

# **The effect of anti-poverty and in-work tax credits for families on self-rated health in parents in New Zealand**

A cohort study of 6,900 participants of the Survey of Family, Income and Employment

Frank Pega

A thesis submitted for the degree of  
Doctor of Philosophy  
at the University of Otago, New Zealand

July 2013



Für meine Eltern Gertrud und Hermann Pega



# Abstract

## Background

Some social protection interventions are promoted as policy tools for addressing the social determinants of health to improve individual and population health and health equity. However, there is considerable controversy over whether publicly funded financial credits have a positive, a negative or no effect on health status in adults in high-income countries. Family Tax Credit (FTC) is a publicly funded financial credit intervention designed to increase income in families living in or at risk of poverty in New Zealand (anti-poverty intervention). In-work Tax Credit (IWTC) is a financial credit intervention that provides additional income to adults in families receiving social assistance (or in low-paid employment) for taking up (or staying in) paid employment (welfare-to-work intervention). This thesis estimates the effect of the FTC and IWTC interventions on self-rated health (SRH) in parents in New Zealand. Rather than analysing cross-sectional data, this thesis uses a cohort study design and fixed effects regression methods to better infer causal effects from observational data.

## Methods

Seven waves of data (Waves 1 to 7, 2002-09) were extracted from the Survey of Family, Income and Employment (N = 29,790) and restricted to a balanced panel of working-age (19 to 64 years) parents in families over two or more consecutive waves (N = 6,900). Linear fixed effects regression analyses were conducted, estimating the association of change in FTC and IWTC with change in SRH at the individual level in the study sample over the short term. These analyses controlled for all time-invariant confounding and adjusted for measured time-varying potential confounding. The exposure variables were eligibility for FTC and IWTC and the amount of FTC and IWTC that the family of an eligible participant was entitled to. The outcome variable was SRH. Potential time-varying confounding variables were the equivalised gross total annual family income (minus FTC or IWTC), family type, number of dependent children in the family and employment status. Subsidiary analyses were conducted to estimate the effect of FTC and IWTC on SRH, where the outcome variable lagged behind the exposure variable by longer periods, and on two other health outcomes (psychological distress and current tobacco smoking), as well as to test for effect modification by ethnicity and level of income.

## **Results**

The best estimate for a change in SRH one year after becoming eligible for FTC was a small, statistically non-significant increase of 0.013 in score over the short term [95% confidence interval (CI) -0.011 to 0.037]. An increase by \$1,000 in FTC amount was also not associated with any discernible change in SRH (effect estimate -0.009, 95% CI -0.057 to 0.039). Likewise, neither becoming IWTC-eligible (effect estimate 0.003, 95% CI -0.020 to 0.027), nor an increase by \$1,000 in IWTC amount (effect estimate 0.000, 95% CI -0.008 to 0.008) were associated with any discernible change in SRH. Subsidiary analyses also found no effect of FTC and IWTC eligibility and amount, when SRH lagged behind the exposure variables by a longer period. No effect of FTC and IWTC was found on the two other health outcomes, psychological distress and current tobacco smoking. Finally, no evidence for effect modification by ethnicity and level of income was found.

## **Conclusions**

This thesis found no discernible effects of FTC or IWTC eligibility and amount on SRH at the individual level, over the short term. No previous studies have investigated the effect of anti-poverty tax credits on parental health. Five studies of welfare-to-work tax credits in the United States found no evidence for an effect of these credits on health status, except for inconclusive evidence on smoking favouring a reduction. This previous evidence for the United States was generally consistent with the finding from this thesis for New Zealand. Strengths of this thesis include its relatively good classification and measurement of the exposure variables and strong control of confounding. Limitations included some risk of bias from misclassification and mismeasurement of the exposure, outcome and confounding variables, likely towards a null finding. The internal validity of the study was judged to be strong, but the thesis had risk of bias from misclassification of the exposure. This study cautions health sector policy makers against investing in anti-poverty and welfare-to-work financial credit interventions to improve adult general health status and health equity in New Zealand and comparable high-income countries. Future research should investigate the effect of FTC and IWTC over the longer term, including the effect of a regime of treatment with FTC and IWTC over several consecutive waves.

# Publications from the thesis

## Peer-reviewed articles in academic journals

1. Pega F, Carter K, Blakely T, Lucas P. (2013). In-work tax credits for families and their effect on health status in adults [Review]. *Cochrane Database of Systematic Reviews*, **8**: CD009963. doi: 10.1002/14651858.CD009963.pub2.

*FP led all aspects of this Cochrane Review, including database searching, abstract screening, extracting data, analysing data, interpreting findings and writing the review. CK contributed to abstract screening, extracting data, analysing data, interpreting findings and review writing. TB and PL contributed to the interpretation of findings and review writing.*

2. Pega F, Carter K, Kawachi I, Davis P, Imlach Gunasekara F, Lundberg O, Blakely T. (2013). The impact of in-work tax credit for families on self-rated health in adults: A cohort study of 6900 New Zealanders. *Journal of Epidemiology & Community Health*, **67** (8): 682-688. doi:10.1136/jech-2012-202300

*FP led all aspects of this research report, including conceptualising the paper, analysing data, interpreting findings and writing the article. KC and TB contributed to all aspects of the article. IK, PD, FG and OL contributed to the interpretation of findings and article writing.*

3. Pega F, Carter K, Blakely T, Lucas P. (2012). In-work tax credits for families and their effect on health status in adults [Protocol]. *Cochrane Database of Systematic Reviews*, **7**: CD009963. doi:10.1002/14651858.CD009963

*FP led to all aspects of this Cochrane Protocol, including registering the title with the Cochrane Collaboration, conceptualising the review topic, developing the search strategy and writing the protocol. KC contributed to all aspects of the protocol. TB and PL contributed to conceptualising the review topic and writing the protocol.*

## Invited commentaries in academic journals

4. Pega F, Kawachi I, Rasanathan K, Lundberg O. (in press). Politics, policies and population health: A commentary on Mackenbach, Hu and Looman (2013). *Social Science & Medicine*, **93**: 176-9. doi:10.1016/j.socscimed.2013.06.007

*FP led all aspects of this invited commentary, including developing the typology of political epidemiology, analysing the comparative advantages and disadvantages of political epidemiological approaches, developing the call for studies taking the individual policy approach and writing the first draft of the commentary. IK, KR and OL contributed to the development of the ideas and the writing of the commentary.*

5. Pega F, Blakely T, Carter K, Sjöberg O. (2012). The explanation of a paradox? A commentary on Mackenbach (2012) with perspectives from research on financial credits and risk factors. *Social Science & Medicine*, **75** (4): 770-3. doi:10.1016/j.socscimed.2012.03.052

*FP led all aspects of this invited commentary, including developing the theory that publicly funded financial credits present one mechanism that could explain the persistence of health inequalities in modern welfare states of Northern Europe and writing the first draft of the invited commentary. TB wrote the section on risk factors. TB, KC and OS contributed to the development of the theory and writing of the commentary.*

## **Conference presentations**

6. Pega F, Carter K, Davis P, Blakely T. (2013). *No effect of In-Work Tax Credit for families on self-rated health in adults in New Zealand*. 9th World Congress on Health Economics. Sydney, Australia, 7-10 July 2013.
7. Pega F, Carter K, Imlach Gunasekara F, Blakely T. (2012). *Measurement error in survey data on income from a publicly funded financial credit*. American College of Epidemiology Annual Scientific Meeting. Chicago, IL, September 9-11.

# Acknowledgements

Many thanks to my supervisors for creating an interesting, challenging and inspiring study and research environment, and for their focused, dedicated and excellent supervision of this thesis research. Dr Kristie Carter from the University of Otago joined me on every step of the thesis research, generously giving her time and knowledge, from helping me develop my computer programming and quantitative data analysis skills over working jointly on the Cochrane Review to providing detailed feedback on the thesis. Professor Tony Blakely from the University of Otago brought clarity, structure and rigor to all aspects of the thesis research and inspired and challenged me at every step, whether teaching theory of the social determinants of health, advanced epidemiological principles or improving my thesis-writing skills. Professor Peter Davis from the University of Auckland provided important insights throughout the thesis research, especially on the links between social policy and health. Again, thanks for such excellent supervision.

Many thanks also to Professor Ichiro Kawachi for hosting me at the Harvard School of Public Health and generously contributing to this thesis research, providing such a wealth of knowledge and ideas, and a fresh pair of eyes, especially on the relationship between income and health. Many thanks also to Professor Olle Lundberg for hosting me at Karolinska Institutet / Stockholm University and adding his expertise on the impact of social policy on health to my thesis research at several points over the last three years.

Thanks also to Dr Fiona Imlach Gunasekara from the University of Otago for providing excellent advice on income and health as well as econometric methods; Dr Ken Richardson from the University of Otago on statistical and methodological issues; Professor Don Matheson from Massey University on the social determinants of health and health equity; and Professor Steven Stillman from the University of Otago on econometric methods.

Thanks also to my collaborators Professor Ola Sjöberg from the University of Stockholm; Dr Kumanan Rasanathan from United Nations Children's Fund (UNICEF); and Dr Patricia Lucas from the University of Bristol for joint research work that contributed greatly to this thesis.

I also want to thank John Upfold, Fiona Wharton and Asheel Ramanlal from Statistics New Zealand for their support in the data lab and for the output checking, as well as Susan Hope from the University of Otago and Dr Paul Bain from Harvard Medical School for their

contribution to helping to develop and implement the search strategy of the Cochrane Review.

Throughout this thesis research, I was privileged to study or work with or alongside many great colleagues that I hope to continue to work with. Particular thanks to Dr Inez Adams, Claudia Ghere, Dr Fiona Imlach Gunasekara, Dr Dolly John, Dr Anna Kolshcheva, Silke Kuehl, Dr Sze Yan Liu, Inga O'Brian, Dr Roman Pabayo, Dr Ruth Gil Prieto, Virginia Signal, Matt Soeberg and Dr Stefan Walter.

Thanks also to Amelia and Steve Corin, Dr John Fenaughty, Dr Elizabeth Heeg, Silke Kuehl and Joseph Leonard Orangias for kindly proof-reading this thesis. All remaining errors remain fully the responsibility of the thesis author.

My wonderful family Gertrud and Hermann Pega, Dr Jochen Pega with Jonas and Joshua Pega and Sabine Pega with Cornelius and Linas Bürkle, as well as Alyx Duncan, Roy Castillo, Scott Summerfield and Joseph Leonard Orangias supported me fantastically through this thesis research. Without you, it would not have been possible.

# Statistics New Zealand Security Statement

Access to the data used in this study was provided by Statistics New Zealand in a secure environment designed to give effect to the confidentiality provisions of the Statistics Act, 1975. The results in this study and any errors contained therein are the work and responsibility of the author, not Statistics New Zealand. Statistics New Zealand are not accountable for any error or inaccurate findings within this work.

## Funding

This thesis research was conducted as part of the SoFIE-Health project within the Health Inequalities Research Programme of the University of Otago, funded by the Health Research Council of New Zealand. The thesis study was principally supported by the University of Otago through a University of Otago Doctoral Scholarship.

A visiting research fellowship at the Centre for Health Equity Studies of the Karolinska Institutet / University of Stockholm in Sweden in September 2011 was funded by the University of Otago, Wellington through a Postgraduate Research Scholarship. A visiting research fellowship to the Department of Social and Behavioral Sciences (formerly Department of Society, Human Development and Health) at the Harvard School of Public Health in the United States between July 2012 and April 2013 was supported by the Harvard School of Public Health through a Visiting Scientist fellowship; the University of Otago through an Elman Poole Travelling Scholarship; and Fulbright New Zealand through a Fulbright-Ministry of Science and Innovation Graduate Award.

Presentations at the 2012 Annual Scientific Meeting of the American College of Epidemiology in Chicago in the United States and at the 9<sup>th</sup> World Congress of Health Economics in Sydney in Australia were supported by the University of Otago, Wellington through Postgraduate Research Scholarships and the University of Otago through a Division of Health Sciences Conference Travelling Scholarship.

## Use of text from publications from the thesis in this document

The five publications in academic journals from the thesis listed above are appended to this thesis. Some of these publications have already published text from this thesis. The below table details which text from this thesis has previously been published. The beginning and end of such text from the thesis that has previously been published is marked by grey boxes in the thesis. No further formal means of citation or quotation was used to ensure flow and readability of the thesis. Some text that has previously been published has been removed, added to or altered in the thesis to ensure the focus, consistency and flow of the thesis is maintained.

<b>Publication</b>	<b>Pages</b>	<b>Thesis chapter</b>	<b>Thesis section</b>	<b>Pages</b>
Pega, Carter, Blakely & Lucas (2012)	pp. 5-7	Chapter 2: Background	Review of causal pathways	pp. 36-43
Pega, Carter, Blakely & Lucas (2013)	pp. 5-7	Chapter 2: Background	Review of causal pathways	pp. 36-43
	pp. 1-56	Chapter 4: Systematic review		pp. 74-108
Pega, Kawachi, Rasanathan & Lundberg (2013)	pp. 1-2	Chapter 3: Methodology	A typology of Political Epidemiology	pp. 47-51
	pp. 2-3		Comparative advantages and disadvantages of the three approaches	pp. 51-54
	pp. 3-4		A call for studies adopting the individual policy approach	pp. 54-55

# Table of Contents

Abstract.....	i
Publications from the thesis .....	iii
Acknowledgements .....	v
Statistics New Zealand Security Statement .....	vii
Funding .....	vii
Use of text from publications from the thesis in this document.....	viii
Table of figures .....	xiv
Table of tables .....	xvi
List of abbreviations .....	xxii
Glossary of terms.....	xxiii
Chapter 1: Social policy interventions as social determinants of health.....	1
The social determinants of health and health equity.....	4
Social policy as a social determinant of health.....	6
Action on the social determinants of health to improve health equity .....	10
Status of evidence on effects of publicly funded financial credits on health.....	12
Research questions.....	16
Structure of the thesis .....	18
Chapter 2: Family Tax Credit and In-Work Tax Credit in New Zealand.....	21
Social policy, protection and assistance .....	22
Anti-poverty and in-work tax credits for families.....	23
Family Tax Credit and In-Work Tax Credit in New Zealand .....	25
The prehistory of the Family Tax Credit and In-Work Tax Credit .....	25
The expansion of Family Tax Credit and introduction of In-Work Tax Credit .....	27
The design of Family Tax Credit and In-Work Tax Credit.....	29
Changes in the eligibility and abatement rates for Family Tax Credit and In-Work Tax Credit over time .....	33
Changes in government spending on Family Tax Credit and In-Work Tax Credit over time.....	34
The causal relationship between Family Tax Credit or In-Work Tax Credit and health .....	35
Conceptual frameworks.....	35
Review of causal pathways.....	36
Conclusions .....	45
Chapter 3: The emergence of political epidemiology.....	46

Political epidemiology.....	47
A typology of political epidemiology .....	48
Comparative advantages and disadvantages of the different approaches.....	51
A call for studies adopting the individual policy approach.....	54
Study designs and methods for the individual policy approach.....	55
Study designs .....	55
Methodological options for studies adopting the individual policy approach.....	59
Rationale for the use of fixed effects regression analyses methods in this thesis.....	71
Conclusions .....	72
Chapter 4: The impact of anti-poverty and in-work tax credits on health status in adults .....	73
Methods.....	75
Criteria for considering studies for this review .....	75
Search methods for identification of studies .....	78
Data extraction, analysis and synthesis.....	80
Results.....	84
Description of studies .....	84
Effects of interventions.....	96
Discussion .....	104
Summary of main results.....	104
Overall completeness and applicability of evidence .....	105
Quality of the evidence.....	106
Potential biases in the review process .....	107
Agreements and disagreements with other studies or reviews.....	107
Conclusions .....	107
Chapter 5: Methods.....	110
Survey of Family, Income and Employment .....	111
Background.....	111
Sampling methods .....	112
Data collection .....	113
Study sample .....	114
Analytical frameworks .....	117
Variables .....	122
The quality of data used for variable derivation .....	122
Exposure variables.....	126

Outcome variable .....	133
Potential confounding variables .....	136
Mediating variables .....	138
Effect-modifying variables .....	138
Analytical methods used.....	139
Basic cross-classified tabular analyses.....	139
Transition matrixes .....	139
Calculation of associations in categorical analyses .....	140
Fixed effects regression analyses .....	141
Conclusions .....	148
Chapter 6: Cross-sectional analyses at baseline.....	149
Time-invariant variables .....	150
Gender and age .....	150
Ethnicity .....	151
Highest qualification.....	151
Exposure variables .....	151
Family Tax Credit eligibility and amount .....	151
In-Work Tax Credit eligibility and amount.....	157
Outcome variable .....	160
Exposure variables by the outcome variable.....	163
Family Tax Credit eligibility and amount by self-rated health.....	163
In-Work Tax Credit eligibility and amount by self-rated health .....	165
Time-varying variables at baseline .....	165
Gross total annual family income.....	165
Equivalised gross total annual family income (minus Family Tax Credit or In-Work Tax Credit) .....	167
Family type .....	167
Number of dependent children in the family.....	172
Employment status.....	172
Loss to follow up.....	177
Conclusions .....	178
Chapter 7: Descriptive analyses of time-varying variables over seven waves .....	179
Change in exposures.....	182
Family Tax Credit eligibility .....	182

Family Tax Credit amount.....	186
In-Work Tax Credit eligibility .....	187
In-Work Tax Credit amount .....	190
Change in outcome.....	192
Change in outcome by change in exposures .....	195
Family Tax Credit .....	195
In-Work Tax Credit.....	195
Change in time-varying confounder variables.....	198
Equivalised gross total annual family income (minus Family Tax Credit or In-Work Tax Credit) .....	198
Family type .....	201
Number of children.....	206
Employment status.....	210
Conclusions.....	216
Chapter 8: Fixed effects regression analyses.....	217
The effect of Family Tax Credit on self-rated health .....	219
Family Tax Credit eligibility .....	219
Family Tax Credit amount.....	222
The effect of In-Work Tax Credit on self-rated health .....	224
In-Work Tax Credit eligibility .....	224
In-Work Tax Credit amount .....	225
Subsidiary analyses .....	226
Longer-term effects.....	227
Associations with other health outcomes .....	228
Effect modification .....	229
Conclusions.....	232
Chapter 9: Internal validity and precision .....	234
Internal validity.....	236
Selection bias .....	236
Misclassification bias and mismeasurement error .....	246
Mismeasurement bias of the outcome .....	260
Misclassification and mismeasurement bias of time-varying covariates .....	260
Misspecification bias in the outcome variable .....	261
Reverse causation.....	261

Overall assessment of internal validity.....	263
Precision .....	263
Conclusions .....	264
Chapter 10: Discussion .....	265
Summary of findings .....	267
Relationship to previous empirical research .....	267
Effect of anti-poverty credits on health status in adults .....	267
Effect of in-work tax credits on health in adults.....	268
Relationship to theories of the effect of income on health .....	270
Income and employment as social determinants of health .....	270
The theory of a minimum income for healthy living .....	272
Effect of publicly funded financial credits on health equity.....	273
Study strengths and limitations.....	274
Study strengths.....	274
Study limitations .....	276
External validity .....	279
Implications for policy .....	281
Future research.....	283
Conclusions .....	285
References .....	286
Appendix 1: Tables of total SoFIE sample at baseline .....	299

# Table of figures

Figure 1: Commission on Social Determinants of Health 2008 framework for social determinants of health and health equity.....	5
Figure 2: Hurrelman 2010 conceptual framework of structural and political factors influencing the health status of the population.....	8
Figure 3: Muntaner 2010 theoretical framework of employment relations and health inequalities .....	9
Figure 4: Navarro 2006 model of relations between politics, labour market and welfare state policies, economic inequality and health indicators .....	10
Figure 5: Timeline of financial credits for families, 1980-2010, New Zealand .....	26
Figure 6: Number of recipient families, Family Tax Credit, In-Work Tax Credit and other Working For Families tax credits, 2002-08 .....	29
Figure 7: Total annual Family Tax Credit amount, by total annual family income and by number of dependent children in the family, 2008 tax year (1 April 2008 to 31 March 2009).....	30
Figure 8: Total annual In-Work Tax Credit amount, by total annual family income and by number of dependent children in the family, 2008 tax year (1 April 2008 to 31 March 2009) .	31
Figure 9: Total annual Family Tax Credit amount that an eligible family was entitled to, by number of children and by income, 2002-08 .....	34
Figure 10: Annual government expenditure, Working For Families tax credits (and predecessors), 2002-08 .....	35
Figure 11: Conceptual framework of the relationship between Family Tax Credit and self-rated health.....	36
Figure 12: Conceptual framework of the relationship between In-Work Tax Credit and self-rated health .....	36
Figure 13: Lundberg 2010 model for pathways and mechanisms linking income and health ...	39
Figure 14: Welfare state regime types .....	49
Figure 15: Imlach Gunasekara’s directed acyclic graph of potential violations (dashed arrows) of assumptions in fixed effects models of an exposure variable (X) on an outcome variable (y) at two time points (1, 2) .....	62
Figure 16: List of searched academic databases, grey literature databases and organisational webpages.....	79
Figure 17: Flow diagram of the systematic search of databases and other sources.....	85
Figure 18: Flow-chart of restriction from survey to study sample .....	115

Figure 19: Directed acyclic graph for the fixed effects model assessing the association between change in Family Tax Credit eligibility and change in health.....	118
Figure 20: Directed acyclic graph for the fixed effects model assessing the association between change in In-Work Tax Credit eligibility and change in health .....	119
Figure 21: Directed acyclic graph showing that adjusting for the variables determining Family Tax Credit eligibility captures the potential effects of other relevant time-varying variables.	120
Figure 22: First page of an entitlement chart with the Family Tax Credit amount by income and number of dependent children for the 2005 tax year (1 April 2005 to 31 March 2006) .....	128
Figure 23: Entitlement chart for the In-Work Tax Credit amount by income and number of dependent children for the 2006 tax year (1 April 2006 to 31 March 2007) .....	132
Figure 24: Directed acyclic graphs of selection bias.....	240
Figure 25: Hernan 2004 classification of selection bias in longitudinal studies .....	241
Figure 26: Hernan 2009 structural classification of measurement error .....	248

# Table of tables

Table 1: Timeline of change in Family Tax Credit, In-Work Tax Credit and other Working For Families tax credits, 2002-08 .....	28
Table 2: Examples of studies taking the three different political epidemiological approaches.	50
Table 3: Comparative advantages and disadvantages of the three political epidemiological approaches .....	52
Table 4: Summary of findings table, in-work tax credit for families interventions, primary outcomes .....	97
Table 5: Summary of findings table, in-work tax credit for families interventions, secondary outcomes .....	102
Table 6: Missing values in gross total annual personal income, total gross annual personal income from employee earnings and total gross annual personal income from government transfers at each cross-section, N=6,900, Waves 1 to 7.....	125
Table 7: Missing values in exposure, outcome, potential time-varying confounding and potential effect-modifying variables at each cross-section, N=6,900, Waves 1 to 7 .....	144
Table 8: Gender by age, N=6,900, Wave 1 .....	150
Table 9: Ethnicity by gender and age, N=6,900, Wave 1 .....	152
Table 10: Highest qualification by gender, age and ethnicity N=6,900, Wave 1.....	153
Table 11: Family Tax Credit eligibility and amount by time-invariant and time-varying variables, N=6,900, Wave 1 .....	154
Table 12: In-Work Tax Credit eligibility and amount by time-invariant and time-varying variables, N=6,900, Wave 4 .....	158
Table 13: Self-rated health by time-invariant and time-varying variables, N=6,900, Wave 1 .	161
Table 14: Family Tax Credit eligibility and amount by self-rated health, N=6,900, Wave 1 ....	164
Table 15: In-Work Tax Credit eligibility and amount by self-rated health, N=6,900, Wave 4..	164
Table 16: Gross total annual family income by time-invariant variables, N=6,900, Wave 1 ...	166
Table 17: Equivalised gross total annual family income (minus Family Tax Credit) by time-invariant and time-varying variables, N=6,900, Wave 1.....	168
Table 18: Equivalised gross total annual family income (minus In-Work Tax Credit) by time-invariant and time-varying variables, N=6,900, Wave 4.....	169
Table 19: Family type by time-invariant and time-varying variables, N=6,900, Wave 1.....	170
Table 20: Number of dependent children by time-invariant and time-varying variables, N=6,900, Wave 1 .....	173

Table 21: Employment status by time-invariant and time-varying variables, N=6,900, Wave 4 .....	175
Table 22: Cross-sectional distribution of Family Tax Credit eligibility, N=6,900, Waves 1 to 7	183
Table 23: Change in Family Tax Credit eligibility between wave <sub>t</sub> and wave <sub>t+1</sub> , N=6,900, Waves 1 to 7.....	185
Table 24: Transition matrix for Family Tax Credit eligibility, N=6,900, Waves 1 to 7.....	185
Table 25: Cross-sectional mean, median and standard deviation of Family Tax Credit amount, N=6,900, Waves 1 to 7.....	186
Table 26: Change in Family Tax Credit amount (by ≥ 1 quintile) between wave <sub>t</sub> and wave <sub>t+1</sub> , N=6,900, Waves 1 to 7.....	188
Table 27: Transition matrix for Family Tax Credit amount, N=6,900, Waves 1 to 7.....	188
Table 28: Cross-sectional distribution of In-Work Tax Credit eligibility, N=6,900, Waves 1 to 7 .....	189
Table 29: Change in In-Work Tax Credit eligibility between wave <sub>t</sub> and wave <sub>t+1</sub> , N=6,900, Waves 1 to 7 .....	189
Table 30: Transition matrix for In-Work Tax Credit eligibility, N=6,900, Waves 1 to 7 .....	190
Table 31: Cross-sectional mean, median and standard deviation of In-Work Tax Credit amount, N=6,900, Waves 1 to 7.....	190
Table 32: Change in In-Work Tax Credit amount (by ≥ 1 quintile) between wave <sub>t</sub> and wave <sub>t+1</sub> , N=6,900, Waves 1 to 7.....	191
Table 33: Transition matrix for In-Work Tax Credit amount, N=6,900, Waves 1 to 7 .....	193
Table 34: Cross-sectional distribution of self-rated health, N=6,900, Waves 1 to 7.....	193
Table 35: Change in self-rated health (by ≥ 1 score) between wave <sub>t</sub> and wave <sub>t+1</sub> , N=6,900, Waves 1 to 7 .....	194
Table 36: Transition matrix for self-rated health, N=6,900, Waves 1 to 7 .....	194
Table 37: Change in self-rated health (by ≥ 1 score) by change in Family Tax Credit eligibility and amount, N=6,900, Waves 1 to 7 .....	196
Table 38: Change in self-rated health (by ≥ 1 score) by change in In-Work Tax Credit eligibility and amount, N=6,900, Waves 1 to 7 .....	197
Table 39: Cross-sectional mean, median and standard deviation of equivalised total annual family income (minus Family Tax Credit), N=6,900, Waves 1 to 7 .....	199
Table 40: Change in equivalised total annual family income (minus Family Tax Credit) (by ≥ 1 quintile) between wave <sub>t</sub> and wave <sub>t+1</sub> , N=6,900, Waves 1 to 7.....	200

Table 41: Transition matrix for equivalised total annual family income (minus Family Tax Credit), N=6,900, Waves 1 to 7.....	200
Table 42: Change in equivalised total gross annual income (minus Family Tax Credit) ( $\geq 1$ quintile) by change in exposure and outcome variables, N=6,900, Waves 1 to 7 .....	202
Table 43: Cross-sectional mean, median and standard deviation of equivalised total annual family income (minus In-Work Tax Credit), N=6,900, Waves 1 to 7.....	203
Table 44: Change in equivalised total annual family income (minus In-Work Tax Credit) (by $\geq 1$ quintile) between wave <sub>t</sub> and wave <sub>t+1</sub> , N=6,900, Waves 1 to 7 .....	203
Table 45: Transition matrix for equivalised total annual income (minus In-Work Tax Credit), N=6,900, Waves 1 to 7.....	204
Table 46: Change in equivalised total gross annual family income (minus In-Work Tax Credit) ( $\geq 1$ quintile) by change in exposure and outcome variables, N=6,900, Waves 1 to 7 .....	204
Table 47: Cross-sectional distribution of family type, N=6,900, Waves 1 to 7.....	205
Table 48: Change in being in a family between wave <sub>t</sub> and wave <sub>t+1</sub> , N=6,900, Waves 1 to 7 ...	207
Table 49: Change in family type (one-parent family, two-parent family) between wave <sub>t</sub> and wave <sub>t+1</sub> , N=6,900, Waves 1 to 7 .....	207
Table 50: Transition matrix for family type, N=6,900, Waves 1 to 7 .....	208
Table 51: Change in family type by change in exposure and outcome variables, N=6,900, Waves 1 to 7 .....	208
Table 52: Cross-sectional distribution of number of dependent children, N=6,900, Waves 1 to 7 .....	211
Table 53: Change in number of dependent children between wave <sub>t</sub> and wave <sub>t+1</sub> , N=6,900, Waves 1 to 7 .....	211
Table 54: Transition matrix for number of dependent children, N=6,900, Waves 1 to 7 .....	212
Table 55: Change in number of dependent children by change in exposure and outcome variables, N=6,900, Waves 1 to 7 .....	212
Table 56: Cross-sectional distribution of employment status, N=6,900, Waves 1 to 7 .....	213
Table 57: Change in employment status between wave <sub>t</sub> and wave <sub>t+1</sub> , N=6,900, Waves 1 to 7 .....	214
Table 58: Transition matrix for employment status, N=6,900, Waves 1 to 7.....	214
Table 59: Change in employment status by change in exposure and outcome variables, N=6,900, Waves 1 to 7.....	215

Table 60: Crude (Model 1) and fully adjusted (Model 2) linear fixed effects model with Family Tax Credit eligibility, potential time-varying confounding variables, outcome self-rated health, N=6,900 (39,580 observations), seven waves (Waves 1 to 7).....	220
Table 61: Stepwise adjustment for potential time-varying confounding variables in a linear fixed effects models with Family Tax Credit eligibility, outcome self-rated health, N=6,900 (39,580 observations), seven waves (Waves 1 to 7) .....	222
Table 62: Crude (Model 1) and fully adjusted (Model 2) linear fixed effects model with Family Tax Credit amount, time-varying co-variates, outcome self-rated health, N=6,900 (39,580 observations), seven waves (Waves 1 to 7).....	223
Table 63: Crude (Model 1) and fully adjusted (Model 2) fixed effects model with eligibility for In-Work Tax Credit, time-varying covariates, outcome self-rated health, N=6,900 (38,260 observations, and 38,250 observations respectively), seven waves (Waves 1 to 7) .....	224
Table 64:Crude (Model 1) and fully adjusted (Model 2) fixed effects model with In-Work Tax Credit amount, time-varying covariates, outcome self-rated health, N=6,900 (38,260 observations, and 38,250 observations respectively), seven waves (Waves 1 to 7) .....	226
Table 65: Type of other issue, hypothesis and subsidiary analyses .....	226
Table 66: Fully adjusted linear fixed effects models with Family Tax Credit and In-Work Tax Credit eligibility and amount, outcome self-rated health, N=6,900, seven waves (Waves 1 to 7) .....	228
Table 67: Fully adjusted linear fixed effects analyses with Family Tax Credit and In-Work Tax Credit eligibility and amount, psychological distress as outcome, time-varying co-variates, N=6,900, three waves (Wave 3, 5 and 7).....	229
Table 68: Fully adjusted logistic fixed effects estimates (odds ratio) with Family Tax Credit and In-Work Tax Credit eligibility and amount, current tobacco smoking as outcome, time-varying co-variates, N=6,900, three waves (Wave 3, 5 and 7).....	229
Table 69: Issue, research question, hypothesis and analysis .....	229
Table 70: Fully adjusted linear fixed effects models with Family Tax Credit and In-Work Tax Credit eligibility and amount, outcome self-rated health, Māori subgroup (N = 885) and non-Māori subgroup (N = 6,015), N=6,900, seven waves (Waves 1 to 7) .....	231
Table 71: Fully adjusted linear fixed effects models with Family Tax Credit and In-Work Tax Credit eligibility and amount, outcome self-rated health, stratified by low versus middle/high income group, N = 6,900, seven waves (Waves 1 to 7).....	232
Table 72: Type of bias, hypothesis and sensitivity analysis .....	237

Table 73: Fully adjusted fixed effects regression model, with Family Tax Credit and In-Work Tax Credit eligibility and amount, outcome self-rated health, balanced panel (N = 6,900) and unbalanced panel (N = 9,360), seven waves (Waves 1 to 7) .....	244
Table 74: Fully adjusted fixed effects regression model, with Family Tax Credit and In-Work Tax Credit eligibility and amount, outcome self-rated health, weighted longitudinally for gender and age, N = 6,900, seven waves (Waves 1 to 7) .....	244
Table 75: Types of misclassification bias in income measures .....	251
Table 76: Fully adjusted fixed effects regression model, with Family Tax Credit and In-Work Tax Credit eligibility and amount, outcome self-rated health, extreme income values removed, N = 6,900, seven waves (Waves 1 to 7) .....	258
Table 77: Fully adjusted fixed effects regression model, with Family Tax Credit and In-Work Tax Credit eligibility and amount, outcome self-rated health, extreme changes in income removed, N = 6,900, seven waves (Waves 1 to 7) .....	258
Table 78: Fully adjusted hybrid (fixed effects) proportional odds model with Family Tax Credit eligibility, outcome SRH, N = 6,900, seven waves (Waves 1 to 7) .....	261
Table 79: Effect estimate (means-centred parameter), fully adjusted hybrid (fixed effects) proportional odds model, with Family Tax Credit and In-Work Tax Credit eligibility, outcome SRH, N = 6,900, seven waves (Waves 1 to 7) .....	262
Table 80: Fully adjusted linear fixed effects models, with Family Tax Credit and In-Work Tax Credit eligibility and amount, outcome self-rated health, N=6,535 (balanced panel of participants reporting excellent, very good or good SRH at Wave 1), seven waves (Waves 1 to 7) .....	263
Table 81: Gender by age, N=9,360 (unbalanced panel), Wave 1 .....	299
Table 82: Ethnicity by gender and age, N=9,360 (unbalanced panel), Wave 1 .....	299
Table 83: Highest qualification by gender, age and ethnicity N=9,360 (unbalanced panel), Wave 1 .....	300
Table 84: Family Tax Credit eligibility and amount by time-invariant and time-varying variables, N=9,360 (unbalanced panel), Wave 1 .....	300
Table 85: In-Work Tax Credit eligibility and amount by time-invariant and time-varying variables, N=9,360 (unbalanced panel), Wave 4 .....	302
Table 86: Self-rated health by time-invariant and time-varying variables, N=9,360 (unbalanced panel), Wave 1 .....	304
Table 87: Family Tax Credit eligibility and amount by self-rated health, N=9,360 (unbalanced panel), Wave 1 .....	306

Table 88: In-Work Tax Credit eligibility and amount by self-rated health, N=9,360 (unbalanced panel), Wave 4.....	307
Table 89: Gross total annual family income by time-invariant variables, N=9,360 (unbalanced panel), Wave 1.....	307
Table 90: Equivalised total annual family income (minus Family Tax Credit) by time-invariant and time-varying variables, N=9,360 (unbalanced panel), Wave 1.....	308
Table 91: Equivalised total annual family income (minus In-Work Tax Credit) by time-invariant and time-varying variables, N=9,360 (unbalanced panel), Wave 4.....	310
Table 92: Family type by time-invariant and time-varying variables, N=9,360 (unbalanced panel), Wave 1.....	311
Table 93: Number of dependent children by time-invariant and time-varying variables, N=9,360 (unbalanced panel), Wave 1 .....	312
Table 94: Employment status by time-invariant and time-varying variables, N=9,360 (unbalanced panel), Wave 4.....	314

# List of abbreviations

CI – confidence interval

EITC – Earned Income Tax Credit

FTC – Family Tax Credit

IWTC – In-Work Tax Credit

OSM – original sample member

OR – odds ratio

SD – standard deviation

SDH – social determinants of health

SoFIE – Survey of Family, Income and Employment

SRH – self-rated health

# Glossary of terms

**Anti-poverty tax credit** – a tax credit principally designed to reduce income poverty

**Attrition** – a “reduction in the number of participants in a study as it progresses” (p. 12) [1]

**Attrition bias** – “a type of selection bias due to systematic differences between the study groups in the quantitative and qualitative characteristics of the processes of loss of their members during study conduct” (p. 12) [1]

**Balanced panel** – a sample of participants in a panel study who contribute data to each wave of the entire study period

**Conditional financial credit** – a financial credit, for which eligibility or entitlement are conditional upon the recipient unit engaging in certain actions, such as taking up social or health services or paid employment

**Confounding variable** – a variable that is related to both the exposure and the outcome and is not a mediator of the exposure-outcome relationship

**Difference-in-differences analysis** – a method for analysing policy (natural) experiments where the mean health outcome after the policy change is subtracted from the mean health outcome before the policy change in both the treatment and control group, and then the differenced effect in the control group is subtracted from the differenced effect in the outcome group

**Econometrics** – “a discipline that develops mathematical and statistical methods, applies them to the estimation of economic models, and conducts quantitative analysis of the behaviour of economic data” (p. 88) [2].

**Estimator** – a statistic estimating a parameter from a sample

**Fixed effects regression analysis** – a regression analytic method for assessing the association between change in the exposure variable and change in the outcome variable in repeated measures studies

**Health equity** – the absence of differences in health that are “not only unnecessary and avoidable but, in addition, are considered unfair and unjust” (p. 5) [3]

**Income** – “the amount an individual can spend in a period while leaving his or her capital unchanged [...] [such as] receipts from wages, or earned income, plus receipts from transfers, such as pensions” (p. 157) [2]

**In-work tax credit** – a publicly funded tax credit conditional on the recipient unit taking up or retaining paid employment (see Welfare-to-work policy)

**Epidemiology** – “the study of the occurrence and distribution of health-related states or events in defined populations, including the study of the determinants influencing such states, and the application of this knowledge to control the health problems” (p. 81) [1]

**Minimum income for healthy living theory** – the income threshold theory coined by Morris *et al.* that healthy living requires a specific minimum income [4-8]

**Natural experiment** – the “naturally occurring circumstances in which subsets of the population have different levels of exposure to a supposed causal factor in a situation resembling an actual experiment, where human subjects would be randomly allocated to groups” (p. 164) [1]

**Political Epidemiology** – “the sub-discipline of Epidemiology that studies the effects of the political context on (the distribution of) health and well-being” (p. 1) [9]

**Politics** – the political traditions and ideology, processes, systems and institutions

**Publicly funded financial credit** – a financial credit provided by a government

**Social assistance** – the publicly funded financial credits or in-kind resources provided to socio-economically disadvantaged recipient units to ensure an adequate standard of living (also called social benefit or social welfare)

**Social benefit** – see Social assistance

**Social Epidemiology** – “A branch or sub-speciality of epidemiology that studies the role of social structure and social factors in the production of health and disease in the population” (p. 231) [1]

**Social Policy** – “public policy and practice in the areas of health care, human services, criminal justice, inequality, education and labor” [10]

**Social protection** – “protecting individuals and households during periods when they cannot engage in gainful employment or obtain enough income to secure their livelihoods – due to unemployment, sickness, chronic ill health or disability, old age or care responsibilities” (p. 16) [11]

**Social welfare** – see Social assistance

**Tax credit** – a sum deducted from the total amount a tax payer owes the state [2]

**Triple differences analyses** – a type of difference-in-differences method, where a control variable is used to control for differences in underlying time trends between the treatment and control groups that could lead to confounding in difference-in-differences analyses

**Welfare regime** – a welfare state's type of regime, classified by Esping-Andersen [12] on the basis of the country's degree of decommodification, social stratification and private-public mix of welfare provision

**Welfare security** – a health-enhancing feeling of security that publicly funded financial credits may have in non-recipients of these credits

**Welfare-to-work policy** – a policy principally designed to reduce income poverty and move recipients of social benefits into (or retain them in) paid employment



“Alle Krankheiten haben zwei Ursachen; eine pathologische, die andere politisch.”

[All diseases have two causes; one pathological, the other political.]

„Die Politik ist nichts weiter als Medizin im Großen.“

[Politics is nothing but medicine at a large scale.]

Rudolph Carl Virchow (1821-1902)



# Chapter 1: Social policy interventions as social determinants of health

This chapter introduces this thesis on the effect of two social policy interventions, called Family Tax Credit (FTC) and In-Work Tax Credit (IWTC), on individual health in parents in New Zealand over the 2002-09 period. By improving two social determinants of health (SDH), income and employment, these social policy interventions may improve health and health equity. Thus, the thesis falls within the broad domain of SDH and health equity research.

A policy agenda focused on addressing the SDH to improve health and health equity has gained momentum, both internationally and nationally. Evidence on the effect on health of social policy interventions that may address the SDH is crucial for identifying tools for advancing health and health equity. The discipline of political epidemiology has emerged to help generate such evidence.

Political epidemiological research requires further definitional, conceptual, theoretical and methodological development, such as advancement of conceptual frameworks. Empirical studies quantifying the effect on health of pivotal types of social policy intervention such as publicly funded financial credits are needed, especially for high-income countries. They form a crucial evidence base for decisions in favour of or against health sector involvement and investment in social policy interventions.

This thesis investigated two principal research questions:

1. *What was the association of change in FTC eligibility and amount with change in self-rated health (SRH) in adults at the individual level over the short term?*
2. *What was the association of change in IWTC eligibility and amount with change in SRH in adults at the individual level over the short term?*

Subsidiary research questions investigated effect modification of the health effects of FTC and IWTC on SRH by ethnicity and poverty, as well as the effect of FTC and IWTC on SRH over the longer term and their effect on two additional health outcomes, psychological distress and current tobacco use.

## Chapter 1: Introduction

This chapter introduces the key concepts of SDH and health equity, and locates political factors, including social policy interventions, as structural SDH that form the political context of health equity. The chapter critically reviews key existing conceptual frameworks of the causal relationship between social policy (interventions) and health, evaluating the potential of these frameworks to be used to conceptually guide this thesis. It then provides the rationale for a policy agenda focused on addressing the SDH, including social policy interventions, to improve health equity and describes progress along this policy agenda to date, emphasising the potential relevance of this thesis for public health policy and practice. The chapter assesses the status of evidence on the effects of social policy interventions on health, concluding that this research domain remains under-researched, despite its considerable potential for informing SDH- and health equity-focused policy and action. Finally, the chapter introduces the specific research questions of this thesis and outlines the further structure of the thesis.

The opening quotes attributed to the influential German pathologist Rudolph Carl Virchow (1821-1902) demonstrate that an understanding of the interconnectedness of politics<sup>1</sup> and public health has existed for over a century. However, we still know little about which political factors matter for health and through which causal pathways they influence health. Debates between scientists in this area of scholarship are as heated as debates between the prime minister and leader of the opposition. International interest in deepening our knowledge of whether and how political factors, and especially social policy interventions, influence health and well-being has gained considerable momentum with the global movement towards addressing the SDH to improve health equity.

Governments in many countries have used publicly funded financial credit interventions that provide additional income to low-income individuals and families such as the FTC studied in this thesis as policy tools to reduce income poverty and improve income equity. They often occur within the context of high and increasing levels of income poverty [13, 14] and income inequality [15, 16], which have the potential to result in serious civic unrest and conflict, thereby posing serious threats to the social cohesion of our societies. Publicly funded financial credit interventions are one social policy response to this central challenge facing governments and civil societies. Moreover, governments have designed publicly funded

---

<sup>1</sup> Eikemo and Bambra (2008) define politics for health research in at least four ways: “the art of government and the activities of the state”; “the conduct and management of community affairs”; “the expression and resolution of conflicts through compromise, conciliation, negotiation and other strategies”; and “the process through which desired outcomes are achieved in the production, distribution and use of scarce resources in all the areas of social existence” (p. 573).

financial credits that are conditional on up-take or retention of employment such as the IWTC studied in this thesis to address unemployment and its negative economic and social consequences such as long-term and potentially inter-generational welfare dependence, as well as to reduced government spending on social assistance. Unemployment rates were high in many high-income countries even before the 2008 global economic crisis reduced labour demand [13, 17]. These publicly funded financial credit interventions are attractive to politicians, as demonstrated by the large number of governments that have implemented these interventions in high-income countries [18].

Publicly funded financial credits that improve income and employment, which are key SDH, in socio-economically disadvantaged populations could theoretically improve the health of these populations and, in turn, provide better health equity in the population [19]. The next chapter explores the specific causal pathways through which social policy interventions could impact health and health equity. Because some publicly funded financial credit and other social policy interventions have been assumed to improve health, they have been promoted as policy tools for addressing the SDH to improve health and health equity in a population. The World Health Organisation Commission on Social Determinants of Health in its first overarching recommendation to “improve daily living conditions” called on governments to “create social protection policy supportive of all” and ensure “social protection across the life course” (p. 10) [20]. Some global health experts also recently made the case for increased involvement of the health sector in publicly funded financial credit interventions [20, 21], with some experts explicitly advocating the use of such credits to improve health in high-income countries [22].

Other experts have, however, cautioned policy makers interested in improving health and health equity about the use of publicly funded financial credits, pointing to the lack of evidence that could be used to justify investments in such interventions, at least for high-income countries [23-25]. Westin pointed out that the existing evidence cannot even answer such basic questions as whether social policy interventions should be universal or means-tested in design to have the most positive health effect [26]. This thesis presents conceptual [9], theoretical [27], systematic review [28, 29] and original empirical evidence [30]. The thesis further cautions that some potentially promising publicly funded financial credit interventions may, perhaps surprisingly, *not* influence health and may *not* present effective policy tools for improving health and health equity.

# The social determinants of health and health equity

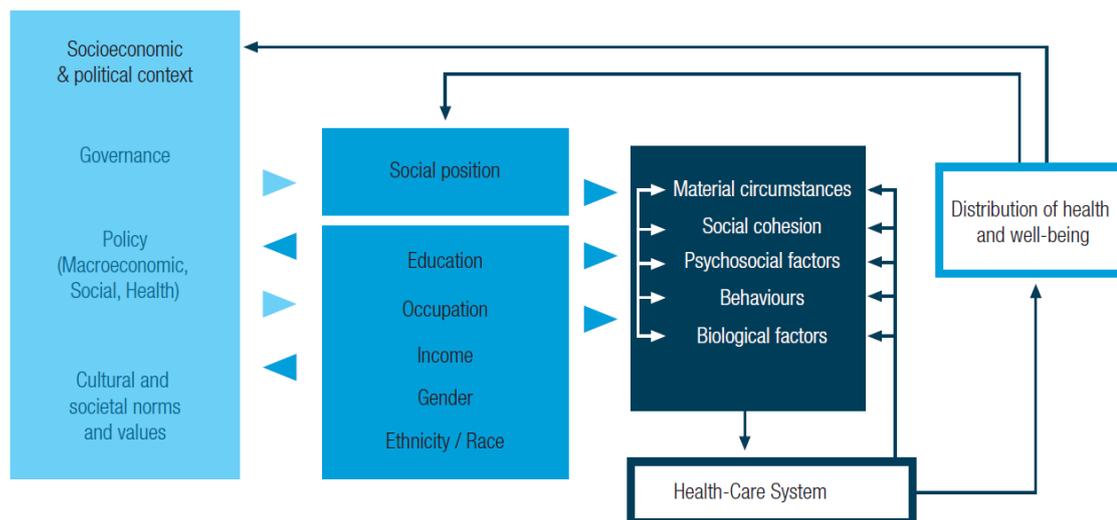
This thesis studies the effect of structural SDH, namely two publicly funded financial credit interventions. The World Health Organization defines SDH as “the circumstances in which people are born, grow up, live, work and age and the systems put in place to deal with illness [...] [, all of which ] are in turn shaped by a wider set of forces: economics, social policies, and politics” [31]. The SDH comprise both “specific features” of such circumstances such as income or employment, but also “pathways by which societal conditions affect health” such as social inclusion or racism (p. 697) [32].

The global Commission on Social Determinants of Health’s conceptual framework for action on the SDH presented in **Figure 1** theorises how the SDH influence the distribution of health and well-being , differentiating three distinct blocks of determinants [20, 33]. The first block (light blue box) are the structural SDH that form the socioeconomic and political context of health equity, comprising governance; macro-economic, social and health policy; as well as cultural and societal norms and values. These structural SDH influence (and are reciprocally influenced by) the intermediary SDH in the second block (bright blue box). These intermediary determinants mark the social position, encompassing conditions such as education, occupation, income, gender and ethnicity. They influence the third block of SDH (dark blue block), the individual-level factors of material circumstances; social cohesion; psychosocial factors; behaviours; and biological factors. The health care system is conceptualised as a separate determinant that also influences this set of individual-level SDH. Both the health care system and individual-level SDH jointly determine the distribution of health and well-being in a population. In turn, the distribution of health and well-being reciprocally affects the socioeconomic and political context as well as an individual’s or population group’s social position. This thesis investigates the impact on health of interventions from the domain of social policy, defined as “public policy and practice in the areas of health care, human services, criminal justice, inequality, education, and labor” [10]. The intermediary and individual-level SDH that social policy interventions may operate through, being such factors as income and employment, are discussed in detail in the next chapter.

Much is known about how the intermediary and individual-level SDH as well as the health care system influence (the distribution of) health and well-being [20, 34]. However, the structural SDH, including social policy interventions, and the causal pathways through which they

influence health are poorly understood and under-researched [20, 35-37]. This thesis aims to advance both theoretical knowledge and empirical evidence on how social policy impacts individual (and, in turn, population) health and well-being. It also contributes theoretical considerations about the effect of social policy on socioeconomic inequalities in health.

**Figure 1: Commission on Social Determinants of Health 2008 framework for social determinants of health and health equity**



Source: Commission on Social Determinants of Health, 2008, p. 51 [20]

This thesis empirically studies and focuses its investigation on the effect of social policy interventions on health at the individual level; it does not directly study the effect of these interventions on population-level health and health equity. However, average effects of a policy intervention on the health of individuals in a population should translate into changes in population-level health. For example, finding no effect of such an intervention on individual health suggests no (large) effects of the intervention on population health. No changes in population health from a policy intervention also suggest no effect on the distribution of health and health equity in the population. Therefore, although this thesis does not empirically study the effect of social policy interventions on population health and health equity, the findings of the effect of such interventions on individual health have important implications for and provide suggestive evidence for intervention effects on population health and health equity.

The unequal distribution of SDH underlies and explains the unequal distribution of health resources and outcomes in the population [20]. For example, one of the social factors that is commonly believed to determine health is income, “the amount an individual can spend in a period while leaving his or her capital unchanged [...] [such as] receipts from wages, or earned

income, plus receipts from transfers, such as pensions” (p. 144) [2]. The role of income is thought to determine health by providing higher-income individuals more opportunities to access health resources than lower-income individuals, thereby generating differential health outcomes. Following this theory, publicly funded financial credits that provide additional income to low-income groups should increase the health status of low-income groups, thereby reducing socio-economic (by-income) inequalities in health. This conceptualisation of the causal effect of publicly funded financial credit interventions on health is common (see e.g. [20, 21]), however this thesis reveals it to potentially be flawed. The next chapter indicates potentially active and powerful causal pathways other than income through which publicly funded financial credit interventions may impact health and its distribution.

According to Whitehead’s seminal definition, health equity refers to the absence of differences in health that are “not only unnecessary and avoidable but, in addition, are considered unfair and unjust” (p. 5) [3]. Another definition of health equity is “the absence of systematic disparities in health (or in the major social determinants of health) between groups with different levels of underlying social advantage / disadvantage - that is, wealth, power, or prestige” (p. 254) [38]. Socially produced and remedial inequalities in health, whether within or between countries, are often unfair and unacceptable, and action on the SDH must be taken to address such health inequalities [20]. This thesis estimates the effects of social policy interventions that could potentially address the SDH and improve health and health equity. It evaluates the effectiveness of these interventions in improving health, providing evidence that can aid decision-making for, or against, investments in these types of social policy interventions from those who are concerned with addressing the SDH to improve health and health equity.

## **Social policy as a social determinant of health**

Several conceptual frameworks have theorised the causal relationship (factors, pathways and mechanisms) between social policy and (the distribution of) health. In *Chapter 3* I argue that approaches to studying the effect of social policy on health can be grouped into three distinct type, namely studies of the effect of: welfare regimes; politics; and specific individual policy interventions [9]. While some of the existing frameworks feature social policy as one of several aspects they conceptualise, other frameworks have been developed with the specific intention to guide cross-country comparative research that assesses the effect of welfare state regimes and politics on population health.

## Chapter 1: Introduction

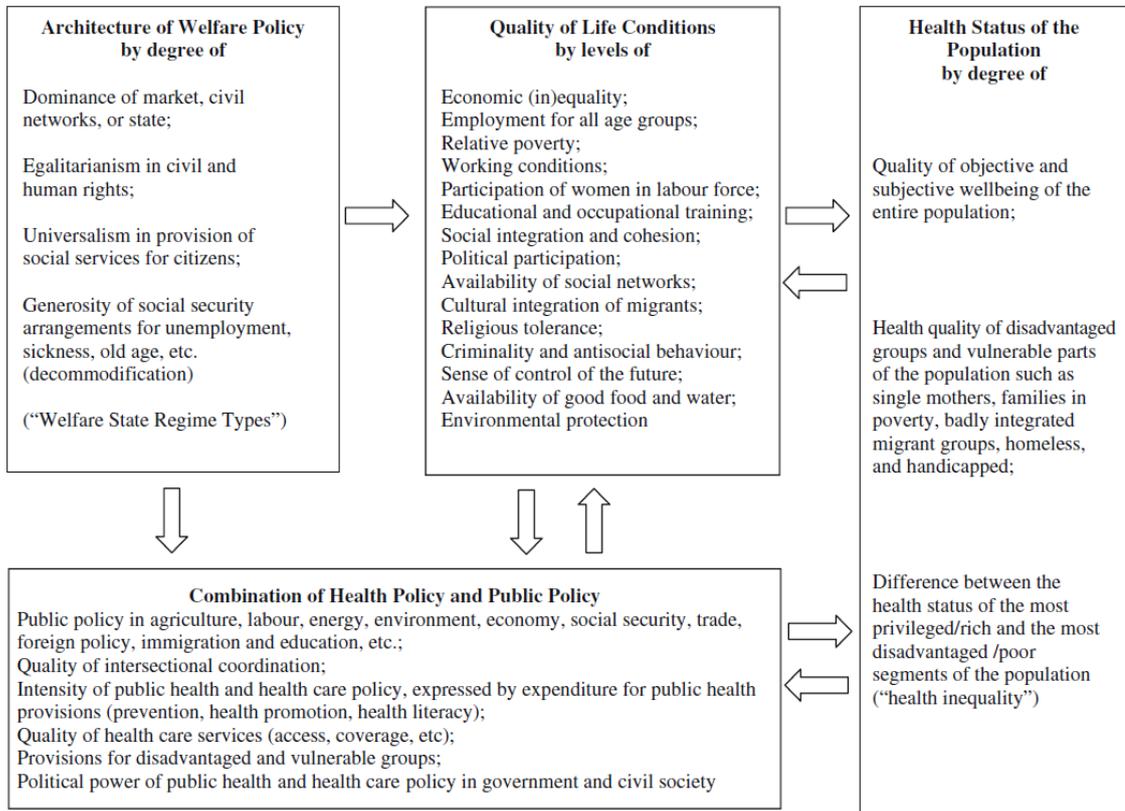
There are four prominent conceptual frameworks of the causal relationship between social policy and health. These are: the Commission on Social Determinants of Health 2008 *Conceptual framework for action on the social determinants of health* [20, 33]; the Hurrelmann 2010 *Model of structural and political factors influencing the health status of adults* [39]; the Muntaner 2010 *Theoretical framework of employment relations and health inequalities* [40]; and the Navarro 2006 *Model of relations between politics, labour market and welfare state policies, economic inequality and health indicators* [35]. I describe, for each framework, its principal function, conceptualisation of the social policy-health relationship and shortcomings in terms of applicability for studying the effect of individual social policy interventions on health.

The Commission on Social Determinants of Health 2008 framework presented in **Figure 1** above was developed as a conceptual framework for action on the SDH and health equity, synthesising previous frameworks [20, 33]. As such, it theorises the causal pathways and mechanisms between SDH and the distribution of health and well-being. Social policy, a structural SDH, is theorised to influence some or all intermediary SDH, which determine individual-level SDH. The individual-level factors, in turn, determine (the distribution of) health, which then feeds back into social policy (and the other structural SDH). The framework understands (social) policy as separate from governance, which is conceptualised to be another structural SDH, and does not explicitly refer to welfare regimes. This framework cannot easily be applied to guide studies of the effect of individual social policy interventions on health, such as this thesis investigating the FTC and IWTC interventions. The framework does not theorise the relationship between social policy and other structural SDH, including other political factors such as governance, as well as health and macroeconomic policy. The framework also does not specify the causal pathways between social policy and health and does not conceptualise causal pathways between social policy and health that are not mediated through intermediary SDH, such as the direct causal pathway that I describe in the next chapter.

The Hurrelmann 2010 framework [39] presented in **Figure 2** theorises the relationship between welfare regimes (or ‘the architecture of welfare policy’) and (the distribution of) health. The welfare regime, which includes the generosity of social protection policies, is seen as influencing a block of determinants of social position (‘quality of life conditions’), which in turn influence (the distribution of) health. The welfare regime is also theorised to influence health and other public policy, including social protection policies. As with the previous

framework, this framework also does not conceptualise the specific causal pathways between different (blocks of) SDH or which specific causal pathways operate between social policies and health.

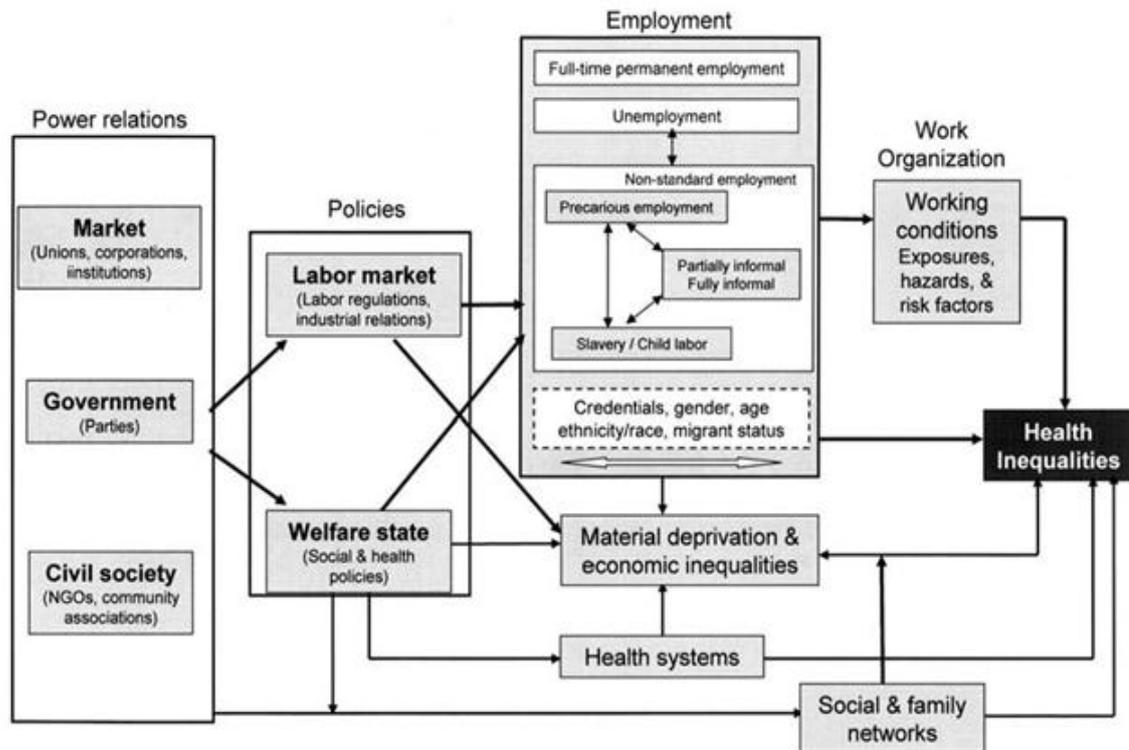
**Figure 2: Hurrelman 2010 conceptual framework of structural and political factors influencing the health status of the population**



Source: Hurrelmann *et al.*, 2010, p. 8 [39]

The Muntaner 2010 framework [40] presented in **Figure 3** principally describes the causal pathways and mechanisms between employment (and its wider determinants) and health inequalities. Power relations, comprising market, government (or politics) and civil society, are seen to influence the welfare regime, which comprises social (and health) policies. Social policies influence employment, material deprivation and economic inequality, the health system and social capital and cohesion ('social and family networks'), as well as, in turn, health inequalities. The framework extends the Commission on Social Determinants of Health framework in that it specifies the employment pathway by differentiating key aspects such as employment status (e.g., full-time, permanent; non-standard; and unemployed), while at the same time retaining the separate causal pathway through material circumstances (i.e., what this thesis terms as the 'income pathway', described in detail in the next chapter).

**Figure 3: Muntaner 2010 theoretical framework of employment relations and health inequalities**



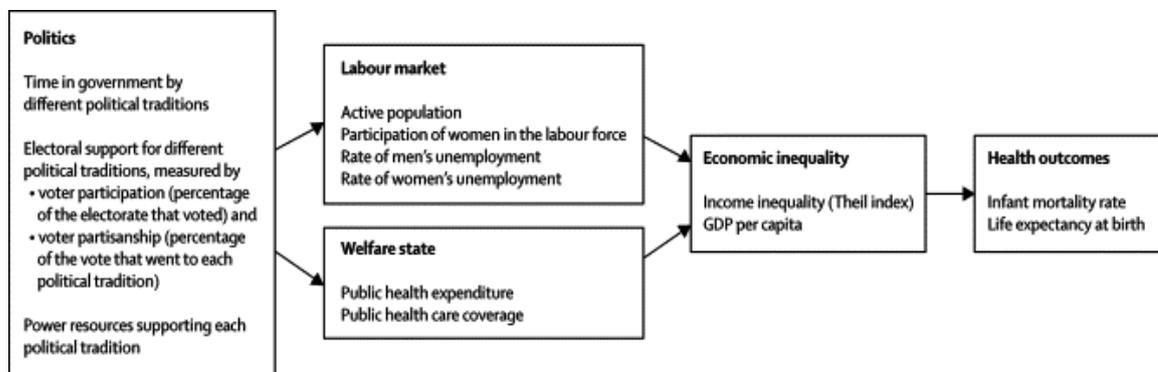
Source: Muntaner *et al.*, 2010, p. 217 [40]

The Navarro 2006 model [35] presented in **Figure 4** conceptualises the causal relationship and pathways between politics and health. Politics are understood as time in government of political traditions, electoral support and power base for political traditions. They influence the labour market and welfare regimes, narrowly conceptualised as public health expenditure and coverage. The labour market and welfare state, in turn, influence the level of economic inequality, which determines health outcomes. Although this model conceptualises the relationship between politics and social policy better than the Commission on Social Determinants of Health 2008 framework, neither of them conceptualise the relationship between specific social policy interventions and individual health. Navarro's model also does not specifically examine the impact of politics and welfare regimes through causal pathways other than through economic inequality, neglecting the impact of important other SDH than income (inequality) such as the employment and direct pathways (as is reviewed in the next chapter).

In summary, while several complex conceptual frameworks of the causal relationship between social policy and health exist, all of these were developed to guide (cross-sectional) research of the relationship between welfare regimes or politics and (population) health. These models,

while informative, cannot easily be applied to studies of the effects of specific individual policy interventions on individual health and to longitudinal (or repeated measures) data. There is a lack of frameworks for the effect of individual social policy interventions on (individual) health. The need for further conceptual research to progress epidemiological research on the relationship between social policy and health has been emphasised [41, 42]. Conceptual frameworks that help us better understand the relationship between individual policy interventions and health are required, and dedicated conceptual frameworks for the social policy interventions studied in this thesis are presented in the next chapter.

**Figure 4: Navarro 2006 model of relations between politics, labour market and welfare state policies, economic inequality and health indicators**



Source: Navarro *et al.*, 2006, p. 1036 [35]

## Action on the social determinants of health to improve health equity

Considerable progress towards coordinated and comprehensive global action on the SDH to improve health equity has been achieved over the last decade. In 2005-08, the global Commission on Social Determinants of Health reviewed evidence, stimulated awareness and reported their findings for addressing the SDH to improve health equity, making three overarching recommendations [43]. In 2009, the Sixty-Second World Health Assembly passed a resolution urging international organisations and World Health Organization member states to adopt the recommendations of the Commission [44]. In 2011, the World Health Organization and its members states drafted and adopted the *Rio Political Declaration on Social Determinants of Health* [45] during the *World Conference on Social Determinants of Health*, emphasising the need to address the SDH to improve health equity in five key action areas. In 2012, the Sixty-Fifth World Health Assembly renewed its call for action on the SDH

and health equity, affirming its commitment to the recommendations of the Commission and endorsing the action areas of the *Rio Political Declaration on Social Determinants of Health* [46].

International organisations have commenced implementing these global calls for action. For example, as part of the implementation of the Commission on Social Determinants of Health's recommendations across the World Health Organization, the European Region Office commissioned the *European review of the social determinants of health and the health divide* of its 53 member states [47]. This international review included a task group on gross domestic product, income and tax, which reviewed evidence on the effect of social policy interventions on health. This thesis contributed a country case study on the effect of the reform of Working For Families tax credits, including the FTC and IWTC, on health in New Zealand to this review [48].

Some national governments have also answered the calls for action on the SDH. The United Kingdom government was amongst the first globally to develop a comprehensive national strategy and policy action on the SDH to improve health equity, commencing in the 1990s [49]. While the success of this strategy and its implementation has been questioned [49], recent data suggest at least some partial improvements to health equity from this concerted government effort [50]. The New Zealand government has also taken considerable steps towards reducing inequalities in health between the late 1990s and late 2000s [51-54]. It launched the *Reducing Inequalities* (later terminology changed to *Reducing the Gaps*) initiative, with several innovative policy tools being developed during this time [51-53]. The health sector was guided by dedicated reducing inequalities in health and Māori<sup>2</sup> health strategies, both of which took a SDH approach [51-53]. This thesis evaluates the health effect of two social protection interventions that were central to the government strategy to reduce social inequalities in New Zealand, namely FTC and IWTC. Thus, the thesis contributes evidence towards an understanding of the effectiveness of using these policy interventions to address the SDH to improve health equity in New Zealand.

Policy makers have been advised to implement social protection interventions to address the SDH to improve individual and population health and health equity. The Commission on Social Determinants of Health's final report chapter on "social protection over the life course" recommended that "governments [...] build universal social protection systems and increase

---

<sup>2</sup> Māori are the Indigenous people of New Zealand.

their generosity towards a level that is sufficient for healthy living" (p. 87) [20]. The United Nations Social Protection Floor Initiative argued that social protection policies are required to achieve the Millennium Development Goals, including Goal 4: Reduce child mortality and Goal 5: Improve maternal health [55]. The World Bank has also argued that "social protection programs [...] are a powerful tool to reduce poverty and vulnerability [...] [and] can have a direct, positive effect on poor families by building human capital through better health, more schooling, and greater skills" [56].

However, specific social protection interventions that effectively (and cost-effectively) improve health (equity) must be identified, so that policy actors (affected communities, policy-makers and health workers) can prioritise, advocate for, design, plan and implement these interventions. In the context of the "policy market", policy actors are increasingly required to justify their priorities, decisions and choices with evidence. For example, policy-makers in the health sector are required to justify investments of health dollars in specific social protection interventions as a means towards improving the SDH and health (equity) [9]. Thus, empirical studies estimating the actual effect on health of those social protection interventions that may improve the SDH and health (equity) have gained considerably in importance [29]. The successful progression of the SDH and health equity-focused policy agenda will depend to some degree on differentiating those social protection interventions that effectively improve health from those that do not.

## **Status of evidence on effects of publicly funded financial credits on health**

Over the last two decades of research activity in the domain of social policy and health, several dedicated research programmes have been carried out. The first major research programme was conducted in the early 2000s by a research team spanning several European countries and headed by Professor Vincente Navarro with funding from the European Commission. It culminated in the publication of the seminal book titled, *The political and social context of health* [57]. In the mid to late 2000s, another important research programme was *The Nordic experience: Welfare states and public health*, conducted by the Centre for Health Equity Studies, led by Professor Olle Lundberg, with funding from the Commission on Social Determinants of Health. This research programme published its findings in a final report [58], as well as a special issue in the *International Journal of Social Welfare* (Volume 19,

## Chapter 1: Introduction

Supplement 1, July 2010). In 2010, the *Fair Society Healthy Lives: Marmot Review* [59], conducted by Professor Sir Michael Marmot with funding from the United Kingdom government, also had a dedicated task force on social protection, headed by Professor Howard Glennerster, that published a final report [60]. In 2012, as mentioned above, the *European review of the social determinants of health and the health divide* [47, 61], headed by Professor Sir Michael Marmot and funded by the World Health Organisation Regional Office for Europe, included a task group on gross domestic product, taxes, income and welfare, headed by Professor Olle Lundberg [61]. These dedicated research programmes mostly studied the effects of welfare regimes or politics (political traditions, processes and institutions) on health. In *Chapter 3* I reflect further on this current evidence base and argue that some of such evidence may not easily be used to stimulate policy and action.

Some research relevant to this thesis has previously been conducted in New Zealand. One study has estimated the effect of income (from all sources) on health in New Zealand, using the same survey data as this thesis, finding a small, positive but statistically insignificant effect of income on SRH [62, 63]. This previous study differs from this thesis in three main ways. First, the time period that the former study investigated was shorter, namely 2002-05, encompassing Waves 1 to 4 of the Survey of Family Income and Employment (SoFIE) (rather than 2002-09, Waves 1 to 7), and did not cover the time during which the Working For Families tax credits reform occurred (2006-08, Waves 5-7), meaning that it could not have assessed the potential effect of the FTC and IWTC on health. Second, it assessed the effect of income from all sources (rather than eligibility for and income from FTC and IWTC specifically), but income from specific sources, especially financial credits from the government, may have a differential effect from other sources of income, such as income from salary earnings or wages. Third, the study assessed the effect of income on any adults (rather than on the subgroup of working-age parents in one-or two-parent families). Therefore, while the previous study provides important insights and background from New Zealand, this thesis tests different hypotheses on a different sample over a different period of time. Another group of studies potentially relevant for this thesis are government reports and evaluations, such as the Ministry of Social Development's program evaluation of the FTC and IWTC conducted in 2010 [64]. However, considering that health effects were not specifically set as an objective of the FTC and IWTC in New Zealand, such previous evaluations have not determined the health impacts of these credits [64].

## Chapter 1: Introduction

The paucity of studies investigating the effect of the upstream and structural SDH of the socioeconomic and political context on (the distribution of) health and well-being has been critically highlighted [20, 34, 65]. The Commission on Social Determinants of Health noted that “data on the association between the magnitude of health inequities within countries and social protection policies remain scarce” (p. 85) [20]. *Chapter 3* outlines that political epidemiology [66, 67], the emerging discipline that studies the causal relationship between the political context and health [9], has focused on studying the effects of welfare state regimes and politics on health. Unfortunately, the chapter indicates that this discipline has contributed little information that would enable us to robustly judge the health effects of individual social protection interventions, the type of policy-relevant evidence that is urgently required. Therefore, despite political epidemiological research gaining considerable momentum, useful evidence and systematic reviews of such evidence [68] on the (cost-) effectiveness of specific social policy interventions in tackling the downstream SDH, individual health, population health and health equity currently remain scarce. To identify intervention points, evidence on the mechanisms by which specific social protection interventions affect health is particularly important [68]. The paucity of such evidence, despite its important role for policy and action, has made such research a priority not only for health equity research [20, 69], but also for epidemiological research in general [67].

The lack of studies investigating the effect on health of publicly funded financial credit interventions, the type of social protection policy intervention studied in this thesis, has also been critically highlighted [23, 24, 68, 70-73], including within the context of research on the SDH and health equity [20, 68, 69]. For low- and middle-income countries, there is a strong body of research on the health effects of conditional financial credits in South America, such as *Oportunidades* in Mexico [74] and *Bolsa Familia* in Brazil [75], synthesised rigorously in a systematic review [76]. However, little evidence currently exists on the effect of conditional publicly funded financial credits on health in regions such as Africa, the Eastern Mediterranean, South-East Asia and the Western Pacific, as well as on unconditional publicly funded financial credits.

For high-income countries, the lack of evidence on publicly funded financial credits is even more pronounced, with researchers highlighting insufficient evidence on unconditional [24, 70] and conditional credits [23, 25], including employment-conditional (or in-work) credits [29, 73]. Researchers have criticised the current lack of high-quality data that would enable robust empirical analyses of the effects of publicly-funded financial credits on health [73]. Several

randomised controlled trials of publicly funded financial credits have failed to collect information on health, which researchers have called “a lost opportunity” (p. 725) [71] for improving the current evidence base [71, 73]. Furthermore, the few existing studies are often of low quality, generally due to facing substantial methodological challenges [24, 29]. In addition, the existing evidence is inconclusive or, at the least, interpreted differently by different experts. Some experts advocate for the use of financial credits intervention in high-income countries to improve health and health equity [22], whereas others argue against applying such credits for this purpose [77], while others caution that the evidence is insufficient to draw robust conclusions [23, 25]. Much of the current evidence for high-income countries is limited geographically to North America [29, 70] and to studies of short-term effects of such credits [9, 30, 78].

Different types of evidence and evaluation of the effects of publicly funded financial credits on health in high-income countries are needed. More research is required to elaborate the definitional, conceptual, theoretical and methodological foundations of such studies [9, 35, 41, 42, 66, 79-81]. Empirical research estimating whether and how much specific financial credit interventions impact individual and population health and health equity are necessary [23, 28, 29, 70]. Subgroup analyses assessing the effects of these credits by relevant variables such as income level, employment status, family type and ethnicity are also required [28, 29], as are studies that provide insights on the effects of specific policy design features, such as whether policies should be universal or means-tested to provide optimal health improvements [26].

As *Chapter 3* indicates, together with experimental and quasi-experimental studies, longitudinal studies designs and methods present particularly promising research avenues for studying the effect of publicly funded financial credits on health. Establishing not only relevant causal effects, but also mechanisms and pathways through which publicly funded financial credits may impact health requires longitudinal studies of how changes in these credits cause changes in health at the individual level over time. Studies investigating the effects of these credits on health at the individual level are required, because they can overcome some of the risks of bias associated with ecological studies, such as the ecological fallacy<sup>3</sup>. The review of the systematic review evidence on structural SDH revealed that reviews of financial credit

---

<sup>3</sup> According to Porta, Greenland and Last (2008), an ecological fallacy is “an erroneous inference that may occur because an association observed between variables on an aggregate level does not necessarily represent or reflect the association that exists at an individual level; a causal relationship that exists on a group level or among groups may not exist among the group individuals” (p. 75)

interventions and their effect on health are scarce and called for more such reviews [68]. In the review authors words, “in terms of the [...] welfare domain, there are still areas in need of further research, particularly in terms of the effects on health of welfare to work policies” (p. 289) [68]. Some government institutions have evaluated the effect of their financial credit interventions on health, but independent evaluations are needed to test these official evaluations.

The potential reasons for the relative lack of existing studies are multiple. The definitional, conceptual, theoretical and methodological foundations of this area of research are still emerging, making research in this field challenging [9, 35, 41, 42, 66, 79-81]. Empirically studying the effects on health of financial credits may require taking an interdisciplinary and intersectoral approach and thus acquiring knowledge from multiple disciplines, including epidemiology, social policy and econometrics. It is difficult to find adequate data, especially suitable individual-level longitudinal data, on both a policy and health, as most longitudinal social surveys set up to evaluate social policy interventions do not collect sufficient health data and cannot be linked to health records. The lack of randomised controlled trials of income supplementation [71], including anti-poverty [24] and in-work tax [73] credit interventions, has previously been noted. It is also difficult to identify suitable natural experiments for study that can overcome some of the risks of bias of observational studies. However, even observational studies of the effect of publicly funded financial credits on health are scarce, meaning that there is a lack of methodological precedent even for observational study design.

## **Research questions**

This thesis estimates the effect of two publicly funded financial credits interventions for the social protection of families, FTC and IWTC, on SRH at the individual level over the short term in New Zealand over the 2002-09 period. Both the FTC and IWTC aim to provide additional income to families at risk of or living in income poverty. An additional goal of the IWTC intervention is to move families on welfare into paid employment, and retain families in low-paid employment respectively. The Statistics New Zealand’s longitudinal SoFIE is analysed, using a cohort study design and individual-level fixed effects analytic methods.

This thesis aimed to answer two principal research questions with regards to FTC:

## Chapter 1: Introduction

1. What was the association between change in FTC eligibility and change in SRH in adults at the individual level over the short term?
2. What was the association between change in the FTC amount that a family is eligible for and change in SRH in adults at the individual level over the short term?

It aimed to answer two principal research questions with regards to IWTC:

3. What was the association between change in IWTC eligibility and change in SRH in adults at the individual level over the short term?
4. What was the association between change in the IWTC amount that a family is eligible for and change in SRH in adults at the individual level over the short term?

There were four sets of subsidiary research questions. One set of questions was concerned with identification of effect modification by income. Morris *et al.*'s theory of a minimum income for healthy living postulates that income should have a more positive health effect in those people whom additional income lifts above the threshold for a healthy living [4-8]. In the absence of a specific measure of this theorised income threshold for New Zealand, income poverty presents a potential measure of this threshold in New Zealand. This analysis tested the theory of minimum income for healthy living.

5. Was the effect of FTC eligibility and amount on SRH modified by income poverty?
6. Was the effect of IWTC eligibility and amount on SRH modified by income poverty?

Māori continue to be overrepresented in low-income groups [82] and to have worse health than non-indigenous New Zealanders [83]. Therefore, the second set of questions was concerned with identification of effect modification by Māori and non-Māori ethnic group. Previous research has found some evidence for treatment effect heterogeneity of similar publicly funded financial credits by ethnicity in the United States [84, 85], reviewed in more detail in *Chapter 4*. This warranted investigation for New Zealand with the current interventions.

7. Was the effect of FTC eligibility and amount on SRH modified by ethnicity?
8. Was the effect of IWTC eligibility and amount on SRH modified by ethnicity?

Previous studies have suggested that financial credits interventions may have a longer-term (rather than immediate) effect on health. Thus, a third set of subsidiary research questions

tested the effect of the delay between the change in FTC and IWTC and the change in the outcome variable, SRH.

9. What were the effects of FTC eligibility and amount on SRH two to six years after change in the credit?
10. What were the effects of IWTC eligibility and amount on SRH two and three years after change in the credit?

The final set of subsidiary research questions estimated the effect of FTC and IWTC on two other health outcomes, psychological distress and current tobacco smoking.

11. What was the association of change in FTC eligibility and amount with change in psychological distress in adults at the individual level over the short term?
12. What was the association of change in IWTC eligibility and amount with change in psychological distress in adults at the individual level over the short term?
13. What is the association of change in FTC eligibility and amount with change in current tobacco smoking in adults at the individual level over the short term?
14. What was the association of change in IWTC eligibility and amount with change in current tobacco smoking in adults at the individual level over the short term?

While estimates of the direction and strength of the causal pathways through which FTC and IWTC may influence health were of interest, the thesis did not attempt to estimate these. There were methodological concerns that decomposing the financial-credit-on-health effect into its component causal effects (operating through income and employment) was impossible, because these financial credits changed several mediating factors (e.g., income and employment) simultaneously, so that these effects could not be isolated [28, 29, 70]. Therefore, this thesis estimated the net effect, through all causal pathways, and did not attempt to decompose it.

## Structure of the thesis

The thesis has the following structure:

**Chapter 2: Background** introduces the FTC and IWTC interventions studied in this thesis and theorises how they could impact health. Social policy, social protection and social assistance are defined and differentiated. The histories, objectives and designs of FTC and IWTC in New Zealand are described. Conceptual frameworks for the effect of FTC and IWTC on health are

developed. Literature on the causal pathways through which anti-poverty and in-work tax credits for families could impact health is critically reviewed.

**Chapter 3: Methodology** locates the thesis in the emerging discipline of political epidemiology and introduces potential study design and methodological options for studies assessing the health effect of specific social policy interventions on health, making transparent the rationale for the design and methods used in this thesis. Political epidemiology is defined and described. The thesis then develops a typology of political epidemiological approaches to studying the impact of social policy on health. The importance of studying the health effect of individual social policies (as opposed to welfare regimes or politics) is argued. Potential study designs (randomised controlled trials, natural experiments and cohort studies) and methods for estimating the effect of individual social policies (fixed effects regression, difference-in-differences, discontinuity regression and marginal structural models) are described, and their respective advantages and disadvantages contrasted. The rationale for the choice of a cohort study design and fixed effects methods for this thesis is outlined.

**Chapter 4: Literature Review** systematically reviews literature on the effect of anti-poverty and in-work tax credits interventions on health status in adults, applying Cochrane Review standards and protocols, as well as a more epidemiological framework of critical appraisal. The methodology of the systematic review is described. The findings of the systematic review are then presented. The search results and the characteristics of the included studies are described, as well as the risk of bias assessed. The findings of the included studies are presented. Finally, the overall quality of the evidence is assessed and the review findings are related to previous systematic review evidence.

**Chapter 5: Methods** describes the practical steps of the thesis. The design, data collection, initial survey response and attrition from the SoFIE are described. The restriction of the survey to the study sample and the characteristics of the study sample are then presented. The analytical frameworks of the fixed effects regression analyses are developed. The exposure, outcome, confounding, mediating and effect modifying variables are described. The individual fixed effects regression analytical methods are elaborated. The methods of main and subsidiary fixed effects regression analyses are described. Finally, the methods of the sensitivity analyses are presented.

**Chapter 6: Cross-sectional analyses at baseline** describes characteristics of the study population at baseline. The distributions of the exposure, outcome and confounding variables

used in the regression analyses at baseline are described. The cross-sectional association of FTC and IWTC with SRH at baseline is calculated.

**Chapter 7: Descriptive analyses of time-varying variables over seven waves** describes dynamics in the time-varying exposure, outcome and confounding variables used in the regression analyses. Basic tabular analyses of change in the exposures and change in the outcome are also presented as preliminary findings. The association of change in the variables hypothesised to be time-varying and potentially confound the exposure-outcome relationships with change in the exposure variables and with change in the outcome variable is determined. This enables prediction on the basis of empirical data of the potential of these variables to act as confounding variables in the fixed effect regression analyses.

**Chapter 8: Fixed effects regression analyses** presents the results of the main fixed effects regression analyses estimating the effect of the FTC and IWTC on SRH at the individual level in adults over the short term. Results from subsidiary analyses are also presented, testing for: effect modification by ethnicity and by poverty; effects of the tax credits on health when change in the outcome variable lagged behind change in the exposure variables by longer periods; and effects of the credits on two other health outcomes, psychological distress and current tobacco smoking.

**Chapter 9: Internal validity and precision** assesses the risk of several biases as a means of assessing the internal validity of the thesis, as well as judges the precision of the thesis findings. The risk of confounding, as well as bias from selection; misclassification of the exposure, outcome and potential confounding variables; misspecification of the outcome; and health selection (or reverse causation) is explored and, where feasible, quantified. For this purpose, several theoretical and empirical sensitivity analyses are presented.

**Chapter 10: Discussion** summarises the key findings of the thesis and relates them to findings of previous studies, as well as to relevant theories. Potential conceptual, methodological and data analytic limitations are outlined and critically discussed. The overall external validity of the study is assessed. Implications for public health policy are drawn out and a future research agenda is proposed.

## Chapter 2: Family Tax Credit and In-Work Tax Credit in New Zealand

This chapter introduces relevant background information on the Family Tax Credit (FTC) and the In-Work Tax Credit (IWTC) interventions in New Zealand, locating these publicly funded financial credit interventions within the domain of social policy and conceptualising the causal relationship between these credits and health in parents. The FTC and IWTC are social assistance interventions, a type of social protection policy that provides financial credits or in-kind resources to individuals or families, generally on a needs basis.

The first social assistance intervention studied in this thesis, called FTC, is primarily concerned with increasing income in families living in or at risk of income poverty, making it an anti-poverty financial credit intervention. The second intervention, called IWTC, has the dual goal of increasing income and moving welfare recipients into (or keeping them in) paid employment and is thus a welfare-to-work policy intervention. As part of the Working For Families tax credit reform, the FTC intervention, which had existed for two decades, was expanded in its population coverage and generosity (in terms of dollar amount provided to families) between 2005 and 2007. As part of the same reform, IWTC was introduced in 2006.

The principal causal pathways through which these credits could impact health are the income pathway for FTC and the income and employment pathways for IWTC. The existence of these credits may also create a general sense of 'welfare security', especially in non-recipients, and a sense of stigma in recipients, both of which could have a health effect (direct causal pathway). However, the net effects of publicly funded financial credits for families through these causal pathways remain unclear.

The purpose of this chapter is to provide the reader with a detailed understanding of the publicly funded financial credit interventions studied in this thesis, as well as how these may impact health status in parents. For this purpose, the chapter first defines social policy, protection and assistance, as well as describes two types of social assistance policies, namely anti-poverty and welfare-to-work financial credits. The publicly funded financial credits of the Working For Families tax credit package studied in this thesis, the FTC and IWTC, are then introduced. A description of their prehistory; social and economic context; timeline of implementation; and specific design, including their population coverage and generosity, as well as changes in these interventions over the 2002-09 study period is provided. Conceptual frameworks for the causal relationship between the FTC and IWTC interventions and health are developed. Finally, the chapter reviews evidence for the causal pathways, through which FTC and IWTC may have an effect on health in parents.

### **Social policy, protection and assistance**

According to the Malcolm Wiener Centre for Social Policy at the Harvard Kennedy School of Government, social policy is “public policy and practice in the areas of health care, human services, criminal justice, inequality, education and labor” [10]. Social protection policy is one aspect of social policy, falling under the human services, inequality and labour policy domains. The United Nations Research Institute for Social Development defines social protection as “protecting individuals and households during periods when they cannot engage in gainful employment or obtain enough income to secure their livelihoods due to unemployment, sickness, chronic ill health or disability, old age or care responsibilities” (p. 16) [11]. Therefore, social assistance (also called social benefit or welfare) is one of three domains of social protection, the other two being labour market and social insurance.

Social assistance interventions provide financial credits or in-kind resources to socio-economically disadvantaged individuals, families or households to ensure an adequate standard of living. For the United Nations Economic and Social Council, the standard of living refers to “the human person’s rights to certain fundamental freedoms, including freedoms to avoid hunger, disease, and illiteracy” (p. 7) [86]. Living standards therefore refer to the availability of economic and social goods, comforts and necessities such as income, education, employment and housing that are available to a specific socioeconomic group in a specific geographic area. “With an adequate standard of living, people are well-placed to participate fully in society and to exercise choice about how to live their lives” (p. 7) [82].

One way of structuring thinking about social assistance is to consider its design choices: “between payments in cash [also called financial credits] and provision of benefits in kind; between universal benefits and means-tested benefits; between unconditional benefits and benefits conditional on work or training; and between benefits as of right and benefits at the discretion of officials” (p. 24) [2]. One form of financial credits are tax credits, defined as “a payment from the government to an individual or a family” made through the tax system (p. 316) [2].

## **Anti-poverty and in-work tax credits for families**

This thesis is concerned with two different types of publicly funded financial credits. The first is an anti-poverty tax credit for families, the second an in-work tax credit for families. Anti-poverty tax credits aim to provide additional income to families living in or at risk of poverty for the purpose of moving or keeping them out of poverty. These financial credits are common in many high-income countries. They are unconditional in that they are tested on the basis of need, especially low income or income poverty, but no conditions are attached to the eligibility for or receipt of these tax credits. Such credits have received considerably less research attention to date, as demonstrated in the literature review on the impact of these credits on health status in adults presented in *Chapter 4*.

In-work tax credits for families have the dual objective of reducing (the risk of) income poverty and moving individuals or families dependent on welfare into paid employment [87-89]. They are popular and common publicly funded financial credit interventions implemented in more than 34 member states of the Organisation for Economic Co-operation and Development in 2009 [18]. Their appeal to governments is their ability to redistribute income to low-income and / or middle-income groups, while at the same time creating additional (and financial) incentives for employment [87-89]. A detailed economic rationale for conditional financial credits has been provided elsewhere [90].

In short, eligibility for in-work tax credits for families is generally dependent on family income, family type (one-parent versus two-parent family) as well as the number and age of dependent children in the family [18]. I have elsewhere defined an IWTC for families as “a tax credit implemented as part of a welfare-to-work policy; received by families (at least one parent or principal child carer with at least one dependent child); received by adults currently in work; and not time limited (permanent; individuals in theory continue receiving the credit over time, as long as they still qualify)” (p. 7) [28]. Examples for in-work tax credits for families

include the Earned Income Tax Credit (EITC) in the United States; Prime Pour L'Emploi in France; the Working Tax Credit in the United Kingdom, as well as the Child Tax Credit in the Slovak Republic; Earned Income Tax Credit in the Republic of Korea; and Working Income Tax Benefit in Canada [18].

In-work tax credits for families differ from two other types of in-work financial credits. They differ from in-work tax credits paid to an individual, such as those of Belgium, Finland, Germany, Hungary, Netherlands and Sweden [18]. Because these credits are not family-targeted, they may have a different health pathway and impact on adults in families [29]. In-work tax credits for families also differ from in-work cash payments, generally one-off or time-limited publicly funded employment-conditional cash benefits designed to exert an immediate impact following uptake of employment [18]. Examples of in-work cash payments are listed in Table 2 in [18]. These credits may differ from in-work tax credits for families in their duration (permanent versus one-off or time limited) and temporal impact (medium- to long-term versus immediate-term) [18]. The short-term impact of these credits may be different from the longer-term changes brought about by the more permanent in-work tax credits for families [28, 29]. Also, tax credit from earned income may be considerably more socially acceptable than income from a public cash credit provided through the benefit system [91] and, because it may cause less social stigma for its recipient, may have a relatively more positive health effect, at least in countries that do not take a social rights approach to social assistance [28, 29].

Empirical research on the health effects of financial credits has mostly been conducted on financial credits conditional on the uptake of health and social services in low- and middle-income countries, aided by randomised controlled trials credits such as *Oportunidades* in Mexico [74] and *Bolsa Familia* in Brazil [75]. A systematic review of the effect of these conditional credits in low- and middle-income countries found that receipt of these credits was associated with improved child and adult health outcomes, but could not establish whether these improvements resulted from the additional income or from the utilisation of social and health services that credit receipt was conditional on [76]. The health effect, and thus application and effectiveness, of conditional credits in high-income countries on the other hand is contested [25, 77], with two systematic reviews, including the review of this thesis research presented in *Chapter 4*, finding no evidence for an effect [24, 29].

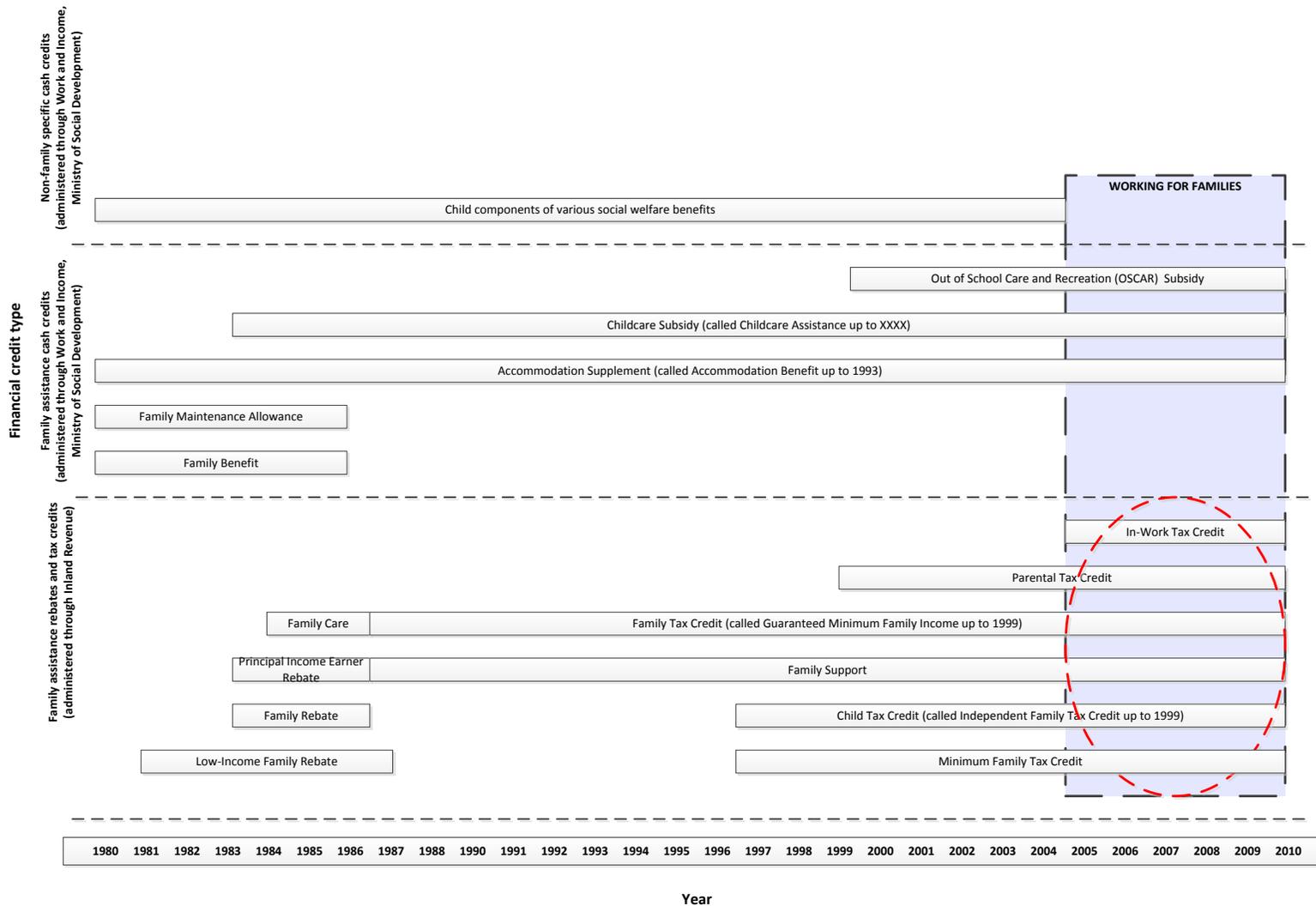
# Family Tax Credit and In-Work Tax Credit in New Zealand

## The prehistory of the Family Tax Credit and In-Work Tax Credit

The New Zealand government has a long history of providing social assistance financial credits to families [92]. A timeline of publicly funded financial credits for families between 1980 and 2010 in New Zealand is presented in **Figure 5**. A more detailed description of the evolution and operation of publicly funded financial credits for families in New Zealand can be found elsewhere [92]. Three principal types of such publicly funded financial credits for families can be differentiated according to whether they are administered through the welfare or tax system and whether or not they are specifically targeted at families. These three types of financial credits are: not family-specific benefits (administered through the social assistance system); family-specific benefits (administered through the social assistance system); and tax rebates or credits (administered through the tax system).

The 1980-2010 period in New Zealand was marked by three distinct phases of reform of publicly funded financial credits for families (**Figure 5**). The first phase of reform occurred in the 1980-87 period within the context of successive governments considerably restructuring the economy in the face of economic crisis by radically opening the national market to international trade and privatising public assets, while at the same time reducing the provision of public services in a move towards 'greater personal responsibility'. As employers such as meat processing factories collapsed, unemployment and poverty rates grew considerably, especially though amongst socio-economically disadvantaged groups. The marked socio-economic inequalities in health in New Zealand also widened considerably [93]. Social assistance for families was reduced, with four rebates disestablished: *Family Care*; *Principal Income Earner*; *Family*; and *Low-Income Family Rebates*. Moving away from provision of funds directly to parents, *Childcare Subsidy*, a means-tested financial credit paid directly to the provider of childcare, was established. To compensate (at least partially) for the loss of income that the disestablished rebates had previously provided to low-income families, two rebates called *Family Support Credit* and *FTC*, were established. A rebate is "a deduction from an amount to be paid or a return of part of an amount given in payment" [94]. The *FTC*, despite its names implying otherwise, was a rebate, not a tax credit. The *Accommodation Benefit* (later called *Accommodation Supplement*) and not family-specific benefits remained unchanged.

Figure 5: Timeline of financial credits for families, 1980-2010, New Zealand



The second phase of reform occurred in the 1995-99 period, marked by the growth after recession of the New Zealand economy, with increasing labour demand and reduced unemployment. Socio-economic health inequalities were marked and widened [93], but monitoring and policy initiatives did not focus on these inequalities [52, 53]. This was a less severe restructuring of social family assistance payments. The Independent Family Tax Credit (later called the Child Tax Credit) and Parental Tax Credit rebates were established in 1997 and 1999, respectively. Again, these credits were rebates, not tax credits. The Out of School Care and Recreation Subsidy was also established in 1999 to subsidise costs of children attending before- and after-school programmes as well as school holiday programmes for school-aged children (five to 13 years).

### **The expansion of Family Tax Credit and introduction of In-Work Tax Credit**

The third phase of reform occurred over the 2000-08 period. Economically, this period was characterised by a buoyant economy, with steady and strong economic growth and low unemployment rates creating considerable labour demand. Despite the rapid economic development and labour demand, several social indicators such as child poverty and child mortality rates remained high in absolute terms, although they were comparable with other reference countries. A recent report of the Organization of Economic Development and Cooperation showed that 12.2% children were living in poverty (defined as in a household income below 50% of the median) in New Zealand in 2008, compared to 14.0% in Australia, 12.5% in the United Kingdom and 21.6% in the United States [95]. A World Health Organization report showed that the infant mortality rate per 1,000 live births was five for New Zealand in 2004, compared to five in Australia, five in the United Kingdom and six in the United States [96]. Ethnic and socio-economic inequalities in health and other social outcomes were marked and continued to widen [97]. As stated above, one prominent policy initiative of this period was the Reducing inequalities (later called Closing the gaps) policy programme which aimed to reduce ethnic and socio-economic inequalities [51, 52]. Therefore, health policy-making was also focused on SDH and reducing inequalities in health [51, 52].

The third reform was brought about by the 2004 Taxation (Working for Families) Act [98]. This reform provided four tax credits to families, administered through the tax system by the Inland Revenue Department for non-beneficiaries and through the social benefit system by the Ministry of Social Development for beneficiaries (**Figure 5**). This tax credit package overall aimed to “make work pay, ensure income adequacy and support people into work” (p. 14)

[99]. The Working For Families tax credits were introduced and implemented in a staggered fashion over the 2004-07 period (**Figure 5**). One principal change brought about by the policy reform was the expansion in generosity and population coverage of the FTC, an anti-poverty tax credit for families living in or at risk of poverty. The second principal change was the introduction of the IWTC, an employment-conditional financial credit aiming to increase income in families living in or at risk of poverty and (uptake of) paid employment in low- and middle-income families. Other changes included the expansion of the Parental Tax Credit, a credit aiming to support families over the first eight weeks after the birth of a child to assist with the costs of a new baby. A fourth change was the introduction of the Minimum Family Tax Credit to provide a base income to a small number of working families on very low income. The fifth change was that Child Tax Credit was slowly phased out. Furthermore, child components of various not family-specific benefits were also disestablished, but some family-specific benefits were increased in generosity and population or geographical coverage, such as the Childcare Subsidy, Out of School Care and Recreation Subsidy and Accommodation Supplement.

**Table 1: Timeline of change in Family Tax Credit, In-Work Tax Credit and other Working For Families tax credits, 2002-08**

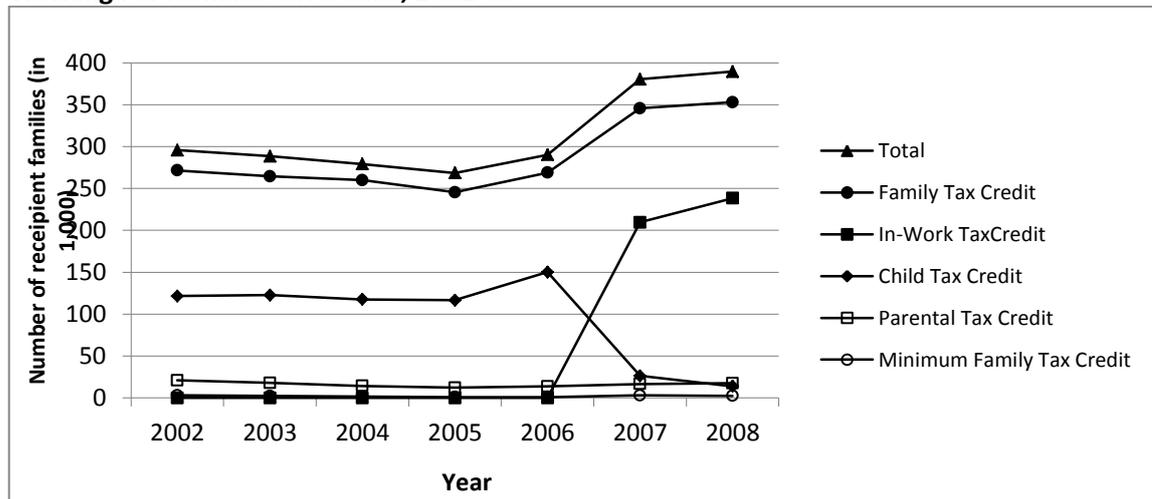
Time	Change in FTC, IWTC and other Working For Families tax credits
1 April 2005	FTC rates increased.
1 April 2006	IWTC replaced Child Tax Credit for eligible families. Income limits for the Minimum Family Tax Credit increased. Higher income thresholds were set for FTC, IWTC and Parental Tax Credit, and the abatement rate reduced.
1 April 2007	FTC rates increased. Income limits for the FTC increased. Future payments were regularly adjusted for inflation.
1 April 2008	The Minimum Family Tax Credit rate increased to ensure that a family's after-tax income did not fall below \$18,460 (\$355 per week after tax).

Source: Adapted from Work and Income and Inland Revenue, n.d. [100]

The annual population coverage of the FTC, IWTC and other Working For Families tax credits changed considerably over the 2002-08 period, as shown in **Figure 6**. The FTC expanded its population coverage by 51.8% from before the Working For Families reform in 2005 to after the reform in 2008 (from 245,400 to 372,600 recipient families). This expansion was most rapid over the 2005-07 period and levelled out to a small, steady increase in recipient numbers over 2007-10. The IWTC was introduced in 2006 to about 209,600 recipient families and increased its population coverage between 2006 and 2008 by 18.8% to 249,100 families.

Recipient family numbers were consistently small for Parental Tax Credit (between 13,800 and 20,900) and Minimum Family Tax Credit (between 1,000 and 3,700). The total number of families receiving any of the four Working For Families tax credit was marginally higher than the number of families receiving FTC, highlighting the particularly large population coverage of this credit. Of the four Working For Families tax credits, the two credits investigated in this thesis, the FTC and the IWTC, had by far the largest population coverage, and both the FTC and the IWTC affected sizeable numbers of families. Both tax credits are delivered to their recipients through the tax system as either a lump sum payment at the end of the tax year as a tax return or as advanced weekly or fortnightly payments in smaller portions throughout the year [101]. Beneficiary families can apply to receive FTC through the welfare system, as weekly payments from the Work and Income department [102].

**Figure 6: Number of recipient families, Family Tax Credit, In-Work Tax Credit and other Working For Families tax credits, 2002-08**



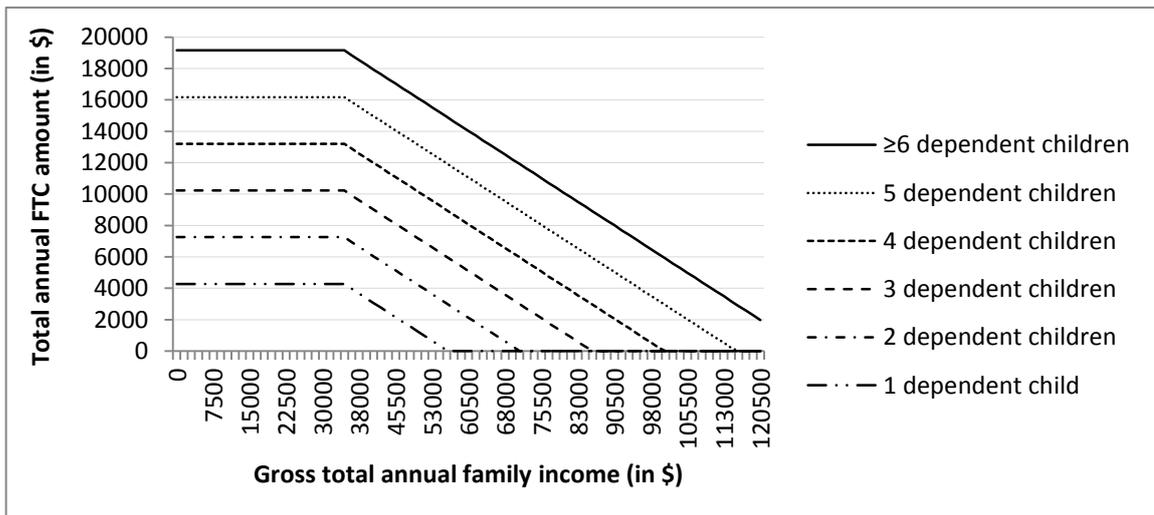
Data source: Inland Revenue Department, n.d. [103].

## The design of Family Tax Credit and In-Work Tax Credit

The FTC is “a payment for each dependent child aged 18 or younger” [104]. The policy goal of the FTC is to reduce income poverty by increasing income in low- and middle-income families. The amount of FTC that an eligible family is entitled to is dependent on total annual family income, the number of dependent children in the family and the age of the dependent children [104]. A dependent child for Working For Families tax credit purposes is defined as a child living with her parents, who is under 16 or 16-18 years old, working less than 30 hours per week, not receiving social assistance and, if 18 years old, attending secondary or tertiary education [98].

The total annual amount of FTC that a family is eligible for by the gross total annual family income and by the number of dependent children in the family, for the 2008 tax year is presented in **Figure 7**. The eldest dependent child is assumed to be under the age of 16 and all other dependent children under the age of 13 in **Figure 7**, as well as associated examples. The credit does not phase in, but all families on a gross total annual family income of less than \$38,500 are eligible for the maximum FTC amount, after which the credit phases out linearly. The process of phase out of a tax credit in tax terms is called abatement, defined as “the cancellation of all or part of an assessed tax” (p. 1) [105]. For a family with one dependent child, the maximum amount of FTC that a family is entitled to is \$4,278 for those on \$38,500 or less, and is fully phased for annual family incomes of \$56,000 or higher. For families with more than one dependent child, the amount received by families with one dependent child is simply supplemented by an additional \$2,974 per additional dependent child, extending phase-out to higher family incomes (e.g., \$71,000 for families with two dependent children, 86,000 for those with three dependent children, etc.). Families with six or more dependent children on a family income of \$38,500 or less are entitled to the overall maximum amount of FTC, \$19,149, and the credit is only fully phased out for these families for family incomes of over \$120,500. Changes in these abatement rates over the 2002-08 period are described in the following sections.

**Figure 7: Total annual Family Tax Credit amount, by total annual family income and by number of dependent children in the family, 2008 tax year (1 April 2008 to 31 March 2009)**



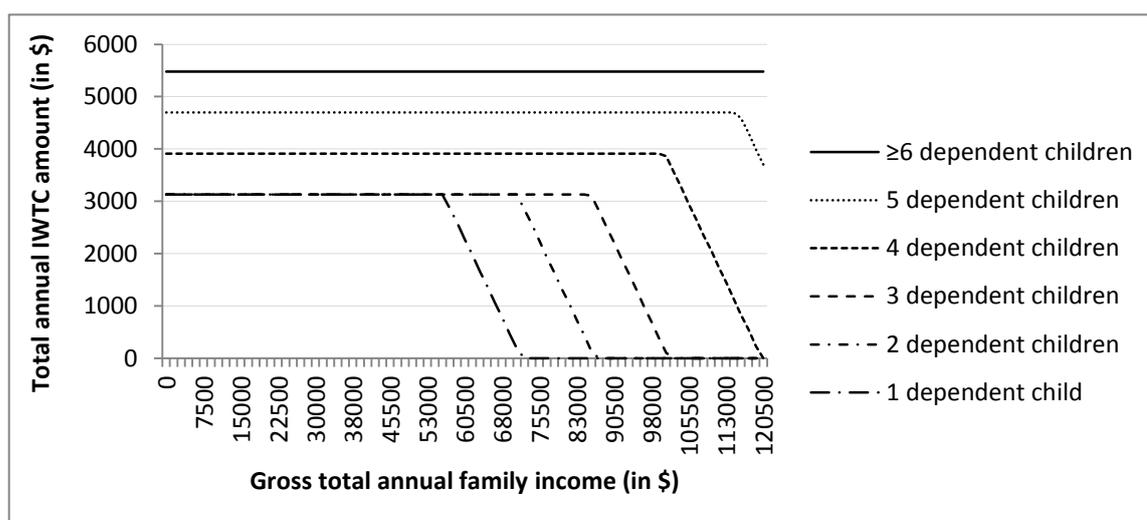
Data source: N. Oloapu-Atoni, Inland Revenue Department, personal communication, 13 September 2010. Notes: The eldest dependent child is assumed to be under the age of 16 and all other dependent children under the age of 13.

The IWTC is “a payment for families who are in paid work” [106]. The dual policy goals of the IWTC are to increase income in families living in or at risk of poverty and to move welfare

recipients into paid employment. The IWTC intervention is the tax credit component that makes the Working For Families tax credits package a welfare-to-work policy. Eligibility for IWTC is dependent on total annual family income, the number of dependent children; the number of hours in paid employment and no receipt of an income-tested benefit, a student allowance or a parent's allowance [106]. Minimum thresholds for hours of employment are 30 hours (jointly) per week for two-parent families (e.g., one parent working five hours and the other working 25 hours) and 20 hours each week for one-parent families, respectively [106].

The annual amount of IWTC that an eligible family is entitled to is presented for the 2008 tax year by gross total annual family income and by number of dependent children in the family in **Figure 8**. The IWTC is not phased in, but families under a certain family income threshold (dependent on the number of dependent children in the family) are eligible for the maximum amount of the credit, after which the IWTC is phased out relatively steeply. For a family with one dependent child, the maximum amount of IWTC that a family is entitled to is \$3,130 for those on \$56,000 or less, and is fully phased for annual family incomes of \$72,500 or higher. The maximum amount of IWTC is the same for families with one, two and three dependent children, but family income thresholds for phase out are higher for families with large numbers of dependent children. The maximum amount of IWTC, \$5,479, is available to families with six or more dependent children, with phase out starting beyond total family incomes of \$120,500. Again, changes in these abatement rates over the 2002-08 period are described in the following sections.

**Figure 8: Total annual In-Work Tax Credit amount, by total annual family income and by number of dependent children in the family, 2008 tax year (1 April 2008 to 31 March 2009)**



Data source: N. Oloapu-Atoni, Inland Revenue Department, personal communication, 13 September 2010. Notes: The eldest dependent child is assumed to be under the age of 16 and all other dependent children under the age of 13.

## Chapter 2: Background

Similar to in-work tax credits in other countries such as in the Prime Pour L'Emploi in France, the IWTC in New Zealand targets both low- and middle-income groups [18]. However, IWTC interventions principally target low-income families in several other countries, such as the EITC in the United States and the Working Families Tax Credit in the UK, for which the credit is phased-out starting at between 10% and 44% of average income from wages [18]. The IWTC in New Zealand is also relatively generous in international comparison, with IWTC in New Zealand (as EITC in the United States) providing a maximum amount of 7%-11% of average income from wages, whereas the Prime Pour L'Emploi in France only provides up to a maximum of 3% [18].

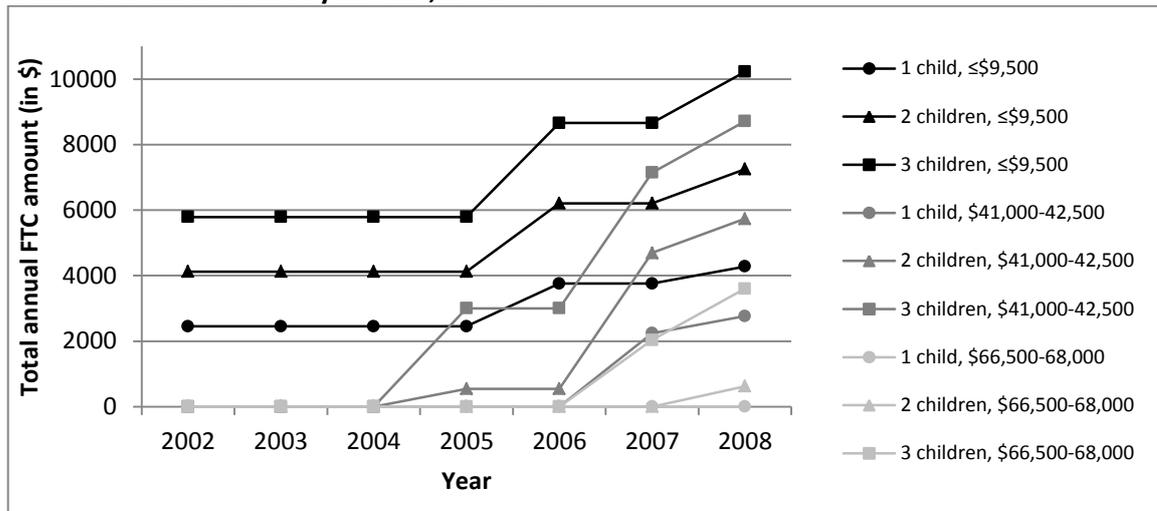
There are several prominent critiques of and concerns about in-work tax credit designs. One critique of these interventions is that they may paradoxically reduce working hours due to the marginal tax rate, “the rate of tax on an additional unit of the tax base” (p. 173) [105]. The marginal tax rate of an individual can be calculated by determining the change in payable taxes that increasing or decreasing the individual’s total personal income results in. For example, in the case of an IWTC, the marginal tax rate is the rate of tax levied on additional hours worked per week. Because marginal tax rates determine the net reward in income such as the amount of income received due to engaging in additional work, they have the potential to influence economic behaviour [105]. In-work tax credits may under certain circumstances dis-incentivise families to increase their working hours [107]. To secure a higher amount of in-work tax credit, a family earning \$56,000 may not increase their working hours, because the income from additional working hours would increase their income above the \$56,000 thresholds for receiving the highest amount of in-work tax credit. Even worse, in-work tax credits may paradoxically incentivise families to reduce their working hours [107]. For example, to secure the highest amount of in-work tax credit, a two-parent family with both parents working a total of 40 hours per week and earning \$58,000 may reduce their working hours to reduce their income to move below the threshold of \$56,000 for receiving the maximum amount of credit. A second prominent critique of in-work tax credit interventions is that they fail to reduce income poverty in recipients of social assistance, one of the population groups most affected by poverty. It has been argued in this regard that in-work tax credits discriminate on the basis of income, failing to adhere with human rights protected under human rights legislation. The Child Poverty Action Group has argued that the IWTC in New Zealand breaches the *1993 Human Right Act* [108], a view not supported by the Human Rights Review Tribunal and the New Zealand High Court, but currently before the Court of Appeal [109]. A concern is that whereas in-work tax credits may succeed in moving recipients of social

assistance into paid employment during labour *demand*, they may unfairly disadvantage recipients of social assistance in times of labour *supply* (such as during the 2008 global economic recession), when social assistance recipients, who are likely relatively less competitive than other low-income groups in the labour market, are unable to find work. Another concern is that IWTC interventions may generate socio-economic inequalities in health, if they have a more positive (or less negative) effect on health in non-beneficiaries than on beneficiaries, which would constitute a case of what Lorenc *et al.* have called “intervention-generated inequalities” (p. 190) [110]. They may generate additional inequalities in health, if they had no effect in their recipients, but increased health in non-recipients through ‘welfare-security’ [27], a causal mechanism that is described in a later section of this chapter.

### **Changes in the eligibility and abatement rates for Family Tax Credit and In-Work Tax Credit over time**

The dollar amount of FTC that an eligible family was entitled to increased markedly after the Working For Families tax credit reform (**Figure 9**). For example, a family with three dependent children and on a very low income (less than \$9,500) was eligible for \$5,791 over the 2002-05 period, \$8,661 in 2006-07 and \$10,226 in 2008, an overall increase in the amount of FTC by 76.6% from 2002 to 2008. The amount of FTC that an eligible family was entitled to increased similarly over the 2002-08 period for families with different numbers of children. For example, the increase was 75.9% for a low-income family with two children (from \$4,122 in 2002 to \$7,252 in 2008) and 74.5% for a low-income family with one child (from \$2,452 in 2002 to \$4,278 in 2008). However, as the FTC was expanded to middle-income families and high-income families with the Working For Families tax credit reform, the amount of FTC that an eligible family was entitled to increased considerably more for middle-income and high-income families than for low-income families. A family with three dependent children and on a middle income (\$41,000 to \$42,500) was not eligible for any FTC over the 2002-04 period, but was entitled to \$3,000 in 2005-06, to \$7,148 in 2007 and \$8,713 in 2008, a strong increase of 190.4% from 2005 to 2008. A family with three dependent children and on a high income (\$66,500 to \$68,000) was not eligible for any FTC over the 2002-06 period, but was eligible to \$2,034 in 2007 and to \$3,600 in 2008, a 77.0% increase between 2007 and 2008. Thus, this differential expansion of FTC by income level constitutes a natural experiment, considering that it exposes subsets of the population to different levels of income supplementation, a potential causal factor influencing health status, in a situation akin to an actual experiment.

**Figure 9: Total annual Family Tax Credit amount that an eligible family was entitled to, by number of children and by income, 2002-08**

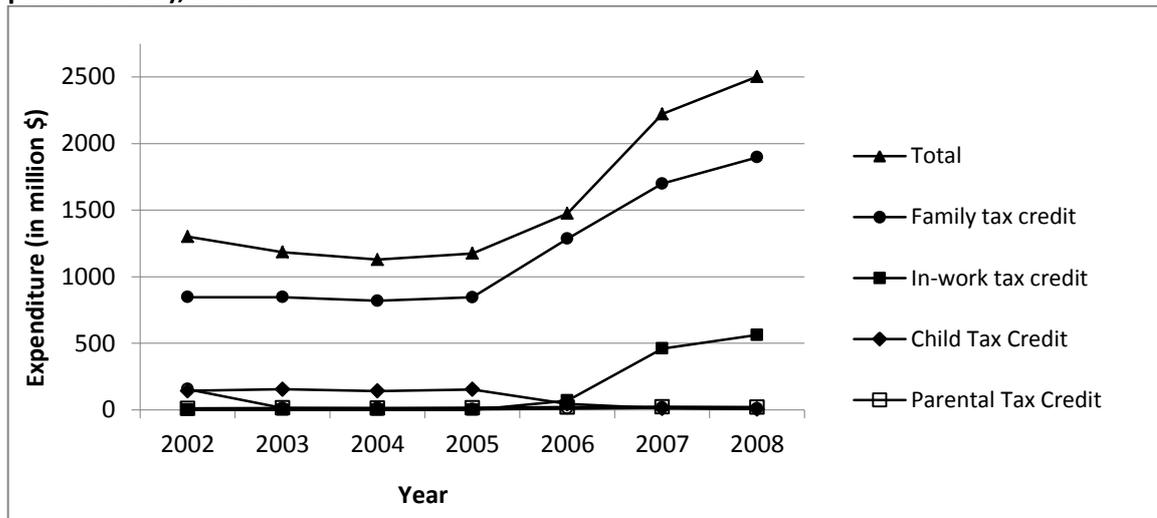


Data source: N. Oloapu-Atoni, Inland Revenue Department, personal communication, 13 September 2010. Notes: The eldest dependent child is assumed to be under the age of 16 and all other dependent children under the age of 13.

### Changes in government spending on Family Tax Credit and In-Work Tax Credit over time

Government spending on the FTC and IWTC increased considerably over the 2002-08 period (**Figure 10**). Spending on FTC increased by 124.2% from 2005 (\$846 million; before the Working For Families tax credit reform) to 2008 (\$1,897 million; when the FTC had been expanded in both population coverage and generosity). Government expenditure on the IWTC was small in 2006 due to the policy being introduced late in 2006 (\$70 million), but increased by 22.1% from 2007 (\$461 million) to 2008 (\$563 million) as the policy was being implemented. By 2008, of the total government spending on social assistance, FTC consumed 11.6% and IWTC 3.5% [111].

**Figure 10: Annual government expenditure, Working For Families tax credits (and predecessors), 2002-08**



Data source: S. Carey, The Treasury, Personal communication, 21 May 2013.

## The causal relationship between Family Tax Credit or In-Work Tax Credit and health

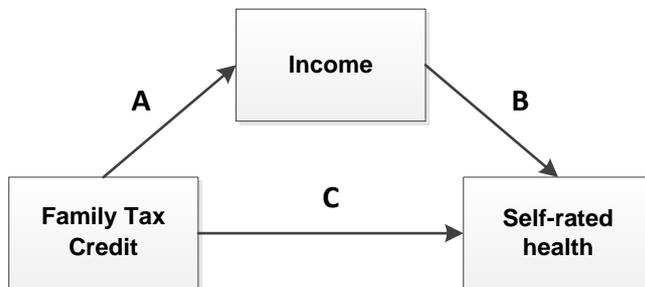
### Conceptual frameworks

The conceptual framework of the causal relationship between FTC and health presented in **Figure 11** posits that FTC could impact SRH through two principal causal pathways. The first FTC-to-health pathway (pathway A through B) operates through income. For example, operating on the income pathway, changes in income could influence health through changes in consumption behaviours or employment decisions. The second pathway is the direct pathway, mediated by other third variables such as through social stigma attached to receiving social assistance and welfare security, the psychologically beneficial sense of economic security from knowing that social assistance is in place (C). Theoretical and empirical evidence for each theorised causal pathway is reviewed below. The aim of the conceptual frameworks is to provide introductory conceptual guidance for the analytical model presented in *Chapter 5* that is more comprehensively discuss all potentially relevant causal factors, including potential confounding variables.

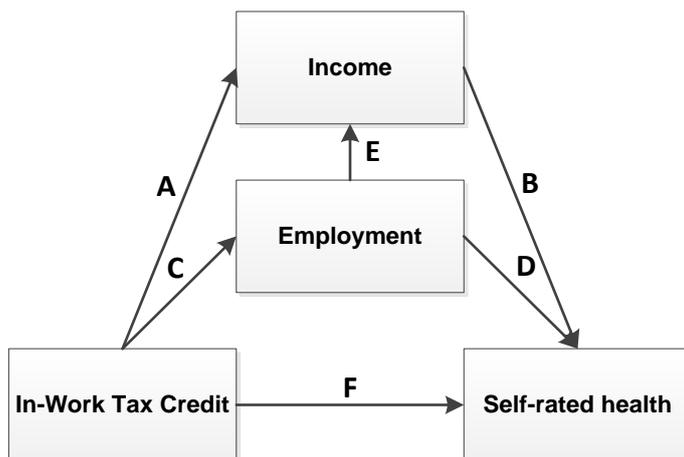
The conceptual framework of the causal pathways between IWTC and SRH is presented in **Figure 12** [28]. The first causal pathway between IWTC and SRH operates through income (pathway A through B). The second causal pathway is through paid employment, either directly (pathway C through D) or mediated by income (pathway C through E through B). A

third causal pathway is a direct pathway (pathway F), which is again other pathways not stated, such as stigma and welfare security.

**Figure 11: Conceptual framework of the relationship between Family Tax Credit and self-rated health**



**Figure 12: Conceptual framework of the relationship between In-Work Tax Credit and self-rated health**



Source: Pega *et al.*, 2013, p. 2 [28]

The following text is adapted from the Cochrane Review written as part of this thesis, led by myself and co-authored by Dr Kristie Carter, Professor Tony Blakely and Dr Patricia Lucas [28, 29].

## Review of causal pathways

A review of the theoretical and empirical evidence for the main causal pathways through which FTC and IWTC could impact health provides further conceptual basis for this thesis. As

stated briefly before, Waldfogel [70] and I [28, 29] have argued that it is not feasible to decompose empirically the total effect of in-work tax credits (including the IWTC in New Zealand) on health into its component effects through the theorised causal pathways (principally income and employment). The reason is that eligibility and entitlement for an IWTC depends on a *combination* of income and employment, so that changes in one variable are linked to changes in the other variable [28, 29, 70].

### ***The income pathway***

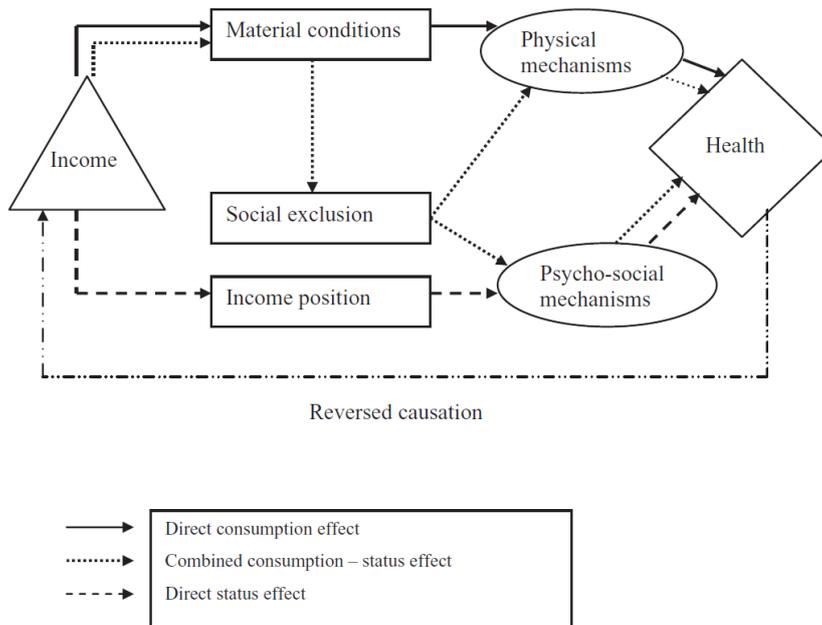
Empirical evidence that could provide an understanding of the strength of the impact pathway in FTC and IWTC is currently inconclusive. A review of empirical evidence concluded that in-work tax credits increased income in their target populations [18]. Empirical evidence, as synthesised in systematic reviews, for whether additional income affects health remains inconclusive, and neither the direction nor the size of such an effect has been established. Randomised controlled trials of income supplementation [71] and randomised and non-randomised studies of interventions aiming to increase income in low socio-economic individuals and families [112] produced mixed evidence for an income effect on health and were plagued by major methodological and other study limitations. A systematic review of the effect of financial credits on child physical and mental health in low-income or socially disadvantaged families in high-income countries also concluded that the current evidence was insufficient to determine whether financial credit interventions are effective at improving health in children over the short term [24]. A non-systematic review of longitudinal studies on the effect of income on health over time concluded that income is positively related to health [113]. On the other hand, a more recent review of longitudinal studies investigating the impact of changes in income on changes in SRH concluded that there was a small and significant positive effect of increased income on health over the short term which, after controlling for unmeasured and residual confounding and health selection, became statistically non-significant [114]. However, equivalent evidence for analyses over the long run is lacking.

Theoretical evidence could provide insights for whether FTC and IWTC affect health through the income pathway. Conceptual models of the relationship between income (from social assistance financial credits such as FTC and IWTC) and health at the individual level suggest that income impacts health through three types of causal effects. These are direct consumption effects, direct status effects, and combined consumption and status effects, shown in **Figure 13** taken from [19]. Direct consumption effects are those by which income

impacts on material conditions which, in turn, affect health through a physical mechanism. For example, if adults receiving FTC invested the additional income they received in goods and services that promote their own health, such as health care and nutritious food, then FTC would be expected to improve their health status. Alternatively, if adult recipients of FTC spent the additional income on health damaging goods and services such as tobacco and alcohol products or energy-dense foods, then the credits would be expected to exert a negative impact on parental health status. Empirical evidence suggests that some anti-poverty and in-work tax credits increase family expenditure on health promoting goods and services such as nutritious food, transport, adult clothing, housing, and educational resources in lower-income families [115-118]. For example, Romich and Weisner's 1999 ethnographic study of spending of EITC among 42 low-income families in Milwaukee, WI, found that families often reported purchasing larger-ticket goods, especially furniture, cars, appliances or houses, as well as entertainment equipment [118]. The same study found further that some families were able to put the EITC payments towards cash savings [118], which also may have increased a sense of financial security and psychological wellbeing in these families. These spending patterns suggested that recipient families perceive their income from EITC more as an annual bonus than as permanent income [118]. Empirical evidence for the impact of anti-poverty and in-work financial credits on family expenditure on health challenging goods and services is mixed. Some studies found that low-income families decreased their spending on alcohol and tobacco after welfare reform in the United Kingdom [116]. Other studies found that the expansion such credits for families had no impact on poor [117] and increased medium-educated [115] sole mothers' expenditure on alcohol and tobacco in the United States.

The second type of effect, termed direct status effect, includes those effects of income on health that are mediated by an individual's relative income position through psychosocial mechanisms [19]. For example, the additional income from IWTC could increase recipient adults' income position relative to relevant individuals or comparison groups and, therefore, enhance their social status, leading to a reduction in psychosocial stress and in turn improved health status. While such changes would be expected to improve individual health for those individuals who improve their social status, it would arguably also decrease health in individuals whose status would be relatively lessened (as others 'move up' in the social hierarchy), and the net effect on population health would be zero.

**Figure 13: Lundberg 2010 model for pathways and mechanisms linking income and health**



Source: Lundberg *et al.*, 2010, p. 57 [19]

The third type of effect is combined consumption and status effects, where income effects on health are mediated by material conditions and in turn social exclusion, and thereby through both physical and psychological mechanisms [19]. For example, if the additional income was used to purchase goods and services that enhanced recipients' inclusion in a social group, such as sports team membership, then this would likely produce a positive impact on health. The level to which a social group, to which the credits and consumption of these credits provides access, promotes health is likely to mediate the level to which the additional income from these credits increases health. Social inclusion in groups that promote healthy behaviours such as exercising and eating nutritious food are likely to impact more positively on health status, compared to inclusion in social groups that promote health damaging behaviours such as tobacco and alcohol consumption. Combined consumption and status effects are active in countries and population groups of different income levels, because these effects originate from relative social status and consumption in comparison with relevant groups, in line with Sen's understanding of poverty equating to reduced capability to participate in society [119, 120].

Another relevant pathway, through which additional income could impact health status, is that changes in income may influence employment decisions, which in turn may impact health status in parents. For example, assuming that leisure time is a normal good (i.e., a good that is more preferred at higher income levels), if a family received additional income from FTC, then

it is plausible that the parents in the family reduced the number of working hours. Thus, employment could be added as a mediating pathway between income and health in the Lundberg 2010 model [19] presented in **Figure 13**.

Morris *et al.* have proposed the theory of a minimum income for healthy living [4-8], an income threshold required to live healthy. The minimum income for healthy living has been calculated for working-age adults [4] and older people [8] for the United Kingdom, but not yet for other countries [121]. Therefore, a financial credit intervention that lifts income of individuals on an income below the minimum income for healthy living to above this threshold should increase health in these individuals, an exaggerated income threshold argument. However, providing financial credits to people whose income is already above the minimum income for healthy living should improve health status less in these people. Therefore, financial credits designs with population coverage of low-income families (e.g., the EITC in the United States discussed further in *Chapter 4*) would be expected to have a relatively larger and positive effect on health, whereas those with broader population coverage of low- and middle-income families (e.g., the FTC and IWTC in New Zealand) would be expected to have a smaller positive or even no effect, depending on the proportion of individuals being lifted above the minimum income for healthy living and the strength of the effect of being lifted above this threshold. The theory of a minimum income for healthy living is tested in *Chapter 8* by estimating the effect of the FTC and IWTC on health in individuals living below the poverty line, assuming - in the absence of a national measure of the minimum income for healthy living for New Zealand - that the poverty line presents a suitable proxy for this threshold.

There is some evidence suggesting that adults in families receiving in-work tax credits for families do not spend the additional income from these financial credits on goods and services that impact their own health but instead invest in health promoting goods and services for their children. Waldfogel's 2007 comparative review concluded that welfare reform which included the introduction or expansion of in-work tax credits for families increased child-related spending in the United Kingdom, whereas it did not in the United States [122]. On the other hand, Waldfogel's review [122] did not include studies such as Romich and Weisner's 1999 study [118], which suggested that low-income families in Milwaukee, WI, prioritised spending of income from EITC on essential items for children, such as clothes, educational tuition fees and to establish savings accounts for their children. However, it may be that the impact of in-work tax credits for families on investments in one's own versus one's children's health might be differential by country or region and the type of in-work tax credit intervention. The degree to which adults spend the additional income from anti-poverty and

in-work tax credits for families on enhancing or damaging their own versus their children's health might modify the impact that such financial credits have on adult health status. However, it has been shown that adults in families who did not use additional income from a move from welfare to work to improve their personal material well-being, but invested it in their children, tended to have an indirect health benefit from their children's improved well-being [123]. They also tended to experience significant improvements in their psychological well-being in the form of boosted self-esteem, sense of self-worth and confidence, an associated reduction of stress and improvements in family functioning [123]. Thus, even if FTC and IWTC did not have a direct effect on parental health, but did have a positive effect on children, then parental health should also improve as a spin-off effect.

### ***The employment pathway***

Economic theory suggests that publicly funded financial credit interventions have an effect on labour supply [124]. It suggests that in-work tax credit intervention such as the IWTC in New Zealand should have different impacts on labour supply in different groups of potential recipients [124]. For non-workers, additional income from an in-work tax credit should motivate them to take up employment to increase their net incomes [124]. However, for persons who already participate in the workforce, additional income from the IWTC should decrease the number of working hours, especially so for those with working hours just above or around the eligibility threshold [124]. One reason for the hypothesised reduction in work hours among the latter recipient group is the effect of receiving additional income from the tax credit, which enables recipients to afford additional leisure time for the same net income. The second reason is the overtaxing of additional earnings in the abatement phase of the tax credit (see **Figure 8** for abatement rates for IWTC), which means that increasing working hours return increasingly smaller net gains in income [124]. Thus, standard labour supply theory [124] theorises that IWTC eligibility and entitlement criteria directly cause changes in employment status, namely increase the up-take of employment in non-workers and reduce working hours among workers. Moreover, FTC should reduce labour supply by increasing net income, enabling recipients to afford more leisure time.

In New Zealand, the Inland Revenue Department and Ministry of Social Development evaluated the effect of the Working For Families tax credit package on labour supply, including the FTC and IWTC, concluding that the package increased employment in its target population

[64].<sup>4</sup> An international review of empirical evidence also concluded that in-work tax credits increased the uptake of employment in their targeted population groups [18], and uptake of work has been shown to improve health [125]. Many factors remain unknown: The level to which in-work-tax credits move individuals from unemployment to full-time permanent employment versus precarious employment; whether working conditions of the employment entered into are advantageous or disadvantageous for health; and whether in-work tax credits impact positively or negatively on work-life balance is unknown and will vary by societal context and personal circumstances.

The health impact of moving from welfare to work is likely to depend on the employment condition moved into from unemployment [126]. Unemployment is defined as the employment condition of those working age people who are available for and seeking work, but are not in paid employment during a reference period [127]. Since eligibility for in-work tax credits for families requires one or both parents in a family to have taxable income, welfare recipients can move from unemployment to either full-time permanent employment or to precarious employment [126]. Full-time permanent employment is defined as standard employment that is characterised by a contract of an undetermined duration that covers at least 35 working hours per week [127]. Precarious employment is non-standard employment (in terms of contract duration and contractual conditions) such as temporary, contingent or home-based employment that is generally characterised by instability, unsustainable income, higher worker flexibility and limited workers' rights [127]. A review of the association between employment conditions and health concluded that unemployment is associated with poorer physical and mental health [127]. The same review found that full-time permanent employment is associated with more advantageous working conditions and better physical and mental health than precarious employment conditions [127]. Consequently, recipients of in-work tax credits for families who moved from unemployment to full-time permanent employment may, on average, experience a relatively more beneficial health effect than those moving into precarious employment. However, the modifying effect of employment conditions on the in-work tax credits -health relationship was beyond the scope of this thesis and was not studied empirically in this thesis.

---

<sup>4</sup> However, the difference-in-differences analyses that these conclusions for New Zealand are principally based on must be regarded with caution. They assumed that one-parent and two-parent families are exchangeable, i.e., do not differ in their baseline characteristics and in the changes in characteristics over time. So, if these family types are not exchangeable, which is likely, then the drawn conclusions carry a high risk of bias from confounding. Therefore, it remains to be demonstrated more convincingly that Working For Families tax credits actually improved employment in New Zealand.

Working conditions, which are often determined by employment conditions, are a group of factors that are likely to mediate the health impact of a move from welfare to work [128]. They are potential occupational exposures, hazards and risk factors that can further be classified into physical, chemical, biological, ergonomic and psychosocial categories [126]. Examples of health-affecting working conditions include exposure to chemical substances (for example, carcinogens); physical hazards such as demanding physical labour; and psychosocial risk factors such as a lack of control over the work environment and its processes. Taking up employment with more advantageous working conditions is likely to have a more positive impact on health than taking up employment with less advantageous working conditions. This was evident in a recent systematic review where it was found that interventions that created flexible working conditions, thereby increasing worker control and choice, have a positive effect on health [128]. So, if in-work-tax credits for families move individuals from welfare to employment with disadvantageous working conditions, they could have less beneficial or potentially negative health effects for their recipients. For example, qualitative research from the United Kingdom found that some parents, who had moved from welfare to work, experienced negative effects on their mental health from work-associated stress [123].

Another point is that taking up employment might have either positive or negative effects on the health of adults in families through differences in work-life balance. For example, one study found that recipients moving from welfare to work in the United Kingdom reported increased self-esteem, confidence and self-worth; reduced household stress; increased partnership satisfaction; and the positive effect of seeing their children benefiting from the increased material well-being [123]. These positive psychological effects are likely to increase family functioning and social inclusion and, in turn, to have a positive impact on health status. On the other hand, the same study found that negative psychological effects included additional stress from having to juggle work and homemaking and from increased financial responsibility [123]. Some recipients reported that as a result of spending less time with their children, due to having to work, their mental health had worsened [123]. These negative psychological effects are likely to decrease family functioning and social inclusion and therefore to decrease health status. The differential minimum weekly working hour requirements for one-parent families (20 hours) and two-parent families (30 hours jointly for both parents) may lead to inequalities in work-life balance between these family types.

The text adapted from the Cochrane Review written as part of this thesis, led by myself and co-authored by Dr Kristie Carter, Professor Tony Blakely and Dr Patricia Lucas, ends here [28, 29].

### *The direct pathway*

Relatively little is known about any direct effects (pathways C in **Figure 11** and F in **Figure 12**) of publicly funded financial credits on health, whether for recipients or non-recipients. It is argued that receipt of social assistance is likely to carry some social stigma in New Zealand, a liberal welfare state that does not take a social rights approach to social assistance. However, this stigma should not be large, but may be moderate to small for FTC and IWTC for two reasons because of the universality of these tax policies. Firstly, FTC and IWTC are paid through the tax system, and not through the benefit system, at least for those not on other needs-tested benefits, meaning that these credits are not necessarily regarded as social benefits in the public eye. Secondly, the policy was designed to have wide population coverage (including middle-income families), which would have increased the acceptability of the credits, and reduced the potential social stigma attached to them. However, the potential (assumed small) social stigma from FTC and IWTC for recipients may (likely negatively) affect health in recipients.

In non-recipients, the pure knowledge that publicly funded financial credits exist and are available to them during times of social need may create a sense of 'welfare security', which could be argued to have a positive health effect [27, 129]. Empirical validation of this theory comes from a recent study of the European Social Survey that showed that the generosity of unemployment benefits was positively associated with subjective well-being in employed individuals [129]. I argue that welfare security may provide one potential causal mechanism for explaining persistent and widening socio-economic inequalities in health in the modern welfare states of Northern Europe [27]. If the effect of a financial credit is less positive in recipients (principally low-income groups) than in non-recipients (principally middle and high-income groups), then financial credits would, paradoxically, result in intervention-generated-inequalities [27].

## Conclusions

This chapter introduced relevant background information on the FTC and IWTC interventions in New Zealand, locating these publicly funded financial credit interventions within the domain of social policy and conceptualising the causal relationship between these credits and health in parents. The FTC and IWTC are social assistance interventions, a type of social protection policy that provides financial credits or in-kind resources to individuals or families, generally on a needs basis.

The first social assistance intervention studied in this thesis, called FTC, is primarily concerned with increasing income in families living in or at risk of income poverty, making it an anti-poverty financial credit intervention. The second intervention, called IWTC, has the dual goal of increasing income and moving welfare recipients into (or keeping them in) paid employment and is thus a welfare-to-work policy intervention. As part of the Working For Families tax credit reform, the FTC intervention, which had existed for two decades, was expanded in its population coverage and generosity (in terms of dollar amount provided to families) between 2005 and 2007. As part of the same reform, IWTC was introduced in 2006.

The principal causal pathways through which these credits could impact health are the income pathway for FTC and the income and employment pathways for IWTC. The existence of these credits may also create a general sense of 'welfare security', especially in non-recipients, and a sense of stigma in recipients, both of which could have a health effect (direct causal pathway). However, the net effects of publicly funded financial credits for families through these causal pathways remain unclear.

# Chapter 3: The emergence of political epidemiology

This chapter locates the thesis within the emerging discipline of political epidemiology and discusses study designs and methods for studying the health effect of specific social policies. Political epidemiology is defined as the study of the effect of the political context on health. The sub-discipline can be divided into three types of approaches, differentiating studies of the health effects of: welfare state regimes; political factors; and specific individual policies. The individual policy approach, taken in this thesis, has been least practiced, but is particularly promising for SDH-focused policy-making because of its potential to inform policy development.

Randomized controlled trials, natural experiments and repeated measures studies are promising study designs for investigating the effect of individual policies on health. Randomised controlled trials of publicly funded financial credits are extremely rare, albeit feasible. Few credible natural experiments of publicly funded financial credits have been identified. This thesis uses a cohort study design to study the effect of FTC and IWTC on health.

Four methods for studying the effect of time-varying exposures on time-varying outcomes, using repeated measures of individuals are described and contrasted: Fixed effects regression, difference-in-differences, discontinuity regression and marginal structural models. Fixed effects regression analytic methods were prioritised in this thesis. The rationale for this choice included the method's suitability for answering the research question of this thesis; the efficiency of its estimator; and its strong control of confounding.

This chapter locates the thesis within the emerging discipline of Political Epidemiology. A typology of political epidemiological approaches for studying the effects of social policies on health is presented. The comparative advantages and disadvantages of the three approaches are discussed. A call is made for further research adopting the individual policy approach. Different methodological options (study designs, methods) for studying the effect on health of specific, individual social policies are discussed in relation to this thesis.

The following text was adapted from the invited commentary written as part of this thesis, led by myself and co-authored by Professor Ichiro Kawachi, Dr Kumanan Rasanathan and Professor Olle Lundberg [9].

### Political epidemiology

The idea of a political epidemiological discipline and the term “political epidemiology” (p. 1396) recently emerged in a commentary written by Muntaner *et al.* in 2010 [66]. While there is currently no standard definition of Political Epidemiology, I understand Political Epidemiology as “the sub-discipline of Epidemiology that studies the impact of the political context on (the distribution of) health and wellbeing” (p. 1) [9]. In line with the Commission on Social Determinants of Health 2008 conceptual framework **Figure 1** [20], I understand the political context as comprising policies (social, economic, health) and politics (governance). As other conceptual frameworks [35, 39, 40], I expand this understanding of the political context to also include welfare regimes. Thus, Political Epidemiology is the study of the effects of welfare regimes, politics and individual policies on health and the distribution of health [9].

Even though it has only recently been conceptualised as such, much political epidemiological research has been conducted over the last two decades. A 2012 review of studies on the effect of welfare regimes on health (inequalities) identified 33 such studies [130] and a recent review of studies of the effect of politics on health identified 42 such studies [131]. Studies on individual policies are less common, but ten studies have investigated the effect on health of conditional financial credits [76] and nine of anti-poverty financial credits [24]. This type of research has especially increased momentum and gained prominence over the last decade. As noted previously, the Commission on Social Determinants of Health was amongst the critical global actors who have explicitly called for research studying the effect of the structural SDH of the political context, especially social protection policies, on health [20]. Political

Epidemiology responds to these calls, filling the relative void of research in this research highlighted in *Chapter 1*.

Despite considerable research activity in the political epidemiological research domain over the last two decades, the definitional, conceptual, theoretical and methodological foundations of the discipline are still emerging [9, 35, 41, 42, 66, 79-81]. Political epidemiologists have repeatedly called for the elaboration of these foundations [41, 42, 66, 80, 81, 132]. This thesis presents a political epidemiological study and aims to contribute to the development of this emerging sub-discipline.

Social epidemiology is closely related to political epidemiology. Social epidemiology is defined as “a branch or sub-speciality of epidemiology that studies the role of social structure and social factors in the production of health and disease in the population” (p. 231) [1]. As such, political epidemiology may be understood as a sub-discipline of social epidemiology. However, social epidemiologists have to date mostly focused on the study of more downstream, individual-level SDH such as ethnicity, gender, income and occupation and have less so studied the upstream SDH of the political context, which ‘opens the space’ for political epidemiology to emerge as a distinct discipline.

### **A typology of political epidemiology**

Three broad approaches to studying the health impact of the political context can be identified: the welfare regime approach, the politics approach and the individual policy approach. These approaches vary according to the level at which they conceptualise the causal processes linking the political context and health, in their methodology and in their application. While the welfare regime and politics approaches have been debated [41, 42, 79, 80, 133], the individual policy approach has received less attention.

#### ***The welfare regime approach***

The well-established welfare regime approach is largely grounded in Esping-Andersen’s classification [12]. This macro-level approach classifies societies based on their degree of de-commodification (state efforts that reduce individuals’ reliance on the market for their well-being), social stratification and (private versus public) welfare provision. Esping-Andersen mainly suggested three regimes: social-democratic, Christian-democratic and liberal (and later added a fourth type, the Mediterranean) [12]. These and additional welfare regime types are listed and described below in **Figure 14** taken from [134]. Others subsequently developed and

expanded this classification to consider regimes outside of Europe and North America (but excluding those in regions such as Africa and South America) [134].

**Figure 14: Welfare state regime types**

<p><b>Liberal/residual</b> In the <i>welfare states</i> of the liberal regime (United Kingdom, United States, Ireland, Canada, Australia), state provision of welfare is minimal, social transfers are modest and often attract strict entitlement criteria; and recipients are usually means tested and stigmatised.<sup>15</sup> In this model, the dominance of the market is encouraged both passively, by guaranteeing only a minimum, and actively, by subsidising private welfare schemes.<sup>16</sup> The liberal <i>welfare state regime</i> thereby minimises the <i>decommodification</i> effects of the <i>welfare state</i> and a stark division exists between those, largely the poor, who rely on state aid and those who are able to afford private provision.</p>
<p><b>Conservative/corporatist/Bismarckian</b> The conservative <i>welfare state regime</i> (Germany, France, Austria, Belgium, Italy and, to a lesser extent, The Netherlands) is distinguished by its “status differentiating” welfare programmes in which benefits are often earnings related, administered through the employer; and geared towards maintaining existing social patterns. The role of the family is also emphasised and the redistributive impact is minimal. However, the role of the market is marginalised.<sup>15</sup></p>
<p><b>Social democratic</b> The Social Democratic regime type (Nordic countries) is characterised by <i>universalism</i>, comparatively generous <i>social transfers</i>, a commitment to full employment and income protection; and a strongly interventionist state. The state is used to promote social equality through a redistributive social security system.<sup>52</sup> Unlike the other <i>welfare state regimes</i>, the Social Democratic regime type promotes an equality of the highest standards, not an equality of minimal needs and it provides highly <i>decommodifying</i> programmes.<sup>15</sup></p>
<p><b>Southern</b> It has been proposed that the southern European <i>welfare states</i> (Italy, Greece, Portugal and Spain) comprise a distinctive, southern, <i>welfare state regime</i>.<sup>53-55</sup> The southern <i>welfare states</i> are described as “rudimentary” because they are characterised by their fragmented system of welfare provision, which consists of diverse income maintenance schemes that range from the meagre to the generous, and welfare services, particularly the healthcare system, that provide only limited and partial coverage.<sup>54</sup> Reliance on the family and voluntary sector is also a prominent feature.</p>
<p><b>Radical/targeted</b> Castles and Mitchell argue that the United Kingdom, Australia and New Zealand constitute a radical, targeted form of <i>welfare state</i>, one in which the welfare goals of poverty amelioration and income equality are pursued through redistributive instruments rather than by high expenditure levels.<sup>56</sup> In the same vein, Korpi and Palme describe the existence of a targeted <i>welfare state regime</i>.<sup>57</sup></p>
<p><b>Confucian</b> The Confucian <i>welfare state</i> (Japan, South Korea, Taiwan, Hong Kong and Singapore) is characterised by low levels of government intervention and investment in social welfare, underdeveloped public service provision, and the fundamental importance of the family and voluntary sector in providing social safety nets. This minimalist approach is combined with an emphasis on Confucian social ethics (obligation for immediate family members, thrift, diligence and a strong education and work ethic).<sup>58</sup></p>
<p><b>Eastern European</b> According to Esping-Andersen, these countries are clearly the most underdefined and understudied region in terms of <i>welfare state</i> development.<sup>39</sup> The formerly Communist countries of Eastern Europe have experienced extensive economic upheaval and have undertaken extensive social reforms throughout the 1990s.<sup>59</sup> These have seen the demise of the <i>universalism</i> of the Communist <i>welfare state</i> and a shift towards policies associated more with the liberal <i>welfare state regime</i>, notably marketisation and decentralisation. In comparison with the other member states of the European Union, they have limited health service provision and overall population health is relatively poor.<sup>60</sup></p>

Source: Eikemo and Bambra, 2008, p. 5 [134]

Studies in the welfare regime tradition are generally cross-country or cross-regional comparative studies aiming to determine the differential impact of welfare regimes on population health and/or health equity in high-income countries. Welfare regime theory points to systematic differences across countries in social rights and protection that generate higher levels of population health and health equity [42]. A 2011 review concluded that population health status is generally best in the social-democratic regime, whereas (relative) health inequalities are comparable across regimes [131]. However, a 2012 review concluded that empirical evidence does not consistently support welfare regime theory [130]. The authors of the review concluded that “measurement of policy instruments or outcomes of welfare regimes may be more promising for public health research than the use of typologies alone” (p. 397) [130]. Nonetheless, this approach continues to be active (examples in **Table 2**).

**Table 2: Examples of studies taking the three different political epidemiological approaches**

Approach	Study	Topic
Welfare regime approach	Chung & Muntaner (2007) [135]	Welfare regimes and child mortality, birth weight
	Muntaner <i>et al.</i> (2011) [131]	Welfare regimes and population health, health inequalities (review, 31 studies)
	Brennenstuhl <i>et al.</i> (2012) [130]	Welfare regimes and population health, health inequalities (review, 33 studies)
	Chuang <i>et al.</i> (2012) [136]	Welfare regimes and child mortality, life expectancy
Politics approach	Lynch <i>et al.</i> (2001) [137]	Trade union membership, political representation of women and child mortality
	Navarro <i>et al.</i> (2006) [35]	Political tradition and child mortality, life expectancy
	Granados (2010) [138]	Political tradition and population health
	Muntaner <i>et al.</i> (2011) [131]	Political tradition, globalisation, democracy and population health (review, 42 studies)
	Chen & Cammett (2012) [139]	Political activism and health care access
	Lin <i>et al.</i> (2012) [140]	Democracy and live expectancy
	Mackenbach, Hu & Looman (2013) [141]	Democracy and life expectancy
Individual policy approach	Lucas <i>et al.</i> (2008) [24]	Anti-poverty financial credits and child health (review, nine studies)
	Lagarde <i>et al.</i> (2009) [76]	Conditional financial credits and health (review, 10 studies)
	Pega, Carter, Kawachi <i>et al.</i> (2013) [30]	IWTC and individual health
	Pega, Carter, Blakely <i>et al.</i> (2013) [29]	IWTC and health (review, five studies)

### ***The politics approach***

Beyond the organising framework of the welfare regime approach, political epidemiology broadens out to a diverse range of potential SDH that is difficult to encapsulate in one descriptive term. I have nonetheless summarised these studies under the umbrella descriptor of ‘the politics approach’. These studies have investigated the health effects of political traditions and ideology (e.g., neoliberalism), processes (e.g., democratisation, globalisation, corruption, privatisation and trade liberalisation), systems (e.g., democracy versus autocracy) or institutions (e.g., unions, political parties and bureaucracy). Research programs on political traditions, globalisation and democracy remain the most prominent in this area [131]. The Mackenbach, Hu and Looman 2013 study [141] of the relationship between democratisation and change in life expectancy over time in European countries is an example of this approach (further examples in **Table 2**).

### ***The individual policy approach***

The third and so far least prominent approach in political epidemiology involves studies evaluating a clearly defined, discrete (often social) policy’s impact on individual- or population-level health. In economics and econometrics there is an established tradition of evaluating the impacts of individual social policies on various outcomes. Examples include the effect of raising the minimum wage on employment rates [142] and of an extra year of schooling on earnings, based on changes in compulsory schooling laws [143]. These studies analyse the introduction of new legislation or a policy change as a natural experiment, attempting to identify the causal effect of change in the exposure on change in the outcome, using such causal inferential methods as instrumental variable, fixed effects or discontinuity regression analysis. In the field of population health, this approach has only recently begun to be applied to examine the health impacts of social policies (**Table 2**).

## **Comparative advantages and disadvantages of the different approaches**

Drawing on scientific debates [41, 42, 66, 80, 81, 132], I have drawn out comparative advantages and limitations of the three approaches in **Table 3**. The approaches differ in the strength of their theoretical foundations and the robustness of their concepts and measurements.

In terms of theories, the welfare regime approach and, to a lesser extent, the politics approach are grounded in well-developed theories, but their propositions are much harder to

**Table 3: Comparative advantages and disadvantages of the three political epidemiological approaches**

Approach	Advantages	Disadvantages
Welfare regime approach	<p>Is founded on established definitions, concepts and theories.</p> <p>Can estimate associations of welfare regimes and health (equity) embedded within the historical, political and economic context.</p> <p>Can determine which welfare regimes are overall better at improving population health and health equity [66].</p> <p>Can determine net effects accruing from multiple sectors at the same time [66].</p>	<p>Relies on contested foundational definitions, concepts and theories [41].</p> <p>Cannot explain changes in health when welfare regimes are stable over time (assuming welfare regimes do determine health).</p> <p>Cannot commonly establish causal effects due to paucity of natural experiments [42].</p> <p>May mask (such as average out) differential (by country) health effects of social policies by grouping countries by regime [41].</p> <p>Generally relies on ecological data due to researching the relatively most aggregated study unit.</p> <p>Cannot determine which individual policies improve health.</p> <p>Has limited direct application in health policy, because cannot propose effective interventions.</p> <p>Cannot be applied for all countries.</p>
Politics approach	<p>Is founded on established concepts, definitions and theories.</p> <p>Can determine which politics are overall better at improving population health and health equity [66].</p> <p>Can determine net effects accruing from multiple sectors at the same time [66].</p> <p>Can be applied for all countries.</p>	<p>Cannot commonly establish causal effects due to the paucity of natural experiments [42].</p> <p>Does not necessarily estimate associations of politics and health (equity) embedded within the historical, political and economic context.</p> <p>Generally relies on ecological data due to researching an aggregated study unit.</p> <p>Cannot determine which individual policies improve health.</p> <p>Has limited direct policy application, because cannot propose effective interventions [41, 42].</p>
Individual policy approach	<p>Can establish causal effects by leveraging natural experiments.</p> <p>Has direct policy application, because can propose effective interventions [42].</p> <p>Can be applied for all countries.</p> <p>Can identify effects of individual policies on individual health.</p>	<p>Requires natural experiments.</p> <p>May be confounded by the impact of other policies occurring at the same time or other sectors [66].</p> <p>Cannot determine net effects accruing from multiple sectors at the same time [66]</p> <p>May disregard the historical, political and economic context [66].</p> <p>May lead to limited understanding of the individual policy's political origin [66].</p> <p>Cannot determine which macro-level political factors improve health (equity) [66].</p> <p>May lead to a limited set of policy alternatives [66].</p>

test for causality. The welfare regime approach, in particular, faces several theoretical criticisms, including that existing theories and classifications are based on single aspects of the welfare state (e.g., public financial credits) [39]; assume that diverse welfare resources are organised similarly within (clusters of) countries [39]; assume that welfare states are stable over time (despite policies changing); and are not established for most low- and middle-income countries. The empirical evidence does not align well with theory [130]. By comparison, the individual policy approach lends itself to the counterfactual framework for testing causality, but it misses the “big picture” of why politics matter for health.

In terms of concepts and measurements, I argue that the welfare regime approach faces distinct challenges. Firstly, the application of relatively static welfare regime concepts may fail to capture important changes of a country’s social policies over time. Sweden was an exemplary social-democratic welfare regime, yet it decreased the generosity of its social policies considerably after the mid-1980s. Despite this, Sweden is generally classified as a social-democratic regime before and after the mid-1980s. Second, the approach fails to incorporate potentially important time dimensions (e.g., lag effects of policy). If certain social policies have a long-term (rather than short-term) effect, changes in outcomes may have resulted from welfare changes many years previously.

The three approaches also differ methodologically. The welfare regime [42] and politics approaches generally do not fit well into counter-factual frameworks. The reason is that finding instances when a *change* in welfare regime or a *change* in political institution (e.g., from dictatorship to democracy) occurred to permit a natural experiment on population health change is challenging. However, all approaches share the problem of overcoming endogeneity (presence of unobserved variables that drive demand for certain policies and population health). Furthermore, teasing apart the impacts of specific policies, which may be confounded by the impact of other policies occurring at the same time or in other sectors, may be difficult [66]. While the welfare regime and politics approaches can address this issue in part, the individual policy approach cannot determine synergic effects accruing from multiple sectors at the same time [66]. The flip side of this argument is that any association between a macro-level political exposure (e.g., ‘liberal democracy’) and a population health outcome cannot pin down the exact policies that were responsible for the health difference.

Arguably, the welfare regime and politics approaches’ potential for informing policy-making and practice is limited. They do not usually provide practicable information on individual social policies or policy features that policy makers could modify [41, 66]. Suggesting interventions for changing a state’s (usually stable) welfare regime, the degree of democracy or the

populace's political preferences is difficult. In contrast, by providing evidence of the health impact of defined policy interventions, the individual policy approach has direct application in and implications for policy development. However, studying an individual policy risks disregarding the historical, political and economic context, within which the policy affects health [66]. For example, whilst studies suggest that income inequality is a predictor of poor health, these studies have been criticised for overlooking the influence of those more 'upstream' political forces (e.g., neoliberalism) that produce inequality in the first place and are associated with a 'package' of other likely also health-deleterious policies (e.g., de-unionisation, fiscal austerity, privatisation) [144]. Finally, whereas the welfare regime approach has limited country coverage (European, North American and Asian high-income countries), the politics and individual policy approaches can be applied to all countries.

### **A call for studies adopting the individual policy approach**

I believe that the welfare regime and politics approaches have yielded important insights and that the political epidemiological evidence base is strongest if it draws on results from all approaches. By contrast, the individual policy approach (i.e., evaluating the impact of individual policy change) has been less often practiced with respect to evaluating population health. Despite caveats about the narrow interpretation of results, I believe it promises to yield policy-relevant insights in ways that the other approaches do not.

This potential of the individual policy approach is linked to the increasing drive for evidence-based policy. While policy-making is by definition a political, rather than purely rational or technocratic, process, policy actors are increasingly required to justify their decisions through empirical evidence. Furthermore, in the context of the policy 'market', there is increasing potential to deploy evidence as a tool to advocate for specific policy options over others.

Proponents of action on SDH have been pressed to provide practical guidance and be more pragmatic. The individual policy approach, by providing evidence that is easier to apply in the policy process, is arguably better placed to address this call than the welfare regime and politics approaches. It can generate evidence that policy makers and marginalised communities can directly use to advocate for specific policy interventions. Furthermore, because of its direct applicability, the usefulness of such evidence for SDH- and health equity-focused policy development can more easily be demonstrated to important policy actors, which should increase translation of the evidence into policy and action.

Therefore, I cautiously call for greater attention and application of the individual policy approach, mindful of its limitations. By building on the insights of the welfare regime and

politics approaches, accumulating more evidence on the impacts of individual social policies may be a constructive direction for political epidemiology to drive action on SDH and reduce health inequities.

The text adapted from the invited commentary written as part of this thesis, led by myself and co-authored by Professor Ichiro Kawachi, Dr Kumanan Rasanathan and Professor Olle Lundberg, ends here [9].

## **Study designs and methods for the individual policy approach**

The established hierarchy of study designs and methods also applies to studies adopting the individual policy approach, where experimental studies are superior to non-experimental studies. However, experiments of individual social policies in randomised controlled trials (RCTs) remain scarce. Therefore, a goal of studies adopting the individual policy approach is to identify and analyse natural experiments to study causal effects of policies. Repeated measures studies, while non-experimental, study changes over time in the policy and the health outcome, making these a promising study design that should provide information that comes closer to causal effect than associational, cross-section studies can.

### **Study designs**

Political epidemiologists study different types of changes in social policy. One type of social policy change commonly studied is the effect of a change in a policy due to a natural policy experiment. Such natural policy experiments include such scenarios when the introduction or expansion of a policy resulted in treatment (e.g., with an additional amount of income) of one group, but not a comparable other group.

The expansion of FTC and the introduction and subsequent expansion of IWTC during the study period of this thesis was described in the previous chapter. However, these policy changes did not constitute natural experiments, because these changes were non-differential, not affecting groups differentially. Therefore, methods specifically tailored for analysing policy experiments were not suited to this study. Rather than studying the effect of one major policy change, this thesis studied changes in the specific trajectory of individuals over time in their eligibility for FTC or IWTC or the amount of FTC or IWTC that their family was entitled to.

### ***Randomised Controlled Trials***

Randomised controlled trial study designs are the gold standard for assessing the health effect of any intervention in epidemiological research. A randomised controlled trial is “an epidemiological experiment in which subjects in a population are randomly allocated into groups, usually called study and control groups, to receive or not to receive an experimental preventive or therapeutic procedure, maneuver, or intervention” (p. 206) [1]. Randomised controlled trial designs enable an assessment of the causal effect of an policy intervention exposure on a health outcome, because their random allocation of participants to study group and control group theoretically controls for any confounding (assuming that the study and control groups do not differ in their characteristics at baseline, which is likely when the sample size is large and participation high). Randomised controlled trial designs are more comprehensively discussed and critiqued elsewhere [145].

Oakley *et al.* argue that scientific, ethical and feasibility concerns explain the scarcity of randomised controlled trials of social policy interventions, including publicly funded financial credits [146]. The scientific concerns they note include that such trials cannot robustly assess the effects of the totality of complex social policy interventions; ignore the role of scientific theory in understanding intervention effects; and cannot be used in settings which prohibit ‘blinding’ of study participants [146]. However, one could argue that many existing randomised controlled trial ignore the role of scientific theory, such as trials of medicinal drugs ignoring the theories of cellular chemistry. Furthermore, while non-blinding in randomised controlled trials is a weakness, many such trials are conducted without blinding of the intervention, and this limitation can be overcome, at least partially, with the use of objective outcomes measures and blinding of assessors. Feasibility concerns noted by Oakley *et al.* include that randomised controlled trials cannot commonly be carried out in complex institutional and social settings; are usually unacceptable to policy-makers; and are expensive [146]. The next paragraph reviews randomised controlled trials of financial credit interventions, demonstrating that such trials have previously been conducted successfully in low- and middle-income countries and, more recently, in high-income countries. Finally, Oakley *et al.* argue that such trials are unethical, if they withhold a treatment (such as income from a financial credit) from study participants allocated to the control groups [146]. However, I argue that is ethically justifiable to conduct randomised controlled trials of financial credit interventions, if all participants receive at least some additional income or if the treatment (additional income) is also provided to the control group at a later point.

Randomised controlled trials of income supplementations as one crucial type of financial credit intervention have been described as “a lost opportunity for assessing health outcomes” (p. 725), with none identified in the late 1990s [71]. However, trials of several social protection interventions (especially publicly funded financial credits) have since been conducted, initially in low- and middle-income countries. *Oportunidades* is an intervention of the Mexican government, providing financial credit to families conditional on regular school attendance, visits of health clinic and participation in a nutritional support program [74]. The randomised controlled trial of *Oportunidades* was the first such trial in a low- and middle-income. It has established the generally positive effects of this conditional financial credit on health outcomes such as birth weight [147]; growth [148, 149]; and cognition, language and behaviour [149] in children, as well as body mass index and blood pressure in adults [150].

An example (perhaps the first and only) of a randomised controlled trial of a financial credit intervention in a high-income country is the recent *Opportunity NYC* initiative of Mayor Bloomberg of New York City. *Opportunity NYC - Family Rewards* is a financial credit for families conditional on uptake of education and health services, as well as workforce participation and job training activities [151]. This trial demonstrated some positive health effects of this intervention [152, 153].

Randomised controlled trials of the financial credit interventions studied in this thesis, publicly funded anti-poverty and welfare-to-work tax credits for families, have not previously been conducted [73]. However, at least one experimental study of in-work financial credits interventions not administered through the public administration system has been conducted, namely Card and Hyslop’s experiment of an earnings subsidy for exiting social assistance in Canada [154]. However, this experiment also did not study effects on health [154]. In summary, randomised controlled trials have proven a suitable design for studying the health effects of financial credits interventions, but no such evidence exists for the type of anti-poverty and in-work tax credits interventions studied in this thesis.

### ***Natural experiments***

Natural experiments have the potential to provide gold standard evidence with a causal interpretation from observational data. A natural experiment is “naturally occurring circumstances in which subsets of the population have different levels of exposure to a supposed causal factor in a situation resembling an actual experiment, where human subjects would be randomly allocated to groups” (p. 164) [1]. As Wooldridge put it, “in the natural; experiment literature, people [...] find themselves in the treatment group essentially by

accident” (p. 129) [155]. In practice, natural experiments often do not randomize individuals the study and control groups, but to qualify as natural experiments they must distribute participants independent of potential confounding variables [1].

The role of government policy in creating natural experiments is made explicit in Angrist and Krueger’s definition of natural experiments as “situations where forces of nature or government policy have conspired to produce an environment somewhat akin to a randomised experiment” (p. 73) [156]. Changes in publicly funded financial credits may potentially be regarded as providing natural experiments for studying the impact of these credits on health, as long as they provide a differential change for the treatment and a comparable control group. However, as Rodgers has noted, “substantial changes in income are regularly made by governments via taxation, benefits, minimum wage policies, etc., but in such a way that their effects on health cannot be evaluated reliably” (p. 1439) [157]. The core challenge is thus to identify policy changes of publicly funded financial credits where treatment assignment was either entirely random or at least independent of confounding factors.

Four [84, 85, 158-160] out of five studies estimating the effect of in-work tax credit for families interventions on health, which are reviewed systematically in the next chapter, have exploited a differential policy change of the EITC in the United States as a natural experiment to assess the net effect of EITC on health outcomes. The policy change was a relatively larger increase in the generosity of the EITC for mothers with two or more children than for mothers with one child through the *1993 Omnibus Budget Reconciliation Act* [161], described in more detail in the next chapter and elsewhere [160]. Unfortunately, the next chapter indicates that even this established natural policy experiment of a public funded financial credit may be biased from confounding. This demonstrates the difficulty in identifying natural experiments that make it possible to robustly estimate the health effect of publicly funded financial credits.

Information presented in Chapter 2 suggested that both the expansion of FTC and the introduction of IWTC in New Zealand may provide natural experiments for studying the effects of FTC and IWTC on health. Although it could be argued that the provision of the FTC and IWTC interventions was not independent of potential confounding factors, the expansion of FTC and the introduction of IWTC nevertheless were natural experiments, as suggested in the previous chapter. However, the lack of natural control groups may prohibit the application of certain methods, such as difference-in-differences methods. Furthermore, there are no obvious instrumental variables for estimating the casual effect of FTC and IWCT eligibility and

amount on SRH, so that instrumental variable analysis is also not a feasible methodological option.

### ***Repeated measures studies***

Considering that randomised controlled trials and (convincing) natural experiments did not exist for the financial credits studied in this thesis, a most promising study design was repeated measures studies. Epidemiologists commonly refer to studies of two or more repeated measures as cohort studies, whereas econometricians may rather refer to these as panel studies. Cohort studies are studies “in which subsets of a defined population [, generally a large number of participants,] can be identified [and followed over a long period] who are, have been, or in the future may be exposed or not exposed, or exposed to different degrees, to a factor or factors hypothesised to influence the occurrence of a given disease or other outcome” (p. 44) [1]. This thesis applied a cohort study design.

### **Methodological options for studies adopting the individual policy approach**

Political epidemiologists have emphasized the need for further methodological development [42, 66, 79]. This section outlines a selection of salient methodological options for political epidemiological studies that analyse the effect of individual social policy interventions on health (the individual policy approach). It focuses on econometric and epidemiological methods for causal inference from observational studies for repeated measures of individuals in panel studies. This provides insights into methodological options for this thesis.

A number of methods were carefully considered for application in this thesis, but ultimately fixed effects regression analytic methods were prioritized. This section provides an introduction to fixed effects regression analysis. More ‘mathematical’ detail is provided in *Chapter 5*, when the methods of this thesis are described. This section further describes the three alternative methods that were also more closely considered for application in this thesis, namely difference-in-differences, discontinuity regression and marginal structural model methods. This provides the opportunity to compare and contrast the prioritised fixed effects regression methods with the three alternative options, thereby providing a transparent rationale for the choice of fixed effects regression methods.

Each method is outlined by firstly describing its standard applications, such as which types of research questions it answers and which data sources it is best suited to. Secondly, the underlying statistical principles of the method or ‘what it does’ are described. Third,

assumptions of the method are drawn out. Fourth, strengths and weaknesses of the method are described. Finally, the specific suitability of the method for policy and program evaluation is discussed and related to its potential application in this thesis. Where feasible, the fixed effects methods are contrasted with the alternative method that was also considered for, but not implemented in this thesis.

### ***Individual fixed effects regression analysis***

Individual fixed effects regression analytic methods are standard econometric method for the analysis of panel data with repeated measures from individuals, where the exposure and outcome variables are time-varying and the focus is short-run associations [156, 163].<sup>5</sup> Their main purpose is to evaluate the causal effect of a time-varying exposure variable on a time-varying outcome variable at the individual level, controlled for all time-invariant variables (measured and unmeasured), and adjusted for measured time-varying (confounding) variables [156, 163]. They are thus well-suited to research that aims to determine the association of *change* in the exposure variable with *change* in the outcome variable [156, 163].

The essence of fixed effects regression analytic methods is that they eliminate all time-invariant confounding by removing the time-invariant variation within individuals, because “each individual serves as his or her own control” [163]. Accordingly, the fixed effects regression estimator is sometimes referred to as the ‘within’ estimator, because it quantifies the change in the outcome variable that resulted from change in the exposure variable ‘within the individual’ [156, 163].<sup>6</sup> As it eliminates between-individual variation, this estimator cannot determine the effects of between-individual differences, but relies entirely on ‘within-individual’ variation [156, 163]. Because pooled analyses of repeated measures using standard ordinary least squares regression analysis methods do not control for clustering (or serial correlation) of measures in individuals over time, they may produce biased estimates of the exposure-outcome association and imprecise standard errors in panel studies [156]. In contrast, for panel data, fixed effects regression analytic methods provide unbiased (and consistent) estimates, that under some circumstances are the most efficient (least variance) [156].

---

<sup>5</sup> Fixed effects regression methods can also be conducted on other units than the individual such as states or neighbourhoods, but this sections focuses on individual fixed effects as the method most relevant for this thesis that focuses on individual-level exposure and outcome variables.

<sup>6</sup> In contrast, random effects analyses estimate change in the outcome that is due to change in the exposure variable ‘between individuals’. For a discussion of the differences between fixed and random regression analytic effects methods see Wooldridge (2012).

In practice, these methods use means-centring transformation, where the individual mean of all repeated measures (pooled) is subtracted from each single measure from the individual at each time point [156, 163]. This eliminates each individual's set of fixed time-invariant parameters or, in other words, all time-invariant factors, including between-individual differences (or random effects) and all time-invariant confounding variables [156, 163]. Participants who experience no change in their exposure variable are eliminated from the analysis [156, 163]. A formal derivation of the fixed effects regression analytic model used in this thesis is presented in *Chapter 5*.

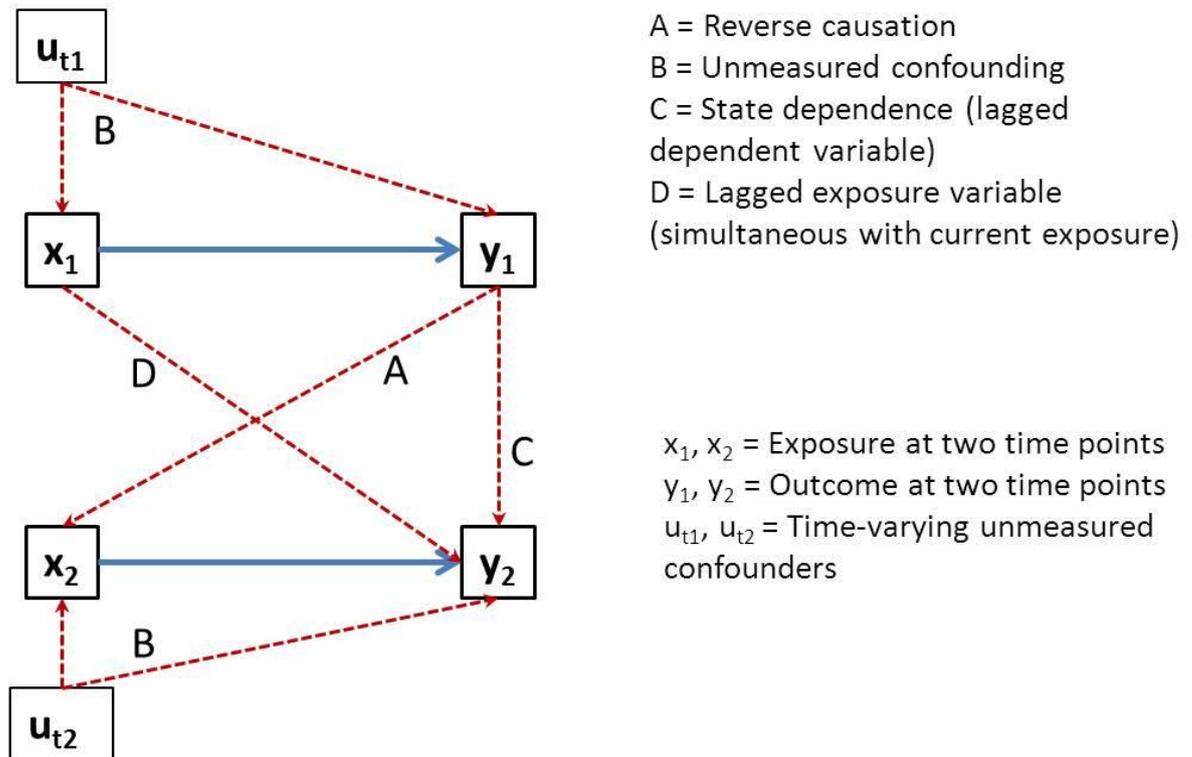
Fixed effects regression analyses rely on several assumptions, violations of which are marked in dashed arrows in **Figure 15** taken from Imlach Gunasekara *et al.* [164]. The first such violation is reverse causation (pathway A in **Figure 15**), where the outcome variable at the first time point causes the exposure at the second time point [164]. The second violation is unmeasured time-varying confounding of the exposure-outcome relationship at either or both of time points one and two (pathway B) [164]. The third type of violation of the assumptions occurs with state dependence (pathway C), where the outcome variable is lagged, i.e. the outcome variable at time point one causes the outcome variable at time point two [164]. The fourth violation is a lagged exposure variable (pathway D), where the exposure variable at time point one causes the outcome at time point two [164].

The main strength of fixed effects regression analytic methods is their ability to eliminate all time-invariant factors from an analysis [156, 163-165]. As Kaufman has noted, “this is the really remarkable promise of the fixed effects model, and one that makes it so attractive for social epidemiology, where exposures are often heavily confounded by myriad contextual, behavioural and attitudinal quantities that would be difficult to assess exhaustively” (p. 625) [165]. A second strength of fixed effects models is their statistical efficiency, compared to some alternative methods for causal inference from observational studies reviewed below, as long as substantial change in the exposure variable occurred in the study sample [156].

There are several limitations of fixed effects regression analytic methods. One limitation is that fixed effects regression analyses can only estimate the average treatment effect within the subset of people experiencing change in the exposure variable [164, 165]. If the counterfactual treatment effect differs between individuals experiencing change and those not experiencing change, then fixed effects regression analytic methods may provide biased estimators of an average treatment effect in the treated [165]. While fixed effects regression analyses may under certain conditions be more efficient than many alternative methods [156], a second limitation of these methods is that their estimators nevertheless lose some efficiency

due to eliminating persons without change from the analysis [164, 165]. A third limitation of these methods is their inability to determine between-individual (random) effects, because these effects are eliminated through the means-differencing transformation [164]. A fourth limitations is the methods' inability to control for reverse causation and unmeasured time-varying confounding [164]. Finally, they can also not deal well with time-varying confounding variables that could simultaneously also be time-varying mediators.

**Figure 15: Imlach Gunasekara's directed acyclic graph of potential violations (dashed arrows) of assumptions in fixed effects models of an exposure variable (X) on an outcome variable (y) at two time points (1, 2)**



Source: Imlach Gunasekara *et al.*, under review, [164]

Fixed effects methods are useful econometric tools for policy analysis and programme evaluation [156]. Wooldridge notes the superiority of fixed effects regression analyses over pooled ordinary least square regression analyses or mixed models in policy evaluation, “where participation in a program is determined by preprogram attributes that also affect [the outcome variable]” (p. 279) [156]. In other words, fixed effects regression analytic methods are suitable tools for determining the effects of eligibility for an intervention on an outcome, where factors determining eligibility may also affect the outcome (making these time-varying confounding variables).

Different methodological options were considered for this thesis, but ultimately fixed effects regression analytic methods were evaluated as most tailored to this thesis. These methods have been specifically designed to answer the types of research questions posed in this thesis, such as the principal question, *Is change in FTC and IWTC eligibility and amount associated with change in SRH at the individual over the short term?*. One of the conclusions of the systematic review of literature presented in the next chapter is that bias from confounding may present one of the challenges for the existing evidence on the effects of anti-poverty and in-work tax credit for families interventions on health. The complete elimination of time-invariant confounding effects in fixed effects regression analyses thus provided considerable promise for this thesis, especially coupled with the ability to identify time-varying confounding variables and adjust well for these, as is described in *Chapter 5*. The thesis was interested in determining an average treatment effect in the treated (which the fixed effects estimator provides for the sizeable subset of individuals experiencing any change), and the level of consistency and statistical efficiency afforded by fixed effects models for such an estimate was also considered a substantial advantage.

### ***Difference-in-differences methods***

Difference-in-differences (or differences-in-differences) methods are econometric methods for causal inference from observational studies, generally in the context of estimating the causal effect of a treatment on an outcome [155, 165]. The treatment is generally the introduction or expansion of an intervention, that differentially effected comparable treatment and control groups [155, 165]. Wooldridge describes these methods as “simple yet powerful methods for program evaluation”, when “a two-year panel data set with control and treatment groups [are] available at two points in time” (p. 283) [155]. They have proven particularly useful in scenarios where a policy change has affected one state or geographic region, but not a comparable such as neighbouring region, thus ensuring a policy change in one geographic region (treatment group) and at the same time no change in the same policy in a comparable geographic region (control group) [165]. A bibliometric search of the Ovid MEDLINE(R) 1946 to Present with Daily Update electronic academic database conducted in July 2013 identified 264 articles listed with ‘difference-in-differences’ or ‘difference-in-differences’ as a keyword. Of the four methods described in this section, difference-in-differences was most commonly indexed in the database, possibly indicating its relative good uptake in health research.

The standard scenario for application of difference-in-differences methods is where an outcome is observed for two groups at two time points [155]. The first group is exposed to a treatment during the second time period, but not the first period (treatment group) [155]. The second group is not exposed to the treatment at either of the two time periods [155]. If the same units are observed within the same group over the two time periods, then the difference-in-differences estimator is calculated by subtracting the mean change between time periods one and two in the control group from that in the treatment group through the first-differencing transformation [155]. Thus, this estimator allows for group-specific effects, the within-group changes in the treatment and control groups [155]. At the same time it allows for time-specific effects, removing bias by eliminating the underlying time-trends that are shared between the two groups [155]. If the treatment and control groups differ in important characteristics that would result in differing trends in the absence of treatment, then additional variables can be included in the regression analysis that may explain this between-group variability, thereby potentially reducing or eliminating bias from these potentially confounding differences [155].

Difference-in-differences methods are traditionally used to analyse repeated cross-sectional time-series data or repeated measures from different individuals [155]. In this setting, they are akin to the study type termed interrupted time series in epidemiology. Interrupted time-series studies are defined as "a research design that collects observations at multiple time points before and after an intervention (interruption) [...] to detect, whether the intervention has had an effect significantly greater than the underlying trend" [166]. However, more recently dedicated difference-in-differences methods for the analysis of the effect of an intervention on an outcome with repeated measures on the same individual from panel data have been developed. The epidemiological equivalent of these difference-in-differences methods are controlled before and after studies. These methods are defined as "a non-randomised study design, where a control population of similar characteristics and performance as the intervention group is identified [...] [and] data are collected before and after the intervention in both the control and intervention groups" [166].

Difference-in-differences and fixed effects methods share many of the same assumptions, especially strict exogeneity of the outcome of each individual at each time point, conditional on all unmeasured time-varying variables [155, 165]. A variable is referred to as exogenous, if it is not caused by any of the other variables in the model, but can cause change in other variables, as long as the former are not influenced by the latter [167]. They also assume that the outcome variable is uncorrelated with the unobserved time-varying variables in all

individuals at each time point, in practice ruling out time-invariant exposure variables and collinearity among any of the time-varying variables included in the model [155]. In simpler terms, the difference-in-differences estimator is unbiased, if the policy change that it evaluates was not systematically related to other factors that affected the outcome variable, including unmeasured time-varying confounding variables [155]. In Athey and Imbens' words, "the underlying assumption is that the time trend in the control group is an adequate proxy for the time trend that would have occurred in the treatment group in the absence of the policy intervention" (p. 1) [168].

The main strength and appeal of difference-in-differences methods is their statistical simplicity and the ease with which they can be computed [155]. Furthermore, if the method is used to analyse a natural experiment, where strict exogeneity holds, then this relatively simple method produces an unbiased estimate. However, the main disadvantage of difference-in-differences methods is their assumptions of no unmeasured time-varying confounding between the treatment and control groups. Thus, the difference-in-differences estimator is biased, if the control and study group have different underlying time trends [155, 165]. Furthermore, standard methods for estimating the difference-in-differences estimators have been shown to considerably underestimate the standard error of the estimator, leading to an estimated 45% type I error in conventional difference-in-differences analyses [169]. Type I (or alpha) error is "the error of wrongly rejecting a null hypothesis, i.e., declaring that a difference exists when it does not" (p. 85) [1].

Wooldridge has compared fixed effects and difference-in-difference estimators and he has argued that the fixed effects and difference-in-difference estimators are unbiased, consistent and identical, if repeated measures from two time points are analysed [155, 170]. If repeated measures from three or more time points are analysed, then the fixed effects and difference-in-difference point estimates are still unbiased, consistent and identical, but their standard errors differ [155, 170]. However, for panel studies with large sample sizes and a small number of time points (as in this thesis), the choice between fixed effects and difference-in-differences methods centres on the relative efficiency of their estimators, which is determined by the presence of serial correlation in the idiosyncratic errors [170]. Idiosyncratic errors are "unobserved factors that change over time and affect  $y_{it}$  [the outcome variable]" (p. 420) [170]. In Wooldridge's own words, "when the  $u_{it}$  [idiosyncratic errors] are serially uncorrelated, fixed effects is more efficient than first differencing (and the standard errors reported from fixed effects are valid)" (p. 447) [170]. Wooldridge further points out that "it is

difficult to test whether the  $u_{it}$  are serially uncorrelated after FE [fixed effects] estimation” (p. 447) [170].

Difference-in-differences methods have been used to study the effect of publicly funded financial credits on health. The next chapter identified these methods as those employed most commonly to determine the effect of in-work tax credit for families interventions in the United States. For example, Averett and Wang’s 2012 study of the effect of the differential expansion of such an in-work tax credit as natural experiment to study the effect of the credit on current smoking in mothers in the United States [84, 85]. Difference-in-differences were also considered for use in this thesis, but it was concluded that they presented a less well suited methods than individual fixed effects. The reason was that the idiosyncratic errors appear to be uncorrelated in this thesis, because of the strong control of time-varying confounding variables, as described in *Chapter 5*. In essence, because the time-invariant confounding variables blocked many back door pathways, unmeasured time-varying confounding was considered minimal (mostly only residual confounding due to measurement error in the adjusted confounding variables). Thus, as Wooldridge suggested for these circumstances, fixed effects methods were expected to be advantageous over difference-in-differences methods in terms of efficiency.

### ***Discontinuity regression analysis***

Discontinuity regression (also referred to as regression discontinuity) analytic methods are quasi-experimental methods for casual inference of the effect of a binary treatment variable (generally, treatment assignment or no treatment assignment) on an outcome, using repeated measures of individuals [171-173]. These methods find application in a setting where treatment is based on whether an observed ‘assignment’ variable<sup>7</sup> exceeds a known and fixed threshold [171-173]. The defining feature is a threshold effect, where “the probability of receiving treatment changes discontinuously as a function of one or more underlying variables” (p. 201) [171]. For example, the seminal 1960 Thistlethwaite and Campbell study [174] estimated the effect of scholarship awards (treatment) on academic success (outcomes) by comparing the academic outcomes of persons who received a scholarship with those whose merit points (assignment variable) fell just short of the threshold for the award. Other, more recent applications include evaluating the social and economic impacts of policy interventions with a threshold effect [171-173], including conditional publicly funded financial credits such as social assistance for the unemployed [175]. The Ovid MEDLINE(R) 1946 to

---

<sup>7</sup> The assignment variable is sometimes alternatively referred to as the ‘forcing’ or ‘running’ variable.

Present with Daily Update electronic academic database lists 43 academic journal articles with 'discontinuity regression' or 'regression discontinuity' as a keyword, demonstrating that this method has not commonly been applied in health research to this date.

According to Hahn *et al.*, regression discontinuity methods require relatively weak assumptions compared with those necessary in other observational, including quasi-experimental, methods [171]. Hahn *et al.* identified two principal assumptions of discontinuity regression methods. The first assumption was monotonicity, namely that every participant whose assignment variable crosses over the threshold for treatment must receive the treatment [171]. The second assumption is that of excludability [171]. This refers to the requirement that the crossing of the assignment variable (or, in other words, moving into treatment) must affect the outcome exclusively through receipt of the treatment, and not through any other factors [171].

As with difference-in-differences methods, if the assumptions of a regression discontinuity design are fulfilled, then the data can be analysed with experimental methods and the regression discontinuity estimator is unbiased. However, Lee argues that regression discontinuity is superior to methods that exploit natural experiments such as difference-in-differences methods, because the researcher actively manipulates the assignment of participants to treatment and control groups, rather than purely observes random assignment as in a natural experiment [173]. He showed formally that regression discontinuity methods do not require the treatment variation that they isolate to be akin to random allocation [176]. But it suffices for this method that the observed random variation is indeed a direct consequence of the inability of participants to precisely control the assignment variable around the treatment threshold [176]. In other words, the strengths of discontinuity regression methods hinges on identifying treatments that are assigned on the basis of a known, fixed threshold and that participants are automatically assigned to, without them having the opportunity of selecting into or out of the treatment.

One limitation of discontinuity regression is that it can only estimate the effect of a treatment exposure variable on an outcome in participants locally at the point where the probability of treatment receipt changes discontinuously [171]. A related, second weakness of the method is that it confines data usage to participants located within the defined boundaries of the space around the threshold of the assignment variable. The estimator is thus necessarily less precise than that from methods that use a larger set of the data in the model. A third limitation is the limited applicability of discontinuity regression methods in situations where an individual can precisely manipulate the assignment variable [173]. For example, if recipients of an in-work

tax credit intervention were able to influence receipt of the credit by increasing their work hours to qualify for the credit, then this could lead to non-random differences between participants above and those below the threshold, potentially causing bias from (unmeasured) confounding. If participants who may have increased their work hours (the assignment variable) to receive a larger amount of credit have more motivation to work than those who did not increase their working hours, then motivation to work may confound the exposure-outcome relationship. Despite these concerns, several studies have nevertheless used discontinuity regression methods to estimate the effect of social assistance receipt on social outcomes [175, 177].

While I am not aware of a paper that has directly compared the advantages or disadvantages of fixed effects and discontinuity regression methods, I tentatively attempt such a comparison. Discontinuity regression analyses may be perceived as providing a more causal estimate than fixed effects regression analyses. However, fixed effects regression analytic methods overcome many of the limitations that may affect discontinuity regression methods. Because fixed effects models use a larger range of data (although only from participants experiencing change in the exposure variable), they provide a measure closer to an average treatment effect than the local average treatment effect that discontinuity regression provides. Related to this, due to capturing a larger extent of variation due to the increased study sample, fixed effects regression analyses are likely to produce more precise estimates, making them more statistically efficient. Finally, in the special case of research with the aim of estimating the effect of social policy on health, discontinuity regression may be biased, if participants are able to manipulate their selection into assignment to treatment, whereas this is less of a concern for fixed effects models.

The application of discontinuity regression methods was considered in this thesis, but there were reasons for prioritizing fixed effects methods. One reason was that this thesis aimed to determine an effect most akin to an average treatment effect in the treated, rather than a local treatment effect. Fixed effects regression analyses provided such a more generalizable average treatment effect in the treated, whereas a discontinuity regression design would have provided a more local estimate (i.e., the treatment effect in those participants with a marginal income around the maximum income threshold for FTC and IWTC eligibility). A second reason was the concern that regression discontinuity methods may substantially reduce the statistical power of the thesis findings, because they would have required restriction of the sample to person around the eligibility criteria. A third reason was that the risk of measurement error in the income data (see *Chapter 9*) challenged the application of a discontinuity regression

design. The perhaps main reason was though that participants may have potentially been able to manipulate their FTC and IWTC eligibility and amount, for example by increasing their employment or decreasing their income levels. As described above, discontinuity regression methods are not well equipped to deal with this issue.

### ***Marginal structural model analysis***

Marginal structural models are dedicated epidemiological methods for assessing the effect of a time-varying exposure variable (generally a treatment) on a time-varying, binary outcome variable (generally onset of a disease) [178, 179]. They have been developed to improve confounder adjustment in the specific scenario where a potential time-varying confounding variable may simultaneously also be a potential time-varying mediating variable, so that conventional methods for confounder adjustments are biased [178]. In July 2013, the *Ovid MEDLINE(R) 1946 to Present with Daily Update* electronic academic database listed 85 academic journal articles with ‘marginal structural model’ as a keyword. Of the four described methods, marginal structural modelling is thus the least commonly used in health research – at least according to this rather simple bibliometric ‘test’.

In essence, marginal structural modelling in practice involves artificially creating a ‘pseudo-population’ through a method called inverse-probability weighting [178]. This pseudo-population is exchangeable with participants who have received the treatment (or other exposure) and their ‘counterfactuals’, who are participants who are exchangeable, but have not received the treatment [178]. By creating exchangeable treatment and control groups, conventional survival analytic methods, where participants who develop the disease (or other outcome) are censored, can then be applied to estimate the effect of the time-varying exposure variable on the outcome variable [178].

The principal and strong assumption of marginal structural modelling methods is that of no unmeasured confounding (or exchangeability) [178, 179]. Thus, unbiased marginal structural model estimators essentially require either an experiment or a natural experiment, where participants have been randomly assigned to the exposure variable [178, 179]. The second strong assumption of marginal structural models is positivity (or the experimental treatment assignment assumption) [179]. Positivity is defined as the requirement that there are “both exposed and unexposed participants at every combination of the values of the observed confounder(s) in the population under study” (p. 1) [180]. Or, in other words, “observed treatment levels must vary within confounder strata” (p. 31) [181]. So, when the exposure variable is stratified by levels of the confounding variables, then each cell within each level

must contain at least one participant observation in order for a marginal structural model estimator to be unbiased.

The main strength of marginal structural models is their improved adjustment for confounders in scenarios where a potential confounding variable may potentially also mediate the exposure-outcome relationship, compared to other methods [178]. Another strength of the method is that it can be used to determine the effects of different treatment regimens [179]. For example, it can be used to compare the effect of a three-year treatment with an exposure or the effect of a pattern of exposure-nonexposure-exposure over the three year period. This may provide important comparative information for the effects of different treatment regimens, for example if the goal is to assess the longer-term effects of a policy.

The perhaps largest limitation of conventional marginal structural models is their assumptions of no unmeasured confounding and positivity [178]. Furthermore, a potential limitation in the context of panel studies may be that in conventional marginal structural models a participant is censored and thus exits the study, once she changes her outcome. Therefore, any additional changes in the outcome that occur after the first change are lost for the analysis. In panel studies with more than two repeated measures of the outcome, the marginal structural model estimator may thus be relatively less statistical efficient than alternative estimators.

To my knowledge, the comparative advantages and disadvantages of fixed effects regression analytic methods and marginal structural models have not previously been described. The principal advantage of fixed effects regression methods over marginal structural model methods may be that the former controls for all time-invariant confounding, whereas the latter cannot control for unmeasured time-invariant confounding. On the other hand, marginal structural models provide improved adjustment for confounding variables that may simultaneously also be time-varying mediators. Furthermore, in panel studies with measures repeated at more than two time points, fixed effects models may use more observations or changes in the outcome variable over time than marginal structural models, so that the former may produce more statistically efficient estimators than the latter.

This thesis prioritised fixed effects regression analytic methods over marginal structural methods for three main reasons. First, the control of unmeasured time-invariant confounding that fixed effects methods provide was seen as potentially more beneficial than the control for confounding due to adjustment of confounding variables that could also potentially be mediating variables. Furthermore, considering that this thesis analysed panel data, the fixed effects regression estimator may have potentially been advantageous in terms of statistical

efficiency. Finally, there was a concern that the marginal structural model applied in this thesis may have suffered from a breach of the positivity assumption. The reason is that the exposure variable of every person is determined fully by the combination of the potential confounding variables included in the model, meaning that observed treatment levels do not vary within confounder strata, which is a structural violation of the positivity assumption (see section 11 on page 557 in [178]).

### **Rationale for the use of fixed effects regression analyses methods in this thesis**

Several econometric and epidemiological methods are potentially tailored to the individual policy approach in political epidemiology. Described above were four of these methods for evaluating the effect of specific social policy interventions on health. This thesis introduced and explored each of these four methods with view of their suitability for application in this thesis to study the effect of FTC and IWTC on SRH in adults in New Zealand.

Ultimately, fixed effects regression analytic methods were prioritised in the thesis. The first and perhaps strongest attraction of the fixed effects regression method was its ability to control all time-invariant confounding. A second reason for prioritising fixed effects methods was that they provided a measure akin to the average treatment effect (although only in participants experiencing change in the exposure), which was regarded as preferable to the local treatment effect measures provided by other methods, such as discontinuity regression methods. A second reason was that the fixed effects regression estimator were expected to provide better statistical efficiency than other methods, such as difference-in-differences and regression discontinuity methods, as long as the idiosyncratic error was uncorrelated (which was expected) and there was substantial variation in the exposure variable (which is demonstrated in *Chapter 7*). Furthermore, concerns existed for regression discontinuity that arose from the fact that participants may potentially have been able to manipulate their selection above or below the threshold of the assignment variable (e.g., income and employment status), which is a problem that this method is not equipped to deal with. Concerns also existed that the application of marginal structural model in the thesis may have caused a structural violation of the positivity assumption, which could have biased the marginal structural model estimator. Finally, while fixed effects models themselves have multiple and strict assumptions, these may nevertheless be weaker than some of those of the other three models.

## Conclusions

This chapter locates the thesis within the emerging discipline of political epidemiology and discusses study designs and methods for studying the health effect of specific social policies. Political epidemiology is defined as the study of the effect of the political context on health. The sub-discipline can be divided into three types of approaches, differentiating studies of the health effects of: welfare state regimes; political factors; and specific individual policies. The individual policy approach, taken in this thesis, has been least practiced, but is particularly promising for SDH-focused policy-making because of its potential to inform policy development.

Randomized controlled trials, natural experiments and repeated measures studies are promising study designs for investigating the effect of individual policies on health. Randomised controlled trials of publicly funded financial credits are extremely rare, albeit feasible. Few credible natural experiments of publicly funded financial credits have been identified. This thesis uses a cohort study design to study the effect of FTC and IWTC on health.

Four methods for studying the effect of time-varying exposures on time-varying outcomes, using repeated measures of individuals are described and contrasted: Fixed effects regression, difference-in-differences, discontinuity regression and marginal structural models. Fixed effects regression analytic methods were prioritised in this thesis. The rationale for this choice included the method's suitability for answering the research question of this thesis; the efficiency of its estimator; and its strong control of confounding.

## Chapter 4: The impact of anti-poverty and in-work tax credits on health status in adults

This chapter systematically reviews evidence of the effect of anti-poverty and in-work tax credit for families interventions on health status in adults. Sixteen electronic academic databases, including the Cochrane Public Health Group Specialized Register, Cochrane Database of Systematic Review (*The Cochrane Library 2012, Issue 7*), MEDLINE, and EMBASE, as well as six grey literature databases were searched between July and September 2012 for records published between January 1980 and July 2012. Key organizational websites and reference lists of included records were also searched and academic experts contacted.

No study on the effect of anti-poverty tax credit on health status in adults was found. Five studies on the effects of one in-work tax credit (EITC in the US) on health status in adults comprising a total of 5,677,383 participants (all women) fulfilled the inclusion criteria. All included studies carried a high (as per Cochrane Collaboration protocols) risk of bias (especially from confounding and insufficient control for underlying time trends). Due to the small number of (observational) studies with a high risk of bias, this body of evidence was judged very low overall quality (as per GRADE criteria). From a more nuanced epidemiological critique, though, the overall quality may best be viewed as moderate. While most of the studies are well-designed and analyse large, nationally representative samples, evidence is limited geographically to the US; the number of studies is very limited for most outcomes; and there is considerable risk of misclassification bias of the exposure towards a null-finding, as well as some risk of confounding.

One study found that EITC had no detectable effects on SRH and mental health / psychological distress five years after its implementation (i.e., a considerable change in the generosity of EITC) and on overweight / obesity eight years after implementation. One study found no effect of EITC on tobacco use five years after implementation, one a moderate, but statistically significant reduction in tobacco use one year after implementation [odds ratio (OR) 0.949; 95% confidence interval (CI) 0.937 to 0.961] and one differential effects,

with no effect in African-Americans and a large reduction in European-Americans two years after implementation (risk difference -11.1%, 95% CI -20.9% to -1.3%). No evidence was available for the effect of in-work tax credits on mental illness and alcohol use. No adverse effects of in-work tax credits were identified.

In conclusion, the review found no evidence on the effect of anti-poverty tax credit interventions for families on health in adults and weak evidence of no effect of one in-work tax credit (EITC) interventions on health status, except for mixed evidence for tobacco use, in adult women. Most studies included in this review suffered from misclassification or mismeasurement bias of the exposure variables, which may have biased the study findings towards findings of no effect.

The following text was adapted from the Cochrane Review written as part of this thesis, led by myself and co-authored by Dr Kristie Carter, Professor Tony Blakely and Dr Patricia Lucas [28, 29].

This chapter reviews evidence on the effect of anti-poverty and in-work tax credit for families interventions on health status in adults. The chapter describes previous reviews of such tax credits and how they differed from the review presented in this thesis. It describes the methods that this review used, including the types of studies, participants, interventions and outcomes; the search strategy; and the data extraction, analysis and synthesis. It then assesses the quality of the evidence, including of the risk of bias, and presents and discusses the results of the review.

A lack of systematic reviews evidence of the health effect of publicly funded financial credit interventions has recently been identified [68]. Systematic reviews on anti-poverty tax credits and in-work tax credits on health in adults in high-income countries have not previously been conducted. Lucas *et al.* reviewed the effect of publicly funded financial credits on health in children, not adults. Ludbrook and Porter reviewed the health effects of publicly funded financial credits on adults [112], but did not tease apart the impact of anti-poverty and in-work tax credits from the impact of other types of publicly funded financial credits. Gibson *et al.*'s forthcoming systematic review, currently in protocol stage, will review the effect of welfare-to-work interventions on health and well-being in lone parents and their children [72], covering a broader range of welfare-to-work interventions (rather than focus specifically on in-work tax credits), be restricted to randomised and quasi-randomised controlled trials, (rather than include a broader range of study designs) and focus on sole parents (rather than working-age adults). The objectives of the review presented in this chapter were to assess the effects of anti-poverty and in-work tax credit for families interventions on health outcomes in working-age adults. The review of the effect of in-work tax credit for families was a Cochrane Review, following standard Cochrane Collaboration systematic review procedures [182].

## Methods

### Criteria for considering studies for this review

#### *Types of studies*

The scope of the review included randomised and quasi-randomised controlled trials, cohort studies, interrupted time series studies and controlled before-and-after studies. However, the review only included a controlled before and after study and interrupted time series studies. To reiterate, controlled before and after studies are "a non-randomised study design, where a control population of similar characteristics and performance as the intervention group is

identified. Data are collected before and after the intervention in both the control and intervention groups" [166]. Controlled before and after studies were included because they provide an opportunity to examine differences in outcomes before and after an intervention such as an anti-poverty or in-work tax credit intervention has been implemented. Including a control or comparison group provides some information about what might have happened in the absence of the intervention. To minimise the risk of bias associated with this study design type, this review only included controlled before and after studies that met the minimum methodological criteria defined in the Cochrane Effective Practice and Organisation of Care Group guidelines [183]: at least two sites in each intervention arm (that is, studies included at least two cities with the intervention versus at least two cities without); contemporaneous collection from the intervention and control groups; and comparable intervention and control sites (for example, exclude studies comparing two urban versus two rural sites).

Interrupted time series studies are "a research design that collects observations at multiple time points before and after an intervention (interruption). The design attempts to detect, whether the intervention has had an effect significantly greater than the underlying trend" [166]. Interrupted time series studies were included, because they are designed to assess the impact of interventions on health while controlling for underlying time trends. In keeping with the Cochrane Public Health Group recommendations [184], only those interrupted time series studies were included that fulfilled the following minimum methodological criteria: at least three time points before and after the intervention and a clearly defined intervention point.

A study was included, if control or comparison data were available from a group not in receipt of the tax credit intervention (for example, where the credit was newly introduced or where eligibility for the credit was expanded). Studies were also included, if the control or comparison group received a significantly smaller income amount from the tax credit than the intervention or exposure group. One example of a situation, where such a control or comparison group was used, is where the government had significantly (that is, well beyond adjustment for inflation) increased the generosity of a tax credit from one year to the next, such as the change in EITC through the *1993 Omnibus Budget Reconciliation Act* [161] described in *Chapter 3*. For studies comparing a group receiving a smaller income amount than the intervention, the review advisory panel (Professor Peter Davis and Professor Ichiro Kawachi) was consulted to establish, whether the income amount received from the intervention by the control or comparison group was 'significantly smaller' than that received by the intervention or exposure group.

### ***Types of participants, interventions and outcomes measures***

To be included, study participants had to be working-age (18 to 64 years) adults. The types of intervention that were included were anti-poverty tax credit and in-work tax credit interventions, as defined in *Chapter 2*. Eligible anti-poverty tax credits for families were: tax credits designed to reduce income poverty; unconditional (i.e., they could be needs-tested, but not conditional on up-take of employment or social services); permanent (i.e., not one-off payments) and paid to the family (not the individual). Eligible in-work tax credit interventions were: tax credits designed to reduce income poverty and increase paid employment in families on social assistance; conditional on employment; permanent; and paid to the family. Because minimum thresholds for eligibility vary by type of anti-poverty and in-work tax credit intervention, no restriction with respect to variables defining eligibility for the credits (family income, family type, number and age of dependent children, number of working hours) was applied. Tax credits paid to the individual were excluded, because they are not family-targeted and could potentially have a different health pathway and impact on adults in families than permanent tax credits. Cash payments were excluded, because they are commonly designed to exert an immediate impact and tend to be one-off or time limited, meaning that they may also have a different health effect.

The primary health outcomes of this review were SRH; mental health / psychological distress; mental illness; overweight / obesity; alcohol use; and tobacco use. This thesis assesses the effect of FTC and IWTC on SRH in its primary analyses and on psychological distress and tobacco use in subsidiary analyses, as described in detail in the next chapter. However, a review of other key health outcomes that FTC and IWTC interventions may influence, namely mental illness, overweight / obesity and alcohol use, provides important additional background information. Measures of mental health status (primary outcomes) were prioritised, because anti-poverty and in-work tax credits are likely to have a more immediate impact (that is, shorter time lag) on these measures than on measures of physical health (primarily secondary outcomes), making these more sensitive measures over the short to medium term. Potential adverse effects or harms of the interventions such as mental illness, alcohol use and tobacco use were included.

The secondary outcomes of this review were any other measures of physical health status; change in income; and change in employment. The effect of the tax credit interventions on change in income and change in employment provides key information on evidence of an effect of the tax credits on key individual-level SDH through the income pathway (pathway A-B

in **Figure 11** and **Figure 12**) and the employment pathway (pathway C-D in **Figure 12**) described in *Chapter 2*.

Subjective and objective measures of individual-level outcomes were included; any aggregate population-level outcomes were excluded. Only studies measuring at least one primary outcome were included. If studies measured several outcomes, then each outcome measured was included in the review. If studies used several measures for the same outcome, then the measure most consistent with the measure used in the other studies included in the review was used.

### **Search methods for identification of studies**

Searches were conducted between June 2012 and September 2012. The 16 electronic data bases listed in **Figure 16** were first searched to source academic journal articles. The search strategy for the *Ovid MEDLINE(R) 1946 to Present with Daily Update* is presented in Appendix 1 on p. 42 of the Cochrane Review [29] appended to this thesis. This search strategy was sufficiently broad in design to identify papers on any tax credit and specifically tailored to identifying papers on in-work tax credits. The subject heading terminology and syntax of search terms were adapted according to the requirements of individual databases. The six electronic databases of grey literature listed in **Figure 16** were searched to ensure identification of research reports not (yet) published in academic journals. The producers of an international database of anti-poverty policies, the *Poverty Reduction Database* [185], that is currently under development were also contacted for emerging findings from their review of policy. Targeted internet searches of websites of ten key international organisations and federal government departments of health and social developments in selected countries (New Zealand, United Kingdom and United States) listed in **Figure 16** were searched for publications referring to relevant tax credit interventions, such as relevant health impact evaluations. The first 30 hits on Internet search engines GoogleScholar and Scirus were also screened, using terms similar to those used for searches of bibliographic databases. Records written in any language were sought to minimize the risk of publication bias in the review.

I hand searched the issues published over the last 12 months of the three journals with studies included in the review (*American Sociological Review*, *Economic Journal*, *Health Economics*) and, for all included studies, the reference lists of all their records for additional relevant studies and records. A panel of experts in the domain of social policy and health (Professor Peter Davis and Professor Ichiro Kawachi) was also convened to inform the review. These two panel members were asked at the selection and data synthesis stages to alert me

**Figure 16: List of searched academic databases, grey literature databases and organisational webpages**

Electronic academic databases	Grey literature databases	Organisational webpages
<ul style="list-style-type: none"> <li>• Academic Search Complete</li> <li>• Business Source Premier;</li> <li>• The Campbell Library: The Campbell Collaboration (The Campbell Library, current issue)</li> <li>• CINAHL</li> <li>• Cochrane Central Register of Controlled Trials (CENTRAL) (The Cochrane Library, Issue 7)</li> <li>• Cochrane Public Health Specialised Register</li> <li>• EconLit</li> <li>• EMBASE</li> <li>• Ovid MEDLINE(R) 1946 to Present with Daily Update</li> <li>• PsycINFO</li> <li>• PubMed</li> <li>• Scopus</li> <li>• Social Sciences Citation Index</li> <li>• Sociological Abstracts</li> <li>• TRoPHI</li> <li>• WHOLIS</li> </ul>	<ul style="list-style-type: none"> <li>• The ProQuest Dissertations &amp; Theses Database</li> <li>• System for Information on Grey Literature in Europe - OpenGrey (<a href="http://www.opengrey.eu/">www.opengrey.eu/</a>)</li> <li>• The Directory of Open Access Repositories - OpenDOAR (<a href="http://www.open_doar.org/">www.open_doar.org/</a>)</li> <li>• EconPapers (<a href="http://www.econpapers.repec.org">www.econpapers.repec.org</a>)</li> <li>• Social Science Research Network - SSRN eLibrary (<a href="http://www.ssrn.com/">www.ssrn.com/</a>)</li> <li>• National Bureau of Economic Research (<a href="http://www.nber.org/">www.nber.org/</a>)</li> </ul>	<ul style="list-style-type: none"> <li>• International Labor Organisation (ILO; <a href="http://www.ilo.org/">www.ilo.org/</a>), and World Bank (<a href="http://www.worldbank.org">www.worldbank.org</a>).</li> <li>• OECD (<a href="http://www.oecd.org">www.oecd.org</a>),</li> <li>• United Nations Economic and Social Council (ECOSOC; <a href="http://www.un.org/en/ecosoc/">www.un.org/en/ecosoc/</a>)</li> <li>• World Bank (<a href="http://www.worldbank.org">www.worldbank.org</a>).</li> <li>• federal government departments of health and social development (New Zealand, United Kingdom, United States))</li> </ul>

to any relevant published or unpublished completed or on-going studies they were or became aware of during the course of the review.

## **Data extraction, analysis and synthesis**

### ***Data extraction***

A research librarian assisted the database search for relevant literature. After removal of duplicates, the titles and abstracts of all identified records were initially screened for relevance. All records of interest, including those without abstracts, but with titles suggesting their potential relevance, were selected for further consideration. Abstracts of potentially relevant records were then screened for eligibility. All records selected for full text screening were retrieved. Records written in languages other than those spoken by the authors (English, French and German) were translated into English. It was then determined whether records undergoing full text screening met eligibility criteria for inclusion in the review.

Data from the included studies were then extracted for analysis. The data extraction form recommended by the Cochrane Public Health Group [184] was modified to suit the purposes of the review and used to extract data. The categories of data extraction included: study eligibility (including study characteristics such as study type, participants, type of intervention, duration of intervention and types of outcomes measures), study details (including study intention and methods), results (including participants and subgroups), intervention groups (including group names) and other relevant information (including potential harms of the intervention, potential conflicts of interest and issues affecting directness). Information on the context, implementation, cost and sustainability of the tax credits was also extracted, where available. Data on key socio-demographic characteristics of participants at baseline and at the endpoint within and beyond the PROGRESS framework [184] were also extracted to enable equity impact assessment, as were data on potential measured confounders and comparison groups. Data were stored and managed using RevMan5 [186].

The risk of bias in the controlled before and after study and the interrupted time series studies was assessed by applying the risk of bias criteria of the Cochrane Effective Practice and Organisation of Care Group [187]. An item assessing whether the study appropriately controlled for confounding was added to these criteria for assessing studies with a separate control group (that is, the controlled before and after study). Risk of bias was assessed and reported at the outcome level, first for each outcome for each study and then for each outcome across all studies.

## Chapter 4: Systematic review

In the included study with separate control groups (that is, the controlled before and after study), measures of treatment effect for the dichotomous health outcome were reported as risk differences between treatment or exposure and control groups. In the included studies without separate control groups (that is, interrupted time series studies), measures of treatment effect for dichotomous and continuous outcomes were reported as risk ratios, odds ratios or risk differences. The records of the four included studies that reported risk differences or odds ratios did not provide data that enabled calculation of the risk ratio. Therefore, the principal study authors of these studies were contacted by e-mail and a risk ratio measure or the information needed to calculate the risk ratio was requested. Since the risk ratio could not be established for any of these studies, the measure of treatment effect that the studies reported in the records were reported in this thesis. It is worth noting that these risk ratio and difference measures differed from the fixed effect regression estimator that I calculated in this thesis, which assesses the change in the outcome that becoming eligible for the tax credit or an increase by \$1,000 in the tax credit amount and is described in detail in the next chapter. Therefore, the effect measures presented in this chapter are not directly comparable with those presented on FTC and IWTC in *Chapter 7*.

If studies presented either or both of adjusted and unadjusted measures of treatment effect, the adjusted treatment effect measures were used for data synthesis purposes. If only unadjusted measures of treatment effect were presented, the crude effect measures were adjusted for baseline between-group differences in covariates and potential confounders or the principal study author was contacted by e-mail or phone and the adjusted treatment effect measures requested. If intention-to-treat analyses were conducted, then treatment effect measures from these analyses were prioritised. For example, if a cohort study presented effect estimates both for the impact of an amount of tax credit that a family was *eligible for* and of the amount that the family *received*, then the former estimate was prioritised.

For studies that did not report 95% confidence intervals, but reported standard errors, the 95% confidence interval was calculated. For studies that neither reported confidence intervals nor standard errors, the principal study author was contacted by email and the 95% confidence interval or standard error was requested. The studies were screened for unit of analysis issues arising from randomisation or allocation of participant clusters, individuals undergoing more than one intervention and multiple observations for the same outcome. No unit of analysis issues were identified in any of the included studies.

All relevant missing information on the study methods, outcomes and statistical measures required were requested from the principal study authors by e-mail. For the included studies, detailed information on the following missing data was requested: individuals missing from the study due to survey non-response; missing outcome, exposure and covariate data for each survey or at each survey wave; risk ratio measures; and subgroup analyses for all relevant characteristics (ethnicity, family type, gender, income). The requested information (except for the risk ratio measure and subgroup analyses) was received for the Strully 2010 study [188] (K. Strully, personal communication, 18 January 2013), but not for the other studies. If missing information and data could not be retrieved, only the available data were analysed, and the potential impact of the missing data assessed and reported.

### ***Data analysis and synthesis***

The review experienced both methodological heterogeneity and clinical heterogeneity of the included studies. Methodological heterogeneity included differences in study designs (that is, controlled before and after study versus interrupted time series studies), features of studies that affected their risk of bias and statistical methods. Clinical heterogeneity included variability in the specific definition and measurement of participants, interventions and outcomes. Methodological and clinical heterogeneity resulted in significant statistical heterogeneity of studies included in the review. Because studies differed considerably in their study designs, participants and outcomes (and especially also because all studies had a high risk of bias), they were not combined in meta-analysis.

Missing eligible studies from this review could lead to publication bias. To avoid missing eligible studies, I employed a comprehensive search strategy that included not only academic databases, but also several databases of grey literature and dissertations and theses as well as the Cochrane Central Register of Controlled Trials and the Cochrane Public Health Specialised Register. Furthermore, key experts, including the review advisory board members, were contacted and requested to identify unpublished studies. Any eligible, unpublished studies that were identified were included in the review. To minimize language bias, the review included records written in any language. Because the review identified fewer than 10 eligible studies, it was not feasible to produce a funnel plot to more formally investigate the risk of reporting bias in this review. However, the risk of reporting bias was assessed on the basis of the information available such as the number of eligible, unpublished studies and non-English language records identified in the review and whether statistically insignificant effect estimates were reported in the included studies.

The quality of the evidence for each outcome and, ultimately, of the entire body of evidence included in this review was assessed by considering risk of bias, study limitations, consistency of effect, imprecision, and indirectness [184]. For this purpose, the strict Cochrane Collaboration standard protocols laid out in the Cochrane handbook for systematic review of interventions were applied [182]. For example, with regards to critical appraisal of the risk of bias, these standard protocols require that a 'low', 'high' or 'unclear' rating is assigned [182]. The protocols advise against a more finely nuanced assessment of risk, referring to evidence that demonstrates inconsistent interpretation of and appraisal along a larger number of more nuanced categories [182]. 'Low risk of bias' thus indicates no or negligible risk of bias, whereas a moderate to high risk of bias must be graded as 'high risk' [182]. If the risk of bias cannot be judged (e.g. due to the study providing insufficient data), this is classified as an 'unclear' risk [182]. Considering the few grading options available, presence of some risk of bias requires that the assessor grades a study as 'high risk of bias'. Similarly, unlike epidemiological frameworks, Cochrane protocols prohibit informed estimates of the size or direction of an identified bias [182].

Cochrane Collaboration protocols require that the GRADE criteria [189] are applied to assess the overall quality of the evidence included in a review [182]. According to GRADE guidelines, the quality of a body of evidence is defined as "the extent to which one can be confident that an estimate of effect or association is close to the quantity of specific interest" (p. 361) [182]. Assessing the quality of a body of evidence involves considering risk of bias within studies, the directness of the evidence, heterogeneity between studies, precision of effect estimates and risk of publication bias [182]. The GRADE criteria are a four-level grading system (from 'high' over 'moderate' and 'low' to 'very low'). A 'high' quality assessment is generally reserved for randomized controlled trials, while observational studies are generally classed as 'low' quality. Considerable strength of study methods can lead to an up-grading of studies, while considerable limitations (such as risk of bias) can lead to a downgrading of the evidence. The GRADE criteria are applied to assess the quality of the evidence on each outcome, even if this body only includes one study, as is the case for several outcomes in the review of this thesis. In a second step, the same criteria are then applied to judge the entire body of evidence in a review.

These Cochrane Collaboration protocols for critical appraisal of included studies and assessing the overall quality of the review evidence are strict, perhaps reflecting a bias for clinical trials and against observational, epidemiological studies. Therefore, while grading according to the standard protocols ensures inter-review comparability, when the criteria are rigorously

applied, many observational studies are necessarily graded as carrying a 'high' risk of bias and being of 'low' or 'very low' overall quality, sometimes despite being well designed. The Cochrane protocols were adhered to in this review, but some information is presented in this thesis that would not be included in the review to bridge the gap between the Cochrane protocols and an epidemiological framework of critical appraisal. The very low (as per Cochrane Collaboration criteria) quality of the included studies (and also the considerable level of heterogeneity in study design, participants and outcome measures) prohibited meta-analysis of two or more studies reporting the same outcome measure. Consequently, the study results were summarised in a narrative synthesis. The narrative synthesis reports results separately for each outcome. No one particular study is emphasised, to avoid introducing bias. Since no meta-analysis was conducted, sensitivity analyses were also not performed.

The impact of anti-poverty and in-work tax credit for families interventions might meaningfully differ between populations defined by ethnicity, family type (one-parent family, two-parent family), gender (female, male) and income (for example, after-tax personal income or family income). However, the small number of studies included in the review prohibited meaningful subgroup analyses.

## Results

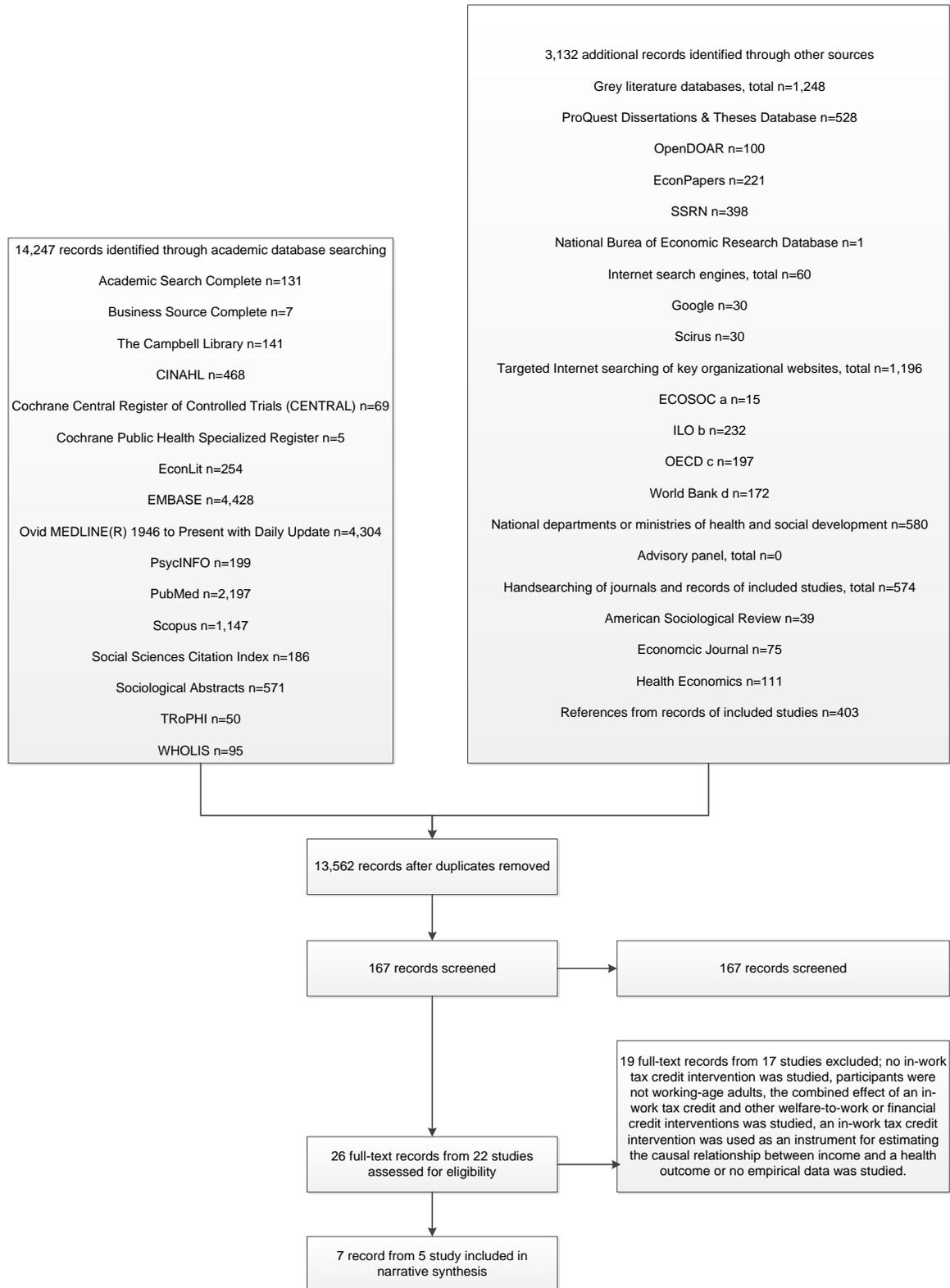
### Description of studies

#### *Results of the search*

A flow-chart of the results of the search, including detailed break-downs of results by individual database or other searched sources is presented in **Figure 17**. The search of the 16 electronic academic databases identified a total of 14,247 records. After removing duplicates, a total of 10,430 records remained. Of these records, after initial title and abstract screening, 122 records were considered potentially relevant. After detailed abstract screening, 16 records were still considered potentially relevant. After full-text screening of these records, no record of an eligible anti-poverty tax credit intervention and one record of one study of an eligible in-work tax credit intervention fulfilled inclusion criteria and was included in the review [188].

Searches of other sources identified a total of 3,132 additional records. The five electronic grey literature databases identified a total of 1,248 potentially relevant records; the search of the international *Poverty Reduction Database* identified 0 records; the two Internet search

Figure 17: Flow diagram of the systematic search of databases and other sources



engines identified 60 records; and the targeted searches of the ten web sites of international organisations and federal departments or ministries of health and social development identified 1,196 records. Of these records, nine records of seven studies were considered potentially relevant and underwent full-text screening. No record of an eligible anti-poverty tax credit intervention and five records of four additional studies of eligible in-work tax credit interventions met inclusion criteria and were included in the review [84, 158-160]. I became aware of one additional record of the Averett 2012 study published in an academic journal during the course of the review [85], which was also included in the review.

Hand-searching of the 225 publications published over the previous 12 months (January 2012 to December 2012) in the three academic journals with an eligible study (American Sociological Review, Economic Journal, Health Economics) and of the 403 references of the seven records of the five included studies of in-work tax credit interventions identified no additional eligible records or studies. The advisory group and other experts contacted also did not identify any additional eligible studies. Meta-analyses of two or more included studies with the same outcome were not conducted because of the high risk of bias in each of the included studies and the considerable statistical heterogeneity of the included studies. The included studies were synthesised narratively.

### ***Included studies***

Five studies with a total of 5,677,383 participants fulfilled the inclusion criteria. Four included studies were interrupted time series studies, namely the Cowan 2011 [158], Evans 2011 [160], Gomis-Porqueras 2011 [159] and Strully 2010 studies [188], and one study was a controlled before and after study, namely the Averett 2012 study [84, 85]. Two studies used difference-in-differences methods [159, 188], and three studies used triple differences methods [84, 85, 158, 160], with fixed effects.

If a study reported both difference-in-differences and triple differences models, then the triple differences model was prioritised. The rationale was that whereas the difference-in-differences model did not, the triple differences model did control for the differences in underlying trends between the treatment and control groups, as discussed in *Chapter 3*. For example, the Evans 2011 study [160] estimated the effect of EITC on the prevalence of excellent or very good SRH. It first subtracted the difference in the prevalence of excellent or very good SRH in women with two or more children (potentially affected by an increase in EITC) from that in women with one child (not affected by the EITC increase), within the subgroup of low-education women, who were considered to be EITC-eligible (the first

differences). It then calculated the same difference for the subgroup of medium-education women (EITC-ineligible), and subtracted the latter difference from the former (difference-in-differences). Finally, the same difference was calculated in a third subgroup, high-education women (never EITC eligible) and was subtracted from the difference-in-differences estimate to adjust for underlying time trends between low- and medium-educated women (triple differences). Thus, the triple differences model provided the highest level of control of different (by subgroup) underlying time trends that could confound then estimate of EITC impact on SRH.

Because triple differences models assess the differential impact of a policy intervention on health by the control variable, triple differences estimators are also a measure of the impact of the policy on equity in health by the control variable. In the above example taken from the Evans 2011 study [160], the EITC increased for adults with two or more children, but did not increase for parents with one child. The study differenced the differences between these groups in three educational sub-groups. The rationale for studying the differences in educational subgroups was that education was seen as a marker for EITC eligibility status. Low-education women were seen as potentially eligible, while medium educated women were seen as potentially EITC-ineligible and high-education women as never EITC-eligible. Thus the estimate for the effect of policy is assumed to be differential by these educational groups, and the estimator of this analysis is one of both policy impact and differential impact on different education groups (impact on equity).

For the primary outcomes analyses, three interrupted time series studies extracted data from repeated nationally representative cross-sectional surveys of the United States population (the Cowan 2011 [158] and Evans 2011 [160] studies: the Behavioral Risk Factor Surveillance System, 1993-2001; the Gomis-Porqueras 2011 study [159]: the National Health Interview Survey, 1982-2004). The fourth interrupted time series study, the Strully 2010 study [188], used birth records collected routinely as part of vital statistics of the United States population (the United States Natality Detail File, 1988-2002). While suitable data were available from vital statistics for most United States states for all years studied, such data were unavailable for selected United States states (that is, California, Indiana, Louisiana, Nebraska, New York, Oklahoma, South Dakota, Washington) for selected years of the study period. The controlled before and after study, the Averett 2012 study [84, 85], used two waves of a longitudinal survey (National Longitudinal Survey of Youth 1979 cohort, 1992 and 1998).

The secondary outcomes analyses of the four interrupted time series studies used data from repeated nationally representative cross-sectional surveys of the United States population,

that is the Behavioural Risk Factor Surveillance System, 1993-2001 [158, 160]; the National Health Interview Survey, 1982-2004 [159]; the National Health and Nutrition Examination Survey III, 1999-2004 [160]; and the March Current Population Survey, 1980-2002 [188]. The controlled before and after study, the Averett 2012 study [84, 85], used the 1992 and 1998 waves of the National Longitudinal Survey of Youth 1979 cohort.

The included studies defined comparator groups in two different ways. Four studies defined the control group as participants receiving a significantly smaller increase in the amount of income from the in-work tax credit intervention than the exposure group in the 1996 expansion of the EITC [84, 85, 158-160]. These studies took advantage of the *1993 Omnibus Reconciliation Act* [161], described in *Chapter 2*, which increased the maximum amount of income from EITC by between \$800 and \$1,327 more for families with two or more children than for those with one child in 1996 [160]. One study defined the control group as participants residing in states without a federal EITC and the exposure group as participants residing in states with federal EITC [188].

### Participants

In their primary outcome analyses, the Averett 2012 study [84, 85] analysed data from 3,365 participants; the Cowan 2011 study [158] from 173,811 participants; the Evans 2011 study [160] from 127,209 participants; the Gomis-Porqueras 2011 study [159] from 111,301 participants; and the Strully 2010 study [188] from 5,260,202 participants. Most studies used the same samples for their secondary outcomes analyses; however, in addition, the Evans 2011 study [160] also analysed data from 2,683 and 3,090 participants from the National Health and Nutrition Examination Survey III, and the Strully 2010 study [188] analysed 66,542 participants from the Current March Population Survey. The country setting of all included studies was the US, and all studies were nation-wide. However, as mentioned above, the Strully 2010 study lacked data from selected United States states for selected years [188].

All studies were restricted to women, despite the intervention being available to women and men. The Gomis-Porqueras 2011 [159] and Strully 2010 [188] studies were further restricted to women not co-habiting with a partner and unmarried mothers, respectively. This restriction is justifiable, considering that the EITC was specifically focused on improving the economic and social well-being of single mothers. All samples were of working age, defined as 20 to 64 years [159]; 21 to 40 years [158, 160, 188]; and 27 to 35 years in 1992 and 33 to 41 years in 1998 [84, 85]. The Averett 2012 sample was stratified by African-American and European-

American ethnicity [84, 85], and the Gomis-Porqueras 2011 sample by high school or less education and some college education [159].

The authors did not report equity impact directly. However, the equity impact by ethnicity could be determined in the Averett study [84, 85] due to the provided stratified analyses. Furthermore, the difference-in-differences or triple differences estimators of the Averett 2012 [84, 85]; Cowan 2012 [158], Evans 2011 [160], Gomis-Porqueras 2011 [159] by design provided estimates of impact on equity by education, as explained above.

### Interventions

The primary and secondary outcomes analyses of all included studies investigated the same in-work tax credit for families intervention: federal and/or state EITC in the US. The federal EITC was introduced in 1975 as a permanent in-work tax credit. It has since been extended in its generosity several times, most notably in 1996 through the *1993 Omnibus Budget Reconciliation Act* [161]. State EITC have been introduced and their generosity changed in various states at various times. The EITC intervention is means-tested to low-income groups, with phase out starting at 17-42% of average income from wages, depending on family type [18]. The intervention is amongst the most generous in-work tax credit interventions internationally, providing up to 7% (\$3,298) and 11% (\$5,128) of an average income from wages (\$47,116 in 2012; [190]) for families with one dependent child and with two dependent children, respectively [18]. In 2011, over 27 million individuals received nearly \$62 billion in EITC, lifting an estimated 3.3 million adult individuals out of poverty [191]. The EITC achieved a high up-take (four out of five individuals eligible for EITC also received the credit), and its administration costs were less than one per cent of its total costs [191].

### Outcomes

The included studies investigated the impact of in-work tax credit on the following four primary outcomes: self-rated general health [160]; mental health / psychological distress (number of bad mental health days; [160]); overweight / obesity [159]; and tobacco use (current: [84, 85, 158]; during pregnancy: [188]). No data were available on two primary outcomes: mental illness and alcohol use. Outcomes falling within the three secondary outcomes categories were also investigated: physical health outcomes (number of bad physical health days; number of risky biomarkers for inflammation, cardio-vascular disease and metabolic disease; [160]); change in income [188]; and change in employment [84, 85, 158-160, 188]. Outcomes lagged behind the implementation of (i.e., a considerable increase in) the EITC intervention by one year in the Strully 2010 study [188]; two years in the Averett

2012 study [84, 85]; five years in the Cowan 2011 [158] and Evans 2011 [160] studies and eight years in the Gomis-Porqueras 2011 study [159].

### ***Excluded studies***

The 19 records from 17 studies that underwent full text screening, but did not fulfil the inclusion criteria, were excluded for five principal reasons. Nine studies were excluded because they did not specifically examine in-work tax credit for families interventions [192-200]. Three of these studies reported that they had combined two or more non-specified publicly funded financial credits [80, 192, 198, 199]. I contacted the principal authors of the primary records for these three studies by email and requested information on whether the combination of publicly funded financial credits they studied included one or more in-work tax credit interventions. For the Ajrouch 2010 study, information on the types of publicly funded financial credits included in the study was not available (K.J. Ajrouch, personal communication, 29 August 2012). Three records from three studies [78, 201, 202] were excluded because they used an in-work tax credit intervention as an instrumental variable to estimate the impact of income on health, so that the research question and regression equations did not estimate the impact of in-work tax credit on health. (Using EITC as an instrument, the studies found that income increased smoking [201]; had no effect on SRH and functional limitations [78]; and had no effect on obesity in men, but increased obesity in women [202].) Four records from four studies were excluded, because they did not study working-age adults [188, 203-205]. One study with two records was excluded, because it investigated the combined effect of an in-work tax credit and a set of other welfare-to-work interventions [116, 206] and another, because it did not study empirical data [207].

### ***Risk of bias in included studies***

The general risk of bias in this review was high (as explained below), with little variability in the risk of bias across the included studies. All studies carried an unclear or high risk of bias from selection; high risk of bias from misclassification of the exposure; unclear or high risk of bias from attrition; high risk of bias from unmeasured or unadjusted confounding; high risk of bias due to insufficient control for underlying time trends (except the Averett 2012 study [84, 85]); and did not control for reverse causation.

#### **Selection bias**

Selection bias occurs when the effect measure for the exposure variable on the outcome variable (e.g., risk ratio, odds ratio or risk difference) is different in the group of participants

who participated in the analysis than in the wider group of persons who were eligible for inclusion in the analysis [1, 208] (for a detailed description see *Chapter 9*). None of the included studies collected or reported information on non-responders or those lost to follow up that would enable an empirical investigation of whether the in-work tax credit intervention effect in study participants differed from that in non-responders or those lost to follow up. In the absence of such information, although the extent of initial survey non-response and loss to follow up *per se* do not indicate the lack or presence of selection bias, it could be argued that relatively larger non-response and relatively larger loss to follow up carry a relatively larger risk of selection bias. Four studies did not report survey response rates (for the controlled before and after study, at wave one) [84, 85, 158, 160], adding further to the inability to assess the extent of selection bias in these studies. One study used a near-complete sample of the United States population of women giving birth, but a high percentage of the sample had missing values for the outcome (23%, 1,577,080 records; K. Strully, personal communication, 18 January 2013) [188]. If participants who had missing values had a different EITC-smoking effect than those who contributed data, then this introduced selection bias, suggesting some risk of selection bias in this study. Thus, if values were not missing at random, but were missing systematically, then this carries some risk of selection bias. The controlled before and after study, the Averett 2012 study [84, 85], did not report the percentage of participants who were lost to follow up, indicating an unclear risk of selection bias due to loss to follow up. In summary, the overall risk that this review suffers from selection bias due to initial non-response and loss to follow up is therefore unclear, with some risk of bias (to be classified as 'high' according to Cochrane Collaboration protocols) established for the Averett 2012 [84, 85] and Strully 2010 [188] studies. However, from an epidemiological perspective, considering that most of the studies found very small effects, a large effect would have to have occurred in non-responders or participants lost to follow up to meaningfully change the findings of these studies.

### Misclassification bias of the exposure and the outcome variables

Misclassification is defined as the "the erroneous classification of an individual, a value or an attribute into a category other than that to which it should be assigned" (p. 157) [1] (for a detailed description see *Chapter 9*). Regarding misclassification in the exposure assessment, four studies [84, 85, 158-160] defined their exposure as eligibility for an increase in EITC after the implementation in 1996 of the 1993 *Omnibus Reconciliation Act* [161]. These studies operationalised this definition as: having a low level of education and co-habiting with two or more dependent children. A low level of education was defined as: high school or less

education and some college education [159]; some college education or less [158, 160]; and 12 or fewer years of education [84, 85], respectively. That the definition includes having two or more children is based on the fact that the *1993 Omnibus Reconciliation Act* [161] provided a relatively larger increase in EITC to families with two or more children than to those with one child, as described in *Chapter 3*. However, the second aspect of the exposure definition (that is, low education) is extremely crude and not robust, because EITC eligibility is defined by factors other than level of education, such as family income, family type and number of dependent children in the family. Therefore, I consider these four studies to have a high risk of bias from misclassification of the exposure. The Strully 2010 study [188] defined the exposure as living in a state that had an EITC in a given year. Considering that a substantial and (potentially systematically) varying (over time and per state) proportion of study participants included in this study will not be eligible for EITC, I consider the Strully 2010 study [188] as also having a high (as per Cochrane Collaboration protocol) risk of bias from misclassification of the exposure. Assuming that many respondents who truly received EITC were falsely classified as not receiving it (i.e., less than perfect sensitivity), and vice versa many respondents truly not receiving EITC were falsely classified as receiving it (i.e., less than perfect specificity), and this misclassification was non-differential by health outcomes, then these studies should have underestimated the effect of EITC (other biases ignored) (for a review of different types of misclassification bias in longitudinal surveys is provided see *Chapter 9*).

Since the primary outcome measures of all studies are self-reported, each has some (but assumed low) risk of bias from misclassification of the outcome. Since tobacco use is socially stigmatised, the three studies of tobacco use have some (but assumed low) risk of social desirability bias of the outcome [84, 85, 158, 188]. For example, if smoking became relatively more socially stigmatized in states without EITC receipt than in states with EITC receipt, then this could have led to differential misclassification bias of the outcome variable by the exposure. A theoretically plausible underlying mechanism may have been that states with more public funds were more likely to provide EITC and at the same time more likely to invest in social marketing campaigns for decreasing the public acceptability of smoking. Thus, if increasingly more smokers misreported their smoking in EITC states (due to increased social stigma attached to smoking), whereas the rate of underreporting remained the same in non-EITC states, leading to differential misclassification bias of the outcome by the exposure. The Gomis-Porqueras 2011 study of obesity adjusts its outcome assessment for social desirability bias, using a validated tool, suggesting a low risk of social desirability bias of the outcome

[159]. Overall, this suggests that this review has a high (as per Cochrane Collaboration protocols) risk of misclassification.

#### Reporting bias

Reporting bias (or publication bias) is “a bias caused by only a subset of all the relevant data being available” [166] This review searched several electronic grey literature databases and web sites of key international organisations and government departments to identify studies not published in records indexed in the several electronic academic databases searched. Several experts were also contacted with the request to identify unpublished studies. Despite these extensive efforts, no eligible unpublished studies were identified. All studies included in the review reported both statistically significant and statistically insignificant results. Therefore, the risk of reporting bias is considered low.

#### Bias from confounding

All interrupted time series studies used state-level and/or county-level fixed effects, controlling for all time-invariant confounding in states and or counties [158-160, 188]. The controlled before and after study used individual and state fixed effects, controlling for all time-invariant confounding in individuals and states [84, 85].

The primary and secondary outcome analyses of the four interrupted time series studies adjusted for several time-invariant and time-varying confounding variables. Most of these studies used state fixed effects to control for some state-level effects. However, because the studies used repeated cross-sections capturing different samples at each cross-section, they were required to consider both time-invariant and time-varying confounding.

All interrupted time series studies adjusted for confounding by some key *individual-level* factors, including age; ethnicity (except the Averett 2012 study [84, 85], which however stratified analyses by ethnicity); education (except the Gomis-Porqueras 2011 study [159], which stratified analyses by education); and number of dependent children in the family (except the Strully 2010 study [188], which adjusted for number of previous births). While none of the analyses adjusted for family type, the Gomis-Porqueras 2011 study [159] was restricted to women not co-habiting with a partner, the Strully 2010 study [188] was restricted to unmarried mothers, and all studies (except for the Averett 2012 study [84, 85]) adjusted for marital status.

Since income and employment determine eligibility for in-work tax credit for families *and* may be changed by in-work tax credit for families over time, these variables are potential time-

varying confounding variables and/or mediators. Therefore, including income and / or employment in regression analyses to adjust for potential confounding could over-adjust for mediation; the direction of this bias is likely to underestimate the association between in-work tax credit and health status, but its exact magnitude is unclear. This also presented a methodological challenge for this thesis and is discussed in more detail later in the thesis, including in the next chapter and in the chapter discussing bias in the main fixed effects regression analyses of this thesis (*Chapter 9*). One study [84, 85] adjusted for income, and no study adjusted for employment status. This suggests that all studies could be biased by confounding by income (except the Averett 2012 study [84, 85]) and employment. Addressing this issue requires dedicated methods such as marginal structural model analysis [178], as discussed in the previous chapter.

Three studies [84, 85, 159, 188] also adjusted for confounding by time-varying *state-level* variables. Variables adjusted for included: Generosity and / or coverage of social assistance payments such as Aid to Families with Dependent Children / Temporary Assistance for Needy Families and food stamps (all three studies); government policies and taxes such as minimum wage policy [159, 188] and cigarette taxes [84, 85]; income variables such as average earnings from wages [159] and poverty rates [188]; unemployment rate [84, 85]; and prices for groceries, fast food and cigarettes [159]. While the Gomis-Porqueras 2011 study [159] adjusted for a wide range of time-varying state-level confounders, the Averett 2012 [84, 85] and Strully 2010 [188] studies adjusted for a limited range of these confounders, and the Cowan 2011 [158] and Evans 2011 [160] studies did not control for any time-varying state-level variables.

This thesis is at lower risk of time-varying ecologic confounding, because New Zealand is one jurisdiction – any change in other policy variables such as FTC and IWTC will affect everyone at the same time. This suggests little risk of time-varying ecologic confounding for individuals changing their FTC and IWTC eligibility over time. However, confounding could have still occurred, if policies changed simultaneously with changes in FTC and IWTC.

The treatment and control groups differed in health outcomes and demographic characteristics at baseline in the Averett 2012 controlled before and after study [84, 85]. In summary, different studies adjusted for a different set of confounders, often despite using the same data. I consider that all included studies had some risk of bias from unmeasured or unadjusted confounding (to be classified as ‘high’ according to Cochrane Collaboration protocols).

### Bias from underlying time trends in interrupted time series studies

For all four interrupted time series studies, the intervention was unlikely to alter data collection. The outcome data used in the analyses as the pre-intervention measure preceded and those used as the post-intervention measure followed the implementation of the intervention. One exception was that the first, 'pre-implementation' measure used in the difference-in-differences analyses of the National Health and Nutrition Examination Survey III was for 1999, 3 years *after* the implementation of the intervention in 1996 [160], which raises questions about the validity of these analyses.

Using the *1993 Omnibus Reconciliation Act* [161] as a natural experiment of the EITC, three studies adjusted for trends over time between in-work tax credit eligible mothers (with two or more dependent children; the treatment group) and ineligible mothers (with one dependent child; control group) in potentially in-work tax credit eligible, low-education mothers [158-160]. However, this adjustment for time trends only produced unbiased estimates under the assumption that trends in health outcomes over time did not differ between mothers with two or more dependent children (treatment group) and mothers with one dependent child (control group). In other words, if trends in the health outcome between the treatment group and control group differ, then this could be misinterpreted as a treatment effect. However, adjusting for time-varying confounding variables may adjust for this bias if these time-varying confounding variables were (correlated with) causes of varying trends. The Cowan 2011 [158] and Evans 2011 [160] studies further adjusted for underlying time trends between the treatment and control groups by using a second control group (that is, never in-work tax credit eligible, high-education mothers). In order for this method to robustly control underlying time trends, the assumption must be made that the underlying trends over time in the outcome did not differ between low-education and high-education mothers. Therefore, all of these three studies carried a risk of bias from unmeasured or unadjusted confounding due to insufficient control for underlying time-trends. While I am not aware of evidence for actual differences in underlying time trends, it is standard Cochrane Collaboration protocol to grade concerns for biases as indicative of (high) risk of bias (being the only alternative to low risk of bias) [182].

The Strully 2010 study [188] adjusted for trends over time in the health outcome between women living in a state with an EITC (treatment group) and women residing in a state without an EITC (control group), but did not adjust for underlying, differential trends between the treatment and control groups. The review carried an overall high (as per Cochrane Collaboration protocols) risk of bias from unmeasured or unadjusted confounding due to

insufficient control for underlying time trends in the four included interrupted time series studies.

#### Reverse causation

Studies indicating no effect of in-work tax credit interventions on health status are unlikely to be affected by reverse causation, because they have not demonstrated any effect. However, none of the included studies formally investigated, controlled or tested for the presence of reverse causation.

### **Effects of interventions**

For the key measure of each primary outcome of the review, the study, statistical model, subgroup, baseline measure,  $\beta$  coefficient (odds ratio, risk difference), 95% confidence interval, number of study participants, number of studies and overall quality assessment (applying the GRADE criteria) are presented in **Table 4**. No studies estimating the impact of in-work tax credit on mental illness and alcohol use were identified.

As shown in **Table 4**, the first outcome, SRH, was presented only in one included study, the Evans 2011 study [160], with 127,209 participants analysed. The study dichotomized SRH into excellent or very good vs. good, fair or poor SRH, and analysed the outcome using an ordinary least square regression model with an identity function (assuming a normal distribution of the error term). The beta coefficient of this analysis of -0.005 (95% confidence interval -0.027 to 0.018) provided a risk difference. Because the outcome is a binary variable (essentially giving the prevalence of excellent or very good SRH), the coefficient can be interpreted as the percentage point change in the outcome that resulted from the EITC increase in mothers eligible for this increase. Thus, the best estimate from this model was a statistically not significant decrease in the prevalence of very good or good SRH by 0.5 percentage points in participants affected by the EITC increase. Or, to provide a more nuanced interpretation, the subgroup of potentially EITC-eligible women (low-education mothers with a high school degree or less) had a 0.5 percentage point decreased prevalence of reporting excellent or very good SRH five years after the 1996 increase, when compared to women ineligible for the EITC increase (those with one child; 95% CI -2.7 to 1.8 percentage points). This equates to a statistically not significant decrease from 58.2 to 57.7 percent of women in the treatment group reporting excellent or very good SRH (assuming the pre-treatment mean in the treatment group and that only the EITC increase had an effect on the SRH over time). This estimate provides an unbiased estimate under the assumption that the pre-treatment and

Chapter 4: Systematic review

**Table 4: Summary of findings table, in-work tax credit for families interventions, primary outcomes**

Outcomes (Measure)	Study	Statistical model (link function)	Sub- groups	Baseline rate / risk	Treatment effect estimate (95% CI)	No of participants / observations (studies)	Quality of the evidence (Cochrane / GRADE)	Comments
Self-rated general health (Excellent or very good vs. good, fair or poor)	Evans 2011	OLS (identity)		0.58	<b>No effect</b> <b>RD -0.005</b> (-0.027 to 0.018)	127,209 participants (1 study)	Very low	
Mental health / psychological distress (Number of bad mental health days in past 30 days)	Evans 2011	Neg. Bin. (logarithmic)		4.52	<b>No effect</b> <b>RR 0.940</b> (0.815 to 1.084)	127,209 participants (1 study)	Very low	
Mental illness								No evidence
Overweight / obesity (Overweight; BMI≥25 vs. BMI<25)	Gomis- Porqueras 2011	OLS (identity)	High school or less education	0.52	<b>No effect</b> <b>RD -0.019</b> (-0.052 to 0.014)	59,756 participants (1 study)	Very low	
	Gomis- Porqueras 2011	OLS (identity)	Some college education	0.41	<b>No effect</b> <b>RD -0.026</b> (-0.061 to 0.009)	51,545 participants (1 study)		
(Obesity; BMI≥30 vs. BMI<30)	Gomis- Porqueras 2011	OLS (identity)	High school or less education	0.25	<b>No effect</b> <b>RD -0.015</b> (-0.050 to 0.020)	59,756 participants (1 study)		
	Gomis- Porqueras 2011	OLS (identity)	Some college education	0.18	<b>No effect</b> <b>RD -0.013</b> (-0.050 to 0.024)	51,545 participants (1 study)		
Alcohol use								No evidence

## Chapter 4: Systematic review

---

Tobacco use							
(Any vs. no current tobacco use)	Cowan 2012	OLS (identity)		0.34	<b>No effect</b> <b>RD -0.012</b> (-0.036 to 0.012)	173,811 participants (1 study)	Very low
	Averett 2012	OLS (identity)	African-American	0.27	<b>No effect</b> <b>RD -0.043</b> (-0.141 to 0.055)	1,404 participants (1 study)	
	Averett 2012	OLS (identity)	European-American	0.34	<b>Positive effect</b> <b>RD -0.111</b> (-0.209 to -0.013)	1,961 participants (1 study)	
(Any vs. no tobacco use during pregnancy)	Strully 2010	Logistic (logit)		0.29	<b>Positive effect</b> <b>OR 0.949</b> (0.938 to 0.960)	5,260,202 participants (1 study)	

---

Notes: RD = risk difference. RR = relative risk; OR = odds ratio. OLS = ordinary least squares. Neg. Bin. = negative binomial. BMI = body mass index. Averett 2012 refers to [84, 85]; Cowan 2012 to [158]; Evans 2011 to [160]; Gomis-Porqueras to [159]; and Strully 2010 to [188]. The GRADE guidelines are presented and described in [189]. GRADE Working Group grades of evidence for very low quality indicate considerable uncertainty about the estimate.

post-treatment trends in potentially eligible (low-education) women and never eligible (high-education) women are comparable, except for the EITC effect..

The second outcome was mental health / psychological distress, measured with the number of bad mental health days over the last 30 days, studied in one study (Evans 2011 [160]). The continuous outcome was modelled in a negative binomial regression analysis with logarithmic link. The study reported the regression coefficient (-0.0089) and standard error (0.0144). Exponentiation of the coefficient provides a relative risk measure, comparing the treated with the untreated. This provided a relative risk of 0.940, with a 95% CI of 0.815 to 1.084. Thus, the subgroup of women who were potentially eligible for the EITC increase (low-education mothers) had reduced their number of bad mental health days in the past 30 days by 6.0 percent five years after the 1996 increase, when compared to women ineligible for the EITC increase. This equates to statistically not significant reduction from the pre-expansion mean of 4.52 to a post-expansion mean of 4.25 bad mental health days over the last 30 days due to the EITC increase (assuming that only the EITC increase had an effect on the variable). Again, this estimate provides an unbiased estimate under the assumption that the pre-treatment and post-treatment trends in potentially eligible (low-education) women and never eligible (high-education) women are comparable, except for the EITC effect. The Gomis-Porqueras 2011 study [159] found no evidence for differences in the probability of overweight (defined as a body mass index of 25 or more) and obesity (defined as body mass index of 30 or more) eight years after a large increase in EITC in 1996 in women eligible for the increase (women with two or more dependent children), compared to women ineligible for such an increase in EITC (women with one dependent child). This was found for two subgroups of women, who were assumed potentially eligible for EITC (low-education mothers): mothers with high school or less education (overweight: risk difference -1.9 percentage points, 95% CI -5.2 to 1.4 percentage points; obesity: -1.5 percentage points, 95% CI -5.0 to 2.0 percentage points) and mothers with some college education, but no college degree (overweight: risk difference -2.6 percentage points, 95% CI -6.1 to 0.9%; obesity: risk difference -1.3 percentage points, 95% CI -5.0 to 2.4 percentage points). These estimates are not adjusted for underlying pre-intervention/post-intervention trends that differ between women assumed eligible (women with two or more dependent children) and those assumed ineligible (women with one dependent child) for an increase in EITC in 1996 and thus risk bias from confounding.

Evidence on the effect of in-work tax credit on tobacco use was mixed. One study found no evidence that a large increase in EITC in 1996 had a discernible effect (risk difference of current smoking -1.2 percentage points, 95% CI -3.6 to 1.2 percentage points) on the

probability of reporting current tobacco use five years after the increase in women eligible for the increase (women with two or more dependent children), compared to women ineligible for this increase (women with one dependent child), within the group of potentially EITC eligible women (low-education mothers defined as those without a college degree), adjusted for trends over time in never eligible women (high-education mothers defined as those with a college degree; [158]). One study found a differential effect by ethnicity two years after the 1996 increase in EITC [84, 85]. In women potentially eligible for EITC (low-education mothers defined as those with less than 13 years of education), those eligible for the increase in EITC (women with two or more children) did not change their current tobacco use in African-Americans (risk difference -4.3 percentage points, 95% CI -14.1 to 5.5 percentage points), but decreased current smoking in European-Americans by an estimated 11.1 percentage points (95% CI -20.9 to -1.3 percentage points), compared to those not eligible for EITC (mothers with one dependent child), adjusted for the trends over time in never eligible women (high-education mothers defined as those with 13 or more years of education). This equates to a reduction in European Americans from a pre-treatment mean of 34 percent of current smokers to a post-treatment mean of 23 percent after the introduction of EITC, assuming no other trends in smoking than those from EITC. I note that the study did not present an estimate of the impact of EITC on tobacco use in the combined sample of both ethnic groups and did not present statistical tests of significance for the differences between the ethnic subsamples. However, given the large overlap in confidence intervals for European- and African-Americans, one must assume that there was no statistically significant interaction, and the finding for European-Americans that excludes the null from the 95% CI is quite likely to be a chance finding. At the least, this finding must be treated with considerable caution.

One study, the Strully 2010 study [188], found 5.1 percentage point reduced odds of tobacco use during pregnancy one year after an increase in EITC for mothers living in states with a state-level EITC, compared to mothers not living in states with an EITC (95% CI -6.2 to -4.0 percentage points). This equates to a reduction in smoking rates from the pre-treatment average of 29 percent to 28 percent after the expansion of EITC, again assuming no other effects on smoking than the EITC effect.<sup>8</sup> The Cowan 2012 [158] and Averett 2012 [84, 85] studies assumed that the underlying trends in smoking were comparable between potentially eligible (low-educated) and never eligible (highly educated) women, whereas the Strully 2010 study [188] did not attempt to control for underlying trends in smoking between EITC-eligible

---

<sup>8</sup> The probability of smoking in the control group of 0.290 equals odds of 0.408. The odds in the treatment group are the odds ratio multiplied by the odds in the control group,  $0.949 * 0.408 = 0.388$ . The prevalence of smoking in the treatment group then is  $0.388 / (1 + 0.388) = 0.280$ .

and EITC-ineligible mothers; both approaches carry risk of bias from confounding. For example, in the Cowan 2012 study, if new tobacco control measures targeted at social housing facilities were implemented in a selection of states included in the study, and if low-educated women were overrepresented in social housing facilities, then this may have caused a different underlying trend in smoking in low-education compared to high-education women in the study.

Review findings on the secondary health outcomes are summarized in **Table 5**. The study found that, for potentially EITC-eligible (low-education) women, women eligible for a large increase in EITC in 1996 (those with two or more dependent children) reported a 13.0 percent increase in the number of bad physical health days in the past 30 days five years after the increase (relative risk 1.13, 95% CI 0.95 to 1.35), compared to women not eligible for the increase in EITC (those with one dependent child), adjusted for the trend in health in never EITC-eligible (high-education) women, assuming that the pre / post-treatment trends in potentially and never eligible women were comparable. This equates to an increase, albeit statistically not significant, of bad physical health days over the last 30 days from a pre-expansion mean of 2.65 to a post-expansion means of 3.00, assuming only changes to EITC influenced the variable. The risk difference for number of risky biomarkers for inflammation, cardiovascular disease and metabolic disease eight years after the implementation of the intervention was large (a reduction by 19.1 percent), but imprecisely measured (risk ratio 0.81, 95% CI 0.53 to 1.23). This equated to a (statistically not significant) reduction of the number of risky biomarkers from a pre-expansion mean of 1.09 to a post-expansion means of 0.86, assuming no effects other than those from EITC. The first data point in these latter analyses followed (rather than preceded) the implementation of the intervention by three years. This raises questions about the interpretation and methodological validity of these results, suggesting these latter analyses provide weak, if any, evidence for a beneficial effect of in-work tax credit for families on this outcome.

The Strully 2010 study [188] found that mothers living in a state with a state-level EITC had an 31.8 percentage point increased log income from employee earnings one year after an increase in EITC, compared to mothers not living in a state with an EITC (95% CI 10.2 to 53.4 percentage points). Evidence on change in employment in women was mixed. Two studies found no evidence for an effect on employment in all women five years after a considerable increase in EITC in 1996 (risk difference 0.013 percentage points, 95% CI -0.011 to 0.037; [16]) as well as in African-American (risk difference -0.010 percentage points, 95% CI -0.137 to 0.157) and European-American women two years after the increase (risk difference 0.119

Chapter 4: Systematic review

**Table 5: Summary of findings table, in-work tax credit for families interventions, secondary outcomes**

Outcomes (Measure)	Study	Statistical model	Sub-groups	Baseline rate	Treatment effect estimate (95% CI)	No of participants / observations (studies)	Quality of the evidence (GRADE)
Physical health							
(Number of bad physical health days in past 30 days)	Evans 2011	Neg. Bin. (logarithmic)		2.65	<b>No effect</b> <b>RR 1.130</b> (0.946 to 1.351)	127,209 participants (1 study)	Very low
(Number of risky biomarkers for inflammation, cardio-vascular disease, and metabolic disease)	Evans 2011	Neg. Bin. (logarithmic)		1.16	<b>No effect</b> <b>RR 0.810</b> (0.531 to 1.234)	3,090 participants (1 study)	Very low
Change in income (Logged wages/salary)	Strully 2010	OLS (identity)		6.05	<b>Positive effect</b> <b>RD 0.318</b> (0.102 to 0.534)	66,542 participants (1 study)	Very low
Change in employment (Currently employed)	Cowan 2012	OLS (identity)		0.60	<b>Positive effect</b> <b>RD 0.020</b> (0.006 to 0.035)	144,477 participants (1 study)	Very low
	Evans 2011	OLS (identity)		0.58	<b>No effect</b> <b>RD 0.013</b> (-0.011 to 0.037)	127,209 participants (1 study)	
	Strully 2010	Logistic (logit)		0.66	<b>Positive effect</b> <b>OR 1.187</b> (1.025 to 1.375)	66,542 participants (1 study)	
	Gomis-Porqueras 2011	OLS (identity)	High school or less education	0.72	<b>Positive effect</b> <b>RD 0.039</b> (0.008 to 0.070)	29,663 participants (1 study)	
	Gomis-Porqueras 2011	OLS (identity)	Some college education	0.90	<b>Positive effect</b> <b>RD 0.033</b> (0.000 to 0.066)	15,773 participants (1 study)	
	Averett 2012	OLS (identity)	African-American	Not provided	<b>No effect</b> <b>RD -0.010</b> (-0.137 to 0.157)	1,404 participants (1 study)	
	Averett 2012	OLS (identity)	European-American	Not provided	<b>No effect</b> <b>RD 0.119</b> (-0.013 to 0.251)	1,961 participants (1 study)	

Notes: RD = risk difference. RR = relative risk; OR = odds ratio. OLS = ordinary least squares. Neg. Bin. = negative binomial. Averett 2012 refers to [84, 85]; Cowan 2012 to [158]; Evans 2011 to [160]; Gomis-Porqueras to [159]; and Strully 2010 to [188]. The GRADE guidelines are presented and described in [189]. GRADE Working Group grades of evidence for very low quality indicate considerable uncertainty about the estimate.

percentage points, 95% CI -0.013 to 0.251; [84, 85]). Two studies found that EITC moderately increased the prevalence of current employment. The first study found that EITC increased the employment rate in all women by 2.0 percentage points five years after the increase in EITC in 1996 ([158]; 95% CI 0.006 to 0.035), equating to a statistically significant increase from the pre-expansion mean of 66% to a post-expansion mean of 68%. The second study found that EITC increased the employment rate in women with high school or less education by 3.9 percentage points (95% CI 0.008 to 0.070), as well as in women with some college education, but no college degree eight years after the increase by 3.3 percentage points (risk difference 0.033 percentage points, 95% CI 0.000 to 0.066); [159]). These increases equated to statistically significant changes from a pre-expansion mean of 72% to a post-expansion mean of 76% in women with high school or less education, and from 90% to 93% in women with some college education (assuming only EITC effects). These latter suggests that EITC achieved its goal of moving individuals into paid employment. One study found 18.7% increased odds in the employment rate in women living in an EITC state (95% CI 1.0025 to 1.375) [188], equating to an difference in employment rates of 70 percent in mothers in states with EITC, compared to 66 percent in mothers in states without EITC (assuming only EITC effects).<sup>9</sup> This suggests that in-work tax credits increase income and may increase employment, as previously found in another review [18], as well as that the income and employment pathways were active in these previous studies, as discussed in *Chapter 2*.

### ***Impact on equity***

The triple differences estimators reported above as estimators of policy impact also showed no effect on equity by level of education in SRH [160] and number of bad metal health days [160]. In other words, the gaps in these outcomes between educational groups did not close. While the Gomis-Porqueras 2011 study [159] did provided separate difference-in-differences estimates for the group of people with a low level and for those with a high level of education, but it did not test whether these estimates differed, e.g. by using interaction terms. Triple differences estimates demonstrated no effect on health equity by level of education in current tobacco use in the Cowan 2011 study [158]. The Averett 2012 study [84, 85] reported a differential impact on equity in current tobacco smoking with the low-education group considerably decreasing its current tobacco use in European-Americans (compared to the high-education group), but not having a discernible equity impact in African Americans by

---

<sup>9</sup> The probability of employment in the control group of 0.660 equals odds of 1.941. The odds in the treatment group are the odds ratio multiplied by the odds in the control group,  $1.187 * 1.941 = 2.304$ . The prevalence of smoking in the treatment group then is  $2.304 / (1 + 2.304) = 0.697$ .

level of education. However, I have previously cautioned that these reported differences may not actually be statistically significant.

The triple differences estimators of number of bad physical health days and number of risky biomarkers for inflammation, cardio-vascular disease and metabolic disease showed no impact on equity by level of education [160]. Two studies found no evidence for any discernible impact on equity (by level of education) in current employment in all women [160], and in African-American and European-American women respectively [84, 85]. One study found evidence for a larger impact of the policy on employment status in low-education than in high-education women [158], suggesting the intervention may reduce by-education inequalities in employment status. One study found increased prevalence of current employment in low-education women and no effect on high-education women, but did not formally test whether these estimates differed statistically significantly [159].

## Discussion

### Summary of main results

No studies on the effect of anti-poverty tax credits for families on health status in adults were found. Seven records from five studies estimating the impact of in-work tax credit interventions for families (not individuals) as a standalone intervention to reduce income poverty and unemployment (not operating alongside other welfare-to-work or financial credit interventions) on health status in working-age adults were found. All of these included studies investigated the EITC intervention in the United States in women. The review found low-quality evidence suggesting that the EITC intervention had no statistically discernible effect on self-rated general health and mental health/psychological distress five years after policy implementation and overweight/obesity eight years after implementation (**Table 4**). Evidence of the effect of EITC on tobacco use was mixed, with one study finding no effect five years after implementation and one finding a moderate reduction one year after implementation (**Table 4**). No evidence was available on the impact of in-work tax credit for families interventions on mental illness and alcohol use. No adverse effects were identified.

One study also found no detectable effect of in-work tax credit on the number of bad physical health days and of risky biomarkers for inflammation, cardio-vascular disease and metabolic conditions eight years after implementation (**Table 5**). One study found that in-work tax credit had a large, positive effect on income from employee earnings one year after implementation (**Table 5**). Two studies found no effect on employment two and five years after

implementation, whereas two found a moderate increase five and eight years after implementation and one a large increase in employment due to in-work tax credit one year after implementation (**Table 5**).

This small body of evidence is limited to non-experimental studies of the impact on health status of one in-work tax credit intervention, one country setting, female participants and four of the six primary outcomes of this review. It also has a high (as per Cochrane protocols) risk of bias (especially from misclassification and confounding, including confounding due to insufficient control for underlying time trends). However, from a more nuanced epidemiological framework, there was little evidence for selection bias; some bias towards a null finding from misclassification bias of the exposure and outcome variables; and some risk of confounding. Additional studies are required of a range of in-work tax credit interventions; of different country settings; of male participants; of mental illness and alcohol use outcomes; and that control more rigorously for risks of biases.

### **Overall completeness and applicability of evidence**

The current body of evidence is not sufficient to address the objective of this review. There is no previous evidence on the effect of anti-poverty tax credits for families on health status in adults. Previous studies of the effect of in-work tax credit on health status are limited to non-experimental studies; one in-work intervention (EITC); one country setting (United States); female participants; and four of the six primary outcomes of this review. Information is unavailable about several relevant participant subgroups: men; ethnic groups (except the Averett 2012 study [84, 85] for tobacco use in African-Americans and European-Americans); family types (one-parent families, two-parent families); and income groups (low income, middle income). Therefore, subgroup analyses could not be carried out. Moreover, statistical power for subgroup analyses is likely limited. The only information on key population characteristics is level of education in three studies [84, 85, 158, 160]. Further information on these variables is required to better assess the health equity impact of in-work tax credits.

These multiple limitations of the current body of evidence have implications for the external validity of this evidence. One important consideration in this regard is that all studies included in the review examined the effect of the EITC in the United States. Therefore, this evidence is specific to one in-work tax credit design (EITC) and the policy context of one country (United States). Therefore, the external validity of the evidence reviewed in this chapter is limited.

## Quality of the evidence

The existing body of evidence does not permit a robust conclusion regarding the review objective for anti-poverty tax credit and in-work tax credit interventions. While the review includes seven records from five studies of in-work tax credit interventions with large participant numbers, all included studies are non-experimental and carry risk of bias (especially from misclassification bias of the EITC exposure variable, and to some extent confounding). Because evidence on the primary outcomes of SRH, mental health / psychological distress and overweight / obesity (and mental illness and alcohol use, respectively) is limited to one (and no study, respectively), the consistency of this evidence cannot be judged. The evidence of an impact of EITC on tobacco use is inconsistent, although the evidence points towards a reduction in smoking.

In line with the GRADE considerations described above [184], the current evidence base for the impact of in-work tax credit on the health outcomes SRH, mental health / psychological distress and overweight / obesity was judged to be of 'very low' quality. Each of these outcomes included only one study. All included studies were observational (to be graded 'low' quality as per GRADE). Furthermore, each included study had some risk of bias, meaning that the quality of the evidence for each of these outcomes was downgraded one level to 'very low', adhering with GRADE criteria [189] and Cochrane Collaboration protocols [182]. Evidence on tobacco use, despite a larger number of studies and large participant numbers, was also judged to be of very low quality due to limitations in the design and implementation of the included studies suggesting high (as per Cochrane protocols) risk of bias and unexplained heterogeneity or inconsistency in results (GRADE considerations, [184]). Due to the risk of bias in all included studies, it was not feasible to meta-analyse common outcomes. In summary, future research is very likely to have an important impact on my confidence in and is very likely to change estimates of the effect that in-work tax credit interventions have on health status in adults.

However, when the overall quality of the evidence is judged from an epidemiological rather than a Cochrane Collaboration framework, then the evidence would perhaps best be judged to be of moderate quality. The evidence is mainly from studies analysing a natural experiment, which may provide superior evidence than many other epidemiological, observational study designs, such as cross-sectional and cohort studies. Furthermore, the studies were generally based on large samples drawn from the general population (likely at random). Risk of bias was likely towards a null-finding, mostly due to misclassification bias of the exposure and, to some degree, from confounding. However, the small number of studies (mostly only one per

outcome) was a concern, when the overall quality of the evidence is judged, as were the inconsistent findings from the three studied on smoking. Therefore, overall, from an epidemiological point of view, the quality of the evidence may best be described as moderate. More research is required to strengthen this evidence base.

### **Potential biases in the review process**

I am confident that the review identified all completed eligible studies. The search strategy was designed to be broad to ensure that all potentially relevant records would be identified from the large number of relevant academic and grey literature databases that were searched. In addition, several leading experts were consulted throughout the review with the request to identify any missing studies. All academic and several grey literature database searches were conducted by an independent reference librarian. Much of the grey literature included in this review was recently published (in the last six months), and some of these records are currently undergoing the peer review publication process. That some data could not be obtained from the study authors may have potentially introduced some bias. For example, the lack of data on survey response rates concealed the degree to which response or attrition bias may have influenced findings in some studies.

### **Agreements and disagreements with other studies or reviews**

Ludbrook and Porter's review is the only other review of the impact of publicly funded financial credits on health status in adults that I am aware of [112]. This review was not a systematic review. It also included all publicly funded financial credits, not just anti-poverty and in-work tax credits for families, and did not conduct subgroup analyses of the studies for these types of financial credits [112]. It was thus unable to review the effects of individual anti-poverty and in-work financial credit for families on health status. Therefore, the finding from this review of no evidence on anti-poverty tax credits and an inconsistent relationship between increases in income from in-work financial credits and health status in adults [112] are neither directly comparable, nor in disagreement with findings from the review presented in this thesis.

## **Conclusions**

This chapter systematically reviews evidence of the effect of anti-poverty and in-work tax credit for families interventions on health status in adults. Sixteen electronic academic databases, including the Cochrane Public Health Group Specialized Register, Cochrane

## Chapter 4: Systematic review

Database of Systematic Review (*The Cochrane Library 2012, Issue 7*), MEDLINE, and EMBASE, as well as six grey literature databases were searched between July and September 2012 for records published between January 1980 and July 2012. Key organizational websites and reference lists of included records were also searched and academic experts contacted.

No study on the effect of anti-poverty tax credit on health status in adults was found. Five studies on the effects of one in-work tax credit (EITC in the US) on health status in adults comprising a total of 5,677,383 participants (all women) fulfilled the inclusion criteria. All included studies carried a high (as per Cochrane Collaboration protocols) risk of bias (especially from confounding and insufficient control for underlying time trends). Due to the small number of (observational) studies with a high risk of bias, this body of evidence was judged very low overall quality (as per GRADE criteria). From a more nuanced epidemiological critique, though, the overall quality may best be viewed as moderate. While most of the studies are well-designed and analyse large, nationally representative samples, evidence is limited geographically to the US; the number of studies is very limited for most outcomes; and there is considerable risk of misclassification bias of the exposure towards a null-finding, as well as some risk of confounding.

One study found that EITC had no detectable effects on SRH and mental health / psychological distress five years after its implementation (i.e., a considerable change in the generosity of EITC) and on overweight / obesity eight years after implementation. One study found no effect of EITC on tobacco use five years after implementation, one a moderate, but statistically significant reduction in tobacco use one year after implementation [odds ratio (OR) 0.949; 95% confidence interval (CI) 0.937 to 0.961] and one differential effects, with no effect in African-Americans and a large reduction in European-Americans two years after implementation (risk difference -11.1%, 95% CI -20.9% to -1.3%). No evidence was available for the effect of in-work tax credits on mental illness and alcohol use. No adverse effects of in-work tax credits were identified.

In conclusion, the review found no evidence on the effect of anti-poverty tax credit interventions for families on health in adults and weak evidence of no effect of one in-work tax credit (EITC) interventions on health status, except for mixed evidence for tobacco use, in adult women.

## Chapter 4: Systematic review

The text adapted from the Cochrane Review written as part of this thesis, led by myself and co-authored by Dr Kristie Carter, Professor Tony Blakely and Dr Patricia Lucas, ends here [29].

## Chapter 5: Methods

This chapter describes the data and methods that were used in this thesis to estimate the effect of FTC and IWTC on SRH. Seven waves of data were extracted from the SoFIE, an official household panel survey conducted by Statistics New Zealand between 2001 and 2010. The total SoFIE sample was restricted to a balanced panel of 6,900 adult (19 to 65 years) parents in one- or two-parent families over two or more consecutive waves.

The four exposure variables of this thesis were FTC and IWTC eligibility and amount, and the outcome was SRH. The potential time-varying confounding variables, identified *a priori* in the analytical frameworks of the thesis, were household-equivalised gross total annual family income (minus FTC or IWTC), family type, number of dependent children in the family and, for IWTC also, employment.

Linear fixed effects analyses were conducted to answer the principal and subsidiary research questions of the thesis. The main fixed effects regression analyses estimated the effect of FTC and IWTC eligibility and amount on SRH in adults at the individual level over the short term (one year lag). These analyses controlled for all time-invariant and adjusted for potential time-varying confounding.

Subsidiary analyses estimated effect modification by Māori / non-Māori ethnicity and minimum income for healthy living; the effect of FTC and IWTC over a longer time period (two to six year lags); and the effect of FTC and IWTC on two other relevant health outcomes, psychological distress and current tobacco smoking. Sensitivity analyses were conducted that assessed the risk of bias from selection, misclassification of the exposure variable, misspecification of the outcome variable and reverse causation in the main fixed effects analyses.

The objective of this chapter is to describe the data and methods that were used in this thesis to estimate the effect of FTC and IWTC and SRH. The chapter first overviews the background, aims, sampling methods and data collection of the SoFIE. It then describes the restriction of the survey sample to the study sample. Analytical frameworks developed to guide the main fixed effect regression analyses are presented and explained. The exposure, outcome, potential confounding, mediating and effect modifying variables are described. The chapter then outlines the underlying theory, specific type and application in this thesis of linear fixed effects regression analytic methods. The chapter ends by describing the methods of subsidiary and sensitivity analyses.

## **Survey of Family, Income and Employment**

### **Background**

In New Zealand, official statistics are collected by around 60 government agencies through the Official Statistics System under the leadership of the Government Statistician and Statistics New Zealand, as mandated through the *1975 Statistics Act* [209]. The Official Statistics System aims to contribute to an informed society that uses official statistics by ensuring trust and confidence in the Official Statistics System and providing access to official statistics that are timely, relevant and of high-quality [210]. Statistics New Zealand requires data users to adhere with strict data access, confidentiality and data security rules and protocols to ensure trust and confidence in official data collected as part of the Official Statistics System is maintained.

To comply with the *1975 Statistics Act* [209] and Statistics New Zealand's data confidentiality rules, all non-regression outputs in this thesis such as counts in tables were rounded to the nearest multiple of five and a minimum value of five. A cell in a table that contains the value of five may have a value of between zero and seven, with all values smaller than or equal to five imputed to five, and the values six and seven rounded to five. All percentages from the SoFIE are calculated from the rounded counts. Due to these imputation and rounding protocols, totals in tables may appear inconsistent with the sum of individual cells. For example, the cells in individual columns and rows may not sum exactly to column and row totals. Grand totals may not equal precisely the known size of the study sample. Furthermore, rounding to a base of five can result in an artificial increase of counts and percentages in tables.

The SoFIE was an annual longitudinal household panel survey conducted by Statistics New Zealand under its Programme of Official Social Statistics over a period of eight years between

October 2002 and September 2010 [211]. All surveys conducted under the *1975 Statistics Act* [209], including SoFIE, are exempt from the general requirement to undergo ethics review and approval through a dedicated New Zealand ethics committee. However, ministerial approval for such official surveys is only granted, if the need for the survey is established and the survey design, methodology, questionnaire and analysis method have been reviewed independent of the issuing government agency. The principal aim of the SoFIE was to “provide information about changes over time in the economic well-being of individuals and their families, and about factors influencing those changes” (p. 10) [212]. Question modules that collected detailed information on the household, key demographic characteristics, family, income, employment and education at all eight waves formed the core of the SoFIE [213]. A module collecting information on assets and wealth was added in Waves 2, 4, 6 and 8, and a module collecting health information added in Waves 3, 5 and 7 [213]. The health areas collected through the dedicated health module included physical diseases such as asthma, diabetes, cardio-vascular disease and stroke; mental health status such as diagnosis with a psychological illness, psychological distress and happiness; health behaviours such as alcohol and tobacco use and fruit and vegetable consumption; and health care access and utilisation such as access to primary health care [211].

This thesis research was conducted within the SoFIE-Health project [211]. This project was initiated by Professor Tony Blakely, Professor Alistair Woodward and Dr Jackie Fawcett through the University of Otago’s Health Inequalities Research Program with seeding funding from the University of Otago, the Accident Compensation Corporation and the Alcohol Advisory Council of New Zealand. The project was co-led by Professor Tony Blakely and Dr Kristie Carter under programme funding from the Health Research Council of New Zealand. One of the principal aims of the SoFIE project was to assess the association of changes in family circumstances, employment status and income, including income from social assistance, with changes in health status and behaviour. The project sought and was granted ethics approval from the University of Otago Ethics Committee. It led the design of the health module included in SoFIE and the analysis of the data collection in the health module.

## **Sampling methods**

The target population of the SoFIE was “the usually [12 or more months at the time of the interview] resident New Zealand population living in [permanent,] private dwellings” (p. 17), excluding institutions (i.e., prisons, hospitals and rest homes) and non-private dwellings (e.g., hotels, boarding houses, educational institutions) [212]. The target population excluded

foreign visitors, non-New Zealand diplomats and non-New Zealand defence force personnel as well as persons living on off-shore islands (except Waiheke Island) and in remote, rural areas [212].

Survey participants for the SoFIE were sampled randomly from Statistics New Zealand's standard sampling frame, using a three-stage stratified cluster sampling strategy [211, 212]. Statistics New Zealand's standard sampling frame consists of about 19,000 geographic units called primary sampling units, each of which cover between 30 and 260 (on average 70) dwellings in the North, South and Waiheke Islands of New Zealand. In the first stage, primary sampling units were allocated to strata defined by demographic factors such as place of residency, urban / rural residency and density of Māori residents, using figures from the latest New Zealand Census of Population and Dwellings. Primary sampling units were then randomly sampled from each of the strata, ensuring an independent sample of primary sampling units. In the second stage, households were sampled randomly from the selected primary sampling units. In the third stage, all eligible residents of the sampled household were selected.

The SoFIE attempted to follow its 29,790 original sample members (OSMs), the adult (>15 years) survey participants interviewed at Wave 1, over the full length (eight years) of the survey. Children of OSMs were treated as OSMs from when they became 15 years of age. All OSMs were interviewed annually and followed over the length of the survey, even if they moved into a new household. However, if OSMs missed two waves, they were no longer followed. All adult members of the household that an OSM resided in at the time of the interview were interviewed. Basic demographic information on children in the household such as their gender, age and ethnicity was collected from a nominated household member. Non-OSMs were not followed over time, but only interviewed as long as they resided with an OSM. The level of information sought from participants also differed between OSMs and non-OSMs; OSMs were asked the full SoFIE questionnaire, whereas non-OSMs were asked a limited set of questions only.

### **Data collection**

The SoFIE collected data annually within a three months window after the anniversary of the last interviewing date. Computer-assisted face-to-face interviews were conducted in the participants' home. An interviewer visited the household, read the survey questions off a computer screen and entered the participants' answers into the computer. Some information from past interviews was fed back to the participants during an interview. Enforced consistency checks at the point of data entry were also conducted to reduce measurement

error during the data collection. Moreover, upon induction into interviewing teams, each interviewer was trained by Statistics New Zealand on the administration of the survey in a standardised manner, aiming to ensure consistent survey administration over interviewers and time.

Selected questions in the SoFIE were administered, using show cards that displayed the response categories encoded with numbers (or letters). The use of show cards enabled participants to answer sensitive questions such as questions on income without having to disclose the answer to others who may be in the room with the participants, including the interviewer. The use of show cards in the SoFIE may have increased both the comprehension and confidentiality of survey questions, thereby potentially reducing item non-response rates and systematic misreporting.

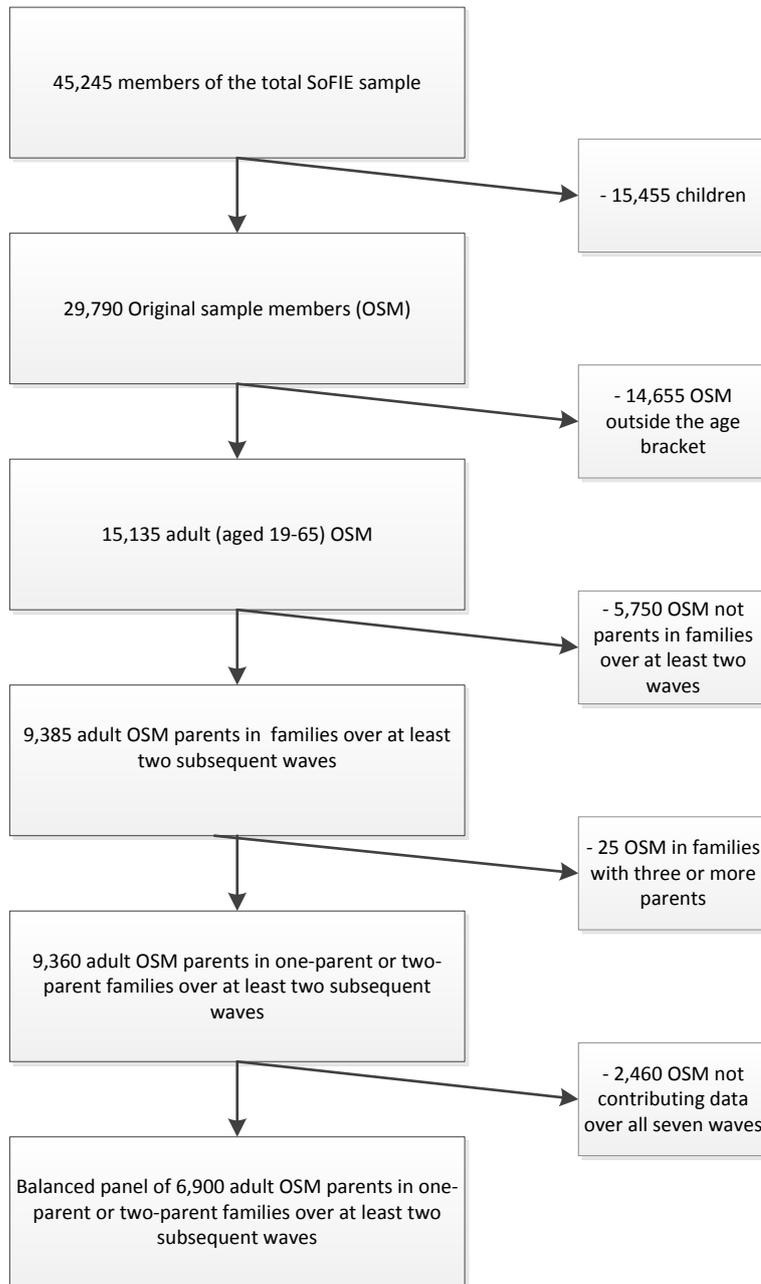
Of the about 15,000 households invited to participate in the survey, about 11,500 agreed to participate, giving an initial survey response rate of 76.7% [211]. Of the total of 29,790 participants from whom the SoFIE collected data in Wave 1, about 22,000 were adults aged 15 years or over, the OSMs [211]. By Wave 3, 79% of respondents interviewed at Wave 1 had remained in the survey, giving an estimated effective response rate of 61% [211]. A detailed description of the demographic composition of the total SoFIE sample at Wave 1 baseline was provided elsewhere [211].

## **Study sample**

Seven waves of data from the SoFIE, Waves 1 to 7 covering the years 2002 to 2009, were extracted from the *SoFIE Waves 1 to 7 data version 2*. The data were cleaned and relevant variables derived as described below. The final dataset contained all relevant variables in one row per participant per wave.

Research on the effect of publicly funded financial credits for families on health in adults generally studies samples of adults who are potentially eligible for these credits, since this is the population group whose health is likely affected by the credits [29]. In line with this approach, the extracted data was restricted to a sample of adults who were potentially eligible for FTC or IWTC, *a balanced panel of adult (19 to 65 years) parents in one- or two-parent families at two or more consecutive waves* [30]. A flowchart demonstrating the restriction of the total SoFIE sample of N = 45,245 participants to the study sample of N= 6,900 participants is presented in **Figure 18**.

**Figure 18: Flow-chart of restriction from survey to study sample**



An adult was defined as a person aged 19 to 65 years, with very young (under 19 years) and elderly (over 65 years) people excluded to ensure a representative study sample of working-age adults. A parent in a family was an adult who reported being a parent of one or more dependent or independent children living in the same household. A family was defined as one or two parents living with one or more of their dependent or independent children, with

adults in families with three or more parents excluded to ensure the representativeness of the study sample.

Only those working-age parents were included who were in one- or two-parent families for two or more consecutive waves, because the fixed effects regression analytic methods applied in this study only used data from participants with at least two measurement points. Including participants who were parents in families in some, but not all of the seven waves ensured that those survey participants were included who during the survey period moved from being in a family to living single or with their partner only, such as participants whose children left the household. It also ensured that participants who moved into a family during the study period were included, such as participants who had their first child.

The sample was restricted from an unbalanced to a balanced sample, because this provided the opportunity to conduct full case analysis. While an unbalanced panel contains all participants who contribute data to any wave of a survey over a study period, a balanced panel is a panel of participants contributing data to all waves of the study period. As shown in the flowchart (**Figure 18**), the unbalanced panel consisted of  $N = 9,360$  and the balanced panel of  $N = 6,900$  adult OSM parents in one-parent or two-parent families over at least two subsequent waves, giving an attrition rate of 26.3%.<sup>10</sup> Attrition in the sample, including which population groups were disproportionately lost to follow-up, is described in *Chapter 6*. The restriction to a balanced panel may have increased the risk of bias from selection bias, being the risk that the FTC- or IWTC-SRH effects in the total sample differed from these effects observed in the study population, because data from participants who left the survey over the course of the survey were lost. Furthermore, restricting the unbalanced sample to a balanced study sample reduced the sample size, which may have reduced the study precision. However, running the main fixed effects estimates on the unbalanced panel produced near-identical effect estimates as running the model on the balanced panel, suggesting that the restriction to a balanced panel did not have these potential disadvantages in this study. Further analysis of the risk of selection bias emanating from restriction to a balanced panel is also provided in *Chapter 9*.

The demographic features of the sample are described in detail in the following two chapters. A whole chapter, *Chapter 6*, is dedicated to describing the demographic characteristics of the study sample at baseline. *Chapter 7* describes dynamics in relevant time-varying characteristics in the study sample over the seven waves

---

<sup>10</sup> The attrition rate is the sample size in the unbalanced panel minus the sample size of the balanced panel, divided by the sample size of the unbalanced panel,  $(9,360 - 6,900) / 9,360 = 26\%$ .

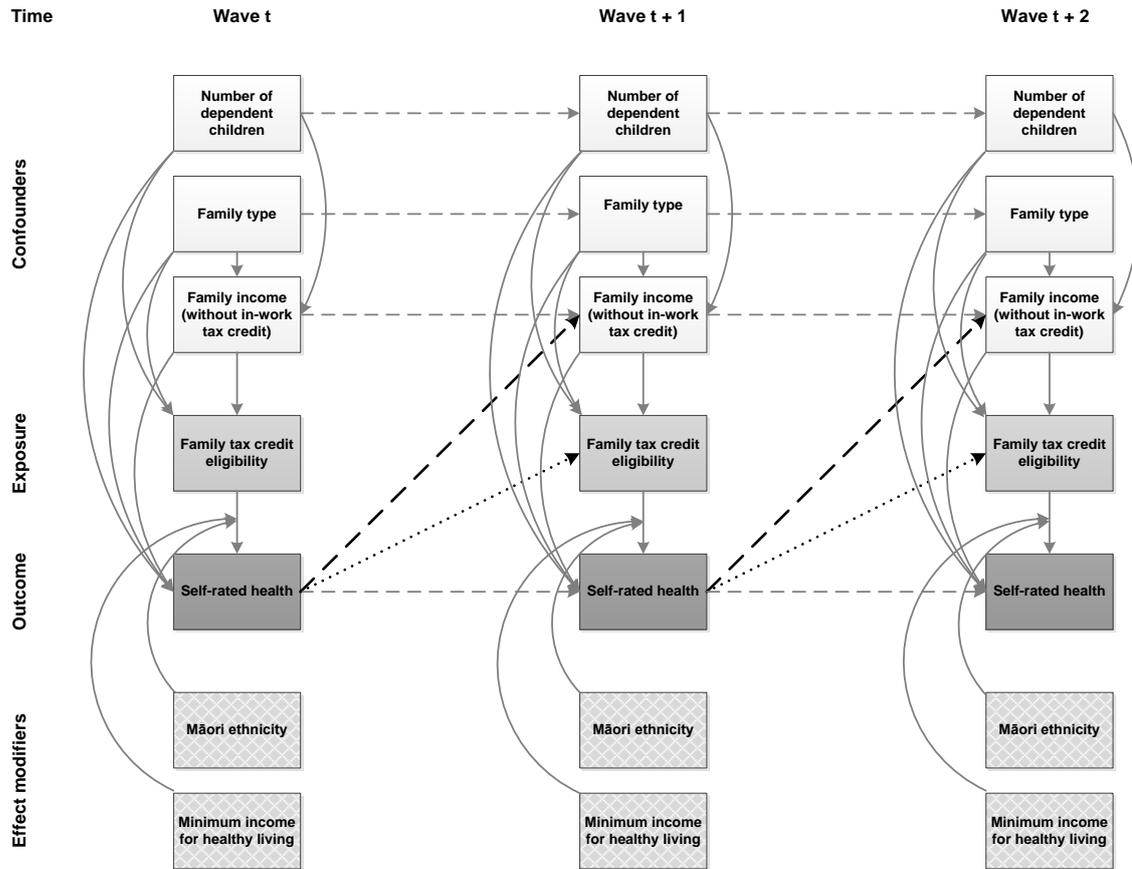
## Analytical frameworks

Directed acyclic graphs are causal diagrams in which each arrow is directed, meaning that each arrow represents a causal association, and in which each variable is acyclic, meaning that no variable affects itself [1]. Another feature of directed acyclic graphs is that all causal relationships between variables included in such a graph are assumed to be shown [1]. Because directed acyclic graphs are well suited for visualizing the hypothesised cause-effect relationship between variables, they are commonly used in epidemiological research and a useful choice of graph for analytical frameworks

The directed acyclic graphs below present the analytical framework for the main fixed effects analyse of this thesis presented in *Chapter 8*. The analytical framework of analyses studying the effect of FTC on SRH is shown in **Figure 19** and that of analyses studying the IWTC-on-SRH effect in **Figure 20**. These frameworks, developed prior to the analyses being carried out, hypothesise the causal relationship between FTC or IWTC and SRH over three consecutive waves. Wave<sub>t</sub> refers to the wave at a time t and wave<sub>t+1</sub> to the wave at time t+ 1. So, for the example of t = 2, wave<sub>t</sub> refers to Wave 2, and wave<sub>t+1</sub> denotes Wave 3

The light grey boxes in the conceptual frameworks represent the tax credit exposure variables (exemplified by FTC and IWTC eligibility), and the dark grey boxes the outcome variable (SRH). The white boxes mark the variables hypothesised to vary over time and to potentially confound the FTC-SRH or IWTC-SRH relationships. In order for these variables to introduce confounding in the fixed effects regression analyses, change in these variables needed to be associated with change in both the relevant exposure variable and the outcome variable. The criteria for FTC eligibility or amount, and IWTC eligibility or amount respectively, were described in *Chapter 2*. As per these criteria, the factors that determine FTC and IWTC eligibility and amount were gross total annual family income (household-equivalised, minus FTC and IWTC), family type and number of dependent children. Employment status was theorised to be a time-varying confounding factor for IWTC (but not FTC), because it determined IWTC eligibility and amount (but did not determine FTC exposure variables). Change in these determinants of FTC and IWTC was hypothesised to be associated with change in FTC or IWTC eligibility and amount. It was also hypothesised that change in each of these variables could potentially be associated with change in the outcome, SRH. Since all of these variables potentially fulfilled the criteria for a time-varying confounding variable, they were included in the analytical framework and adjusted for in the fixed effects regression analyses.

**Figure 19: Directed acyclic graph for the fixed effects model assessing the association between change in Family Tax Credit eligibility and change in health**

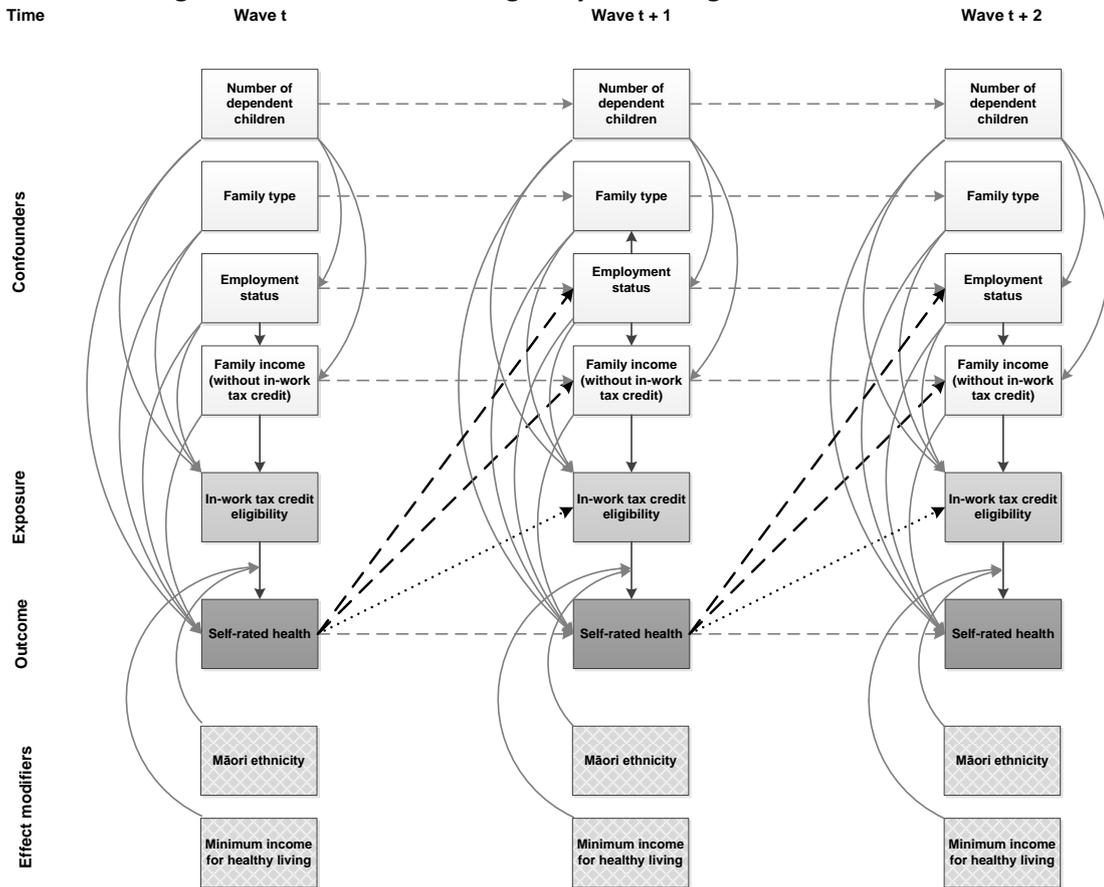


Notes: Wave<sub>t</sub> refers to the survey wave at time t (such as Wave 1), whereas wave<sub>t+1</sub> refers to the wave at time t+1 (such as Wave 2). An arrow that points from one box to another indicates a causal effect from the variable in box from which the arrow originates on the variable in the box that the arrow points to. A thick arrow indicates a potential causal relationship between factors across waves. A light grey, dashed arrow indicates a potential causal effect across waves between repeated measures of the same variable. A black, dotted arrow indicates potential reverse causation. An arrow A that points to an arrow B indicates effect modification originates of the casual relationship between the variables that arrow B connects by the variable from which arrow A.

Considering that fixed effects regression analyses control for all time-invariant confounding, as described in more detail below, no time-invariant confounding variables were included in the analytical framework as confounders. Several potentially relevant time-varying variables other than the determinants of FTC and IWTC eligibility and amount were considered for inclusion in the framework. Most of these variables were unmeasured and could potentially have carried a considerable risk of bias for the main fixed effects regression analyses, considering that these models cannot adjust for unmeasured time-varying confounding variables. However, when these variables were further scrutinized, it was found that their effect on SRH through the FTC or IWTC exposure variables acted exclusively through the identified time-varying potential confounding variables. As shown for the example of the FTC eligibility-SRH relationship in the

directed acyclic graph in **Figure 21**, if a participant changes from infertile to fertile then this only translates into a change in the participant’s FTC eligibility, if the change in fertility causes a change in the number of dependent children

**Figure 20: Directed acyclic graph for the fixed effects model assessing the association between change in In-Work Tax Credit eligibility and change in health**

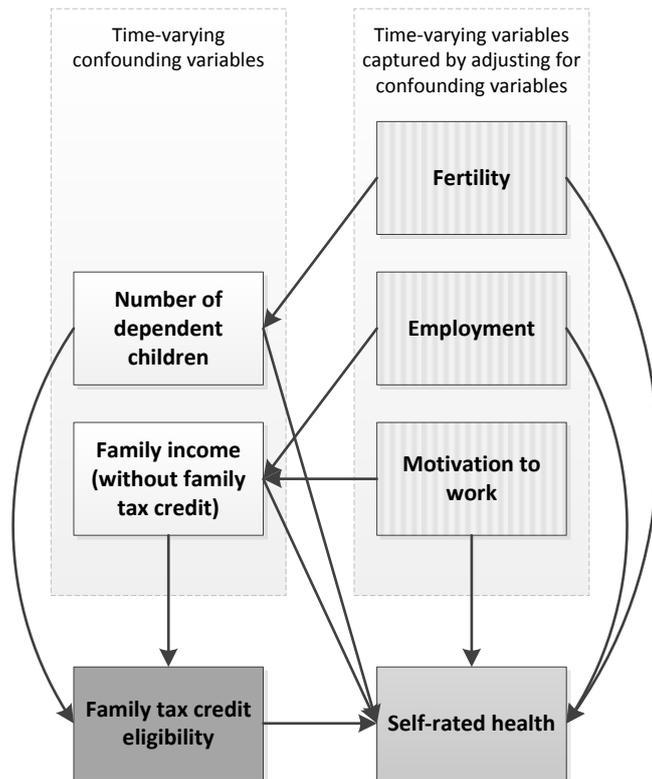


Notes: Wave<sub>t</sub> refers to the survey wave at time t (such as Wave 1), whereas wave<sub>t+1</sub> refers to the wave at time t+1 (such as Wave 2). An arrow that points from one box to another indicates a causal effect from the variable in box from which the arrow originates on the variable in the box that the arrow points to. A thick arrow indicates a potential causal relationship between factors across waves. A light grey, dashed arrow indicates a potential causal effect across waves between repeated measures of the same variable. A black, dotted arrow indicates potential reverse causation. An arrow A that points to an arrow B indicates effect modification originates of the casual relationship between the variables that arrow B connects by the variable from which arrow A.

Similarly, if a participant reduces her motivation to work, this change only results in a change in FTC eligibility, if it results in a change in her family income. In other words, adjusting for the determinants of the exposure variables blocked all back door pathways, so that adjustment for additional time-varying confounding variables was unnecessary. Change in employment status determines change in IWTC and thus constitutes a potential time-varying confounding variable for the IWTC-SRH relationship. But **Figure 21** shows that analyses of the FTC-SRH relationship captured change in employment status through changes in family income, so that

they did not require independent adjustment in these analyses. Considering that all determinants of the exposure variables could be identified and were adjusted for, the fully adjusted main fixed effects regression analyses of the thesis should have adjusted for all potential time-varying confounding variables.

**Figure 21: Directed acyclic graph showing that adjusting for the variables determining Family Tax Credit eligibility captures the potential effects of other relevant time-varying variables**



Thick arrows in the analytical frameworks indicate potential associations between factors across waves. The light grey, dashed arrows are such associations between repeated measures of the same variable, which are unproblematic for fixed effects regression analyses. Black, dotted-line arrows indicate such potential for reverse causation in the main fixed effects regression analyses, if the outcome variable (SRH) at wave<sub>t</sub> is related to the exposure variable (such as FTC eligibility) at wave<sub>t+1</sub>. In econometrics terms, this potential for reverse causation would be described as an issue of endogeneity. For example, it could be argued that employment status is an endogenous variable to health status in analyses of the effect of IWTC on health status. Fixed effects regression analyses assume no reverse causation, and this thesis may theoretically potentially violate this assumption. However, I present a sensitivity

analysis in *Chapter 9* that finds no evidence for the presence of reverse causation, suggesting that the risk of bias from reverse causation in this thesis is low, if there is any.

Black, dashed arrows in the frameworks indicate potential time-varying mediation, where the effect of the outcome (SRH) at wave<sub>t</sub> on the outcome at wave<sub>t+1</sub> is mediated through a determinant of the exposure variable at wave<sub>t+1</sub>. Such potential time-varying mediators are family income and, for IWTC also, employment status. This thesis does not conduct mediation analyses that would enable an assessment of whether family income and mediated the FTC-SRH relationship and family income or employment the IWTC-SRH relationship. I am also not aware of any studies that have assessed the level of mediation of the effect of a publicly funded financial credit on SRH through family income or employment. Therefore, the extent of potential violation that arose from adjusting for variables that may potentially be time-varying mediating variables cannot be established with confidence. However, as is shown in *Chapter 8*, adjusting for these potential mediating variables in the fixed effects regression analyses did not change the effect estimates, suggesting little, if any, mediation by these variables and therefore a small risk of violation of the fixed effects regression analytic assumptions.

Moreover, the potential time-varying mediating variables were above also posited as a potential time-varying confounding variable. These variables pose a potential methodological catch-22 situation. On the one hand, adjusting for these variables in the main fixed effects regression analyses controls for their potential time-varying confounding effects, but it at the same time biases the effect estimate by adjusting for mediating variables. On the other hand, not adjusting for such variables to prevent 'adjusting away' their potential mediating effects would result in effect estimates biased by the potential time-varying confounding effects of the variables. This methodological problem needs to be considered even if a fixed effects analysis finds no effect of the exposure on the outcome. The confounding effect of a variable could be smaller or larger in size than or equal in size as the mediating effect, and confounding and mediating effects could act in the same direction or in opposite directions. Therefore, if confounding and mediating effects of the two variables were equal in size, but acted in opposite directions, then fixed effects regression analyses would have been expected to erroneously estimate no discernible effect, when an effect may indeed have existed. One epidemiological method for causal inference developed to address this very methodological problem commonly affecting studies of time-varying interventions, marginal structural modelling (see *Chapter 3*) [178].

The marbled boxes represent the two potentially effect-modifying variables, Māori ethnicity and poverty. As discussed in *Chapter 4*, there is evidence suggesting that the effect of tax credits for families interventions on health may be modified by ethnicity [84, 85]. Therefore, Māori / non-Māori ethnicity was theorised to potentially modify the effects of FTC and IWTC on SRH. In line with Morris *et al.*'s theory of a minimum income for healthy living [4-8] discussed in *Chapter 3*, additional income from FTC and IWTC may have a more positive effect on health in income-poor participants, whom the additional income lifted closer to or above the minimum income threshold for a healthy living, than in participants with incomes already above the minimum income threshold.

## Variables

The ability to evaluate the validity of the derivation of a variable relies considerably on a thorough understanding of the quality of the data from which the variable was created. Therefore, this section assesses the quality of data used for variable derivation, raising any key data quality concerns upfront. The section then describes the exposure, outcome, confounding, mediating and effect modifying variables of the fixed effects regression analyses, including descriptions of their collection in the survey or derivation from survey data, respectively.

### **The quality of data used for variable derivation**

All exposure variables, as well as some confounding, mediating and effect modifying variables were derived by Statistics New Zealand or me from other variables. Several of the variables used to derive variables for this thesis themselves had previously been derived by Statistics New Zealand. An assessment of these variables with a focus on potentially quality-impaired variables is therefore a useful prelude to a description of the final, derived variables used in the thesis. Importantly, misclassification in the exposure variable can also lead to misclassification bias, and the thesis findings are scrutinized in terms of their risk of bias from such misclassification in *Chapter 9*.

The most complex derivation, detailed in the following sections, was that of the exposure variables, FTC and IWTC eligibility and amount, based on complex eligibility and entitlement criteria. Some of the key variables that were used to derive these variables included the gross total annual family income, family type, number of children in the family and number of hours worked per week. The family type and number of hours worked per week variables were considered to be of considerable quality, because both variables were considered non-

sensitive, meaning that they were not prone to non-response or systematic misreporting, and both were collected using standard survey questions and derived by Statistics New Zealand through application of pre-determined official statistical standards. The number of children in the family required information on relationships in a family such as identification of children in the family, which was derived by Statistics New Zealand, as well as information on the age, education and hours worked per week of the children, none of which variables raised quality concerns. When the data was studied, no irregularities in these derived variables or their component variables were found.

The fourth variable that played a key role in the derivation of FTC and IWTC eligibility and amount was a participant's gross total annual family income, which was derived by Statistics New Zealand from a large number of income spells. Gross total annual family income was also the key component variable for deriving two other variables, equivalised gross total annual family income, the potential confounding and potential mediating variable, and poverty measure, the potential effect modifying variable. An income spell is a period over which a participant received a certain income amount and type from a specific source. The multiple types of income sources that participants reported were grouped into eight broader types, such as employee earnings; government transfers; income from self-employment; and interests and dividends. The SoFIE asked each participant at each interview to report each income spell that occurred since the last interview took place, producing a detailed record of all type of income received over the entire study period. For each participant, the income spells from all types of income over the annual reference period were then summed for each type of the eight types of income sources, arriving at such variables as gross total annual personal income from employee earnings or gross total annual personal income from government transfers. These gross total annual personal incomes from the eight types of sources were then summed into gross total annual personal income. Finally, the personal incomes of all members of a family over the one year reference period were summed, arriving at gross total annual family income.

Reporting income data may potentially be sensitive for some participants, which could lead to participants non-responding to income items, introducing measurement error and, therefore, impairing the quality of these data [214]. Another reason for missing data may be that participants did not know the answer to the question or that data was irrelevant for a person, such as asking an unemployed person for their income from employee earnings [214]. Missing variables in longitudinal surveys may also arise from censoring of spell-length data for which the start and end data of the spell is missing [214]. Left censoring occurs when the income

spell was already in progress before the start of the reference period of a survey commenced, whereas right censoring occurs when a spell extends beyond the end date of the reference period [214]. Right censoring is relatively less problematic in terms of bias than left censoring, because data from the survey up to the final missing spell can be used to impute the length of the missing spell, whereas no such data for imputation is available to predict the length of a spell before the survey started [214]. Furthermore, missing values in one income variable is likely to have knock-on effects on aggregate income variables, such as missing data on one source of income for any one person in a family may lead to missing data on total family income in the family as a whole [214].

In the SoFIE, on average, around 11.4% of gross total annual personal income values had at least one income spell missing, with the equivalent percentage for such income from employee earnings being 4.3% of values and for government transfers being 5.1% missing at least one income spell (**Table 6**). Due to the complexity of the derivation of these variables, it is not possible to determine the extent of missing spells in these variables, or for example the percentage of participants who missed more than half of their spells. However, these statistics suggest a considerable level of non-response, raising some concerns about the quality of these income data.

Imputing missing income data was considered for the purpose of addressing the potential bias from these missing values on robust estimation of the effects of FTC and IWTC on SRH, that is selection bias. However, the small number of relevant studies on imputing income in health research that have been conducted do not convincingly demonstrate the benefit of multiple imputation of missing income data for this thesis study. Ryder *et al.*'s 2011 study found that imputing missing income data improved the predictive power of regression analyses that estimated cross-sectional associations between annual personal income and SRH in an American Mexican population [215]. However, Schenkel *et al.*'s 2006 study found that multiple imputation did not have an effect on the association between poverty measures and SRH [216]. The latter study is more relevant for this thesis, as poverty measures are more akin to the income threshold variables used as exposure variables in this thesis than is annual personal income. Furthermore, the effect of imputing missing income data in longitudinal analyses such as the fixed effects analyses conducted in this thesis remains widely unknown. Gottschalk and Huynh's 2010 longitudinal study suggested that imputing income data increased non-classical measurement error in measures of income mobility, when compared with complete-case analysis [217]. In addition, there were concerns for the feasibility of imputing the complex income data collected in spells in the SoFIE within the scope of this

**Table 6: Missing values in gross total annual personal income, total gross annual personal income from employee earnings and total gross annual personal income from government transfers at each cross-section, N=6,900, Waves 1 to 7**

Variable	Wave															
	Wave 1		Wave 2		Wave 3		Wave 4		Wave 5		Wave 6		Wave 7		Total	
	N	%	N	%	N	%	N	%	N	%	N	%	N	%	N	%
Gross total annual personal income	665	9.6	855	12.4	850	12.3	880	12.8	800	11.6	735	10.7	700	10.1	5485	11.4
Employee earnings	260	3.8	340	4.9	340	4.9	360	5.2	295	4.3	245	3.6	240	3.5	2080	4.3
Government transfers	265	3.8	380	5.5	355	5.1	400	5.8	380	5.5	350	5.1	350	5.1	2480	5.1

thesis research. Therefore, considering that there is as yet no clear indication in the literature that imputing missing income data should improve fixed effects estimates of the effect of FTC and IWTC on SRH and the considerable concerns about the feasibility of imputing these complex data as part of this thesis, missing income data were not imputed. This may, however, be a fruitful area of future research beyond the scope of this thesis.

It is not only misreporting that can impact the quality of income data, but systematic misreporting can also lead to non-classical measurement error that has the potential to reduce the quality of these data. Validation studies that compared self-reported income from employee earnings with employer records or tax records have found non-classical measurement error in these income data [217, 218]. The measurement error in these data was means reverting, meaning that participants on incomes above the mean tend to underreport and those below the mean tend to over-report their income [217, 218]. It was not feasible to validate the self-reported income data in the SoFIE, because these data were not linked to objective measures of income. However, some extreme incomes were reported, such as the highest 1.0% of total gross annual family incomes ranging from around \$450,000 up to over \$10,000,000 at Wave 1. Sensitivity analyses that excluded participants reporting the lowest and highest 1.0% of gross total annual family incomes or the largest income changes can thus provide some insights into the potential risk of bias from measurement error and are presented in *Chapter 9*.

## **Exposure variables**

### ***Family Tax Credit***

Two exposure variables were created for FTC, the first being *eligibility* for FTC, a two-level categorical variable (eligible, ineligible). The second exposure variable was the *dollar amount* of FTC that a family was eligible for (scaled at \$1,000). This variable was \$0 for ineligible participants and ranged from \$0 to the maximum FTC value of \$19,410 for eligible participants. The main fixed effects analyses on FTC eligibility estimated the effect of becoming FTC-eligible and those on FTC amount estimated the effect of an increase in the FTC amount by increments of \$1,000, such as an increase from 0\$ to \$1,000 or from \$12,500 to \$13,500. The relationship between the two exposures was that the eligibility exposure provided a binary measure of the FTC concept, whereas the amount exposure gave a continuous measure of the concept for eligible participants. The amount exposure therefore potentially had the advantage over the eligibility exposure that it provided a more accurate

measurement of the exposure variable. Eligibility and entitlement criteria for FTC, described previously in *Chapter 2*, are set out in the *2004 Taxation (Working for Families) Act* [98]. These criteria required an eligible person to be a New Zealand resident; be over 15 years of age; be the principal carer of one or more dependent children; and have a total gross annual family income within bounds defined by the number and age of the dependent children in their family [98]. All participants fulfilled the residency criterion, as only New Zealand residents were eligible to participate in the SoFIE. Furthermore, since only participants over 18 years of age were included in the study sample, all participants fulfilled the age criterion. A small number of parents aged 16 or 17 years that may have potentially been FTC-eligible were excluded from the analysis to ensure the generalisability of the study to the working-age population. Furthermore, all participants in the study sample were parents cohabiting with their children, whom I assumed to be the principal carers of these children. Therefore, all participants included in the study fulfilled the residency, age and co-habitation criteria for FTC eligibility and entitlement.

Participants differed in terms of whether their gross total annual family income from all sources before tax lay below or above the maximum threshold defined by the number (and age) of dependent children in the family. Similarly, they differed in terms of the specific FTC amount that they were entitled to, again defined by whether their gross total annual family fell within income bounds defined by the number of dependent children. The income thresholds for FTC eligibility and the specific FTC amount that a participant was eligible for were taken from the most precise available entitlement charts for FTC. The publicly available entitlement charts for FTC showed the range of FTC amounts that families within a range of total annual family income were eligible for. But they did not show the exact income threshold by the number (and age) of dependent children in the family, so that they could not be used to accurately derive eligibility for FTC. Therefore, the most detailed entitlement charts held by the Inland Revenue Department were sourced from the department (N. Oloapu-Atoni, personal communication, 13 September 2010). For example, the first page of the entitlement chart for the 2005 tax year presented in **Figure 22**, shows that in order to be FTC-eligible a participant in a family with one dependent child needed to have a gross total annual family income of less than \$34,500. It also shows, that a participant in a family with one dependent child, who was on a family income of between \$21,501 and \$23,500 was eligible for a fortnightly payment of \$122 or an annual payment of \$3,183 of FTC.

Chapter 5: Methods

**Figure 22: First page of an entitlement chart with the Family Tax Credit amount by income and number of dependent children for the 2005 tax year (1 April 2005 to 31 March 2006)**

Family income (before tax)		Fortnightly payments - year ending 31 Mar 2005 (including CTC)																			MFTC
Weekly \$	Annual \$	Number of children (including 1 baby for PTC)																			
		1			2			3			4			5			6				
		0-12 years			0-12 years			0-12 years			0-12 years			0-12 years			0-12 years				
\$	\$	FTC	CTC	PTC	FTC	CTC	PTC	FTC	CTC	PTC	FTC	CTC	PTC	FTC	CTC	PTC	FTC	CTC	PTC	\$	
up to 182	up to 9500	144	30	300	238	60	300	332	90	300	426	120	300	520	150	300	614	180	300	269	
up to 221	up to 11500	144	30	300	238	60	300	332	90	300	426	120	300	520	150	300	614	180	300	208	
up to 259	up to 13500	144	30	300	238	60	300	332	90	300	426	120	300	520	150	300	614	180	300	147	
up to 298	up to 15500	144	30	300	238	60	300	332	90	300	426	120	300	520	150	300	614	180	300	86	
up to 336	up to 17500	144	30	300	238	60	300	332	90	300	426	120	300	520	150	300	614	180	300	25	
up to 391	up to 20356	144	30	300	238	60	300	332	90	300	426	120	300	520	150	300	614	180	300		
up to 413	up to 21500	136	30	300	230	60	300	324	90	300	418	120	300	512	150	300	606	180	300		
up to 451	up to 23500	122	30	300	216	60	300	310	90	300	404	120	300	498	150	300	592	180	300		
up to 490	up to 25500	108	30	300	202	60	300	296	90	300	390	120	300	484	150	300	578	180	300		
up to 519	up to 27000	98	30	300	192	60	300	286	90	300	380	120	300	474	150	300	568	180	300		
up to 548	up to 28500	82	30	300	176	60	300	270	90	300	364	120	300	458	150	300	552	180	300		
up to 576	up to 30000	65	30	300	159	60	300	253	90	300	347	120	300	441	150	300	535	180	300		
up to 605	up to 31500	48	30	300	142	60	300	236	90	300	330	120	300	424	150	300	518	180	300		
up to 634	up to 33000	30	30	300	124	60	300	218	90	300	312	120	300	406	150	300	500	180	300		
up to 663	up to 34500	13	30	300	107	60	300	201	90	300	295	120	300	389	150	300	483	180	300		
up to 692	up to 36000		26	300	90	60	300	184	90	300	278	120	300	372	150	300	466	180	300		

Source: N. Oloapu-Atoni, Inland Revenue Department, personal communication, 13 September 2010. Notes: FTC = Family Tax Credit. CTC = Child Tax Credit. PTC = Parental Tax Credit. MFTC = Minimum Family Tax Credit. The fortnightly FTC amount by income and number of children extracted from the chart is marked grey. Dependent children are assumed to be 0 to 12 years old.

To derive eligibility for and the amount of FTC, these income bounds by the number of dependent children taken from the entitlement charts were systematically applied to two variables derived from the SoFIE, gross total annual family income and the number of dependent children in the family. The derivation protocol for gross total annual family income has been provided above. The number of dependent children in the family was derived in two steps. In the first step, each child's dependency status was determined, using the criteria set out in the *2004 Taxation (Working for Families) Act* [98]. The act defined a dependent child as a child living in the same family, who is 16 years of age or 16-18 years of age; working less than 30 hours per week; not receiving any government benefits; and, if 18 years old, attending secondary or tertiary education. In the second step, the number of dependent children per family was derived.

For the FTC eligibility variable, eligible participants were assigned a value of 1 and ineligible persons a value of 0. For the FTC amount, the amount of FTC that a parent's family unit was eligible for was derived and scaled at increments of \$1,000. In two-parent families, rather than allocating half of the FTC amount that the family unit was eligible for to each of the parents, the entire amount of FTC that the family unit was eligible for was allocated to both parents. The rationale was that what mattered was the total amount that a family was eligible for, especially considering that the entitlement unit of FTC families was the family, not individuals within the family. So, an equal value of \$3,000 was given to a participant in a one-parent family receiving a FTC amount of \$3,000 and to each of the participants in a two-parent family that together received a total amount of \$3,000 in FTC. The intention of the thesis was to study the effect of FTC in participants who were potentially FTC-eligible. Therefore, for waves during which a participant was not in a family and, therefore, could not possibly be FTC-eligible, their FTC eligibility and amount variables were set to missing. This meant that participants in the study sample were excluded from the fixed effects analyses for waves during which they were not in a family. It also means that any types of transitions into and out of families were not considered changes in FTC eligibility or amount.

The time point at which the FTC exposure variables were measured was at wave<sub>t</sub>. Since FTC eligibility status and entitlement is assessed on the basis of fulfilment of criteria over the previous year, measurement of the exposure variable at time point t reflects the FTC exposure variables *over the previous year*. The exposures were derived based on gross total annual family income over the previous year and number of dependent children at the current time.

The number of dependent children was considered relatively stable over a one year period, so that the number at the current time was seen as reflecting the number over the previous year.

Several assumptions were made in the derivation of FTC eligibility and amount that may potentially have introduced misclassification bias. Firstly, there may have been cases of misclassification of parents who shared care of a child, because cohabitation with the child at the time of the interview was assumed to indicate primary care of the child. Thus, primary carers whose child was at the child's secondary carer at the time of the interview may have been misclassified as FTC-ineligible, while secondary carers who had the child in their household at the time of the interview may have been misclassified as FTC-eligible. Secondly, the derivation relied on use of the gross total annual family income variable derived from the SoFIE, the data quality of which may be limited by missing values and misreporting, likely mean reverting, as described in the previous section. Missing income values would have resulted in an underestimation of gross total annual family incomes, which could have potentially resulted in misclassification of FTC-ineligible participants as FTC-eligible and an overestimation of their FTC amount. Means-reverting measurement error in this income variable may have led to misclassification of FTC-eligible participants as FTC-ineligible or eligible for smaller FTC amounts in participants on incomes below the mean, as well as misclassification of FTC-ineligible participants as FTC-eligible or eligible for larger FTC amounts. However, as long as this measurement error did not vary over time, it should have not led to misclassification bias in the fixed effects models, as is discussed later in the thesis. Thirdly, for participants who received FTC, their level of family income without FTC was overestimated, because the FTC exposure variables were derived from gross total annual family income, which comprises incomes from all government transfers, including FTC. Consequently eligibility for and amount of FTC may have been underestimated in participants who received FTC. Fourthly, the best available entitlement charts assumed that all dependent children in the family were less than 13 years of age. Although the age of dependent children was one of the determinants of the FTC amount, age could thus not be considered in the derivation of FTC amount, which may have introduced misclassification bias in the FTC amount variable. These potential misclassifications of the exposure variable are further discussed and their potential risk of bias assessed in *Chapter 9*.

### ***In-Work Tax Credit***

Equivalent exposures were also derived for IWTC, the first being IWTC *eligibility*, a two-level categorical variable (eligible, ineligible). The second exposure variable was the IWTC *amount*

that a participant's family was eligible for (scaled at \$1,000), a numerical variable ranging from \$0 to \$5,479. The relatively less variability in IWTC compared to FTC is explained by the fact that IWTC provides a standard payment of \$60 per week for families with three or less children and an additional \$10 per week for each further dependent child.

The *2004 Taxation (Working for Families) Act* [98] also sets out the eligibility and entitlement criteria for IWTC, described in *Chapter 2*. To be eligible for IWTC, a person had to meet the same criteria as for FTC in terms of New Zealand residency, minimum age and being a principal child carer. Again, all participants in the study sample met all these basic criteria, under the assumptions described above. In addition, eligible participants were required to fulfil minimum weekly working hour requirements and to not receive employment-tested social assistance payments. Finally, the gross total annual family income of IWTC-eligible persons needed to fall within a different set of income bounds defined by the number of dependent children in the family.

The SoFIE variables used to derive IWTC eligibility were total annual family income; number of dependent children in the family; total weekly hours worked at the time of the interview; and the type of government transfer from which a participant received income over the last year. The gross total annual family income and number of dependent children variables were derived as described above. The total weekly hours worked at the time of the interview was derived by Statistics New Zealand from information collected in employment spells by adding the number of hours for each type of employment over the reference period, as described in detail elsewhere [219]. The type of government transfer from which a participant received income over the last year was collected annually in the SoFIE [219], enabling identification of receipt of employment-conditional social assistance payments such as Unemployment Benefit and Domestic Purposes Benefit.

All participants interviewed prior to the introduction of the IWTC in October 2006 were set to IWTC-ineligible. Single parents working less than 21 hours per week were set to IWTC-ineligible, as were participants in two-parent families with parents jointly working less than 31 hours per week, using data on the total weekly hours worked at the time of the interview. Participants who received one or more employment-tested social assistance payments were also set to IWTC-ineligible. The other participants' eligibility status was determined based on total annual family income and the number of dependent children in the family. Income bounds for eligibility for and the amount of IWTC were extracted from publicly available entitlement charts such as **Figure 23** for the 2007 tax year [220]. These income bounds were

## Chapter 5: Methods

systematically applied to the derived variables, determining IWTC eligibility and amount, assigning ineligible persons a value of 0 and eligible participants a value of 1, as well as deriving the IWTC amount at wavet covering the previous year, scaled at increments of \$1,000, over the previous year.

**Figure 23: Entitlement chart for the In-Work Tax Credit amount by income and number of dependent children for the 2006 tax year (1 April 2006 to 31 March 2007)**

FAMILY INCOME (BEFORE TAX)		NUMBER OF CHILDREN											
		ONE		TWO		THREE		FOUR		FIVE		SIX	
Weekly \$	Annual \$	FTC \$	IWTC \$	FTC \$	IWTC \$	FTC \$	IWTC \$	FTC \$	IWTC \$	FTC \$	IWTC \$	FTC \$	IWTC \$
to 673	35,000	164	120	278	120	392	120	506	150	620	180	734	210
674 to 702	35,001 to 36,500	152	120	266	120	380	120	494	150	608	180	722	210
703 to 731	36,501 to 38,000	140	120	254	120	368	120	482	150	596	180	710	210
732 to 760	38,001 to 39,500	129	120	243	120	357	120	471	150	585	180	699	210
761 to 788	39,501 to 41,000	117	120	231	120	345	120	459	150	573	180	687	210
789 to 817	41,001 to 42,500	106	120	220	120	334	120	448	150	562	180	676	210
818 to 846	42,501 to 44,000	94	120	208	120	322	120	436	150	550	180	664	210
847 to 875	44,001 to 45,500	83	120	197	120	311	120	425	150	539	180	653	210
876 to 904	45,501 to 47,000	71	120	185	120	299	120	413	150	527	180	641	210
905 to 933	47,001 to 48,500	60	120	174	120	288	120	402	150	516	180	630	210
934 to 962	48,501 to 50,000	48	120	162	120	276	120	390	150	504	180	618	210
963 to 990	50,001 to 51,500	37	120	151	120	265	120	379	150	493	180	607	210
991 to 1,019	51,501 to 53,000	25	120	139	120	253	120	367	150	481	180	595	210
1,020 to 1,048	53,001 to 54,500	14	120	128	120	242	120	356	150	470	180	584	210
1,049 to 1,077	54,501 to 56,000	2	120	116	120	230	120	344	150	458	180	572	210
1,078 to 1,106	56,001 to 57,500		110	104	120	218	120	332	150	446	180	560	210
1,107 to 1,135	57,501 to 59,000		99	93	120	207	120	321	150	435	180	549	210
1,136 to 1,163	59,001 to 60,500		87	81	120	195	120	309	150	423	180	537	210
1,164 to 1,192	60,501 to 62,000		76	70	120	184	120	298	150	412	180	526	210
1,193 to 1,221	62,001 to 63,500		64	58	120	172	120	286	150	400	180	514	210
1,222 to 1,250	63,501 to 65,000		53	47	120	161	120	275	150	389	180	503	210
1,251 to 1,279	65,001 to 66,500		41	35	120	149	120	263	150	377	180	491	210
1,280 to 1,308	66,501 to 68,000		30	24	120	138	120	252	150	366	180	480	210
1,309 to 1,337	68,001 to 69,500		18	12	120	126	120	240	150	354	180	468	210
1,338 to 1,365	69,501 to 71,000		7		120	115	120	229	150	343	180	457	210
1,366 to 1,394	71,001 to 72,500				109	103	120	217	150	331	180	445	210
1,395 to 1,423	72,501 to 74,000				98	92	120	206	150	320	180	434	210
1,424 to 1,452	74,001 to 75,500				86	80	120	194	150	308	180	422	210
1,453 to 1,481	75,501 to 77,000				74	68	120	182	150	296	180	410	210
1,482 to 1,510	77,001 to 78,500				63	57	120	171	150	285	180	399	210
1,511 to 1,538	78,501 to 80,000				51	45	120	159	150	273	180	387	210
1,539 to 1,567	80,001 to 81,500				40	34	120	148	150	262	180	376	210
1,568 to 1,596	81,501 to 83,000				28	22	120	136	150	250	180	364	210
1,597 to 1,625	83,001 to 84,500				17	11	120	125	150	239	180	353	210
1,626 to 1,654	84,501 to 86,000				5		119	113	150	227	180	341	210
1,655 to 1,683	86,001 to 87,500						108	102	150	216	180	330	210
1,684 to 1,712	87,501 to 89,000						96	90	150	204	180	318	210
1,713 to 1,740	89,001 to 90,500						85	79	150	193	180	307	210
1,741 to 1,769	90,501 to 92,000						73	67	150	181	180	295	210
1,770 to 1,798	92,001 to 93,500						62	56	150	170	180	284	210
1,799 to 1,827	93,501 to 95,000						50	44	150	158	180	272	210
1,828 to 1,856	95,001 to 96,500						38	32	150	146	180	260	210
1,857 to 1,885	96,501 to 98,000						27	21	150	135	180	249	210
1,886 to 1,913	98,001 to 99,500						15	9	150	123	180	237	210
1,914 to 1,942	99,501 to 101,000						4		148	112	180	226	210
1,943 to 1,971	101,001 to 102,500								136	100	180	214	210
1,972 to 2,000	102,501 to 104,000								125	89	180	203	210
2,001 to 2,029	104,001 to 105,500								113	77	180	191	210
2,030 to 2,058	105,501 to 107,000								102	66	180	180	210
2,059 to 2,087	107,001 to 108,500								90	54	180	168	210
2,088 to 2,115	108,501 to 110,000								79	43	180	157	210
2,116 to 2,144	110,001 to 111,500								67	31	180	145	210
2,145 to 2,173	111,501 to 113,000								56	20	180	134	210
2,174 to 2,202	113,001 to 114,500								44	8	180	122	210
2,203 to 2,231	114,501 to 116,000								32		176	110	210
2,232 to 2,260	116,001 to 117,500								21		165	99	210
2,261 to 2,288	117,501 to 119,000								9		153	87	210
2,289 to 2,317	119,001 to 120,500										142	76	210

Source: Inland Revenue Department, 2007, p. 1 [220]. Notes: FTC = Family Tax Credit. IWTC = In-Work Tax Credit. For the FTC amounts, the eldest dependent child is assumed to be less than 16 years old and all other children are under 13 years old.

The same assumptions described above for FTC were made when IWTC was derived, potentially leading to misclassification bias. In addition, it was assumed that the number of hours worked per week at the time of the interview reflected the average number of hours per week over the previous year, which may have introduced classical measurement error. Any identified risk of bias from these assumptions is discussed in *Chapter 9*.

## **Outcome variable**

The main outcome variable of this thesis was SRH. However, subsidiary analyses were conducted with two other health outcomes, psychological distress and current tobacco smoking. All of these health outcomes have previously been identified as outcomes of key interest for studies assessing the health effects of tax credits [28, 29].

### ***Self-rated health***

The primary health outcome studied in this thesis was SRH, a subjective measure of general health status. A recent study of panel data from Australia has demonstrated that SRH captures a range of mental and physical health outcomes, most strongly reflecting vitality, followed by physical functioning, bodily pain and anticipated future health [221]. For this thesis, it was hypothesised that the FTC and IWTC exposures could have an effect on SRH through causal pathways through several health outcomes, from both the physical and mental domains. For example, to provide a break-down of one plausible causal pathway between FTC and SRH operating through other health outcomes, FTC may have reduced obesity, which may, in turn, have increased vitality and physical functioning, resulting in improved SRH.

While the ability of SRH to measure ‘true’, underlying general health remains to be debated [222], SRH has repeatedly been shown to reliably predict mortality in a broad range of populations [223]. The SoFIE did not provide the opportunity to validate the subjective SRH measure to objective measures of general health status, because the latter measures were not collected in or linked to the SoFIE. This means that the extent to which SRH measured ‘true’, underlying general health status could not be tested empirically in this thesis. However, a previous validation study found, for the SoFIE, that changes in SRH scores were correlated with changes in another subjective measure of general health status, SF-36, as well as changes in a measure of psychological distress, Kessler-10 [62]. A recent study found that SRH in an Australian survey was most strongly correlated with dimensions of psychological wellbeing, especially vitality, but also correlated strongly with self-assessed physical health status, especially physical functioning [221]. Another study found that change in SRH was not strongly

associated with self-assessed health change over the last year in the SoFIE [224]. Empirical evidence suggests that the predictive power of SRH for mortality risk is modified by socio-economic factors such as education [223, 225], ethnicity [226], occupation [223] and income [223], but it has been argued that this does not minimize the ability of SRH to produce valid and reliable measures of general health suitable for population health and health equity research [227, 228].

Evidence on the extent of the reliability of SRH is mixed, which has implications for measurement error in SRH. For example, while one study from Australia found that 28% of participants of a survey changed their response to the SRH question from before to after being asked a set of other questions about their health status [229], another study from the United States found 40% of participants did not consistently report their SRH in interviews taken within two months [230]. These studies suggest that the extent of measurement error in SRH measures is considerable [230]. Therefore, misclassification bias of the outcome variable is considered for this thesis in *Chapter 9*.

In the SoFIE, SRH was collected in the demographics module for all waves [213]. It was measured with the following standard question: *Looking at show card 9, in general would you say your health is excellent, very good, good, fair or poor?* Question administration with a show card ensured that participants could report their SRH without disclosing their response to the interviewer or other participants who were in the room when the interview was being conducted, ensuring the confidentiality of the response and thus potentially decreasing misreporting. The response categories were coded 5 (Excellent), 4 (Very good), 3 (Good), 2 (Fair) and 1 (Poor). Therefore, positive estimates in the main fixed effects regression analyses for the SRH variable, treated as continuous, can be interpreted as indicating that becoming eligible for or an increase in the amount of FTC or IWTC was associated with an improvement in SRH. Since the SRH question used in the SoFIE does not specify a reporting period, it was assumed that participants reported SRH at the current time. Under this assumption, the SRH outcome lags behind the tax credit exposures by one year in the main fixed effects regression analyses.

SRH was treated as a numeric (continuous) variable in the main regression analyses of this thesis. A recent study found that neighbouring SRH response categories were not evenly spaced, such as a change from poor to fair may be quantitatively different to a change from fair to good [231]. This suggest that treating SRH scores as linear may result in misspecification, and that it is best be treated as an ordinal variable. However, a previous

study using SoFIE data produced comparable estimates for the effect of income on SRH, whether SRH was treated as a continuous or ordinal variable [62]. Sensitivity analyses testing whether treating SRH as ordinal results produced similar results as treating SRH as continuous (being a misspecification) are presented in *Chapter 9*.

This thesis analyses the effect of change in FTC and IWTC on change in SRH, so that a discussion of potential challenges of the validity of using repeated measures of SRH is warranted. One potential challenge was that change in SRH could not capture health improvements in the large proportion of participants who report the highest response category, excellent health (ceiling effect) [224]. Similarly, decreases in health in the small proportion of participants who reported the lowest response category could also not be captured by change in SRH (floor effect) [224]. Since SRH measured SRH along a relatively crude five-score scale, participants with 'borderline' SRH, such as those falling between two SRH categories, may have reported different (neighbouring) categories over time [224]. Participants may also have changed their choice of reference group, in comparison to whom they rated their own health, over time [224]. The risk of bias from these potential sources of misclassification in the outcome variable is assessed in *Chapter 9*.

### ***Psychological distress***

Psychological distress was an outcome in subsidiary fixed effects regression that analysed the effect of the FTC and IWTC on health outcomes other than SRH. In the SoFIE, the Kessler-10 (also called K-10) questionnaire was asked in the health module at three waves, Waves 3, 5 and 7 [213]. This ten-item measure assessed non-specific psychological distress on a score ranging between 10 (lowest) and 50 (highest). A respondent was asked to rate her psychological distress during the last month on a scale between 1 (none of the time) and 5 (all of the time) along ten dimensions. The ten assessment dimensions were feeling worn out for no good reasons; feeling nervous; feeling so nervous that nothing could calm you down; feeling hopeless; feeling restless or fidgety; feeling so restless you could not sit still; feeling depressed; feeling that everything was an effort; feeling so sad that nothing could cheer you up; and feeling worthless. The Kessler-10 score was derived by summing response values over the ten assessment dimensions, arriving at a total Kessler-10 score that ranged between 10 and 50, which was treated as numerical (continuous) in the subsidiary analyses.

### ***Current tobacco smoking***

Current tobacco smoking was a health outcome in subsidiary fixed effects regression that analysed the effect of the FTC and IWTC on health outcomes other than SRH. Current regular tobacco smoking was collected in the health module of the SoFIE at three waves, Waves 3, 5 and 7 [213]. Participants were asked the following standard question: *Do you regularly smoke one or more tobacco cigarettes a day?*, with the following three response options: *Yes*, *No*, and *Don't know*. A participant responding *Yes* was categorised as a current regular tobacco smoker and assigned a 1, and a participant responding *No* was categorised as not a current regular tobacco smoker and assigned 0 (including both never smokers and ex-smokers). *Don't know* responses were set to missing and thus excluded from the analyses.

### **Potential confounding variables**

Several potential time-varying confounding variables for regression analyses of the FTC-SRH relationship were identified *a priori* from the analytical frameworks of the thesis presented in **Figure 19** and **Figure 20**. As mentioned previously, these were the variables that determined FTC and IWTC eligibility. For FTC, these variables were equivalised total gross annual family income (minus FTC or IWTC), family type and number of dependent children in the family. For IWTC, it was the same three variables, plus employment status.

### ***Equivalised gross total annual family income (minus Family Tax Credit or In-Work Tax Credit)***

Equivalised gross total annual family income (minus FTC or IWTC) was gross total annual family income, equivalised for household size and composition, and minus the FTC or IWTC amount, scaled at increments of \$10,000. One equivalised income variable had FTC deducted from it and was used to adjust analyses on FTC exposures for income. The other equivalised income variable had IWTC deducted for use in IWTC analyses.

In the first step, the FTC or IWTC amount was subtracted from gross total annual family income. As mentioned previously, gross total annual family income included income from government transfers, including from FTC and IWTC. As the data collected on the amount of FTC that a was deemed unreliable, as is argued in *Chapter 9*, and data on receipt of income from IWTC was not collected at all, it was not possible to simply deduct reported incomes from FTC and IWTC from gross total annual family incomes. Instead, the amount of income from FTC or IWTC derived for this thesis was subtracted from gross total annual family

income, arriving at gross total family income (minus FTC) and gross total family income (minus IWTC), respectively.

These variables were then equivalised for family composition (number of adults and children in the family), using the Jensen index, a New Zealand -specific tool for equivalising income for the number of children and adults in a family [224]. Equivalisation of a family income value was done by dividing the income by the relevant family-composition specific constant provided in the Jensen Index. For example, if a family with two adult and two dependent children had a gross total annual family income of \$60,000, then the equivalised family income was calculated by dividing the family income by the relevant constant taken from the Jensen index (1.41), giving an equivalised income of  $\$60,000 / 1.41 = \$42,553$ . Finally, the equivalised gross total annual family income (minus FTC or IWTC) variables were scaled at increments of \$10,000.

### ***Number of dependent children in the family***

The number of dependent children in the participant's family, which ranged from 0 to 10, was treated as a numeric (continuous) variable. It is emphasised again that only children who fulfilled the specific dependency criteria set for Working For Families tax credits were classified as dependent children. The derivation of this variable has already been described in the above section on FTC exposure variables.

### ***Family type***

In the SoFIE, detailed information on the composition of a participating household was collected the family module [213]. Participants were asked to report their relationship to each other member in the household, using standard official statistical questions on household composition and family type. Statistics New Zealand derived the type of each family in a household from this information, using derivation protocols that included application of the national official statistical standard for family type [219], arriving at four key family types, *single, couple (without children), one-parent family* and *two-parent family*.

As described above, the study sample was restricted to working-age parents in one-or two-parent families over two or more consecutive waves. Therefore, participants who were single adults or adult couples (without children) over all waves were excluded from the analysis. Participants included in the study were also excluded from the analysis for waves, during which they were single or in an adult couple (without children). Therefore, family type was

treated as a two-level categorical variable in the regression analyses, with the one-parent family category assigned a 0 (reference category) and the two-parent category assigned a 1.

### ***Employment status***

Employment status at the time of the interview was collected in the labour market module in the SoFIE, using the following question: *Using Showcard 13, can you tell me what you have been doing?* [213]. The following were the response categories shown on Showcard 13: 11 (*working - paid employee*), 12 (*working - self employed*), 13 (*working - family business or farm unpaid*), 14 (*working - casual work*), 15 (*not working - looking for work*), 16 (*not working - other activity*), 17 (*not living in New Zealand*), 88 (*don't know*) and 99 (*refused*). A three-level categorical variable was derived, with categories 11 to 14 classified as *employed* and assigned a 0 (reference category), category 15 classified as *unemployed* and assigned a 1, as well as category 16 classified as *inactive* and assigned a 2. The small number of participants reporting categories 17, 88 or 99 was set to missing, and their observation for the respective wave therefore excluded from the analyses.

### **Mediating variables**

The first potential mediating variable of both the FTC- and IWTC-SRH relationships was equivalised gross total annual family income (without FTC or IWTC). A second potential mediating variable, employment status, was theorized to potentially mediate the IWTC-SRH relationship.

### **Effect-modifying variables**

#### ***Māori / non-Māori ethnicity***

Ethnicity was collected independently at each wave from the following standard question *Looking at show card 7, choose as many responses as you need to say which ethnic groups you belong to*. Show card 7 listed the following standard response categories: 11 (*New Zealand European / Pakeha*), 12 (*other European*), 13 (*Māori*), 14 (*Samoan*), 15 (*Cook Island Māori*), 16 (*Tongan*), 17 (*Niuean*), 18 (*Tokelauan*), 19 (*Fijian*), 20 (*other Pacific Peoples*), 21 (*Southeast Asian*), 22 (*Chinese*), 23 (*Indian*), 24 (*other Asian*), 25 (*other ethnic group*) and 88 (*don't know*). Māori / non-Māori ethnicity was derived by longitudinally prioritising ethnicity, classifying a participant reporting her ethnicity as 13 (*Māori*) at one or more waves as *Māori* and assigning her a 1, and classifying participants reporting any other ethnicity or ethnicities

as *non-Māori* and assigning them a 0. The exception was that 88 (*don't know*) and missing responses were coded as missing to exclude them from the analysis.

### ***Poverty***

Poverty was defined as having an equivalised gross total annual family income (without FTC or IWTC) of below 50% of the median income in the study population. The median equivalised gross total annual family income (minus FTC or IWTC) was calculated for each wave. Participants with equivalised gross total annual family income (minus FTC or IWTC) below the poverty threshold were categorized as in poverty, assigned a 1. Participants with incomes above threshold were categorized as not in poverty, coded as 0.

## **Analytical methods used**

### **Basic cross-classified tabular analyses**

Basic tabular analyses cross-classifying change in the FTC and IWTC exposure variables with change in the outcome variable were conducted to provide initial and suggestive analyses for the main fixed effects regression analyses. Ratios akin to odds ratios were calculated as a measure of association between change in exposure variable and change in the outcome variable. For example, for FTC eligibility-SRH analyses, the ratio of the percentage of participants who increased their SRH, divided by the percentage of participants who decreased their SRH in those participants who became FTC-eligible was divided by this ratio in participants who became FTC-ineligible. More information about the measure of association is provided in *Chapter 6*, when it is used. These measures of association are inferior to the fixed effects regression analyses in that their interpretation is very complex and in that they are not adjusted for time-varying confounding. In effect, they provide baseline descriptive analyses presented in *Chapter 7* that help to support the interpretation of the more complex main fixed effects regression analyses.

### **Transition matrixes**

Six tables were produced for each exposure, outcome and potential time-varying confounding variables that cross-classified the variable at one wave with the variable at the consecutive wave. For example, FTC eligibility<sub>Wave 1</sub> was cross-classified with FTC eligibility<sub>Wave 2</sub>, then FTC eligibility<sub>Wave 2</sub> with FTC eligibility<sub>Wave 3</sub>, and so on. The six such tables for each variable were then summed into one transition matrix per variable that showed the average probability of

transitioning from one category to another between  $\text{wave}_t$  and  $\text{wave}_{t+1}$ . These transition matrixes, presented in *Chapter 7*, provide important descriptive statistic of the level of overall change in a variable over time, as well as information about the probability of all possible types of transitions between categories over the study period.

### **Calculation of associations in categorical analyses**

Cross-tabular analyses of change in the SRH outcome by change in the FTC and IWTC exposure variables provided important preliminary analyses for the main fixed effects regression analyses of the thesis. A measure of association akin to an OR was calculated from such tabular analyses that was used to predict the direction and size of the crude effect of the main analyses of this thesis. Ratios were calculated that were not classical odds, where the proportion  $p$  was divided by  $(1-p)$ , but were akin to and are referred to as odds for simplicity. These were the ratio of the percentage of participants who increased their SRH, divided by the percentage of participants who decreased their SRH. For the example of the FTC eligibility exposure, these odds were calculated for participants who became FTC-eligible and for those who became FTC-ineligible. To calculate the (measure of association akin to an) OR, the odds of participants who became FTC-eligible were then divided by the odds of participants who became FTC-ineligible. This OR can be interpreted as the effect of increasing SRH, compared to decreasing SRH in participants who became FTC-eligible, compared to those who became FTC-ineligible. Below a worked example is given. These analyses are pooled over all wave transitions, rather than done separately by wave. Because these analyses do not take into account that multiple changes for the same person are correlated, their 95% confidence intervals are likely to be overly narrow.

This OR estimator is not directly comparable either with the fully adjusted or the crude fixed effects regression estimators from the main fixed effects regression analyses in several ways. The OR estimates the association of two categories of changes of the exposure (becoming eligible and becoming ineligible) and categories of change in the outcome variable (increase versus decrease). In contrast, the linear fixed effects regression analyses treat the exposure and outcome variables as continuous, more precisely categorizing participants, which should cause less misspecification bias and thus provide a superior effect measures. Furthermore, the OR estimator is not adjusted for potential time-varying confounding, while the fully adjusted fixed effects regression estimator is adjusted for such confounding. Time may be the key confounder in these analyses, as both change in one variable and change in the second variable vary by time. This demonstrates the advantages of the more complex fixed effects

regression analyses. Nevertheless, the ‘analytic’ associations presented in this chapter start to reveal likely associations.

## Fixed effects regression analyses

The main analytic method used in this thesis was linear fixed effects regression analysis. Theoretical background information about this method is provided in this section, followed by a description of the specific application of the method in this thesis. Finally, subsidiary and sensitivity analyses are detailed.

### *Theoretical background*

The main analyses of this thesis used a cohort study design and individual-level linear fixed effects regression, as discussed in *Chapter 3*. Fixed effects regression methods are commonly applied in econometric and increasingly also in epidemiological research. They are suited for estimating the short-run association between a time-varying exposure variable and a time-varying outcome variable from repeated measures data [155, 162] and provide stronger evidence of causal association than cross-sectional associations since time invariant confounding ‘drops out’ of the analysis.

The basic model underlying linear fixed effects regression analysis in this thesis is stated in Formula 1 [162].

$$y_{it} = \mu_t + \beta x_{it} + \gamma z_i + \alpha_i + \varepsilon_{it} \quad i = 1, \dots, n; t = 1, \dots, T \quad (1)$$

where  $y_{it}$  is the outcome variable observed for individual  $i$  at time  $t$ ;  $x_{it}$  is a vector of (time-varying) covariates (including exposure variables) observed for individual  $i$  at time  $t$ ;  $z_i$  is a vector of (time-invariant) covariates observed for individual  $i$ ;  $\mu_t$  is an intercept which is allowed to vary over time;  $\beta$  and  $\gamma$  are unknown coefficient vectors of (time-varying and time-invariant) variables;  $\alpha_i$  are all time-invariant differences between individuals that are not accounted for by  $\gamma$ ; and  $\varepsilon_{it}$  is the random error term.

The assumptions of this linear fixed effects regression model are as follows. First, the time-invariant unobserved differences between individuals ( $\alpha_i$ ), which are regarded as fixed parameters (one per individual for all time points) can be correlated with  $x_{it}$ , as required for a confounding variable. Second, the covariance structure of a given individual’s response over all waves is compound symmetric. Third,  $x_{it}$  is independent of the random error terms not only at time point  $t$ , but at all time points (strict exogeneity). A consequence of strict exogeneity is that the covariates (and exposure variable)  $x_{it}$  cannot be associated with the outcome ( $y_{is}$ ) at

any previous (or past) time point  $s$  i.e., no reverse causation). Similarly, there can be no state dependence so that current  $y_{it}$  cannot be associated with the past or future outcomes  $y_{is}$ . If these assumptions are fulfilled, then a linear fixed effects regression analysis estimates the effect of a time-varying exposure on a time-varying outcome efficiently and without bias from unmeasured time-invariant confounders.

A simple case of fixed effects regression analysis occurs for just one repeated measure (two observations and time points) per individual. The following equations present the models for a measure at time point  $t = 1$  (**Formula 2**) and the repeated measure at time point  $t = 2$  (**Formula 3**) [162]:

$$y_{i1} = \mu_1 + \beta x_{i1} + \gamma z_i + \alpha_i + \varepsilon_{i1} \quad (2)$$

$$y_{i2} = \mu_2 + \beta x_{i2} + \gamma z_i + \alpha_i + \varepsilon_{i2} \quad (3)$$

Conventional fixed effects regression methods difference the effects of repeated measures per individual. In the simple case of one repeated measure the model for time point 1 is subtracted from the model for time point 2, as shown in **Formula 4** [162]:

$$y_{i2} - y_{i1} = (\mu_2 - \mu_1) + \beta(x_{i2} - x_{i1}) + (\varepsilon_{i2} - \varepsilon_{i1}) \quad (4)$$

In **Formula 4**, both  $\gamma z_i$  and  $\alpha_i$  no longer appear in the equation, having been differenced out. Therefore,  $\gamma$ , the coefficient for all time-invariant potential confounding variables, cannot be estimated from the equation, but are controlled for in the analysis. Moreover, as long as both random error terms ( $\varepsilon_{i1}$ ,  $\varepsilon_{i2}$ ) satisfy the assumption of the standard linear model, their difference also satisfies this assumption, even when they are correlated. Therefore, applying linear regression to the differenced scores provides estimates of  $\beta$  unbiased by time-invariant confounders. Only time-varying variables (or interactions of time-invariant and time-varying covariates) should be included in a fixed effects regression model, given that all time-invariant variables are differenced out of the model.

Fixed effects regression analyses have the important advantage over other regression approaches that they eliminate the effects of all time-invariant variables, because individuals act as their own controls in these analyses [155, 162]. Potential time-invariant confounding variables in this thesis include social factors such as gender, age and highest level of qualification (usually) as well as biological factors such as intelligence and genetic predispositions to ill-health. The ability to control for all time-invariant confounding variables presents a considerable advance, especially in the political epidemiological research, which

has traditionally relied on cross-sectional, ecological methods that are prone to time-invariant confounding, as described in *Chapter 3*.

However, fixed effects regression analyses also have disadvantages. Firstly, they cannot deal with time-varying mediating variables in the model [155, 162], as discussed previously when the analytical frameworks of the thesis were introduced. Secondly, fixed effects regression estimators are biased, if reverse causation occurs or state dependence (time-varying confounding), that is if the outcome causes the exposure at a consecutive wave [155, 162]. Thirdly, fixed effects regression cannot estimate the effects of time-invariant variables [155, 162]. Fourth, they may incur considerable loss of information resulting from discarding all observations that do not change over time, potentially reducing a study's precision [155, 162]. Fifth, and perhaps most importantly, they are useful for estimating short-run causal associations; it would take very high quality follow-up data over a long period of time to estimate long-run associations (e.g. the effect of income changes on health with ten year time lags).

### ***Main fixed effects regression analyses***

These main linear fixed effects regression analyses of this thesis estimated the association between becoming eligible for FTC or IWTC, and increasing the FTC or IWTC amount by \$1,000 respectively, on change in the SRH the individual level over the short term. As described in *Chapter 4*, other studies on the effect of tax credits on health status outcomes have applied similar fixed effects regression analytic methods, such as the 2010 Strully *et al.* study on the effect of EITC on maternal smoking in adult mothers in the United States [188].

The SAS 9.3 computer software was used to compute, for each exposure-outcome model, a crude and a fully adjusted fixed effects regression estimator, using the GLM Procedure. Fully adjusted analyses included each of the time-varying variables posited in the analytical framework to potentially confound the FTC-SRH or IWTC-SRH relationship. To check for potential collinearity of two or more variables in the fully adjusted models, these models were built by adding one further potential confounding variable at a time and by then checking for considerable changes, such as a doubling of the standard error, from model to model. Residuals for each model were also checked.

The missing values for each of the exposure, outcome, potential confounding and effect-modifying variables included in the main or subsidiary fixed effects model are presented in **Table 7**. An average over all seven waves, 17.6% of participants had missing values for each of

**Table 7: Missing values in exposure, outcome, potential time-varying confounding and potential effect-modifying variables at each cross-section, N=6,900, Waves 1 to 7**

Variable	Wave															
	Wave 1		Wave 2		Wave 3		Wave 4		Wave 5		Wave 6		Wave 7		Total	
	N	%	N	%	N	%	N	%	N	%	N	%	N	%	N	%
Exposure variables																
FTC eligibility, FTC amount, IWTC eligibility, IWTC amount <sup>a</sup>	1065	15.4	1110	16.1	1175	17.0	1220	17.7	1275	18.5	1315	19.1	1340	19.4	8500	17.6
Outcome variable																
SRH	5	0.1	155	2.3	200	2.9	155	2.2	155	2.3	150	2.2	0	0.0	820	1.7
Potential confounding variables																
Equivalised family income (minus FTC), equivalised family income (minus IWTC) <sup>b</sup>	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0
Family type	0	0.0	100	1.5	130	1.9	110	1.6	105	1.5	115	1.7	0	0.0	560	1.2
Number of children in family	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0
Employment status	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0	5	0.0	5	0.0
Potential effect-modifying variables																
Māori / non-Māori ethnicity	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0
Poverty <sup>b</sup>	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0	0	0.0

Notes: <sup>a</sup> The missing values in the four exposure variables are the values for people not in a family. <sup>b</sup> These income variables are derived from family income, for which about 10% of the sample have at least one missing spell (see **Table 6**).

the exposure, because they were not in a family at this wave. A small percentage (1.7%) of participants had missing outcome (SRH) data. The equivalised gross total annual family income (minus FTC or IWTC) had no missing values. However, gross total annual personal income, the income variable from which equivalised gross total annual family income (minus FTC or IWTC) was primarily derived, had 11.4% missing values, mostly explained by missing values in employee earnings (4.3%) and government transfers (5.1%), as described above. Therefore, to emphasise this again, Statistics New Zealand's derivation practice of summing personal income over all family members to calculate family income, without imputing for missing values, produced underestimate, and masked missing values in income components used to derive family income. The potential impact of the limited quality of the income data from the SoFIE on the main fixed effects analyses of this thesis is assessed in *Chapter 9*. The three other time-varying confounding variables had no or near-zero missing values. Finally, while the effect modifying variables also had no missing values, this may have reflected the fact that ethnicity was prioritized over seven waves, meaning that only participants who refused reporting their ethnicity at all seven waves would have occurred as missing values, as information from other waves was transferred through the longitudinal prioritization of ethnicity. Poverty was derived from equivalised gross total annual family income (minus FTC or IWTC), the limitations of which in terms of missing values have been emphasised. Thus, in summary, few participants had missing values in any of the variables, but the limitations of variables derived from gross total annual family income are noted as a potential concern.

The main fixed effects regression analyses estimate the effect of FTC or IWTC on SRH in participants eligible for, but not necessarily receiving the tax credit. Therefore, the fixed effects analyses estimate an effect akin to an intention-to-treat effect, rather than the average treatment effect in the treated. Intention-to-treat is an analytic approach in randomised controlled trials, where for "all patients allocated to each arm of the treatment regimen are analysed together 'as intended' upon randomisation, whether or not they actually received or completed the prescribed regimen" (p. 130) [1]. Intention-to-treat estimates arguably have primacy over other estimates for policy evaluations aiming to determine the effectiveness of a policy intervention regardless of its up-take. However, considering the evidence suggesting a high up-take of Working For Families tax credits, including FTC and IWTC [232], this thesis' intention-to-treat effect estimator should approximate the average treatment effect in the treated (eligible) population.

### ***Subsidiary analyses***

The subsidiary research questions posed in *Chapter 1* were answered in three sets of subsidiary fixed effects regression analyses. The first set of subsidiary analyses investigated effect modifications. Potential effect modification by Māori / non-Māori ethnicity was tested firstly by including an interaction term (e.g., for FTC eligibility, *FTC eligibility x ethnicity* and *year x ethnicity*) in the fully adjusted main fixed effects regression models and secondly by stratifying the main fixed effects regression analyses by Māori / non-Māori ethnicity. Equivalent analyses were also conducted to test for effect modification by poverty.

The second set of analyses estimated the effect of the FTC and IWTC on two other health outcomes, psychological distress and current tobacco smoking. Linear fixed effects regression analyses were run for both FTC and IWTC exposures with psychological distress as a continuous outcome variable. Logistic fixed effects regression analyses were also run for each exposure with current tobacco smoking as a binary outcome.

The third set of subsidiary analyses investigated potential longer-term effects of the exposures on SRH. The main fixed effects regression analyses were repeated with the outcome lagging behind the FTC exposure variables by two to six years. For example, the analyses lagging the effect by six years investigated the effect of change in FTC between Waves 1 and 2 on change in SRH between Waves 6 and 7. The main fixed effects regression analyses were repeated with the outcome lagging behind the IWTC exposure variables by two and three years. Since the first change in IWTC occurred with the introduction of the IWTC between Waves 3 and 4, the longest possible lag time that could be studied in this thesis was a three year lag, i.e. the effect of a change in IWTC exposure between Waves 3 and 4 on a change in SRH between Waves 6 and 7.

### ***Sensitivity analyses***

Several sensitivity analyses were conducted to test the robustness of the findings of the main fixed effects regression analyses. To judge the risk of bias from selection and attrition, firstly, the hypothetical size of the FTC- and IWTC-SRH effect in those participants who non-responded to or were lost to follow up from the survey was estimated that would be required to considerably alter the results of the main fixed effects regression analyses. Secondly, the main fixed effects regression analyses were adjusted for attrition and initial survey non-response by rerunning the main fixed effects regression analyses with longitudinal weights

that weighted the study sample to the New Zealand general population by gender, age and ethnicity.

To judge the potential risk of bias from misclassification bias of the exposure, the main fixed effects regression analyses with FTC as the exposure were repeated with self-reported FTC receipt and received amount as the exposure, as these self-reported survey data may potentially be less affected by misclassification bias. Furthermore, several assumptions of the derivation of the exposure variables were tested. This included re-running the main fixed effects regression analyses on the study sample restricted to families with children aged less than 13 years only to test for the effect of using entitlement charts assuming all children in the family were under 13 years of age. The extent of misclassification in the exposure variable that was required to considerably bias the effect estimate was calculated, simulating different scenarios of extent of measurement error and correlation of measurement error at wave<sub>t</sub> with measurement error at wave<sub>t+1</sub>. The risk of bias from misclassification of the outcome was assessed by estimating the extent of bias that would have been required to considerably bias the results of the main fixed effects regression analyses.

The main fixed effects analyses assumed that SRH could be treated as a continuous variable, but a recent study demonstrated that SRH categories are not even spaced [231], suggesting that SRH may better be specified as an ordinal variable. To judge the potential risk of bias from misspecification of the outcome as continuous in the main fixed effects regression analyses, hybrid (fixed effects) proportional odds models analyses [162] that treated the SRH outcome as ordinal were conducted.

Fixed effects analyses cannot estimate the effect of clustering by families or households, although these can be estimated, using random effects analytic methods. But adjusting for family type in the fixed effects models partially controlled for clustering by family. However, to test for clustering by families, a hybrid (fixed effects) proportional odds model analysis was conducted where a dummy identifying families was included in the random statement.

As a partial test for reverse causation, the main fixed effects regression analyses were repeated on the balanced panel of adult parents in families with good, very good or excellent SRH at baseline (Wave 1). This restriction to a healthy study sample (at baseline) eliminates the effect that bad health could have on the exposure (at baseline), therefore reducing the risk of bias from reverse causation.

## Conclusions

This chapter describes the data and methods that were used in this thesis to estimate the effect of FTC and IWTC on SRH. Seven waves of data were extracted from the SoFIE, an official household panel survey conducted by Statistics New Zealand between 2001 and 2010. The total SoFIE sample was restricted to a balanced panel of 6,900 adult (19 to 65 years) parents in one- or two-parent families over two or more consecutive waves.

The four exposure variables of this thesis were FTC and IWTC eligibility and amount, and the outcome was SRH. The potential time-varying confounding variables, identified *a priori* in the analytical frameworks of the thesis, were household-equivalised gross total annual family income (minus FTC or IWTC), family type, number of dependent children in the family and, for IWTC also, employment.

Linear fixed effects analyses were conducted to answer the principal and subsidiary research questions of the thesis. The main fixed effects regression analyses estimated the effect of FTC and IWTC eligibility and amount on SRH in adults at the individual level over the short term (one year lag), controlled for all time-invariant and adjusted for potential time-varying confounding.

Subsidiary analyses estimated effect modification by ethnicity and income level; the effect of FTC and IWTC over a longer time period (two to six year lags); and the effect of FTC and IWTC on two other relevant health outcomes, psychological distress and current tobacco smoking. Sensitivity analyses were conducted that assessed the risk of bias from selection, misclassification of the exposure variable, misspecification of the outcome variable and reverse causation in the main fixed effects analyses.

## Chapter 6: Cross-sectional analyses at baseline

This chapter describes the characteristics of the study sample at baseline in terms of key time-invariant variables as well as time-varying exposure, outcome and potential confounding variables. About one in six participants (16.5%) were eligible for FTC at Wave 1. The mean amount of FTC that the family of an eligible participant was entitled to was \$2,963 [standard deviation (SD) \$1,912]. Similarly, about one in six participants (15.4%) were IWTC-eligible, with the mean IWTC amount being \$2,722 (SD \$962) at Wave 4. Characteristics of FTC- and of IWTC-eligible participants corresponded with the eligibility criteria for these credits validating the derivation of these exposure variables.

Over three quarters of participants (78.7%) reported excellent, very good or good SRH at Wave 1. FTC-eligible participants were statistically significantly less likely than FTC-ineligible participants to report excellent, very good or good SRH [odds ratio (OR) 0.52, 95% confidence interval (CI) 0.41 to 0.67], but receiving a larger amount of FTC was not associated with SRH at Wave 1. No evidence for an association of IWTC eligibility and amount with SRH at baseline was found. However, these unadjusted cross-sectional tabular analyses are likely biased by time-invariant and time varying confounding.

The rate of loss to follow up in this thesis was 26.3%. Participants who were lost for follow-up were generally comparable in their demographic profile to those who remained in the study. Exceptions were that Māori, Pacific and less educated participants were disproportionately lost to follow-up of. Participants who were eligible and ineligible for FTC and IWTC were also more likely to be lost to follow up, whereas participants not in families were less likely to be lost to follow up, which may be due to the relatively large loss of Māori, Pacific and less educated participants from the study.

This chapter describes the characteristics of the study sample at baseline through tabular cross-sectional analysis. The study sample is the balanced panel of N = 6,900 working-age parents in families described in the previous chapter. The key time-invariant characteristics described are age, gender, ethnicity and highest qualification. Described is also the distribution in the study sample of the time-varying exposure variables, FTC and IWTC eligibility and amount; the outcome variable, SRH; and potential time-varying confounding variables, equivalised gross total annual family income (without FTYC or IWTC), employment status, number of children and family type. Furthermore, the chapter assesses the cross-sectional association between the exposure and the outcome variables at baseline. These cross-sectional analyses are not adjusted for potential confounding variables, so that they may therefore suffer bias from confounding and must therefore be interpreted with caution. However, these cross-sectional analyses provide preliminary analyses for the main fixed effects analyses presented in *Chapter 7*. The next chapter complements the cross-sectional descriptive statistics at baseline presented in this chapter with description of the time-varying variables over all waves of the SoFIE.

## Time-invariant variables

### Gender and age

The sample comprised more women than men (56.3% cf. 43.7%) (**Table 8**). The great majority of the sample was 25 to 54 years old (90.1%), with only around 5% each falling in the youngest (19 to 24 years) and oldest (55 to 64 years) age groups. Women and men were relatively equally spread over the five age groups.

**Table 8: Gender by age, N=6,900, Wave 1**

Age	Gender							
	Women			Men			Total	
	N	Col %	Row %	N	Col %	Row %	N	Col %
19-24	215	5.5	67.2	105	3.5	32.8	320	4.6
25-34	1085	27.9	60.8	700	23.3	39.2	1785	25.9
35-44	1570	40.4	57.5	1160	38.5	42.5	2730	39.6
45-54	880	22.7	49.9	885	29.4	50.1	1765	25.6
55-64	135	3.5	45.8	160	5.3	54.2	295	4.3
Total	3885	100.0	56.3	3010	100.0	43.7	6900	100.0

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator).

## **Ethnicity**

The majority of the sample had a New Zealand European (73.5%) or Māori (12.1%) prioritised ethnicity (**Table 9**). Only a total of 14.4% of the sample identified as Asian, Pacific or Other. Most ethnic groups comprised a larger proportion of women than men. In Māori, 62.7% of participants were women. Māori and Pacific participants were relatively younger than New Zealand European participants. For example, the percentage of Māori and Pacific participants in the youngest age group (19 to 24 years) was about twice that of New Zealand Europeans.

## **Highest qualification**

Most participants had either attained a (secondary) school (26.7%) or post-school (37.8%) qualification as their highest qualification (**Table 10**). Just fewer than one in five people had no qualification or had completed a (tertiary) degree, respectively. Women had achieved a slightly lower level of education than men. A lower percentage of women than men had completed a post- secondary school qualification as their highest qualification (42.1% cf. 34.7%), which was offset by a larger percentage of women with a secondary school qualification only (30.3% cf. 22.1%). However, the percentages of women and men with no qualification and with a degree were comparable.

Older age groups tended to be less highly educated than younger age groups. The proportion of participants with no qualification tended to be higher in older age groups, which was offset by lower proportions with a school qualification and degree or higher. However, similar proportions of all age groups held at least a post-school qualification.

Asian and Other ethnic groups tended to have the highest level of education, whereas Māori and Pacific participants were generally less highly educated. For example, Asians had the largest proportion of the most highly educated participants (with a degree or higher) (43.5%), followed by Other (29.4%) and New Zealand European (17.8%). In contrast, Māori (9.1%) and Pacific participants had considerably lower proportions with a degree.

## **Exposure variables**

### **Family Tax Credit eligibility and amount**

Overall, around 16.5% of participants were FTC eligible and 68.1% of were FTC-ineligible at Wave 1 (**Table 11**). Note that 15% of participants were not in a family and therefore were neither FTC-eligible nor –ineligible at Wave 1, but had missing values for FTC eligibility and did

**Table 9: Ethnicity by gender and age, N=6,900, Wave 1**

	Ethnicity														
	Māori			New Zealand European			Pacific			Asian			Total		
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	
<b>Gender</b>															
Male	330	37.3	11.0	2270	44.7	75.4	145	42.0	4.8	175	41.7	5.8	3010	43.7	
Female	555	62.7	14.3	2810	55.3	72.3	200	58.0	5.1	245	58.3	6.3	3885	56.3	
Total	885	100.0	12.8	5080	100.0	73.7	345	100.0	5.0	420	100.0	6.1	6895	100.0	
<b>Age</b>															
19-24	80	9.0	24.6	200	3.9	61.5	25	7.2	7.7	15	3.6	4.6	325	4.7	
25-34	280	31.6	15.7	1280	25.2	71.9	90	26.1	5.1	95	22.6	5.3	1780	25.8	
35-44	350	39.5	12.8	2010	39.6	73.6	110	31.9	4.0	180	42.9	6.6	2730	39.6	
45-54	140	15.8	7.9	1370	27.0	77.4	100	29.0	5.6	110	26.2	6.2	1770	25.7	
55-64	35	4.0	11.9	215	4.2	72.9	20	5.8	6.8	20	4.8	6.8	295	4.3	
Total	885	100.0	12.9	5075	100.0	73.5	345	100.0	5.0	420	100.0	6.1	6900	100.0	

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). N = 165 (0.0%) participants reporting an *Other* ethnicity are not shown in the table, but are counted in the totals.

Chapter 6: Cross-sectional analyses at baseline

**Table 10: Highest qualification by gender, age and ethnicity N=6,900, Wave 1**

	Highest qualification													
	No qualification			School qualification			Post-school qualification			Degree or higher			Total	
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %
<b>Gender</b>														
Male	515	42.9	17.1	665	36.1	22.1	1270	48.6	42.1	565	45.6	18.7	3015	43.7
Female	685	57.1	17.7	1175	63.9	30.3	1345	51.4	34.7	675	54.4	17.4	3880	56.3
Total	1200	100.0	17.4	1840	100.0	26.7	2615	100.0	37.9	1240	100.0	18.0	6895	100.0
<b>Age</b>														
19-24	55	4.5	16.9	125	6.8	38.5	115	4.4	35.4	30	2.4	9.2	325	4.7
25-34	250	20.7	14.0	510	27.6	28.6	685	26.2	38.4	340	27.4	19.0	1785	25.8
35-44	475	39.3	17.4	740	40.1	27.1	1020	39.0	37.3	500	40.3	18.3	2735	39.6
45-54	340	28.1	19.2	415	22.5	23.4	680	26.0	38.4	335	27.0	18.9	1770	25.6
55-64	90	7.4	30.5	55	3.0	18.6	115	4.4	39.0	35	2.8	11.9	295	4.3
Total	1210	100.0	17.5	1845	100.0	26.7	2615	100.0	37.8	1240	100.0	17.9	6910	100.0
<b>Ethnicity</b>														
Māori	275	22.8	31.3	190	10.3	21.6	335	12.8	38.1	80	6.5	9.1	880	12.7
NZ European	760	63.1	14.9	1385	75.1	27.2	2035	77.7	40.0	905	73.0	17.8	5085	73.6
Pacific	105	8.7	30.0	145	7.9	41.4	80	3.1	22.9	20	1.6	5.7	350	5.1
Asian	55	4.6	12.9	90	4.9	21.2	95	3.6	22.4	185	14.9	43.5	425	6.2
Total	1205	100.0	17.5	1845	100.0	26.7	2620	100.0	37.8	1240	100.0	17.9	6910	100.0

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). N = 165 (0.0%) participants reporting an Other ethnicity are not shown in the table, but are counted in the totals. NZ European = New Zealand European.

Chapter 6: Cross-sectional analyses at baseline

**Table 11: Family Tax Credit eligibility and amount by time-invariant and time-varying variables, N=6,900, Wave 1**

	FTC eligibility			FTC amount									Not in a family			Total	
	Eligible			Not eligible			Q1-Q2 (\$1 to \$2,452)			Q3-Q5 (\$2,453 to \$19,410)							
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %
Time-invariant variables																	
Gender																	
Male	340	29.8	11.3	2145	45.6	71.0	155	35.2	2.2	185	26.4	2.7	535	50.2	17.7	3020	43.7
Female	800	70.2	20.6	2555	54.4	65.8	285	64.8	4.1	515	73.6	7.5	530	49.8	13.6	3885	56.3
Total	1140	100.0	16.5	4700	100.0	68.1	440	100.0	6.4	700	100.0	10.1	1065	100.0	15.4	6905	100.0
Age																	
19-24	80	7.1	25.0	90	1.9	28.1	30	6.9	0.4	50	7.1	0.7	150	14.2	46.9	320	4.6
25-34	390	34.5	21.8	910	19.4	51.0	160	36.8	2.3	235	33.6	3.4	485	45.8	27.2	1785	25.9
35-44	490	43.4	18.0	2050	43.6	75.2	180	41.4	2.6	310	44.3	4.5	185	17.5	6.8	2725	39.6
45-54	155	13.7	8.8	1445	30.7	81.6	60	13.8	0.9	95	13.6	1.4	170	16.0	9.6	1770	25.7
55-64	15	1.3	5.2	205	4.4	70.7	5	1.1	0.1	10	1.4	0.1	70	6.6	24.1	290	4.2
Total	1130	100.0	16.4	4700	100.0	68.2	435	100.0	6.3	700	100.0	10.1	1060	100.0	15.4	6890	100.0
Ethnicity																	
Māori	270	23.7	30.5	505	10.8	57.1	80	18.6	1.2	190	27.7	2.8	110	10.3	12.4	885	12.8
NZ European	630	55.3	12.4	3615	77.0	71.1	285	66.3	4.1	340	49.6	4.9	840	78.9	16.5	5085	73.7
Pacific	105	9.2	30.4	205	4.4	59.4	40	9.3	0.6	65	9.5	0.9	35	3.3	10.1	345	5.0
Asian	110	9.6	26.2	255	5.4	60.7	25	5.8	0.4	90	13.1	1.3	55	5.2	13.1	420	6.1
Total	1140	100.0	16.5	4695	100.0	68.0	430	100.0	6.2	685	100.0	9.9	1065	100.0	15.4	6900	100.0
Highest qualification																	
No qualification	325	28.6	27.0	750	16.0	62.2	120	27.3	1.7	205	29.5	3.0	130	12.3	10.8	1205	17.5
School qualification	320	28.2	17.4	1270	27.0	69.0	135	30.7	2.0	185	26.6	2.7	250	23.6	13.6	1840	26.7
Post-school qualification	385	33.9	14.8	1795	38.2	68.8	150	34.1	2.2	235	33.8	3.4	430	40.6	16.5	2610	37.9
Degree or higher	105	9.3	8.5	885	18.8	71.4	35	8.0	0.5	70	10.1	1.0	250	23.6	20.2	1240	18.0
Total	1135	100.0	16.5	4700	100.0	68.2	440	100.0	6.4	695	100.0	10.1	1060	100.0	15.4	6895	100.0
Time-varying variables																	
Gross total annual family income																	

Chapter 6: Cross-sectional analyses at baseline

Q1 (<\$31,951)	905	19.8	65.6	245	5.2	17.8	260	59.1	3.8	650	92.9	9.4	225	21.2	16.3	1380	20.0
Q2 (\$31,951 to \$51,271)	225	0.4	16.3	945	20.1	68.5	175	39.8	2.5	50	7.1	0.7	210	19.8	15.2	1380	20.0
Q3 (\$51,272 to \$69,861)	5	0.4	0.4	1190	25.3	86.2	5	1.1	0.1	0	0.0	0.0	185	17.5	13.4	1380	20.0
Q4 (\$69,862 to \$99,871)	0	0.0	0.0	1130	24.0	81.9	0	0.0	0.0	0	0.0	0.0	250	23.6	18.1	1380	20.0
Q5 (>\$99,871)	0	0.0	0.0	1190	25.3	86.2	0	0.0	0.0	0	0.0	0.0	190	17.9	13.8	1380	20.0
Total	1135	100.0	16.4	4700	100.0	68.1	440	100.0	6.4	700	100.0	10.1	1060	100.0	15.4	6900	100.0
Equivalised gross total annual family income (minus FTC)																	
Q1 (<\$23,128)	975	14.5	70.7	300	6.4	21.7	275	62.5	4.0	700	100.0	10.1	105	9.8	7.6	1380	20.0
Q2 (\$23,128 to \$37,311)	165	0.0	12.0	1120	23.8	81.2	165	37.5	2.4	0	0.0	0.0	100	9.3	7.2	1380	20.0
Q3 (\$37,312 to \$50,780)	0	0.0	0.0	1235	26.3	89.5	0	0.0	0.0	0	0.0	0.0	150	14.0	10.9	1380	20.0
Q4 (\$50,781 to \$73,298)	0	0.0	0.0	1130	24.0	81.9	0	0.0	0.0	0	0.0	0.0	250	23.4	18.1	1380	20.0
Q5 (>\$73,298)	0	0.0	0.0	915	19.5	66.3	0	0.0	0.0	0	0.0	0.0	465	43.5	33.7	1380	20.0
Total	1140	100.0	16.5	4700	100.0	68.1	440	100.0	6.4	700	100.0	10.1	1070	100.0	15.5	6900	100.0
Family type																	
Single, couple only	0	0.0	0.0	0	0.0	0.0	0	0.0	0.0	0	0.0	0.0	1065	100.0	15.4	1065	15.4
One-parent	465	41.0	54.1	395	8.4	45.9	140	31.8	2.0	330	47.1	4.8	0	0.0	0.0	860	12.5
Two-parent	670	59.0	13.5	4300	91.6	86.5	300	68.2	4.3	370	52.9	5.4	0	0.0	0.0	4970	72.1
Total	1135	100.0	16.5	4695	100.0	68.1	440	100.0	6.4	700	100.0	10.1	1065	100.0	15.4	6895	100.0
Number of children																	
0	0	0.0	0.0	960	20.4	0.0	0	0.0	0.0		0.0	0.0	1065	100.0	15.4	2025	29.3
1	360	31.6	20.2	1425	30.3	79.8	165	37.5	2.4	195	27.9	2.8	0	0.0	0.0	1785	25.9
2	430	37.7	22.1	1515	32.2	77.9	185	42.0	2.7	245	35.0	3.6	0	0.0	0.0	1945	28.2
3	205	18.0	24.0	650	13.8	76.0	55	12.5	0.8	150	21.4	2.2	0	0.0	0.0	855	12.4
4-10	145	12.7	49.2	150	3.2	50.8	35	8.0	0.5	110	15.7	1.6	0	0.0	0.0	295	4.3
Total	1140	100.0	16.5	4700	100.0	68.1	440	100.0	6.4	700	100.0	10.1	1065	100.0	15.4	6905	100.0

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). Q1-Q2 = quintiles 1-2. Q3-Q5 = quintiles 3-5. NZ European = New Zealand European. The subsample of FTC-eligible participants appears twice in the table, namely first in total in the 'FTC eligibility' column under 'Eligible' and second in the 'FTC amount' column, disaggregated into the 'Q1-Q2' and 'Q3-Q5' quintile groupings. N = 165 (0.0%) participants reporting an Other ethnicity are not shown in the table, but are counted in the totals.

thus not contribute data to the main fixed effects regression analyses for Wave 1. These participants, however, moved into a family and stayed in a family for at least two consecutive waves during the study period.

FTC-eligible participants were predominantly female (70.2%), with female participants being nearly twice as likely as male participants to be FTC-eligible, likely reflecting the relatively large percentage of single mothers on relatively low family incomes in the sample. Fewer female than male participants were not in a family. Most eligible participants were 35 to 44 and 25 to 34 years old (43.4% and 34.5%, respectively), followed by 45 to 54 years (13.7%). The youngest and oldest age groups had a low proportion of eligible participants (7.1% and 1.3%, respectively), but had relatively large proportions of participants who were not in a family. Just over half of all eligible participants were New Zealand European (55.3%), about one quarter were Māori (23.7%) and about 10% each Pacific and Asian. The highest proportion of FTC-eligible participants was for the Māori and Pacific ethnic groups (30.5% and 30.4%, respectively), followed closely by the Asian group (26.2%). The lowest proportion of FTC-eligible participants was observed for New Zealand Europeans and participants reporting Other ethnicities (about 15%).

FTC-eligible participants had the following time-varying characteristics at Wave 1. Only participants in the lowest three quintiles of total annual family income were eligible for FTC, with 65.6% in quintile 1 (lowest), 16.3% in quintile 2 and 0.4% in quintile 3. This is consistent with eligibility criteria that reserve FTC eligibility for low- and lower middle-income families. While 59.0% of eligible participants were in a two-parent family, 41.0% were in a one-parent family. However, one-parent families were considerably more likely to be eligible for FTC than two-parent families (54.1% cf. 13.5%). Most eligible participants had one child or two children (31.6% and 37.7%, respectively), followed by three and four to ten dependent children (18.0% and 12.7%, respectively). However, eligibility for FTC tended to be larger for participants with a larger number of dependent children. Again, this is consistent with the eligibility criteria for FTC.

The mean dollar amount of FTC that an eligible family was entitled to was \$2,963 [standard deviation (SD) \$1,914] and the median was \$2,452 at Wave 1. The FTC amount was 0 for all FTC-ineligible participants and set to missing for those not in a family. Female participants tended to fall within higher quintiles of FTC than male participants. For example, female participants had over twice the proportion of the highest three quintiles of FTC of male participants (8.9% cf. 4.0%), again likely reflecting the relative large percentage of single mothers. Age groups were distributed in a complex pattern over FTC amount quintiles.

However, the proportion of participants entitled to the lower two quintiles of FTC was relatively large among the younger age groups, while the proportion of those in the highest three quintiles was largest amongst 25 to 34 and 35 to 44 year olds. There were higher percentages of Pacific, Māori and Asian than of New Zealand European and Other participants in both FTC amount groups. Less highly qualified participants dominated all quintiles of FTC amount. All participants entitled to a larger amount of FTC were on very low income (lowest quintile), whereas those receiving less FTC were on low and very low incomes (lowest two quintiles), which would be expected, considering the design of FTC. One-parent families tended to be considerably more likely than two-parent families to receive both smaller and larger amounts of FTC. Participants with larger numbers of dependent children tended to receive larger amounts of FTC, reflecting the FTC eligibility and entitlement criteria.

### **In-Work Tax Credit eligibility and amount**

The proportion of participants eligible for IWTC at Wave 1 was 0%, because IWTC had not yet been introduced. The baseline for analyses of the effect of IWTC on health was Wave 4, when the IWTC were introduced. Therefore, the characteristics of participants eligible for IWTC are described for Wave 4. A total of 12.9% of the study sample was eligible for IWTC at Wave 4 (**Table 12**). 17.7% of participants were not in a family so were neither eligible nor ineligible and were set to missing, thus contributing no data to the main fixed effects regression analyses.

Female and male participants were equally as likely to be eligible for IWTC. Over one in four (28.1%) of 25 to 44 year-old participants were eligible for IWTC, but only about 10% of participants aged 19 to 24 and 45 to 54 years and only 2.3% within the 55 to 64 age group. Of IWTC-eligible participants, 70.1% were New Zealand Europeans, followed by just over 13.3% being Māori and around 7% being Pacific and Asian. Within their ethnic groups, Pacific people had the largest percentage of IWTC eligible participants (23.2%). All other ethnic groups had smaller and about equal percentages of about 15% of participants being IWTC-eligible. While 40.3% of eligible participants had a post school qualification and 29.4% a secondary school qualification, only around 15% had no qualification and a degree or higher education, respectively. The relatively low proportion of IWTC-eligible participants with a lower level of qualification (e.g., in comparison to the FTC- eligible group described above) reflects the 'welfare-testing' of the IWTC, where participants on employment-tested benefits (i.e., a likely less qualified group) were excluded from eligibility for the IWTC. Eligible participants tended to be on lower and middle-income family incomes, with no participant in the highest quintile

Chapter 6: Cross-sectional analyses at baseline

**Table 12: In-Work Tax Credit eligibility and amount by time-invariant and time-varying variables, N=6,900, Wave 4**

	IWTC eligibility			IWTC amount									Not in a family			Total	
	Eligible			Not eligible			Q1-Q2 (\$1 to \$3,130)			Q3-Q5 (\$3,131 to \$5,479))							
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %
<b>Time-invariant variables</b>																	
<b>Gender</b>																	
Male	485	45.8	16.1	1955	42.3	64.7	155	47.0	5.1	320	44.8	10.6	580	47.5	19.2	3020	43.7
Female	575	54.2	14.8	2670	57.7	68.7	175	53.0	4.5	395	55.2	10.2	640	52.5	16.5	3885	56.3
Total	1060	100.0	15.4	4625	100.0	67.0	330	100.0	4.8	715	100.0	10.4	1220	100.0	17.7	6905	100.0
<b>Age</b>																	
19-24	10	1.0	10.5	40	0.9	42.1	10	2.9	9.5	10	1.4	9.5	45	3.7	47.4	95	1.4
25-34	295	28.1	22.4	760	16.4	57.8	85	24.6	6.4	210	29.4	15.9	260	21.4	19.8	1315	19.1
35-44	525	50.0	20.3	1840	39.8	71.0	155	44.9	6.0	365	51.0	14.1	225	18.5	8.7	2590	37.6
45-54	205	19.5	9.2	1605	34.7	71.8	85	24.6	3.8	120	16.8	5.4	425	35.0	19.0	2235	32.4
55-64	15	1.4	2.3	380	8.2	58.0	10	2.9	1.5	10	1.4	1.5	260	21.4	39.7	655	9.5
Total	1050	100.0	15.2	4625	100.0	67.1	345	100.0	5.0	715	100.0	10.3	1215	100.0	17.6	6890	100.0
<b>Ethnicity</b>																	
Māori	140	13.3	15.9	605	13.1	68.8	40	11.9	4.5	100	14.1	11.3	135	11.1	15.3	880	12.8
NZ European	740	70.1	14.6	3380	73.2	66.5	250	74.6	4.9	485	68.3	9.5	960	78.7	18.9	5080	73.7
Pacific	80	7.6	23.2	215	4.7	62.3	20	6.0	5.8	60	8.5	17.4	50	4.1	14.5	345	5.0
Asian	70	6.6	16.7	305	6.6	72.6	15	4.5	3.6	55	7.7	13.1	45	3.7	10.7	420	6.1
Total	1055	100.0	15.3	4620	100.0	67.0	335	100.0	4.9	710	100.0	10.3	1220	100.0	17.7	6895	100.0
<b>Highest qualification</b>																	
No qualification	155	14.7	13.9	750	16.2	67.3	40	12.1	3.6	115	16.1	10.3	210	17.2	18.8	1115	16.2
School qualification	310	29.4	18.5	1100	23.8	65.7	85	25.8	5.1	220	30.8	13.1	265	21.7	15.8	1675	24.3
Post-school qualification	425	40.3	15.4	1800	39.0	65.3	140	42.4	5.1	280	39.2	10.2	530	43.4	19.2	2755	40.0
Degree or higher	165	15.6	12.2	970	21.0	71.9	65	19.7	4.8	100	14.0	7.4	215	17.6	15.9	1350	19.6
Total	1055	100.0	15.3	4620	100.0	67.0	330	100.0	4.8	715	100.0	10.4	1220	100.0	17.7	6895	100.0
<b>Time-varying variables</b>																	
<b>Family income</b>																	
Q1 (<\$31,951)	185	41.2	13.4	820	17.7	59.4	0	0.0	0.0	185	25.7	13.4	375	30.7	27.2	1380	20.0

Chapter 6: Cross-sectional analyses at baseline

Q2 (\$31,951 to \$51,271)	435	36.5	31.5	720	15.6	52.2	50	14.9	3.6	385	53.5	27.9	225	18.4	16.3	1380	20.0
Q3 (\$51,272 to \$69,861)	385	36.5	28.0	785	17.0	57.1	250	74.6	18.2	135	18.8	9.8	205	16.8	14.9	1375	19.9
Q4 (\$69,862 to \$99,871)	50	4.7	3.6	1090	23.6	79.0	35	10.4	2.5	15	2.1	1.1	240	19.7	17.4	1380	20.0
Q5 (>\$99,871)	0	0.0	0.0	1205	26.1	87.3	0	0.0	0.0	0	0.0	0.0	175	14.3	12.7	1380	20.0
Total	1055	100.0	15.3	4620	100.0	67.0	335	100.0	4.9	720	100.0	10.4	1220	100.0	17.7	6895	100.0
Equivalentised family income (minus IWTC)																	
Q1 (<\$28,307)	275	42.0	19.9	880	19.0	63.8	5	1.5	0.4	270	37.8	19.6	225	18.4	16.3	1380	20.0
Q2 (\$28,307 to \$42,000)	445	31.1	32.4	810	17.5	58.9	60	17.9	4.4	380	53.1	27.6	120	9.8	8.7	1375	19.9
Q3 (\$42,001 to \$58,823)	330	31.1	23.8	890	19.3	64.3	260	77.6	18.8	65	9.1	4.7	165	13.5	11.9	1385	20.1
Q4 (\$58,824 to \$85,728)	10	0.9	0.7	1090	23.6	79.0	10	3.0	0.7	0	0.0	0.0	280	23.0	20.3	1380	20.0
Q5 (>\$85,728)	0	0.0	0.0	950	20.6	68.8	0	0.0	0.0	0	0.0	0.0	430	35.2	31.2	1380	20.0
Total	1060	100.0	15.4	4620	100.0	67.0	335	100.0	4.9	715	100.0	10.4	1220	100.0	17.7	6900	100.0
Family type																	
Single, couple only	0	0.0	0.0	0	0.0	0.0	0	0.0	0.0	0	0.0	0.0	1220	100.0	100.0	1220	17.7
One-parent	100	9.5	12.2	720	15.6	87.8	15	4.5	1.8	85	11.9	10.4	0	0.0	0.0	820	11.9
Two-parent	955	90.5	19.7	3905	84.4	80.3	315	95.5	6.5	630	88.1	13.0	0	0.0	0.0	4860	70.4
Total	1055	100.0	15.3	4625	100.0	67.0	330	100.0	4.8	715	100.0	10.4	1220	100.0	17.7	6900	100.0
Number of children																	
0	0	0.0	0.0	1220	26.4	100.0	0	0.0	0.0	0	0.0	0.0	1220	100.0	79.6	2440	34.9
1	290	27.5	17.5	1365	29.5	82.5	130	38.8	7.9	160	22.1	9.7	0	0.0	0.0	1655	24.0
2	450	42.7	24.3	1405	30.4	75.7	155	46.3	8.4	295	40.7	15.9	0	0.0	0.0	1855	26.9
3	225	21.3	31.5	490	10.6	68.5	45	13.4	6.3	180	24.8	25.2	0	0.0	0.0	715	10.4
4 to ten	90	8.5	39.1	140	3.0	60.9	5	1.5	2.2	90	12.4	39.1	0	0.0	0.0	230	3.3
Total	1055	100.0	15.3	4620	100.0	67.0	335	100.0	5.0	725	100.0	10.8	1220	100.0	17.7	6895	100.0

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). Q1-Q2 = quintiles 1-2. Q3-Q5 = quintiles 3-5. NZ European = New Zealand European. The subsample of IWTC-eligible participants appears twice in the table, namely first in total in the 'IWTC eligibility' column under 'Eligible' and second in the 'IWTC amount' column, disaggregated into the 'Q1-Q2' and 'Q3-Q5' quintile groupings. N = 165 (0.0%) participants (notes continued on next page) reporting an *Other* ethnicity are not shown in the table, but are counted in the totals.

of family income being eligible for IWTC. This pattern suggests, consistent with the aim and design of the tax credit, that IWTC principally covered mostly low- and middle-income groups in the study sample, although not some low-income subgroups such as welfare recipients and retirees. However, these comparisons are likely confounded by age. Two-parent families were relatively more likely to be IWTC-eligible than one-parent families, also reflecting the welfare-testing of the IWTC. Most IWTC-eligible participants had two dependent children (42.7%), followed by one (27.5%) and three (21.3%) dependent children. Again, this is consistent with the eligibility criteria for IWTC, where income boundaries are lower for families with larger number of dependent children.

The amount variable assigns, for IWTC eligible participants, the dollar value of IWTC that they were entitled to. The mean IWTC amount was \$2,715 (SD: \$952), and the median was \$3,131. Similar proportions of women and men were eligible for the lowest two and highest three quintiles of FTC amount. A relatively large proportion of around 15% of 25 to 34 and 35 to 44 year olds were eligible for the highest three quintiles of IWTC. Pacific, Māori and Asian participants also were relatively more likely than New Zealand Europeans and Other ethnic groups to be entitled for larger amounts of IWTC. Around 10%-15% of participants with no, school and post-school qualifications were entitled to the largest three quintiles of IWTC, again reflecting the relatively larger coverage of middle-income groups in the IWTC than in the FTC. Participants entitled to the largest three quintiles of IWTC tended to be in two-parent families. The higher the number of dependent children in their family, the more likely a participant was to be entitled to the largest three IWTC amount quintiles, in line with IWTC eligibility and entitlement criteria.

## Outcome variable

Overall, around 45% of all participants reported excellent, 35% very good, and 15% good SRH at Wave 1. A small proportion of participants reported fair (5%) and poor (1%) SRH. When treated as a linear variable between the bounds of 1 (poor) and 5 (excellent), the mean of the SRH variable was 4.17 (SD: 0.92) and the median was 4. The proportion of female and male participants was comparable across SRH categories. Younger age groups tended to report higher SRH scores, and older age groups lower SRH (**Table 13**). For example, strong age gradients were apparent for the fair and poor SRH categories. However, the proportion of the youngest age group (19-24 years) who reported excellent health was relatively low, and the proportion of participants reporting very good SRH was comparable for all age groups. The

	SRH										
	Excellent, very good			Good			Fair, poor			Total	
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %
Chapter 6: Cross-sectional analyses at baseline											
<i>Time-invariant variables</i>											
Gender											
Male	2390	44.1	79.3	480	43.2	15.9	140	38.9	4.6	3010	43.7
Female	3035	55.9	78.1	630	56.8	16.2	220	61.1	5.7	3885	56.3

**Table 13: Self-rated health by time-invariant and time-varying variables, N=6,900, Wave 1**

	SRH										
	Excellent, very good			Good			Fair, poor			Total	
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %
<i>Time-invariant variables</i>											
Gender											
Male	2390	44.1	79.3	480	43.2	15.9	140	38.9	4.6	3010	43.7
Female	3035	55.9	78.1	630	56.8	16.2	220	61.1	5.7	3885	56.3
Total	5425	100.0	78.6	1110	100.0	16.1	360	100.0	5.2	6895	100.0
Age											
19-24	245	4.5	76.6	65	5.9	20.3	10	2.8	3.1	320	4.6
25-34	1485	27.4	83.4	225	20.4	12.6	70	19.4	3.9	1780	25.9
35-44	2145	39.6	78.4	445	40.3	16.3	140	38.9	5.1	2730	39.7
45-54	1355	25.0	76.8	300	27.1	17.0	110	30.6	6.2	1765	25.6
55-64	190	3.5	65.5	70	6.3	24.1	30	8.3	10.3	290	4.2
Total	5420	100.0	78.7	1105	100.0	16.0	360	100.0	5.2	6885	100.0
Ethnicity											
Māori	620	11.4	70.5	190	17.0	21.6	225	60.8	8.0	5080	73.6
NZ European	4145	76.5	81.5	710	63.7	14.0	70	18.9	4.4	880	12.8
Pacific	230	4.2	66.7	80	7.2	23.2	35	9.5	10.1	345	5.0
Asian	290	5.4	68.2	110	9.9	25.9	25	6.8	5.9	425	6.2
Total	5415	100.0	78.4	1115	100.0	16.1	370	100.0	5.4	6900	100.0
Highest qualification											
No qualification	810	14.9	67.2	270	24.3	22.4	125	34.7	10.4	1205	17.5
School qualification	1450	26.7	78.8	305	27.5	16.6	85	23.6	4.6	1840	26.7
Post-school qualification	2090	38.5	79.9	400	36.0	15.3	120	33.3	4.6	2610	37.9
Degree or higher	1075	19.8	86.7	135	12.2	10.9	30	8.3	2.4	1240	18.0
Total	5425	100.0	78.6	1110	100.0	16.1	360	100.0	5.2	6895	100.0
<i>Time-varying variables</i>											
Gross total annual family income											
Q1 (<\$31,951)	925	17.1	67.3	320	28.8	23.3	135	37.0	9.8	1380	20.0
Q2 (\$31,951 to \$51,271)	1040	19.2	75.4	240	21.6	17.4	100	27.4	7.2	1385	20.1
Q3 (\$51,272 to \$69,861)	1110	20.5	80.4	210	18.9	15.2	65	17.8	4.7	1380	20.0
Q4 (\$69,862 to \$99,871)	1150	21.2	83.3	180	16.2	13	40	11.0	2.9	1380	20.0

Chapter 6: Cross-sectional analyses at baseline

Q5 (>\$99,871)	1195	22.0	86.6	160	14.4	11.6	25	6.8	1.8	1380	20.0
Total	5420	100.0	78.6	1110	100.0	16.1	365	100.0	5.3	6905	100.0
Equivalentised gross total annual family income (minus FTC)											
Q1 (<\$23,128)	765	14.1	65.7	270	24.2	23.2	155	43.1	11.2	1380	20.0
Q2 (\$23,128 to \$37,311)	855	15.8	73.4	225	20.2	19.3	85	23.6	6.2	1380	20.0
Q3 (\$37,312 to \$50,780)	930	17.2	79.5	190	17.0	16.2	60	16.7	4.3	1380	20.0
Q4 (\$50,781 to \$73,298)	960	17.7	82.4	165	14.8	14.2	35	9.7	2.5	1385	20.1
Q5 (>\$73,298)	1025	18.9	88.0	120	10.8	10.3	25	6.9	1.8	1380	20.0
Q1 (<\$31,951)	5420	100.0	78.6	1115	100.0	16.2	360	100.0	5.2	6905	100.0
Equivalentised gross total annual family income (minus IWTC)											
Q1 (<\$28,307)	820	16.1	59.6	310	24.8	22.5	150	38.0	10.9	1380	20.0
Q2 (\$28,307 to \$42,000)	970	19.0	70.5	295	23.6	21.5	85	21.5	6.2	1380	20.0
Q3 (\$42,001 to \$58,823)	1035	20.3	75.0	255	20.4	18.5	80	20.3	5.8	1380	20.0
Q4 (\$58,824 to \$85,728)	1100	21.6	79.4	225	18.0	16.2	50	12.7	3.6	1375	20.0
Q5 (>\$85,728)	1175	23.0	85.1	165	13.2	12.0	30	7.6	2.2	1375	20.0
Total	5100	100.0	74.0	1250	100.0	18.1	395	100.0	5.7	6890	100.0
Family type											
Single, couple only	885	16.3	83.1	145	13.0	13.6	35	9.9	3.3	1065	15.4
One-parent	600	11.1	69.8	185	16.6	21.5	75	21.1	8.7	860	12.5
Two-parent	3940	72.6	79.2	785	70.4	15.8	245	69.0	4.9	4970	72.1
Total	5425	100.0	78.6	1115	100.0	16.2	355	100.0	5.1	6895	100.0
Number of children in family											
0	1580	29.2	78.0	185	16.5	19.3	145	40.3	4.7	3085	44.7
1	1365	25.2	76.5	325	29.0	18.2	95	26.4	5.3	1785	25.9
2	1595	29.4	81.8	275	24.6	14.1	80	22.2	4.1	1950	28.3
3	660	12.2	77.6	145	12.9	17.1	45	12.5	5.3	850	12.3
4 to ten	220	4.1	74.6	45	4.0	15.3	30	8.3	10.2	295	4.3
Total	5420	100.0	78.5	1120	100.0	16.2	360	100.0	5.2	6900	100.0
Employment status											
Unemployed	85	1.6	60.7	35	3.1	25.0	20	5.5	14.3	140	2.0
Employed	4500	82.9	81.5	820	73.5	14.9	195	53.4	3.5	5515	79.9
Inactive	835	15.4	67.3	255	22.9	20.6	150	41.1	12.1	1240	18.0
Total	5425	100.0	78.5	1115	100.0	16.1	365	100.0	5.3	6905	100.0

(Notes provided on next page.)

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). NZ European = New Zealand European. N = 5 (0.0%) participants had missing values for SRH, and these participants are included in total counts. N = 165 (0.0%) participants reporting an Other ethnicity are not shown in the table, but are counted in the totals. Because IWTC was introduced in Wave 4, equivalised family income (minus IWTC) is for Wave 4.

ethnic groups reporting excellent, very good or good health at the highest rates and fair or poor health at the lowest rates were New Zealand European and Other participants. In contrast, Māori participants reported the lowest rates of excellent, very good or good SRH and the highest rates of fair or poor SRH. Pacific participants reported the second lowest rate of excellent health the lowest rate of very good health and the highest rate of poor health. Participants with a higher level of qualification tended to report higher SRH.

Income gradients (i.e., participants with higher total family income quintiles reporting higher SRH), were apparent across most SRH categories. The same pattern was observed for equivalised family income (without FTC and IWTC, respectively). Participants in two parent-families tended to have better SRH than those in one-parent families. Employed participants reported the highest levels of SRH, followed by inactive participants, with unemployed participants reporting relatively low SRH.

## Exposure variables by the outcome variable

### Family Tax Credit eligibility and amount by self-rated health

Unadjusted cross-sectional tabular analyses suggest that FTC-eligible participants were considerably less likely than FTC-ineligible to report excellent, very good or good SRH at Wave 1 (**Table 14**). This association was strong and statistically significant ( $p < 0.01$ ), with an odds ratio (OR) of 0.53 [95% confidence interval (CI) 0.41 to 0.66].<sup>11</sup> There was, however, less evidence that FTC-eligible participants entitled to the highest three quintiles 3 to 5 of FTC amount differed from those entitled to the lowest two quintiles of FTC in their reporting of excellent, very good or good (OR 0.98, 95% CI 0.65 to 1.48).

However, these observed relationships are likely to be affected by confounding and their precision limited by the dichotomisation of the SRH outcome variable. They are likely confounded by time-invariant variables such as gender, age, ethnicity and highest qualification

---

<sup>11</sup> Using data from **Table 14**, the odds of excellent or very good versus good, fair or poor amongst FTC eligible participants were  $770 / 360 = 2.14$  and amongst FTC-ineligible participants were  $3,765 / 940 = 4.01$ , providing an odds ratio of  $2.14 / 4.01 = 0.53$ .

Chapter 6: Cross-sectional analyses at baseline

**Table 14: Family Tax Credit eligibility and amount by self-rated health, N=6,900, Wave 1**

SRH	FTC eligibility						FTC amount						Not in a family			Total	
	Eligible			Not eligible			Q1-Q2 (\$1 to \$2,452)			Q3-Q5 (\$2,453 to \$19,410)			N	Col %	Row %	N	Col %
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %					
Excellent	405	35.8	13.1	2150	45.7	69.8	165	37.5	5.4	240	34.3	7.8	525	49.3	17.0	3080	44.6
Very Good	365	32.3	15.6	1615	34.3	69.0	135	30.7	5.8	230	32.9	9.8	360	33.8	15.4	2340	33.9
Good	260	23.0	23.3	710	15.1	63.7	100	22.7	8.9	165	23.6	14.7	145	13.6	13.0	1115	16.2
Fair	85	7.5	29.3	175	3.7	60.3	35	8.0	11.9	55	7.9	18.6	30	2.8	10.3	290	4.2
Poor	15	1.3	21.4	50	1.1	71.4	5	1.1	7.1	10	1.4	14.3	5	0.5	7.1	70	1.0
Total	1130	100.0	16.4	4705	100.0	68.2	440	100.0	6.4	700	100.0	10.1	1065	100.0	15.4	6900	100.0

Notes: Col % = column percentage (where the column total is the denominator).<sup>b</sup> Row % = row percentage (where the row total is the denominator). Q1-Q2 = quintiles 1-2. Q3-Q5 = quintiles 3-5. N = 10 (0.0%) participants had missing values for SRH, but were included in total counts.

**Table 15: In-Work Tax Credit eligibility and amount by self-rated health, N=6,900, Wave 4**

SRH	IWTC eligibility						IWTC amount						Not in a family			Total	
	Eligible			Not eligible			Q1-Q2 (\$1 to \$3,130)			Q3-Q5 (\$3,130 to \$5,479)			N	Col %	Row %	N	Col %
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %					
Excellent	415	39.2	15.9	1785	38.6	68.4	150	44.1	36.6	260	36.6	63.4	410	33.5	15.7	2610	37.8
Very good	380	35.8	15.2	1680	36.4	67.3	110	32.4	29.3	265	37.3	70.7	435	35.5	17.4	2495	36.1
Good	200	18.9	16.1	860	18.6	69.1	50	14.7	25.6	145	20.4	74.4	185	15.1	14.9	1245	18.0
Fair	50	4.7	15.6	205	4.4	64.1	20	5.9	40.0	30	4.2	60.0	65	5.3	20.3	320	4.6
Poor	10	0.9	12.5	60	1.3	75.0	5	1.5	50.0	5	0.7	50.0	10	0.8	12.5	80	1.2
Total	1060	100.0	15.4	4620	100.0	66.9	340	100.0	32.4	710	100.0	67.6	1225	100.0	17.7	6905	100.0

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). Q1-Q2 = quintiles 1-2. Q3-Q5 = quintiles 3-5. N = 10 (0.0%) participants had missing values for SRH, but were included in total counts.

as well as time-varying variables such as equivalised gross total annual family income (minus FTC), family type and number of children in the family. The following two results chapters indicate the degree of confounding in the effect estimates from these unadjusted, cross-sectional analyses. Secondly, dichotomising the SRH variable loses statistical power, which is addressed by treating SRH as a continuous variable in the main fixed effects regression analyses presented in *Chapter 7*.

### **In-Work Tax Credit eligibility and amount by self-rated health**

The proportion of IWTC-eligible and IWTC-ineligible participants was comparable across SRH categories at baseline (Wave 4) (**Table 15**). OR estimates found no evidence that IWTC-eligible participants differed from IWTC-ineligible participants in their reporting of excellent, very good or good SRH (OR 1.02, 95% CI 0.76 to 1.36). Similar proportions of participants entitled to the highest two quintiles and of those entitled to the lowest three quintiles of IWTC reported excellent, very good or good SRH (OR 0.65, 95% CI 0.38 to 1.10), also suggesting no cross-sectional association. Again, these effect estimates from unadjusted, cross-sectional analyses could be confounded by various time-invariant and time-varying variables and their precision limited due to SRH being dichotomised; these potential limitations are tested in the following two results chapters.

## **Time-varying variables at baseline**

### **Gross total annual family income**

Gross total annual family was not included in the main fixed effects regression analyses, but it was one of the key variables used to derive the FTC and IWTC exposures. The income variables that were included in the main fixed effects regression analyses as potential confounding variables, equivalised gross total annual family income (minus FTC or IWTC), are described in the following sections. The mean gross total annual family income in the study sample was \$76,288 (SD \$990; median \$61,069) at Wave 1. A slightly higher percentage of women than men were in lower income quintiles, which was offset by a lower percentage of women in higher income quintiles (**Table 16**). Younger participants tended to be on lower incomes, and older participants on higher incomes. Pacific, Māori and Asian participants tended have lower income levels than New Zealand European participants, with the highest percentage of participants in the lowest income quintile observed for Asian (40%), Māori (35%) and Pacific participants (30%). More highly qualified participants tended to be on higher incomes.

Chapter 6: Cross-sectional analyses at baseline

**Table 16: Gross total annual family income by time-invariant variables, N=6,900, Wave 1**

	Total annual family income (quintiles)																	
	Q1 (<\$31,951)			Q2 (\$31,951 to \$51,271)			Q3 (\$51,272 to \$69,861)			Q4 (\$69,862 to \$99,871)			Q5 (>\$99,871)			Total		
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	
<b>Sex</b>																		
Male	425	30.8	14.1	605	43.7	20.1	650	47.1	21.6	665	48.2	22.1	670	48.6	22.2	3015	43.7	
Female	955	69.2	24.6	780	56.3	20.1	730	52.9	18.8	715	51.8	18.4	710	51.4	18.3	3890	56.3	
Total	1380	100.0	20.0	1385	100.0	20.1	1380	100.0	20.0	1380	100.0	20.0	1380	100.0	20.0	6905	100.0	
<b>Age</b>																		
19-24	130	9.5	40.0	90	6.5	27.7	50	3.6	15.4	40	2.9	12.3	15	1.1	4.6	325	4.7	
25-34	410	29.8	23.0	470	33.9	26.3	400	29.0	22.4	300	21.7	16.8	205	14.9	11.5	1785	25.9	
35-44	500	36.4	18.3	520	37.5	19.0	610	44.2	22.3	560	40.6	20.5	540	39.3	19.8	2730	39.6	
45-54	270	19.6	15.3	265	19.1	15.0	285	20.7	16.1	415	30.1	23.5	530	38.5	30.0	1765	25.6	
55-64	65	4.7	22.4	40	2.9	13.8	35	2.5	12.1	65	4.7	22.4	85	6.2	29.3	290	4.2	
Total	1375	100.0	19.9	1385	100.0	20.1	1380	100.0	20.0	1380	100.0	20.0	1375	100.0	19.9	6895	100.0	
<b>Ethnicity</b>																		
Māori	300	21.7	34.1	180	13.0	20.5	175	12.7	19.9	145	10.5	16.5	80	5.8	9.1	880	12.8	
NZ European	780	56.5	15.3	975	70.7	19.2	1050	76.1	20.6	1100	79.7	21.6	1180	85.5	23.2	5085	73.7	
Pacific	105	7.6	30.4	100	7.2	29.0	60	4.3	17.4	55	4.0	15.9	25	1.8	7.2	345	5.0	
Asian	165	12.0	39.3	90	6.5	21.4	60	4.3	14.3	50	3.6	11.9	55	4.0	13.1	420	6.1	
Total	1380	100.0	20.0	1380	100.0	20.0	1380	100.0	20.0	1380	100.0	20.0	1380	100.0	20.0	6900	100.0	
<b>Highest qualification</b>																		
No qualification	370	26.9	30.7	315	22.7	26.1	235	17.0	19.5	200	14.4	16.6	85	6.2	7.1	1205	17.5	
School qualification	335	24.4	18.2	420	30.3	22.8	405	29.3	22.0	375	27.1	20.3	310	22.5	16.8	1845	26.7	
Post-school qualification	510	37.1	19.5	490	35.4	18.8	555	40.2	21.3	560	40.4	21.5	495	35.9	19.0	2610	37.8	
Degree or higher	160	11.6	12.9	160	11.6	12.9	185	13.4	14.9	245	17.7	19.8	490	35.5	39.5	1240	18.0	
Total	1375	100.0	19.9	1385	100.0	20.1	1380	100.0	20.0	1385	100.0	20.1	1380	100.0	20.0	6905	100.0	

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). NZ European = New Zealand European. N = 165 (0.0%) participants reporting an *Other* ethnicity are not shown in the table, but are counted in the totals.

## **Equivalised gross total annual family income (minus Family Tax Credit or In-Work Tax Credit)**

The mean of equivalised gross total annual family income, a potential time-varying confounding variable, was \$52,076 (SD: \$55,742), and the median was \$41,514. After equivalising family income and subtracting FTC, the difference in proportion of female participants in the lower quintiles, compared to the proportion of male participants, was relatively larger than it was for family income (**Table 17**). This suggests that a considerable larger proportion of female participants co-habited with others (including dependent children) than did male participants in the sample. The mean of equivalised gross total annual family income (minus IWTC) was \$55,669 (SD \$55,680), and the median was \$43,861. The characteristics of this variable were comparable to that described for the equivalised gross total family income (minus FTC) variable (**Table 18**).

### **Family type**

As in the general population, the great majority of participants were in two-parent families (72.0%), and only 12.5% were in one-parent families at Wave 1 (**Table 19**). One in six participants (15.4%) was not in a family. Male participants were more likely to be in a two-parent family than female participants (52.8% cf. 47.2%). Female participants were four times as likely as male participants to head a one-parent family (18.8% cf. 4.5%). The youngest age group had the highest proportion of participants in one-parent families (18.8%) and the smallest proportion in two-parent families (34.4%). Other age groups had comparable proportions of participants in one-parent families (11-14%). The largest proportion of participants in two-parent families was for the 35 to 44 and 45 to 54 years age groups (80.2% and 79.1%, respectively), followed by the 25 to 34 and 55 to 64 years age groups (62.7% and 60.8%, respectively). Māori participants had the largest proportion of single-parents (25.4%), followed by Pacific (15.9%) and New Zealand European participants (10.7%). The Asian group had the largest rate of two-parent families (81.0%), followed by New Zealand European and Pacific participants (73.9% and 72.7% respectively). The proportion of participants in two parent families was comparable across educational groups. However, participants with lower education levels tended to be in one-parent families, with a strong gradient apparent, which may however be due to confounding by gender, age and ethnicity. Lower income groups had higher proportions of participants in one-parent families, with 37.1% of participants in the lowest income quintile in one-parent families but only 2.2% of those in the highest income quintile in one-parent families. The highest income quintile had the highest proportion of participants in two-parent families (84.1%), followed by quintiles 3 and 4 at around 80%.

Chapter 6: Cross-sectional analyses at baseline

**Table 17: Equivalised gross total annual family income (minus Family Tax Credit) by time-invariant and time-varying variables, N=6,900, Wave 1**

	Equivalised total annual family income (minus FTC) (quintiles)																	
	Q1 (<\$23,128)			Q2 (\$23,128 to \$37,311)			Q3 (\$37,312 to \$50,780)			Q4 (\$50,781 to \$73,298)			Q5 (>\$73,298)			Total		
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	
Time-invariant variables																		
Gender																		
Male	440	31.9	14.6	580	42.2	19.2	650	46.9	21.6	665	48.2	22.1	680	49.3	22.6	3015	43.7	
Female	940	68.1	24.2	795	57.8	20.5	735	53.1	18.9	715	51.8	18.4	700	50.7	18.0	3885	56.3	
Total	1380	100.0	20.0	1375	100.0	19.9	1385	100.0	20.1	1380	100.0	20.0	1380	100.0	20.0	6900	100.0	
Age																		
19-24	35	2.5	35.0	15	1.1	15.0	25	1.8	25.0	15	1.1	15.0	10	0.7	10.0	100	1.5	
25-34	305	22.1	23.2	325	23.6	24.7	260	18.8	19.8	230	16.7	17.5	195	14.2	14.8	1315	19.1	
35-44	555	40.2	21.4	560	40.7	21.6	570	41.2	22.0	465	33.8	17.9	445	32.4	17.1	2595	37.7	
45-54	365	26.4	16.4	385	28.0	17.3	425	30.7	19.1	525	38.2	23.5	530	38.5	23.8	2230	32.4	
55-64	120	8.7	18.5	90	6.5	13.8	105	7.6	16.2	140	10.2	21.5	195	14.2	30.0	650	9.4	
Total	1380	100.0	20.0	1375	100.0	20.0	1385	100.0	20.1	1375	100.0	20.0	1375	100.0	20.0	6890	100.0	
Ethnicity																		
Māori	285	20.7	32.2	200	14.5	22.6	170	12.3	19.2	150	10.9	16.9	80	5.8	9.0	885	12.8	
NZ European	800	58.2	15.8	965	69.9	19.0	1040	75.1	20.5	1085	78.6	21.4	1185	86.2	23.3	5075	73.6	
Pacific	125	9.1	36.2	105	7.6	30.4	65	4.7	18.8	30	2.2	8.7	20	1.5	5.8	345	5.0	
Asian	135	9.8	32.1	85	6.2	20.2	80	5.8	19.0	70	5.1	16.7	50	3.6	11.9	420	6.1	
Total	1375	100.0	19.9	1380	100.0	20.0	1385	100.0	20.1	1380	100.0	20.0	1375	100.0	19.9	6895	100.0	
Highest qualification																		
No qualification	335	24.4	20.0	385	28.0	23.0	365	26.4	21.8	315	22.9	18.8	275	19.9	16.4	1675	24.3	
School qualification	530	38.5	19.2	550	40.0	20.0	585	42.4	21.2	565	41.1	20.5	525	37.9	19.1	2755	40.0	
Post-school qualification	140	10.2	10.4	160	11.6	11.9	225	16.3	16.7	315	22.9	23.4	505	36.5	37.5	1345	19.5	
Degree or higher	370	26.9	33.2	280	20.4	25.1	205	14.9	18.4	180	13.1	16.1	80	5.8	7.2	1115	16.2	
Total	1375	100.0	20.0	1375	100.0	20.0	1380	100.0	20.0	1375	100.0	20.0	1385	100.0	20.1	6890	100.0	
Time-varying variables																		
Gross total annual family income																		
Q1 (<\$31,951)	865	62.7	87.4	95	6.9	9.6	30	2.2	3.0	0	0.0	0.0	0	0.0	0.0	990	14.3	
Q2 (\$31,951 to \$51,271)	465	33.7	38.4	545	39.5	45.0	130	9.4	10.7	70	5.1	5.8	0	0.0	0.0	1210	17.5	
Q3 (\$51,272 to \$69,861)	50	3.6	4.2	565	40.9	47.5	445	32.1	37.4	95	6.9	8.0	35	2.5	2.9	1190	17.2	

Chapter 6: Cross-sectional analyses at baseline

Q4 (\$69,862 to \$99,871)	0	0.0	0.0	170	12.3	10.9	675	48.7	43.1	600	43.5	38.3	120	8.7	7.7	1565	22.7
Q5 (>\$99,871)	0	0.0	0.0	5	0.4	0.3	105	7.6	5.4	615	44.6	31.5	1225	88.8	62.8	1950	28.2
Total	1380	100.0	20.0	1380	100.0	20.0	1385	100.0	20.1	1380	100.0	20.0	1380	100.0	20.0	6905	100.0

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). NZ European = New Zealand European. N = 165 (0.0%) participants reporting an *Other* ethnicity are not shown in the table, but are counted in the totals.

**Table 18: Equivalised gross total annual family income (minus In-Work Tax Credit) by time-invariant and time-varying variables, N=6,900, Wave 4**

	Equivalised total annual income (minus IWTC) (quintiles)																
	Q1 (<\$28,307)			Q2 (\$28,307 to \$42,000)			Q3 (\$42,001 to \$58,823)			Q4 (\$58,824 to \$85,728)			Q5 (>\$85,728)			Total	
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %
Time-invariant variables																	
Gender																	
Male	350	30.6	11.6	485	42.2	16.0	535	46.7	17.7	540	47.2	17.9	550	48.0	18.2	3025	43.8
Female	795	69.4	20.5	665	57.8	17.1	610	53.3	15.7	605	52.8	15.6	595	52.0	15.3	3880	56.2
Total	1145	100.0	16.6	1150	100.0	16.7	1145	100.0	16.6	1145	100.0	16.6	1145	100.0	16.6	6905	100.0
Age																	
19-24	30	2.6	18.8	30	2.6	18.8	15	1.3	9.4	5	0.4	3.1	5	0.4	3.1	160	2.3
25-34	300	26.2	20.0	265	23.0	17.7	265	23.1	17.7	210	18.3	14.0	115	10.0	7.7	1500	21.7
35-44	475	41.5	18.0	495	43.0	18.8	475	41.5	18.0	475	41.5	18.0	465	40.6	17.6	2635	38.2
45-54	280	24.5	13.4	315	27.4	15.0	330	28.8	15.8	385	33.6	18.4	450	39.3	21.5	2095	30.4
55-64	60	5.2	11.8	45	3.9	8.8	60	5.2	11.8	70	6.1	13.7	110	9.6	21.6	510	7.4
Total	1145	100.0	16.6	1150	100.0	16.7	1145	100.0	16.6	1145	100.0	16.6	1145	100.0	16.6	6900	100.0
Ethnicity																	
Māori	245	21.4	27.8	170	14.8	19.3	150	13.2	17.0	120	10.5	13.6	65	5.7	7.4	880	12.8
NZ European	640	55.9	12.6	790	69.0	15.6	855	75.0	16.8	885	77.3	17.4	995	86.9	19.6	5080	73.7
Pacific	115	10.0	33.3	80	7.0	23.2	50	4.4	14.5	40	3.5	11.6	15	1.3	4.3	345	5.0
Asian	120	10.5	28.6	75	6.6	17.9	65	5.7	15.5	65	5.7	15.5	40	3.5	9.5	420	6.1
Total	1145	100.0	16.6	1145	100.0	16.6	1140	100.0	16.5	1145	100.0	16.6	1145	100.0	16.6	6895	100.0
Highest qualification																	
No qualification	310	27.1	27.2	245	21.3	21.5	180	15.7	15.8	145	12.7	12.7	75	6.6	6.6	1140	16.5
School qualification	295	25.8	17.1	335	29.1	19.4	320	27.9	18.6	285	24.9	16.5	230	20.1	13.3	1725	25.0
Post-school qualification	425	37.1	15.7	435	37.8	16.0	485	42.4	17.9	485	42.4	17.9	400	34.9	14.7	2715	39.3

Chapter 6: Cross-sectional analyses at baseline

Degree or higher	115	10.0	8.7	135	11.7	10.2	160	14.0	12.1	230	20.1	17.4	440	38.4	33.3	1320	19.1
Total	1145	100.0	16.6	1150	100.0	16.7	1145	100.0	16.6	1145	100.0	16.6	1145	100.0	16.6	6900	100.0
Time-varying variables																	
Gross total annual family income																	
Q1 (<\$31,951)	930	81.2	67.1	105	9.1	7.6	5	0.4	0.4	0	0.0	0.0	0	0.0	0.0	1385	20.1
Q2 (\$31,951 to \$51,271)	210	18.3	15.2	765	66.5	55.4	195	17.0	14.1	15	1.3	1.1	0	0.0	0.0	1380	20.0
Q3 (\$51,272 to \$69,861)	5	0.4	0.4	250	21.7	18.1	680	59.1	49.3	230	20.2	16.7	5	0.4	0.4	1380	20.0
Q4 (\$69,862 to \$99,871)	0	0.0	0.0	30	2.6	2.2	265	23.0	19.3	695	61.0	50.5	150	13.1	10.9	1375	19.9
Q5 (>\$99,871)	0	0.0	0.0	0	0.0	0.0	5	0.4	0.4	200	17.5	14.5	990	86.5	71.7	1380	20.0
Total	1145	100.0	16.6	1150	100.0	16.7	1150	100.0	16.7	1140	100.0	16.5	1145	100.0	16.6	6900	100.0

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). NZ European = New Zealand European. N = 165 (0.0%) participants reporting an Other ethnicity are not shown in the table, but are counted in the totals.

**Table 19: Family type by time-invariant and time-varying variables, N=6,900, Wave 1**

	Family type										
	One-parent			Two-parent			Single, couple only			Total	
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %
Time-invariant variables											
Gender											
Male	135	15.6	4.5	2345	47.2	77.8	535	50.2	17.7	3015	43.7
Female	730	84.4	18.8	2625	52.8	67.6	530	49.8	13.6	3885	56.3
Total	865	100.0	12.5	4970	100.0	72.0	1065	100.0	15.4	6900	100.0
Age											
19-24	60	6.9	18.8	110	2.2	34.4	150	14.2	46.9	320	4.6
25-34	215	24.7	12.0	1085	21.8	60.8	485	45.8	27.2	1785	25.9
35-44	355	40.8	13.0	2190	44.1	80.2	185	17.5	6.8	2730	39.6
45-54	200	23.0	11.3	1400	28.2	79.1	170	16.0	9.6	1770	25.7
55-64	40	4.6	13.6	185	3.7	62.7	70	6.6	23.7	295	4.3
Total	870	100.0	12.6	4970	100.0	72.0	1060	100.0	15.4	6900	100.0
Ethnicity											
Māori	225	26.2	25.4	550	11.1	62.1	110	10.3	12.4	885	12.8
NZ European	545	63.4	10.7	3695	74.3	72.7	840	78.9	16.5	5080	73.7
Pacific	55	6.4	15.9	255	5.1	73.9	35	3.3	10.1	345	5.0

Chapter 6: Cross-sectional analyses at baseline

Asian	25	2.9	6.0	340	6.8	81.0	55	5.2	13.1	420	6.1
Total	860	100.0	12.5	4970	100.0	72.1	1065	100.0	15.4	6895	100.0
Highest qualification											
No qualification	230	26.6	19.1	845	17.0	70.1	130	12.3	10.8	1205	17.5
Secondary school qualification	215	24.9	11.7	1375	27.6	74.7	250	23.6	13.6	1840	26.7
Post-school qualification	320	37.0	12.3	1860	37.4	71.3	430	40.6	16.5	2610	37.8
Degree or higher	100	11.6	8.1	890	17.9	71.8	250	23.6	20.2	1240	18.0
Total	865	100.0	12.5	4975	100.0	72.1	1060	100.0	15.4	6900	100.0
Time-varying variables											
Gross total annual family income											
Q1 (<\$31,951)	510	59.0	37.1	640	12.9	46.5	225	21.2	16.4	1375	20.0
Q2 (\$31,951 to \$51,271)	190	22.0	13.8	980	19.7	71.0	210	19.8	15.2	1380	20.0
Q3 (\$51,272 to \$69,861)	90	10.4	6.5	1105	22.3	80.1	185	17.5	13.4	1380	20.0
Q4 (\$69,862 to \$99,871)	45	5.2	3.3	1080	21.8	78.5	250	23.6	18.2	1375	20.0
Q5 (>\$99,871)	30	3.5	2.2	1160	23.4	84.1	190	17.9	13.8	1380	20.0
Total	865	100.0	12.6	4965	100.0	72.1	1060	100.0	15.4	6890	100.0
Equalised gross total annual family income (minus FTC)											
Q1 (<\$23,128)	455	52.6	33.1	815	16.4	59.3	105	9.9	7.6	1375	19.9
Q2 (\$23,128 to \$37,311)	205	23.7	14.9	1080	21.7	78.3	95	9.0	6.9	1380	20.0
Q3 (\$37,312 to \$50,780)	95	11.0	6.9	1135	22.8	82.5	145	13.7	10.5	1375	19.9
Q4 (\$50,781 to \$73,298)	65	7.5	4.7	1070	21.5	77.3	250	23.6	18.1	1385	20.1
Q5 (>\$73,298)	45	5.2	3.3	870	17.5	63.0	465	43.9	33.7	1380	20.0
Total	865	100.0	12.5	4970	100.0	72.1	1060	100.0	15.4	6895	100.0
Equalised gross total annual family income (minus IWTC)											
Q1 (<\$28,307)	425	51.8	30.8	730	15.0	52.9	225	18.4	16.3	1380	20.0
Q2 (\$28,307 to \$42,000)	200	24.4	14.5	1055	21.7	76.7	120	9.8	8.7	1375	19.9
Q3 (\$42,001 to \$58,823)	100	12.2	7.2	1120	23.0	80.9	165	13.5	11.9	1385	20.1
Q4 (\$58,824 to \$85,728)	60	7.3	4.3	1040	21.4	75.4	280	23.0	20.3	1380	20.0
Q5 (>\$85,728)	35	4.3	2.5	920	18.9	66.4	430	35.2	31.0	1385	20.1
Total	820	100.0	11.9	4865	100.0	70.5	1220	100.0	17.7	6905	100.0

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). NZ European = New Zealand European. N = 165 (0.0%) participants reporting an Other ethnicity are not shown in the table, but are counted in the totals. Because IWTC was introduced in Wave 4, equalised family income (minus IWTC) is for Wave 4.

## **Number of dependent children in the family**

Overall, around half of all participants had one or two dependent children, and small percentages had three, or four or more children in their family. About one third of participants had no dependent children in their family, of which N = 695 (14.0%) had non-dependent (as per Working For Families criteria) children and N = 1,060 (15.4%) were single or in couple only families, who moved into a family in a later wave of the survey. When the number of dependent children was treated as a linear variable, its mean was 1.63 (SD 1.16), and its median was 2.

Female and male participants had comparable numbers of dependent children (Table 20). The middle age groups tended to be more likely to be in families with any dependent children than both the youngest and oldest age groups. The 35-44 years age group had particularly high percentages of families with 1 and 2 dependent children. Generally speaking, Pacific participants tended to have the largest number of dependent children, followed by Māori. New Zealand Europeans had relatively lower numbers of dependent children, mostly one or two dependent children. Educational groups had about comparable numbers of dependent children. However, those with no qualification tended to have larger numbers of dependent children. Lower income groups tended to have larger numbers of dependent children. One-parent families tended to have smaller numbers of dependent children than two-parent families.

## **Employment status**

Overall, the large majority of the sample was employed (84.0%), a sizeable minority inactive (14.5%) and only 1.4% unemployed (**Table 21**). Nearly all male participants and a large majority of female participants were employed. About one in four female, but only about one in 20 male participants were labour market inactive. The youngest age group had the lowest proportion of employed and amongst the highest proportions of unemployed and inactive participants. The inactive group had around 20-25% of participants from the youngest two age groups, likely reflecting the numbers of tertiary students in these age groups. The relatively high proportion of 55-64 years old participants who were inactive could be explained by this age group transitioning into retirement.

The highest proportion of participants in unemployment was for Asian and Māori, followed by Pacific and Other participants (all around 3%). New Zealand European participants had the lowest unemployment rates. Pacific participants had the highest proportion of inactive participants, followed by Māori and Asian. Other and New Zealand European participants had

Chapter 6: Cross-sectional analyses at baseline

**Table 20: Number of dependent children by time-invariant and time-varying variables, N=6,900, Wave 1**

	Number of dependent children																	
	0			1			2			3			4 to ten			Total		
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	
Time-invariant variables																		
Gender																		
Male	945	46.7	31.3	745	41.7	24.7	835	42.8	27.6	370	43.5	12.3	125	42.4	4.1	3020	43.7	
Female	1080	53.3	27.8	1040	58.3	26.8	1115	57.2	28.7	480	56.5	12.4	170	57.6	4.4	3885	56.3	
Total	2025	100.0	29.3	1785	100.0	25.9	1950	100.0	28.2	850	100.0	12.3	295	100.0	4.3	6905	100.0	
Age																		
19-24	185	9.2	57.8	85	4.8	26.6	40	2.1	12.5	10	1.2	3.1	0	0.0	0.0	320	4.6	
25-34	515	25.6	28.9	470	26.3	26.3	510	26.2	28.6	220	25.9	12.3	70	23.3	3.9	1785	25.9	
35-44	320	15.9	11.7	660	37.0	24.2	1050	54.0	38.5	505	59.4	18.5	190	63.3	7.0	2725	39.5	
45-54	760	37.7	42.9	535	30.0	30.2	335	17.2	18.9	105	12.4	5.9	35	11.7	2.0	1770	25.7	
55-64	235	11.7	79.7	35	2.0	11.9	10	0.5	3.4	10	1.2	3.4	5	1.7	1.7	295	4.3	
Total	2015	100.0	29.2	1785	100.0	25.9	1945	100.0	28.2	850	100.0	12.3	300	100.0	4.4	6895	100.0	
Ethnicity																		
Māori	195	9.6	22.0	250	14.0	28.2	220	11.3	24.9	150	17.6	16.9	70	24.1	7.9	885	12.8	
NZ European	1550	76.5	30.5	1255	70.1	24.7	1510	77.4	29.7	600	70.6	11.8	170	58.6	3.3	5085	73.6	
Pacific	95	4.7	27.1	100	5.6	28.6	65	3.3	18.6	45	5.3	12.9	45	15.5	12.9	350	5.1	
Asian	135	6.7	32.1	145	8.1	34.5	100	5.1	23.8	35	4.1	8.3	5	1.7	1.2	420	6.1	
Total	2025	100.0	29.3	1790	100.0	25.9	1950	100.0	28.2	850	100.0	12.3	290	100.0	4.2	6905	100.0	
Highest qualification																		
No qualification	355	17.6	29.5	310	17.4	25.7	295	15.2	24.5	165	19.4	13.7	80	27.1	6.6	1205	17.5	
School qualification	500	24.8	27.2	455	25.6	24.8	550	28.3	30.0	240	28.2	13.1	90	30.5	4.9	1835	26.6	
Post-school qualification	790	39.1	30.3	665	37.4	25.5	755	38.8	28.9	300	35.3	11.5	100	33.9	3.8	2610	37.9	
Degree or higher	375	18.6	30.2	350	19.7	28.2	345	17.7	27.8	145	17.1	11.7	25	8.5	2.0	1240	18.0	
Total	2020	100.0	29.3	1780	100.0	25.8	1945	100.0	28.2	850	100.0	12.3	295	100.0	4.3	6890	100.0	
Time-varying variables																		
Gross total annual family income																		
Q1 (<\$31,951)	350	17.3	25.5	440	24.6	32.0	360	18.5	26.2	155	18.2	11.3	70	23.7	5.1	1375	19.9	
Q2 (\$31,951 to \$51,271)	350	17.3	25.4	355	19.9	25.7	395	20.3	28.6	190	22.4	13.8	90	30.5	6.5	1380	20.0	
Q3 (\$51,272 to \$69,861)	340	16.8	24.6	355	19.9	25.7	430	22.1	31.2	185	21.8	13.4	70	23.7	5.1	1380	20.0	
Q4 (\$69,862 to \$99,871)	480	23.8	34.9	305	17.1	22.2	380	19.5	27.6	175	20.6	12.7	35	11.9	2.5	1375	19.9	

Chapter 6: Cross-sectional analyses at baseline

Q5 (>\$99,871)	500	24.8	36.1	330	18.5	23.8	380	19.5	27.4	145	17.1	10.5	30	10.2	2.2	1385	20.1
Total	2020	100.0	29.3	1785	100.0	25.9	1945	100.0	28.2	850	100.0	12.3	295	100.0	4.3	6895	100.0
Equivalised gross total annual family income (minus FTC)																	
Q1 (<\$23,128)	250	12.4	18.1	380	21.3	27.5	400	20.6	29.0	210	24.6	15.2	140	46.7	10.1	1380	20.0
Q2 (\$23,128 to \$37,311)	260	12.9	18.8	350	19.6	25.4	440	22.6	31.9	240	28.1	17.4	90	30.0	6.5	1380	20.0
Q3 (\$37,312 to \$50,780)	340	16.8	24.5	380	21.3	27.4	450	23.1	32.5	175	20.5	12.6	40	13.3	2.9	1385	20.1
Q4 (\$50,781 to \$73,298)	505	25.0	36.5	365	20.4	26.4	360	18.5	26.0	135	15.8	9.7	20	6.7	1.4	1385	20.1
Q5 (>\$73,298)	665	32.9	48.4	310	17.4	22.5	295	15.2	21.5	95	11.1	6.9	10	3.3	0.7	1375	19.9
Total	2020	100.0	29.3	1785	100.0	25.9	1945	100.0	28.2	855	100.0	12.4	300	100.0	4.3	6905	100.0
Equivalised gross total annual family income (minus IWTC)																	
Q1 (<\$28,307)	410	16.8	29.6	310	18.7	22.4	380	20.5	27.4	185	25.9	13.4	100	42.6	7.2	1385	20.1
Q2 (\$28,307 to \$42,000)	300	12.3	21.9	340	20.5	24.8	445	24.1	32.5	210	29.4	15.3	75	31.9	5.5	1370	19.9
Q3 (\$42,001 to \$58,823)	420	17.2	30.4	360	21.7	26.1	425	23.0	30.8	145	20.3	10.5	30	12.8	2.2	1380	20.0
Q4 (\$58,824 to \$85,728)	605	24.8	43.7	345	20.8	24.9	330	17.8	23.8	90	12.6	6.5	15	6.4	1.1	1385	20.1
Q5 (>\$85,728)	705	28.9	51.1	305	18.4	22.1	270	14.6	19.6	85	11.9	6.2	15	6.4	1.1	1380	20.0
Total	2440	100.0	35.4	1660	100.0	24.1	1850	100.0	26.8	715	100.0	10.4	235	100.0	3.4	6900	100.0
Family type																	
Single, couple only	1065	52.6	100.0	0	0.0	0.0	0	0.0	0.0	0	0.0	0.0	0	0.0	0.0	1065	15.4
One-parent family	185	9.1	21.4	315	17.6	36.4	245	12.6	28.3	85	10.0	9.8	35	12.1	4.0	865	12.5
Two-parent family	775	38.3	15.6	1470	82.4	29.6	1700	87.4	34.2	765	90.0	15.4	255	87.9	5.1	4965	72.0
Total	2025	100.0	29.4	1785	100.0	25.9	1945	100.0	28.2	850	100.0	12.3	290	100.0	4.2	6895	100.0

Note: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). NZ European = New Zealand European. N = 165 (0.0%) participants reporting an *Other* ethnicity are not shown in the table, but are counted in the totals. Because IWTC was introduced in Wave 4, equivalised family income (minus IWTC) is for Wave 4.

Chapter 6: Cross-sectional analyses at baseline

**Table 21: Employment status by time-invariant and time-varying variables, N=6,900, Wave 4**

	Employment status											
	Unemployed			Employed			Inactive			Total		
	N	Col	Row	N	Col	Row	N	Col	Row	N	Col	
Time-invariant variables												
Gender												
Male	35	36.8	1.2	2830	48.8	93.7	150	15.0	5.0	3020	43.7	
Female	60	63.2	1.5	2975	51.2	76.5	850	85.0	21.9	3890	56.3	
Total	95	100.0	1.4	5805	100.0	84.0	1000	100.0	14.5	6910	100.0	
Age												
19-24	5	5.3	5.3	70	1.2	73.7	20	2.0	21.1	95	1.4	
25-34	15	15.8	1.1	1035	17.8	78.4	270	27.1	20.5	1320	19.1	
35-44	40	42.1	1.5	2195	37.8	84.6	355	35.7	13.7	2595	37.6	
45-54	25	26.3	1.1	1985	34.2	88.8	225	22.6	10.1	2235	32.4	
55-64	10	10.5	1.5	520	9.0	79.4	125	12.6	19.1	655	9.5	
Total	95	100.0	1.4	5805	100.0	84.1	995	100.0	14.4	6900	100.0	
Ethnicity												
Māori	25	25.0	2.8	685	11.8	77.4	175	17.5	19.8	885	12.8	
NZ European	50	50.0	1.0	4400	75.8	86.5	630	63.0	12.4	5085	73.5	
Pacific	10	10.0	2.9	250	4.3	71.4	90	9.0	25.7	350	5.1	
Asian	10	10.0	2.4	330	5.7	77.6	85	8.5	20.0	425	6.1	
Total	100	100.0	1.4	5805	100.0	83.9	1000	100.0	14.5	6915	100.0	
Highest qualification												
No qualification	0	0.0	0.0	25	26.3	2.2	825	14.2	73.7	270	27.1	
Secondary school qualification	15	15.8	0.9	1410	24.3	83.7	255	25.6	15.1	1685	24.4	
Post-school qualification	45	47.4	1.6	2355	40.6	85.5	350	35.2	12.7	2755	39.9	
Degree or higher	10	10.5	0.7	1215	20.9	90.3	120	12.1	8.9	1345	19.5	
Total	95	100.0	1.4	5805	100.0	84.1	995	100.0	14.4	6905	100.0	
Time-varying variables												
Gross total annual family income												
Q1 (<\$31,951)	55	55.0	4.0	890	15.3	64.3	435	43.5	31.4	1385	20.0	
Q2 (\$31,951 to \$51,271)	25	25.0	1.8	1120	19.3	80.9	240	24.0	17.3	1385	20.0	
Q3 (\$51,272 to \$69,861)	10	10.0	0.7	1240	21.4	89.9	130	13.0	9.4	1380	20.0	
Q4 (\$69,862 to \$99,871)	5	5.0	0.4	1290	22.2	93.5	85	8.5	6.2	1380	20.0	

Chapter 6: Cross-sectional analyses at baseline

Q5 (>\$99,871)	5	5.0	0.4	1265	21.8	91.3	110	11.0	7.9	1385	20.0
Total	100	100.0	1.4	5805	100.0	83.9	1000	100.0	14.5	6915	100.0
Equivalised gross total annual family income (minus IWTC)											
Q1 (<\$28,307)	55	57.9	4.0	855	14.7	61.7	470	47.2	33.9	1385	20.1
Q2 (\$28,307 to \$42,000)	20	21.1	1.5	1130	19.5	82.2	225	22.6	16.4	1375	19.9
Q3 (\$42,001 to \$58,823)	10	10.5	0.7	1245	21.4	90.2	125	12.6	9.1	1380	20.0
Q4 (\$58,824 to \$85,728)	5	5.3	0.4	1305	22.5	94.6	70	7.0	5.1	1380	20.0
Q5 (>\$85,728)	5	5.3	0.4	1270	21.9	91.7	105	10.6	7.6	1385	20.1
Total	95	100.0	1.4	5805	100.0	84.1	995	100.0	14.4	6905	100.0
Family type											
Single, couple only	15	16.7	1.2	1070	18.4	87.7	130	13.0	10.7	1220	17.7
One-parent	25	27.8	3.0	565	9.7	68.9	230	23.0	28.0	820	11.9
Two-parent	50	55.6	1.0	4170	71.8	85.8	640	64.0	13.2	4860	70.4
Total	90	100.0	1.3	5805	100.0	84.1	1000	100.0	14.5	6900	100.0
Number of children											
0	35	36.8	1.4	2110	36.3	86.5	290	29.0	11.9	2440	35.3
1	25	26.3	1.5	1390	23.9	84.0	240	24.0	14.5	1655	24.0
2	25	26.3	1.3	1570	27.0	84.4	265	26.5	14.2	1860	26.9
3	5	5.3	0.7	575	9.9	80.4	135	13.5	18.9	715	10.4
4-10	5	5.3	2.1	160	2.8	68.1	70	7.0	29.8	235	3.4
Total	95	100.0	1.4	5805	100.0	84.1	1000	100.0	14.5	6905	100.0

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). NZ European = New Zealand European. N = 165 (0.0%) participants reporting an *Other* ethnicity are not shown in the table, but are counted in the totals.

the lowest proportion of inactive participants. Generally speaking, participants with a higher level of education had lower rates of unemployment and labour market inactivity and higher rates of employment, with strong such gradients apparent for both the employed and inactive employment status categories. With increasing total annual family income quintiles, the proportion unemployed participants decreased and that of employed and inactive participants increased. Groups of participants with higher numbers of children had larger proportions of unemployment and labour market inactivity and smaller proportions of employment, with such gradients apparent across all three employment status groupings.

## Loss to follow up

This thesis studied the balanced panel, whose characteristics at baseline were described in this chapter. The study sample was created when the unbalanced panel of  $N = 9,360$  working-age (19 to 64 years) parents in one-or two-parent families contributing data for at least two consecutive waves from the SoFIE was restricted to those not lost to follow up (or attrition) over the full seven waves of the SoFIE (see *Chapter 5*). Therefore, the rate of loss to follow up among survey participants potentially eligible to participate in this thesis was 26.3%.

Knowing which groups were more likely to be lost to follow up provides important descriptive information for interpreting study findings. Furthermore, knowing the extent of loss to follow up and comparing characteristics of the unbalanced and balanced panels some may provide information (although limited) about the potential risk of selection bias due to loss to follow up in this thesis (see *Chapter 9* for a comprehensive assessment of selection bias). Thus, each descriptive table at baseline Wave 1 presented for the study sample in this chapter was replicated for the unbalanced panel of potentially eligible SoFIE participants in **Appendix 2**.

These tables show that participants who were lost for follow-up were generally comparable in their demographic profile (time-invariant and time-varying variables) to those who remained in the study. Exceptions were that considerably more Māori, Pacific, and lower educated participants were lost to follow-up over the study period. Furthermore, FTC and IWTC eligible and ineligible participants were more likely to be lost to follow up (whereas those not in families were less likely to be lost to follow up), which may partly be explained with the relatively larger attrition of Māori, Pacific, and less educated participants.

## Conclusions

This chapter describes the characteristics of the study sample at baseline in terms of key time-invariant variables as well as time-varying exposure, outcome and potential confounding variables. About one in six participants (16.5%) were eligible for FTC at Wave 1. The mean amount of FTC that the family of an eligible participant was entitled to was \$2,963 [standard deviation (SD) \$1,912]. Similarly, about one in six participants (15.4%) were IWTC-eligible, with the mean IWTC amount being \$2,722 (SD \$962) at Wave 4. Characteristics of FTC- and of IWTC-eligible participants corresponded with the eligibility criteria for these credits validating the derivation of these exposure variables.

Over three quarters of participants (78.7%) reported excellent, very good or good SRH at Wave 1. FTC-eligible participants were statistically significantly less likely than FTC-ineligible participants to report excellent, very good or good SRH [odds ratio (OR) 0.52, 95% confidence interval (CI) 0.41 to 0.67], but receiving a larger amount of FTC was not associated with SRH at Wave 1. No evidence for an association of IWTC eligibility and amount with SRH at baseline was found. However, these unadjusted cross-sectional tabular analyses are likely biased by time-invariant and time varying confounding.

The rate of loss to follow up in this thesis was 26.3%. Participants who were lost for follow-up were generally comparable in their demographic profile to those who remained in the study. Exceptions were that Māori, Pacific and less educated participants were disproportionately lost to follow-up of. Participants who were eligible and ineligible for FTC and IWTC were also more likely to be lost to follow up, whereas participants not in families were less likely to be lost to follow up, possibly due to the relatively large loss of Māori, Pacific and less educated participants from the study.

## Chapter 7: Descriptive analyses of time-varying variables over seven waves

This chapter describes changes in the exposure, outcome and potential confounding variables over the study period of seven waves. Considerable extents of change were found for all four exposure variables, FTC and IWTC eligibility and amount, confirming that these variables were sufficiently time-varying to be used in fixed effects regression analyses. The changes observed in the study population showed the staggered expansion of FTC and the introduction and subsequent expansion of IWTC, which have also been observed for the general population in official statistics. Substantial change over time was also found for SRH, confirming that the time-variability of this variable was adequate for fixed effects regression analysis.

Tabulating change in the exposure variables (FTC and IWTC eligibility and amount) by change in the outcome variable (SRH) found a small, positive association in all four instances, and reached statistical significance for IWTC amount and SRH (OR 1.18, 95% CI 1.01 to 1.38). However, these measures of association carried some risk of bias from misspecification and were not adjusted for potential time-varying confounding (including time itself), both of which issues the fixed effects regression analyses addressed.

All four variables [equivalised gross total annual family income (minus FTC and IWTC), family type, number of dependent children and employment status] hypothesised to vary over time and potentially confound the FTC-SRH or IWTC-SRH relationship were found to be sufficiently time-varying to warrant inclusion in fixed effects regression analyses. Change in FTC and IWTC eligibility and amount was found to be associated with, or driven by, change in all four potential time-varying confounding variables. However, change in three variables [equivalised gross total annual family income (minus IWTC), family type and number of dependent children] had little, if any, association with change in SRH, meaning they were unlikely to be confounders of the FTC-SRH or IWTC-SRH association of interest. But change in equivalised gross total annual family income (minus FTC) was

associated with change in FTC eligibility and amount, and with change in SRH, suggesting that the FTC-SRH relationship could be confounded by equivalised gross total annual family income. Furthermore, change in employment status was associated with change in IWTC eligibility and amount, as well as with change in SRH, suggesting that employment could confound the IWTC-SRH relationship in fixed effects regression analyses.

This chapter describes change that variables included in the fixed effects regression models underwent over the seven waves of SoFIE, using tabular analyses. The chapter first describes change in the exposure and outcome variables. For each variable, its cross-sectional distribution in the study population at each wave; the types of changes between its categories between consecutive waves; and a transition matrix showing average change between wave<sub>t</sub> and wave<sub>t+1</sub> are described. The chapter then presents change in the exposure variables tabulated by change in the outcome variable and calculates the association between change in the exposure variables and change in the outcome variable, providing *preliminary* analyses for the main fixed effects regression presented in the next chapter. Changes in the four variables hypothesised *a priori* in the analytical frameworks of the thesis (**Figure 19** and **Figure 20**) to theoretically vary over time and potentially confound the FTC-SRH or IWTC-SRH relationships are then described. Change in each of these variables is also tabulated by change in each (relevant) exposure variable and the outcome variable. The association of change in these variables with change in the exposure and outcome variables is estimated as a preliminary empirical test of the potential of these variables to confound the FTC-SRH or IWTC-SRH relationship in the fixed effects regression analyses.

These descriptions of change over time in the exposure and outcome variables have two principal purposes. Firstly, since fixed effects regression analyses are methods designed for analysing the effect of a time-varying exposure variable on a time-varying outcome variable, it is important to demonstrate that the variables included as exposure and outcome variables in such analyses indeed varied sufficiently over time. Secondly, detailed descriptions of the types of changes that occurred in the exposure and outcome variables provide an improved understanding of the changes that drove the findings of the fixed effects regression analyses. For example, it is helpful to be able to differentiate the case where 90% of all change in FTC eligibility was due to participants becoming FTC-eligible and 10% was due to participants becoming FTC-ineligible from the qualitatively different case of 50% of change each being due to participants becoming FTC-eligible and –ineligible. Cross-tabulating and estimating the crude association between change in the exposure variables and change in the outcome variable provides a preliminary answer to the principal research question of the thesis, *Is change in FTC (or IWTC) eligibility (or amount) associated with change in SRH over the short term?* It also provides the opportunity to predict the direction and strength of the effect estimate of the main fixed effects regression analyses.

Inclusion of time-invariant variables in fixed effects regression analyses is disadvantageous, because it does not provide additional confounder control, as these models eliminate all time-

invariant confounding, but may decrease the precision of the estimator. This chapter therefore tests whether all potential confounding variables hypothesised to change over time are indeed time-varying. Furthermore, these variables were hypothesised to potentially confound the FTC-SRH or IWTC-SRH relationships. Confounding by such a variable requires that the variable is associated with both the exposure and the outcome. Therefore, tabulating and estimating, for each of the potential confounding variables, the association of change in the variable with change in each (relevant) exposure and the outcome variable provides an empirical ‘test’ of the potential of these variables to indeed confound the FTC-SRH or IWTC-SRH relationships. This provided the opportunity to predict and explain the effect estimates for the potential time-varying confounding variables found in the main fixed effects regression analyses.

## Change in exposures

### Family Tax Credit eligibility

About one in six participants (14.3% to 16.5%) were eligible for FTC at each of the first three waves (**Table 22**). The initial and major expansion of FTC considerably increased the percentage of FTC-eligible participants by 53.1%, from 14.3% at Wave 3 to 21.9% at Wave 4. As FTC was further expanded, one in four (24.4% to 26.8%) participants was FTC-eligible at Waves 5, 6 and 7. The percentage of participants who were not in a family increased steadily over time, from 15.4% at Wave 1 to 19.4% at Wave 7. These cross-sectional patterns in the percentage of FTC-eligible participants in the study sample corresponded with those found in the number of families entitled to FTC in the general population, as reported in official statistics [233] presented in **Figure 6** in *Chapter 2*.

Several types of changes in FTC eligibility were differentiated. Firstly, the two most important categories of change in FTC eligibility for the fixed effects regression analyses were a change from FTC-ineligible at wave<sub>t</sub> to -eligible is at wave<sub>t+1</sub>, referred to as *becoming FTC-eligible*, and a change from FTC-eligible at wave<sub>t</sub> to -ineligible at wave<sub>t+1</sub>, or *becoming FTC ineligible*. Secondly, there are two cases of *no change* in FTC eligibility. The first case is participants who are FTC-eligible or are FTC-ineligible at both of two consecutive waves. The second case of participants referred to as undergoing *no change* in FTC eligibility are those who moved into or out of a family. The FTC eligibility status of a participant is coded as missing for waves in which the participant is not in a family, meaning that, for example, a participant who moved out of a family between Wave 1 and 2 had a missing value for FTC eligibility at Wave 1, which

**Table 22: Cross-sectional distribution of Family Tax Credit eligibility, N=6,900, Waves 1 to 7**

FTC eligibility	Wave															
	Wave 1		Wave 2		Wave 3		Wave 4		Wave 5		Wave 6		Wave 7		Total	
	N	%	N	%	N	%	N	%	N	%	N	%	N	%	N	%
Eligible	1135	16.5	990	14.4	985	14.3	1510	21.9	1850	26.8	1735	25.1	1685	24.4	9890	20.5
Ineligible	4700	68.1	4800	69.6	4740	68.7	4170	60.4	3770	54.7	3850	55.8	3875	56.2	29905	61.9
Not in a family	1065	15.4	1110	16.1	1175	17.0	1220	17.7	1275	18.5	1315	19.1	1340	19.4	8500	17.6
Total	6900	100.0	6900	100.0	6900	100.0	6900	100.0	6895	100.0	6900	100.0	6900	100.0	48295	100.0

resulted in the exclusion of data from the participant for the Wave 1 / Wave 2 transition in the fixed effects regression analysis.

On average, 5.8% of participants became FTC-eligible and 4.8% of participants became FTC-ineligible between consecutive waves (**Table 23**). Over the initial, major expansion of FTC that occurred between Waves 3 and 4, one in ten (9.3%) participants became FTC-eligible, which constituted 27.0% of all changes into eligibility that occurred over the study period. Offsetting this major increase in FTC eligibility was the low percentage (2.3%) of participants who became FTC-ineligible. As FTC was further expanded, the percentage of participants who became FTC-eligible remained high between Waves 4 and 5 (7.8%). While these considerable changes into FTC eligibility occurred, a noteworthy percentage of participants also became FTC-ineligible, especially between Waves 1 and 2 (5.9%); 5 and 6 (6.5%); and 6 and 7 (5.9%). About one in four participants (22.8% on average) was not in a family in one or both consecutive waves. In summary, this suggests dynamic changes in FTC eligibility over time, comprising both considerable change into FTC eligibility, especially around the staggered expansion of FTC, but also considerable change into FTC ineligibility in the time before and after the expansion of FTC.

Over the seven waves, one in three participants (35.4%) changed their FTC eligibility at least once, of whom most experienced either one (44.9%) or two changes (36.7%), and some experienced three or more changes (18.1%). A transition matrix presents, for each possible transition between categories of a variable, the average percentage of participants who underwent this transition between two consecutive waves [234]. Transition probabilities are shown for all possible combinations of categories between two waves, including not only those for changes from one category to another, but also those for remaining in the same category. Because transition matrices enable a straightforward assessment of the overall extent of change in a variable over the total study period, they can be used to judge the extent to which a variable is time-varying. The transition matrix for FTC eligibility is presented in **Table 24**. Its first row shows that of the small number of participants who were FTC-eligible at wave<sub>t</sub> (N = 8,220 over all seven waves), 72.7% were still FTC-eligible at wave<sub>t+1</sub>; 24.1% were FTC-ineligible; and 3.2% were no longer in a family. Shown in the second row, of the large number of participants who were FTC-ineligible at wave<sub>t</sub> (N = 26,035), 9.2% became eligible, 83.0% remained FTC-ineligible and 7.8% moved out of the family. Therefore, the transition matrix demonstrates considerable change in FTC eligibility, confirming that this variable is sufficiently time-varying to be used as an exposure variable in the main fixed effects regression analyses.

Chapter 7: Descriptive analyses of time-varying variables

**Table 23: Change in Family Tax Credit eligibility between wave<sub>t</sub> and wave<sub>t+1</sub>, N=6,900, Waves 1 to 7**

	Change in FTC eligibility																
	Became eligible			Became ineligible			No change: eligible			No change: ineligible			Not in a family at one or both waves			Total	
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	
Waves 1 / 2	280	11.7	4.1	410	20.7	5.9	690	11.5	10.0	4085	18.9	59.2	1440	15.2	20.9	6905	
Waves 2 / 3	305	12.8	4.4	305	15.4	4.4	640	10.7	9.3	4125	19.1	59.8	1525	16.1	22.1	6900	
Waves 3 / 4	645	27.0	9.3	160	8.1	2.3	785	13.1	11.4	3725	17.2	54.0	1590	16.8	23.0	6905	
Waves 4 / 5	540	22.6	7.8	250	12.6	3.6	1210	20.3	17.5	3280	15.2	47.5	1620	17.1	23.5	6900	
Waves 5 / 6	310	13.0	4.5	450	22.7	6.5	1340	22.4	19.4	3160	14.6	45.8	1640	17.4	23.8	6900	
Waves 6 / 7	305	12.8	4.4	410	20.7	5.9	1310	21.9	19.0	3240	15.0	47.0	1635	17.3	23.7	6900	
Total	2385	100.0	5.8	1985	100.0	4.8	5975	100.0	14.4	21615	100.0	52.2	9450	100.0	22.8	41410	

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator).

**Table 24: Transition matrix for Family Tax Credit eligibility, N=6,900, Waves 1 to 7**

Wavet	Wavet+1			
	Eligible	Ineligible	Not in a family	Total
Eligible	0.727	0.241	0.032	8220
Ineligible	0.092	0.830	0.078	26035
Not in a family	0.057	0.225	0.718	7165
Total	8770	25210	7440	41420

## Family Tax Credit amount

A FTC-eligible family was entitled to a mean amount of FTC of between \$2,963 and \$3,543 at Waves 1 to 3, increasing to \$4,042 at Wave 4 with the initial expansion of FTC and steadily thereafter up to \$4,955 at Waves 7 (**Table 25**). There was increasing variability in the FTC amount over time, from a standard deviation of \$1,912 at Wave 1 to \$3,370 at Wave 7. This pattern in the FTC amount in the study population over time corresponded with the increased generosity in the dollar amount of FTC associated with the staggered expansion of FTC over the 2005-08 period shown in **Figure 9** in *Chapter 2*.

**Table 25: Cross-sectional mean, median and standard deviation of Family Tax Credit amount, N=6,900, Waves 1 to 7**

Wave	N	Mean	Median	SD
1	1135	2963	2452	1912
2	990	2818	2452	1927
3	985	3543	3183	2489
4	1495	4042	3757	2592
5	1845	4426	3965	2905
6	1735	4671	4279	3258
7	1685	4955	4487	3370

Four types of changes in the FTC amount variable were differentiated for categorical analyses in this chapter. First, becoming FTC-eligible was considered an *increase* in FTC amount, as becoming FTC eligible involved a change from a \$0 to any (non-zero) amount of FTC. Secondly, becoming FTC-ineligible, a change from any to a \$0 FTC amount, was referred to as a *decrease* in FTC amount. Thirdly, for participants who were FTC-eligible at both of two consecutive waves, an *increase* in the FTC amount was understood to be captured by an increase in FTC amount by at least one quintile. One example is the case of a participant, who was entitled to an amount of FTC that fell within the first (lowest) quintile of all FTC amounts at wave<sub>t</sub> and to a FTC amount within the second quintile at wave<sub>t+1</sub>, experiencing an increase by one quintile over the two consecutive waves. The fourth type of change was, for FTC-eligible participants, a *decrease* in FTC amount, captured by a decrease by at least one quintile of FTC amounts between consecutive waves. Finally, *no change* firstly referred to participants who were eligible for a FTC amount falling within the same quintile at both consecutive waves. I note that analyses in this chapter conflate FTC eligibility with FTC amount; analyses in *Chapter 8* consider these changes separately. As with FTC eligibility, participants who moved into or out of a family were also considered to not change their FTC amount.

On average, 9.6% of participants increased and 8.9% decreased their FTC amount between consecutive waves (**Table 26**). As with FTC eligibility, increases in the FTC amount were

particularly prominent around the initial expansion of FTC, with 13.8% of participants increasing their FTC amount between Waves 3 and 4, contributing one-fourth (23.8%) of all increases over the study period. The considerable proportion of participants who decreased their FTC amount between Waves 5 and 7 is noted and may be due to maturation of the cohort over time. Two in five (41.5%) participants changed their FTC amount once or more times over the study period, most commonly twice (13.0%), three times (9.1%), once (8.9%) or four times (6.8%). The transition matrix demonstrated considerable change, including both increases and decreases, in FTC amount (**Table 27**), confirming the use of FTC amount as a time-varying variable in the fixed effect regression analyses.

### **In-Work Tax Credit eligibility**

Because IWTC had not yet been introduced at the first three waves, all participants were IWTC-ineligible at these waves (**Table 28**). At Wave 4, as IWTC was introduced, one in six (15.3%) participants was IWTC-eligible and at Waves 5, 6 and 7, as IWTC was expanded, one in four (23.6% to 27.4%) participants was IWTC-eligible. These patterns observed in the study sample were consistent with the introduction and expansion of IWTC over the 2006-08 period in the general population demonstrated in official statistics on the number of families receiving IWTC [233] presented in **Figure 6** in *Chapter 2*.

The same types of changes defined above for change in FTC eligibility were differentiated for IWTC eligibility. The 14.6% of all participants who became IWTC-eligible between Waves 3 and 4 due to the introduction of IWTC contributed 36.3% and the 15.0% of participants who became IWTC-eligible due to the expansion of IWTC between Waves 4 and 5 contributed 37.4% of all such changes to IWTC eligibility over the study period (**Table 29**). The percentage of participants who were IWTC-eligible at Wave 4 (15.3%) was larger than the percentage of participants classified as having become IWTC-eligible between Waves 3 and 4 (14.6%), because those participants who moved from not being in a family into being in an IWTC-eligible family were defined as having undergone no change. A much smaller percentage of participants became IWTC-ineligible, and these changes occurred mostly between Waves 5 and 6 (7.0% of all participants contributing 37.5% of all such changes) and Waves 6 and 7 (8.0% of all participants contributing 42.9% of all such changes). The increase in the percentage of participants becoming IWTC-ineligible between Wave 5 and 7 may reflect decreased labour demand and increased unemployment due to the onset of the global economic recession in this period. Because of the employment conditionality of the IWTC participants who became unemployed also became IWTC-ineligible. However, on average,

Chapter 7: Descriptive analyses of time-varying variables

**Table 26: Change in Family Tax Credit amount (by  $\geq 1$  quintile) between wave<sub>t</sub> and wave<sub>t+1</sub>, N=6,900, Waves 1 to 7**

	Change in FTC amount												
	Increase			Decrease			No change			Not in a family at one or both waves			Total
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N
Waves 1 / 2	500	12.5	7.2	565	15.4	8.2	4395	18.1	63.7	1440	15.2	20.9	6900
Waves 2 / 3	480	12.0	7.0	500	13.6	7.2	4395	18.1	63.7	1525	16.1	22.1	6900
Waves 3 / 4	950	23.8	13.8	335	9.1	4.9	4030	16.6	58.4	1590	16.8	23.0	6905
Waves 4 / 5	965	24.2	14.0	475	13.0	6.9	3835	15.8	55.6	1620	17.1	23.5	6895
Waves 5 / 6	585	14.7	8.5	850	23.2	12.3	3825	15.7	55.4	1640	17.4	23.8	6900
Waves 6 / 7	505	12.7	7.3	940	25.6	13.6	3815	15.7	55.3	1635	17.3	23.7	6895
Total	3985	100.0	9.6	3665	100.0	8.9	24295	100.0	58.7	9450	100.0	22.8	41395

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator).

**Table 27: Transition matrix for Family Tax Credit amount, N=6,900, Waves 1 to 7**

Wave <sub>t</sub>	Wave <sub>t+1</sub>							
	Q1	Q2	Q3	Q4	Q5	Ineligible	Not in a family	Total
Q1	0.206	0.131	0.072	0.056	0.044	0.463	0.028	1600
Q2	0.139	0.198	0.149	0.116	0.040	0.330	0.030	1515
Q3	0.051	0.122	0.315	0.086	0.065	0.313	0.048	1680
Q4	0.053	0.100	0.132	0.263	0.221	0.206	0.025	1405
Q5	0.031	0.046	0.054	0.166	0.480	0.186	0.038	1960
Ineligible	0.030	0.019	0.015	0.012	0.011	0.835	0.078	26080
Not in a family	0.010	0.010	0.019	0.007	0.011	0.228	0.716	7190
Total	1605	1520	1680	1470	1850	25840	7465	41430

Chapter 7: Descriptive analyses of time-varying variables

**Table 28: Cross-sectional distribution of In-Work Tax Credit eligibility, N=6,900, Waves 1 to 7**

IWTC eligibility	Wave															
	Wave 1		Wave 2		Wave 3		Wave 4		Wave 5		Wave 6		Wave 7		Total	
	N	%	N	%	N	%	N	%	N	%	N	%	N	%	N	%
Eligible	0	0.0	0	0.0	0	0.0	1055	15.3	1890	27.4	1810	26.2	1630	23.6	6385	13.2
Ineligible	5835	84.6	5790	83.9	5725	83.0	4625	67.0	3735	54.1	3775	54.7	3930	57.0	33415	69.2
Not in a family	1065	15.4	1110	16.1	1175	17.0	1220	17.7	1275	18.5	1315	19.1	1340	19.4	8500	17.6
Total	6900	100.0	6900	100.0	6900	100.0	6900	100.0	6900	100.0	6900	100.0	6900	100.0	48300	100.0

**Table 29: Change in In-Work Tax Credit eligibility between wave<sub>t</sub> and wave<sub>t+1</sub>, N=6,900, Waves 1 to 7**

	Change in IWTC eligibility																
	Became eligible			Became ineligible			No change: Eligible			No change: Ineligible			Not in a family at one or both waves			Total	
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	
Waves 1 / 2	0	0.0	0.0	0	0.0	0.0	0	0.0	0.0	5460	22.2	79.1	1440	15.2	20.9	6900	
Waves 2 / 3	0	0.0	0.0	0	0.0	0.0	0	0.0	0.0	5375	21.9	78.0	1520	16.1	22.0	6895	
Waves 3 / 4	1005	36.3	14.6	0	0.0	0.0	0	0.0	0.0	4305	17.5	62.4	1590	16.8	23.0	6900	
Waves 4 / 5	1035	37.4	15.0	255	19.7	3.7	765	22.9	11.1	3225	13.1	46.7	1620	17.2	23.5	6900	
Waves 5 / 6	390	14.1	5.7	485	37.5	7.0	1345	40.2	19.5	3040	12.4	44.1	1640	17.4	23.8	6900	
Waves 6 / 7	335	12.1	4.9	555	42.9	8.0	1235	36.9	17.9	3140	12.8	45.5	1635	17.3	23.7	6900	
Total	2765	100.0	6.7	1295	100.0	3.1	3345	100.0	8.1	24545	100.0	59.3	9445	100.0	22.8	41395	

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator).

over twice as many participants became IWTC-eligible (6.7%) than became IWTC-ineligible between two consecutive waves (3.1%), suggesting that dynamics in IWTC eligibility were driven by participants becoming IWTC-eligible. A total of 37.9% of participants changed their IWTC eligibility one or more times over the study period, with 20.6% changing once, 14.3% twice and only 2.6% three and 0.4% four times. The transition matrix of IWTC eligibility demonstrated considerable change in IWTC eligibility over time (**Table 30**), demonstrating sufficient change over time in the variable to use it in the main fixed effects regression analyses.

**Table 30: Transition matrix for In-Work Tax Credit eligibility, N=6,900, Waves 1 to 7**

Wavet	Wavet+1			Total
	Eligible	Ineligible	Not in a family	
Eligible	0.703	0.272	0.025	4760
Ineligible	0.094	0.832	0.074	29490
Not in a family	0.038	0.243	0.719	7160
Total	6385	27580	7445	41410

## In-Work Tax Credit amount

At Waves 4 to 6, an IWTC-eligible family was on average eligible for an IWTC of between \$2,715 and \$2,892, with a standard deviation of between \$946 and \$971, and a median of \$3,131 (**Table 31**). At Wave 7, the mean IWTC amount had increased slightly to \$2,911 and there was less variation in IWTC amount (standard deviation \$774). The median had remained the same.

**Table 31: Cross-sectional mean, median and standard deviation of In-Work Tax Credit amount, N=6,900, Waves 1 to 7**

Wave	N	Mean	Median	SD
1	0	0	0	0
2	0	0	0	0
3	0	0	0	0
4	1055	2722	3131	962
5	1885	2745	3131	946
6	1800	2729	3131	971
7	1625	2892	3131	774

Changes in the IWTC amount were defined in the same way as were changes in the FTC amount. Because over 60% of participants were eligible for the maximum IWTC amount (\$3,131) at any of Waves 4 to 7, quintiles 3 to 5 were jointly grouped into the highest IWTC amount category, referred to as quintiles 3 to 5. On average (over all seven waves), 7.5% of all participants increased and only 4.5% decreased their IWTC amount (**Table 32**). The 14.6% of participants who increased their IWTC amount between Waves 3 and 4 were those

**Table 32: Change in In-Work Tax Credit amount (by  $\geq 1$  quintile) between wave<sub>t</sub> and wave<sub>t+1</sub>, N=6,900, Waves 1 to 7**

	Change in IWTC amount												
	Increase			Decrease			No change			Not in a family at one or both waves			Total
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N
Waves 1 / 2	0	0.0	0.0	0	0.0	0.0	5460	20.2	79.1	1440	15.2	20.9	6900
Waves 2 / 3	0	0.0	0.0	0	0.0	0.0	5375	19.9	77.9	1525	16.1	22.1	6900
Waves 3 / 4	1005	32.3	14.6	0	0.0	0.0	4310	16.0	62.4	1590	16.8	23.0	6905
Waves 4 / 5	1135	36.4	16.4	350	19.0	5.1	3800	14.1	55.0	1620	17.1	23.5	6905
Waves 5 / 6	530	17.0	7.7	740	40.1	10.7	3990	14.8	57.8	1640	17.4	23.8	6900
Waves 6 / 7	445	14.3	6.4	755	40.9	10.9	4065	15.1	58.9	1635	17.3	23.7	6900
Total	3115	100.0	7.5	1845	100.0	4.5	27000	100.0	65.2	9450	100.0	22.8	41410

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator).

participants who became IWTC-eligible as IWTC was introduced. The percentage of participants who increased their IWTC amount progressively reduced to 7.7% between Waves 5 and 6 and further to 6.4% between Waves 6 and 7. Offsetting this were the increases in the percentage of participants who decreased their IWTC amount, from 5.1% between Waves 4 and 5 to 10.7% between Waves 5 and 6 to 10.9% between Waves 6 and 7. Two in five (38.5%) participants changed their IWTC amount once or more over seven waves, with most changing twice (15.0%) or once (15.1%) and a small percentage three (7.2%) or four (1.3%) times. The transition matrix demonstrated considerable change in the IWTC amount over time (**Table 33**), confirming that the variable can be used in the fixed effect regression analyses.

## Change in outcome

On average, 37.7% of participants reported excellent, 35.5% very good and 18.9% good SRH at any wave (**Table 34**). The percentage of participants who reported excellent SRH decreased considerably from 44.7% at Wave 1 to around 30.9% at Wave 7, but this trend was offset by an increase in the percentage of participants reporting the other high SRH category, *very good*, at later waves. Only a small percentage of participants reported fair SRH (between 4.0% and 6.0%) and poor SRH (between 1.0% and 1.6%). The mean SRH score reduced over the seven waves from 4.2 to 3.9 with a SD of 0.9. The median was 4.0, the equivalent of the *very good* category, at each wave. The ceiling effect of SRH at cross-sections is noted, where 73.2% of participants reported the highest two response categories.

On average, a similar percentage of participants, that is one in five, increased (19.6%) and reduced (22.4%) their SRH by at least one score between wave<sub>t</sub> and wave<sub>t+1</sub> (**Table 35**). The percentage of all participants who increased their SRH was larger between Waves 3 and 4 (21.4%), when FTC was expanded, than between Waves 2 and 3 (17.0%). This coincided with a smaller percentage of participants decreasing their SRH between Waves 3 and 4 (20.4%) than between Waves 2 and 3 (25.2%). Between Waves 4 and 5, when IWTC was introduced, a somewhat smaller percentage of participants increased their SRH than had increased their SRH between Waves 3 and 4 (18.8% cf. 21.4%). A larger percentage of participants decreased their SRH between Waves 4 and 5 (22.8%) than between Waves 3 and 4 (20.4%). There was considerable change in SRH over time (**Table 36**), confirming that SRH was sufficiently time-varying to be included in the fixed effects regression analyses.

**Table 33: Transition matrix for In-Work Tax Credit amount, N=6,900, Waves 1 to 7**

Wave <sub>t</sub>	Wave <sub>t+1</sub>					Total
	Q1	Q2	Q3, Q4, Q5	Ineligible	Not in a family	
Q1	0.222	0.068	0.159	0.517	0.034	880
Q2	0.219	0.158	0.254	0.342	0.026	570
Q3, Q4, Q5	0.065	0.059	0.657	0.196	0.023	3295
Ineligible	0.021	0.010	0.063	0.832	0.074	29495
Not in a family	0.008	0.003	0.027	0.243	0.718	7165
Total	1220	665	4490	27585	7445	41405

**Table 34: Cross-sectional distribution of self-rated health, N=6,900, Waves 1 to 7**

	Wave 1		Wave 2		Wave 3		Wave 4		Wave 5		Wave 6		Wave 7		Total	
	N	%	N	%	N	%	N	%	N	%	N	%	N	%	N	%
Excellent	3085	44.7	3090	44.8	2610	37.8	2605	37.7	2410	34.9	2300	33.3	2130	30.9	18230	37.7
Very Good	2340	33.9	2145	31.1	2385	34.6	2495	36.1	2555	37.0	2545	36.9	2705	39.2	17170	35.5
Good	1115	16.2	1155	16.7	1280	18.6	1250	18.1	1335	19.4	1450	21.0	1545	22.4	9130	18.9
Fair	290	4.2	275	4.0	345	5.0	320	4.6	360	5.2	345	5.0	415	6.0	2350	4.9
Poor	70	1.0	80	1.2	80	1.2	80	1.2	85	1.2	110	1.6	105	1.5	610	1.3
Total	6905	100.0	6900	100.0	6900	100.0	6905	100.0	6900	100.0	6900	100.0	6900	100.0	48310	100.0

Notes: A total of N = 820 participants had missing values, but these were included in the total counts.

**Table 35: Change in self-rated health (by  $\geq 1$  score) between wave<sub>t</sub> and wave<sub>t+1</sub>, N=6,900, Waves 1 to 7**

	Increase			Decrease			No change			Missing			Total
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N
Waves 1 / 2	1480	18.3	21.5	1435	15.4	20.8	3825	17.1	55.5	155	9.5	2.2	6895
Waves 2 / 3	1175	14.5	17.0	1740	18.7	25.2	3630	16.2	52.6	355	21.8	5.1	6900
Waves 3 / 4	1475	18.2	21.4	1410	15.2	20.4	3660	16.4	53.0	355	21.8	5.1	6900
Waves 4 / 5	1300	16.0	18.8	1575	17.0	22.8	3715	16.6	53.8	310	19.0	4.5	6900
Waves 5 / 6	1365	16.8	19.8	1525	16.4	22.1	3700	16.5	53.7	305	18.7	4.4	6895
Waves 6 / 7	1310	16.2	19.0	1605	17.3	23.3	3830	17.1	55.5	150	9.2	2.2	6895
Total	8105	100.0	19.6	9290	100.0	22.4	22360	100.0	54.0	1630	100.0	3.9	41385

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator).

**Table 36: Transition matrix for self-rated health, N=6,900, Waves 1 to 7**

Wave <sub>t</sub>	Wave <sub>t+1</sub>						Total
	Excellent	Very good	Good	Fair	Poor	Missing	
Excellent	0.648	0.257	0.063	0.008	0.002	0.020	16105
Very good	0.244	0.526	0.184	0.024	0.004	0.018	14465
Good	0.104	0.323	0.448	0.089	0.011	0.024	7565
Fair	0.041	0.153	0.350	0.365	0.073	0.018	1930
Poor	0.048	0.067	0.181	0.286	0.371	0.048	525
Missing	0.352	0.352	0.206	0.061	0.030	0.000	825
Total	15155	14825	8010	2055	540	830	41415

## Change in outcome by change in exposures

### Family Tax Credit

As described above, cross-tabular analyses of change in the SRH outcome by change in the FTC and IWTC exposure variables provide important preliminary analyses for the main fixed effects regression analyses of the thesis presented in the next chapter. A measure of association akin to an OR was calculated from such tabular analyses that could be used to predict the direction and size of the crude effect of the main analyses of this thesis (see *Chapter 5*).

The change in SRH tabulated by change in FTC eligibility and amount is presented in **Table 37**. The odds of increase in SRH by decrease in SRH were less (0.91) among those who became FTC-eligible. However, among those who became FTC-ineligible, the odds was less again at 0.84. This gives an OR of  $0.91 / 0.84 = 1.08$  with a 95% CI of 0.94 to 1.25, suggesting a small, positive and statistically insignificant effect of becoming FTC-eligible on change in SRH. For the FTC amount exposure variable, the odds of increase in SRH by decrease in SRH was less among those who increased their FTC amount (0.92), but was even less among those who decreased their FTC amount (0.85), giving an OR of 1.08 (95% CI 0.98 to 1.19), suggesting a small, positive, but statistically insignificant association between change in FTC amount on change in SRH. In summary, these preliminary analyses suggest small, positive and statistically insignificant associations of change in FTC eligibility and amount with change in SRH. However, there is some risk of misspecification bias and no adjustment for potential confounding.

### In-Work Tax Credit

With regards to IWTC, the odds of increase in SRH by decrease in SRH was less among those who became IWTC-eligible (0.90) and was even less among those who became IWTC-ineligible (0.80), giving an OR of 1.14 (95% CI 0.96 to 1.36) (**Table 38**), suggesting a small and positive, but statistically insignificant association between change in IWTC-eligibility and change in SRH. For IWTC amount, the odds of increase in SRH by decrease in SRH was less (0.91) among participants who increased their IWTC amount, but even less in participants who decreased their IWTC amount (0.77), with an OR of 1.18 (95% CI 1.01 to 1.38) - a small, positive and statistically significant association. In summary, these preliminary analyses suggest a small and positive association of change in IWTC amount with change in SRH, without adjusting for any potential confounding, reaching statistical significance for the IWTC amount variable, with some risk of bias from some misspecification and confounding (e.g., by time).

Chapter 7: Descriptive analyses of time-varying variables

**Table 37: Change in self-rated health (by  $\geq 1$  score) by change in Family Tax Credit eligibility and amount, N=6,900, Waves 1 to 7**

	Change in SRH												Odds increase / decrease	OR (95% CI)		
	Increase			Decrease			No change			Missing					Total	
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %			N	Col %
<b>Change in FTC eligibility</b>																
Increase	1710	21.1	18.1	1880	20.2	19.9	4620	20.7	48.9	1240	75.8	13.1	9450	22.8	0.91	1.08
Decrease	420	5.2	21.2	500	5.4	25.2	1035	4.6	52.1	30	1.8	1.5	1985	4.8	0.84	(0.94 to 1.25)
No change	5445	67.1	19.7	6350	68.3	23.0	15450	69.1	56.0	325	19.9	1.2	27570	66.6		
Not in a family	540	6.7	22.5	570	6.1	23.8	1245	5.6	52.0	40	2.4	1.7	2395	5.8		
Total	8115	100.0	19.6	9300	100.0	22.5	22350	100.0	54.0	1635	100.0	3.9	41400	100.0		
<b>Change in FTC amount</b>																
Increase	1660	20.5	18.5	1810	19.5	20.2	4370	19.5	48.8	1120	68.5	12.5	8960	21.6	0.92	1.08
Decrease	1310	16.2	20.1	1545	16.6	23.7	3590	16.1	55.1	65	4.0	1.0	6510	15.7	0.85	(0.98 to 1.19)
No change	4060	50.1	19.5	4750	51.1	22.8	11740	52.5	56.4	270	16.5	1.3	20820	50.3		
Not in a family	1075	13.3	21.1	1190	12.8	23.3	2660	11.9	52.1	180	11.0	3.5	5105	12.3		
Total	8105	100.0	19.6	9295	100.0	22.5	22360	100.0	54.0	1635	100.0	3.9	41395	100.0		

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). The presented odds are the row percentage for increase in SRH divided by the row percentage for decrease in SRH, within a category of change in FTC eligibility. These ratios of one proportion divided by another are similar to classical odds of  $p / (1-p)$ . Because participants are included more than once in these analyses, the 95% CI are likely too narrow.

**Table 38: Change in self-rated health (by  $\geq 1$  score) by change in In-Work Tax Credit eligibility and amount, N=6,900, Waves 1 to 7**

	Change in SRH												Total	Odds increase/ decrease	OR (95% CI)	
	Increase			Decrease			No change			Missing						
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %				N
<b>Change in IWTC eligibility</b>																
Increase	1710	21.1	18.1	1880	20.2	19.9	4620	20.7	48.9	1240	76.1	13.1	9450	22.8	0.91	1.14
Decrease	255	3.1	19.7	320	3.4	24.7	705	3.2	54.4	15	0.9	1.2	1295	3.1	0.80	(0.96 to 1.36)
No change	5530	68.2	19.8	6445	69.4	23.1	15570	69.7	55.8	335	20.6	1.2	27880	67.4		
Not in a family	615	7.6	22.3	645	6.9	23.4	1455	6.5	52.8	40	2.5	1.5	2755	6.7		
Total	8110	100.0	19.6	9290	100.0	22.5	22350	100.0	54.0	1630	100.0	3.9	41380	100.0		
<b>Change in IWTC amount</b>																
Increase	1710	21.1	18.1	1880	20.2	19.9	4620	20.7	48.9	1240	75.8	13.1	9450	22.8	0.91	1.18
Decrease	350	4.3	19.0	455	4.9	24.7	1015	4.5	55.2	20	1.2	1.1	1840	4.4	0.77	(1.01 to 1.38)
No change	5355	66.1	19.8	6220	66.9	23.0	15075	67.4	55.9	335	20.5	1.2	26985	65.2		
Not in a family	685	8.5	22.0	740	8.0	23.8	1645	7.4	52.9	40	2.4	1.3	3110	7.5		
Total	8100	100.0	19.6	9295	100.0	22.5	22355	100.0	54.0	1635	100.0	4.0	41385	100.0		

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). The presented odds are the row percentage for increase in SRH divided by the row percentage for decrease in SRH, within a category of change in FTC eligibility. These ratios of one proportion divided by another are similar to classical odds of  $p / (1-p)$ . Because participants are included more than once in these analyses, the 95% CI are likely too narrow.

## Change in time-varying confounder variables

Four variables were hypothesised to be time-varying and potentially confound the FTC-SRH or IWTC-SRH relationships. These were equivalised gross total annual income (minus FTC or IWTC); family type, number of dependent children in the family; and employment status. The derivation of these variables has been described in *Chapter 5* and their cross-sectional distribution at baseline in *Chapter 6*. This description of the changes of these variables over the entire study period aims to test empirically, as a preliminary analysis for the fixed effects regression analyses, whether these variables do considerably change over time and whether there is any evidence of confounding in simple cross-tabular analyses.

### **Equivalised gross total annual family income (minus Family Tax Credit or In-Work Tax Credit)**

The income variable that was considered to potentially confound the FTC-SRH or IWTC-SRH relationship was equivalised gross total annual income (minus FTC or IWTC). This variable was the gross total annual family income from all sources that all members of a family received, minus the amount of the FTC or IWTC that the family was entitled to, and equivalised for household composition, using the New Zealand-specific Jensen Index [235]. The limitations of the quality of these income data and the potential risk of bias of these data described in *Chapter 5* are again noted. The implications of these data limitations for the findings of the thesis are discussed in detail in *Chapters 9 and 10*.

The mean equivalised gross total annual family income (minus FTC) was larger at each consecutive wave, increasing steadily from \$54,374 (median of \$43,489, standard deviation of \$56,176) at Wave 1 to \$73,607 (median of \$59,675, standard deviation of \$75,983) at Wave 7 (**Table 39**). The considerably variability in equivalised gross total annual family income (minus FTC) is noted for Waves 3 and 5. This variability is due to extreme changes captured in the lowest (1%) and highest (99%) percentiles of changes, which encompassed all reductions by more than \$142,368 and increases by more than \$155,778. While some of these extreme changes may have been plausible, a small number of participants within these percentiles reported very large and likely implausible changes such as increases of up to around \$10,000,000, as well as decreases of up to \$8,000,000. These participants remained in the cross-tabular and fixed effect regression analyses. However, sensitivity analyses are presented in *Chapter 9*, where participants reporting extreme income changes are removed. The observed increases in family income over time partially reflected net increases in mean family

income over the study period that resulted from the growth of the New Zealand economy over the study period. However, these increases are not changes in ‘real’ income, considering that the family income variable is unadjusted for inflation captured by factors such as the consumer price index.

**Table 39: Cross-sectional mean, median and standard deviation of equivalised total annual family income (minus Family Tax Credit), N=6,900, Waves 1 to 7**

wave	N	Mean	Median	SD
1	6900	54374	43489	56176
2	6900	57833	45383	68182
3	6900	62813	47718	181044
4	6900	64166	51104	84309
5	6900	68400	53466	102662
6	6900	72618	56178	89246
7	6900	73607	59675	75983

Consistent with the approach taken to define change in FTC and IWTC amount, a participant was considered to change their equivalised gross total annual family income (minus FTC) when the participant increased or decreased their amount of this income by at least one quintile between two consecutive waves. On average, 20.6% of participants increased and 21.0% decreased their equivalised gross total annual family income (minus FTC) between consecutive waves (**Table 40**). Change in equivalised gross total annual family income (minus FTC) was substantial, with higher level of mobility occurring in the middle-income quintiles (**Table 41**), suggesting that the variable could be used in fixed effects regression analyses.

The analytical framework of this thesis for the FTC-SRH relationship presented in **Figure 19** in *Chapter 5* included equivalised gross total annual income (minus FTC) as a potential time-varying confounding variable. In order for this variable to indeed confound the FTC-SRH relationship in the fixed effects regression analyses, change in the variable should be associated with change in the exposure variable, FTC, and should also be associated with change in the outcome variable, SRH. Because this and the other potential confounding variables are determinants of FTC or IWTC, an association between change in these variables and change in FTC and IWTC is almost certain. If no such association is found, then this suggests that change in the exposure variable is not driven by change in the determinant, pointing towards the relative higher importance of other determinants in driving changes in the exposure. These associations were tested empirically by calculating the ratio of the odds of increase by decrease in the exposure variable in participants who increased their equivalised gross total annual income (minus FTC), divided by the odds in those who decreased their equivalised gross total annual income (minus FTC). The equivalent odds ratio

**Table 40: Change in equivalised total annual family income (minus Family Tax Credit) (by  $\geq 1$  quintile) between wave<sub>t</sub> and wave<sub>t+1</sub>, N=6,900, Waves 1 to 7**

	Increase			Decrease			No change			Total
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N
Waves 1 / 2	1375	16.1	19.9	1555	17.9	22.5	3970	16.4	57.5	6900
Waves 2 / 3	1610	18.9	23.3	1460	16.8	21.2	3830	15.8	55.5	6900
Waves 3 / 4	1530	17.9	22.2	1480	17.0	21.4	3890	16.1	56.4	6900
Waves 4 / 5	1505	17.6	21.8	1400	16.1	20.3	3995	16.5	57.9	6900
Waves 5 / 6	1435	16.8	20.8	1480	17.0	21.4	3985	16.5	57.8	6900
Waves 6 / 7	1075	12.6	15.6	1320	15.2	19.1	4505	18.6	65.3	6900
Total	8530	100.0	20.6	8695	100.0	21.0	24175	100.0	58.4	41400

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator).

**Table 41: Transition matrix for equivalised total annual family income (minus Family Tax Credit), N=6,900, Waves 1 to 7**

Wave <sub>t</sub>	Wave <sub>t+1</sub>					Total
	Q1	Q2	Q3	Q4	Q5	
Q1	0.548	0.293	0.086	0.039	0.033	7990
Q2	0.185	0.428	0.256	0.089	0.042	7565
Q3	0.070	0.167	0.402	0.268	0.093	7630
Q4	0.074	0.057	0.173	0.444	0.253	8225
Q5	0.146	0.029	0.049	0.148	0.628	9985
Total	8380	7605	7605	8155	9650	41395

was also calculated for the association between the variable and the outcome variable, namely the odds of an increase by a decrease in SRH in participants who increased was divided by these odds in participants who decrease their equivalised gross total annual income (minus FTC). If change in equivalised gross total annual income (minus FTC) was associated both with change in FTC and change in SRH, then it is plausible that the variable significantly confounded the FTC-SRH relationship in the fixed effects regression analyses. However, confounding would only be guaranteed, if the potential confounding variable was associated with the outcome variable within strata of exposure *and* the confounder variable was neither rare nor ubiquitous.

Change in equivalised gross total annual income (minus FTC) was strongly associated with change in FTC eligibility (OR 0.02, 95% CI 0.02 to 0.02) and amount (OR 0.02, 95% CI 0.01 to 0.02) (**Table 42**). Change in the variable may have been associated with change in SRH (OR 1.07, 95% CI 0.99 to 1.15). This suggests that equivalised gross total annual family income (minus FTC) may have confounded the FTC-SRH relationship in the main fixed effects regression analyses.

The mean equivalised gross total annual family income (minus IWTC) was larger at each consecutive wave, increasing steadily from \$54,722 (median of \$43,489, standard deviation of \$55,920) at Wave 1 to \$73,957 (median of \$59,548, standard deviation of \$75,714) at Wave 7 (**Table 43**). The same pattern of change was observed for this variables as for equivalised gross total annual family income (minus FTC), with an average decrease in the variable observed in 21.8% and an average decrease in 20.8% of participants (**Table 44**). The variable also changed considerably over time (**Table 45**). Change in equivalised total gross annual family income (minus IWTC) was associated with change in IWTC eligibility (OR 0.87, 95% CI 0.81 to 0.93) and amount (OR 0.86, 95% CI 0.80 to 0.92), but was not associated with change in SRH (OR 1.0, 95% CI 0.91 to 1.10) (**Table 46**). This suggested that confounding by this variable in the main fixed effects regression analyses was unlikely.

## **Family type**

On average, seven in ten (70.3%) participants were in a two-parent family, with a somewhat declining trend over time from 72.0% at Wave 1 to 68.4% at Wave 7 (**Table 47**). One in eight (12.1%) participants was in a one-parent family on average at each cross-section. Participants not in families comprised an average of 16.4%, one in six participants, with a slight increase in their percentages over time offsetting the decrease in participants in two-parent families. The large majority of participants, three of four (77.3%), were in one- or two-parent families at

Chapter 7: Descriptive analyses of time-varying variables

**Table 42: Change in equivalised total gross annual income (minus Family Tax Credit) ( $\geq 1$  quintile) by change in exposure and outcome variables, N=6,900, Waves 1 to 7**

	Change ( $\geq 1$ quintile) in equivalised total gross annual income (minus FTC)											Odds increase / decrease	OR (95 CI)
	Increase			Decrease			No change			Total			
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %		
Change in exposure variables													
Change in FTC eligibility													
Increase	190	2.2	8.0	1345	15.7	56.6	840	3.5	35.4	2375	5.7	0.14	0.02
Decrease	1065	12.1	53.9	145	1.7	7.3	765	3.2	38.7	1975	4.8	7.35	(0.02 to 0.02)
No change	5560	63.4	20.2	4930	57.4	17.9	17100	71.2	62.0	27590	66.7		
Not in family	1955	22.3	20.7	2170	25.3	23.0	5325	22.2	56.3	9450	22.8		
Total	8770	100.0	21.2	8590	100.0	20.8	24030	100.0	58.1	41390	100.0		
Change in FTC amount													
Increase	1205	14.1	12.3	3320	38.2	33.8	5285	21.8	53.9	9810	23.7	0.36	0.02
Decrease	1925	22.6	47.6	85	1.0	2.1	2035	8.4	50.3	4045	9.8	22.65	(0.01 to 0.02)
No change	4400	51.6	20.1	4105	47.3	18.7	13425	55.5	61.2	21930	53.0		
Not in family	1005	11.8	17.9	1175	13.5	20.9	3450	14.3	61.3	5630	13.6		
Total	8535	100.0	20.6	8685	100.0	21.0	24195	100.0	58.4	41415	100.0		
Change in the outcome variable													
Change in SRH													
Increase	1735	20.4	21.4	4675	19.3	57.6	1700	19.6	21.0	8110	19.6	0.37	1.07
Decrease	1895	22.2	20.4	5445	22.5	58.5	1960	22.6	21.1	9300	22.5	0.35	(0.99 to 1.15)
No change	4460	52.3	19.9	13355	55.2	59.7	4545	52.3	20.3	22360	54.0		
Missing	435	5.1	26.7	710	2.9	43.6	485	5.6	29.8	1630	3.9		
Total	8525	100.0	20.6	24185	100.0	58.4	8690	100.0	21.0	41400	100.0		

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). The presented odds are the row percentage for increase in SRH divided by the row percentage for decrease in SRH, within a category of change in FTC eligibility. These ratios of one proportion divided by another are similar to classical odds of  $p / (1-p)$ . Because participants are included more than once in these analyses, the 95% CI are likely too narrow.

**Table 43: Cross-sectional mean, median and standard deviation of equivalised total annual family income (minus In-Work Tax Credit), N=6,900, Waves 1 to 7**

Wave	N	Mean	Median	SD
1	6900	54722	43489	55920
2	6900	58121	45383	67998
3	6900	63175	47718	180948
4	6900	64502	50797	84091
5	6900	68755	53027	102452
6	6900	73005	55506	88972
7	6900	73957	59548	75714

**Table 44: Change in equivalised total annual family income (minus In-Work Tax Credit) (by  $\geq 1$  quintile) between wave<sub>t</sub> and wave<sub>t+1</sub>, N=6,900, Waves 1 to 7**

	Increase			Decrease			No change			N
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	
Waves 1 / 2	1480	16.4	21.4	1430	16.6	20.7	3990	16.8	57.8	6900
Waves 2 / 3	1600	17.7	23.2	1450	16.8	21.0	3850	16.2	55.8	6900
Waves 3 / 4	1535	17.0	22.2	1465	17.0	21.2	3895	16.4	56.4	6900
Waves 4 / 5	1495	16.6	21.7	1440	16.7	20.9	3965	16.7	57.5	6900
Waves 5 / 6	1495	16.6	21.7	1440	16.7	20.9	3965	16.7	57.5	6900
Waves 6 / 7	1420	15.7	20.6	1405	16.3	20.4	4075	17.2	59.1	6900
Total	9025	100.0	21.8	8630	100.0	20.8	23740	100.0	57.3	41400

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator).

**Table 45: Transition matrix for equivalised total annual income (minus In-Work Tax Credit), N=6,900, Waves 1 to 7**

Wave <sub>t</sub>	Wave <sub>t+1</sub>					Total
	Q1	Q2	Q3	Q4	Q5	
Q1	0.662	0.221	0.062	0.033	0.022	8265
Q2	0.203	0.498	0.213	0.057	0.028	8285
Q3	0.069	0.183	0.484	0.209	0.054	8285
Q4	0.036	0.066	0.187	0.521	0.190	8280
Q5	0.031	0.030	0.056	0.178	0.705	8280
Total	8285	8280	8295	8265	8270	41395

**Table 46: Change in equivalised total gross annual family income (minus In-Work Tax Credit) ( $\geq 1$  quintile) by change in exposure and outcome variables, N=6,900, Waves 1 to 7**

	Change in equivalised total gross annual income (minus IWTC)											Odds increase/ decrease	OR (95% CI)
	Increase			Decrease			No change			Total			
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %		
Change in exposure variables													
Change in IWTC eligibility													
Increase	2215	24.6	23.5	2250	26.1	23.8	4980	21.0	52.7	9445	22.8	0.98	0.87
Decrease	5810	64.6	20.8	5105	59.1	18.3	16990	71.5	60.9	27905	67.4	1.14	(0.81 to 0.93)
No change	335	3.7	12.1	1160	13.4	42.0	1265	5.3	45.8	2760	6.7		
Not in family	640	7.1	49.8	120	1.4	9.3	525	2.2	40.9	1285	3.1		
Total	9000	100.0	21.7	8635	100.0	20.9	23760	100.0	57.4	41395	100.0		
Change in IWTC amount													
Increase	2215	24.6	23.5	2250	26.1	23.8	4980	21.0	52.7	9445	22.8	0.98	0.86
Decrease	5545	61.6	20.8	4830	55.9	18.1	16250	68.4	61.0	26625	64.3	1.14	(0.80 to 0.92)
No change	345	3.8	10.4	1410	16.3	42.7	1550	6.5	46.9	3305	8.0		
Not in family	895	9.9	44.3	145	1.7	7.2	980	4.1	48.5	2020	4.9		
Total	9000	100.0	21.7	8635	100.0	20.9	23760	100.0	57.4	41395	100.0		
Change in the outcome variable													
Change in SRH													
Increase	1790	19.9	22.1	1710	19.8	21.1	4610	19.4	56.8	8110	19.6	1.05	1.00
Decrease	1985	22.0	21.4	1900	22.0	20.4	5410	22.8	58.2	9295	22.5	1.05	(0.91 to 1.10)

Chapter 7: Descriptive analyses of time-varying variables

No change	4785	53.1	21.4	4540	52.6	20.3	13035	54.9	58.3	22360	54.0
Missing	445	4.9	27.2	485	5.6	29.7	705	3.0	43.1	1635	3.9
Total	9005	100.0	21.8	8635	100.0	20.9	23760	100.0	57.4	41400	100.0

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). The presented odds are the row percentage for increase in SRH divided by the row percentage for decrease in SRH, within a category of change in FTC eligibility. These ratios of one proportion divided by another are similar to classical odds of  $p / (1-p)$ . Because participants are included more than once in these analyses, the 95% CI are likely too narrow.

**Table 47: Cross-sectional distribution of family type, N=6,900, Waves 1 to 7**

	Wave 1		Wave 2		Wave 3		Wave 4		Wave 5		Wave 6		Wave 7		Total	
	N	%	N	%	N	%	N	%	N	%	N	%	N	%	N	%
Two-parent family	4970	72.0	4945	71.7	4890	70.9	4860	70.4	4795	69.5	4755	68.9	4715	68.4	33930	70.3
One-parent family	865	12.5	845	12.2	835	12.1	820	11.9	825	12.0	830	12.0	840	12.2	5860	12.1
Not in a family	1065	15.4	1010	14.6	1045	15.1	1110	16.1	1170	17.0	1200	17.4	1340	19.4	7940	16.4
Total	6900	100.0	6900	100.0	6900	100.0	6900	100.0	6895	100.0	6900	100.0	6895	100.0	48290	100.0

Notes: A total of N = 560 participants had missing values for family type, but are included in the total counts.

consecutive waves (**Table 48**). The percentage of participants who were excluded from the fixed effects regression analyses, because they moved out of a family or into a family was very small (4.4% and 3.8%, respectively). Furthermore, family types were relatively stable, with 65.0% of participants remaining in two-parent families and 10.1% in one-parent families, and only one of hundred participants transitioning from a one-parent to a two-parent family (1.2%) and from a two-parent to a one-parent family (0.9%) (**Table 49**). The largest change in family type of 7.8% was observed in the small number of participants who were in one-parent families at wave<sub>t</sub> and transitioned into two-parent families at wave<sub>t+1</sub>; only 1.7% of the large number of participants in two-parent families were in a one-parent family at the next wave, demonstrating relatively more dynamics in one-parent families (**Table 50**).

Change in family type was associated with change in both FTC exposure variables, change in FTC eligibility (OR 0.06, 95% CI 0.03 to 0.10) and FTC amount (OR 0.08, 95% CI 0.06 to 0.13), but there was little evidence that it was also associated with change in the SRH outcome (OR 1.30, 95% CI 0.87 to 1.94) (**Table 51**). Given the strong association of change in family and no evidence for a (strong) association with change in the outcome, it is unlikely that family type confounded the FTC-SRH relationship in fixed effects regression analyses. However, if the OR is interpreted less strictly as potentially indicative of a weak association with change in SRH, considering the strong association found for change in the exposure variables with change in family type, then change in family type could perhaps confound the FTC-SRH relationship. No evidence was found for an association between change in family type and change in IWTC eligibility (OR 1.33, 95% CI 0.77 to 2.32) or change in IWTC amount (OR 1.0, 95% CI 0.59 to 1.70), as well as with change in SRH (OR 1.30, 95% CI 0.87 to 1.94) suggesting confounding of the IWTC-SRH relationship by family type is unlikely.

These findings also suggest that changes in family type did not drive changes in IWTC, despite family type influencing the required number of hours worked per week for IWTC eligibility and entitlement. One plausible explanation for this perhaps surprising finding is that participants who changed their family type may have adapted their employment to ensure that they stayed within the eligibility criteria for IWTC.

## **Number of children**

All participants included in the study sample lived with children, whether these were dependent or independent children as per Working For Families criteria described in *Chapter 2*, at two or more consecutive waves. On average, at each wave, one in three (35.3%)

Chapter 7: Descriptive analyses of time-varying variables

**Table 48: Change in being in a family between wave<sub>t</sub> and wave<sub>t+1</sub>, N=6,900, Waves 1 to 7**

	No change: In family			Moved out of a family			Moved into a family			No change: Not in a family			Missing			Total
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	
Waves 1 / 2	5460	17.1	79.1	290	15.9	4.2	330	21.1	4.8	720	14.6	10.4	100	9.0	1.5	6900
Waves 2 / 3	5375	16.8	77.9	305	16.7	4.4	270	17.3	3.9	725	14.7	10.5	225	20.2	3.3	6900
Waves 3 / 4	5310	16.6	77.0	325	17.8	4.7	260	16.6	3.8	765	15.5	11.1	240	21.5	3.5	6900
Waves 4 / 5	5280	16.5	76.5	320	17.5	4.6	250	16.0	3.6	835	16.9	12.1	215	19.3	3.1	6900
Waves 5 / 6	5260	16.5	76.2	260	14.3	3.8	250	16.0	3.6	910	18.4	13.2	220	19.7	3.2	6900
Waves 6 / 7	5260	16.5	76.2	325	17.8	4.7	205	13.1	3.0	995	20.1	14.4	115	10.3	1.7	6900
Total	31945	100.0	77.3	1825	100.0	4.4	1565	100.0	3.8	4950	100.0	12.0	1115	100.0	2.7	41400

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator).

**Table 49: Change in family type (one-parent family, two-parent family) between wave<sub>t</sub> and wave<sub>t+1</sub>, N=6,900, Waves 1 to 7**

	From two- to one-parent family			From one- to two-parent family			No change: One-parent family			No change: Two-parent family			Total
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	
Waves 1 / 2	105	21.4	1.5	70	18.0	1.0	705	16.9	10.2	4580	17.0	66.4	5460
Waves 2 / 3	75	15.3	1.1	60	15.4	0.9	700	16.8	10.1	4540	16.9	65.8	5375
Waves 3 / 4	85	17.4	1.2	70	18.0	1.0	670	16.1	9.7	4485	16.7	65.0	5310
Waves 4 / 5	75	15.3	1.1	70	18.0	1.0	685	16.4	9.9	4455	16.6	64.5	5285
Waves 5 / 6	75	15.3	1.1	60	15.4	0.9	700	16.8	10.1	4425	16.5	64.1	5260
Waves 6 / 7	75	15.3	1.1	60	15.4	0.9	710	17.0	10.3	4415	16.4	64.0	5260
Total	490	100.0	1.2	390	100.0	0.9	4170	100.0	10.1	26900	100.0	65.0	31950

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator).

**Table 50: Transition matrix for family type, N=6,900, Waves 1 to 7**

Wave <sub>t</sub>	Wave <sub>t+1</sub>				Total
	One-parent family	Two-parent family	Not in a family	Missing	
One-parent family	0.832	0.078	0.069	0.021	5010
Two-parent family	0.017	0.920	0.050	0.012	29225
Not in a family	0.033	0.204	0.749	0.014	6610
Missing	0.221	0.593	0.186	0.000	565
Total	5005	28975	6875	555	41410

**Table 51: Change in family type by change in exposure and outcome variables, N=6,900, Waves 1 to 7**

	Change in family type										Odds increase / decrease	OR (95% CI)	
	From one-parent to two-parent			From two-parent to one-parent			No change			Total			
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N			Col %
Change in exposure variables													
Change in FTC eligibility													
Increase	35	8.8	1.5	155	31.6	6.5	2200	5.4	92.1	2390	5.8	0.23	0.06
Decrease	115	28.8	5.8	30	6.1	1.5	1850	4.6	92.7	1995	4.8	3.83	(0.03 to 0.10)
No change	250	62.5	0.9	305	62.2	1.1	27020	66.7	98.0	27575	66.6		
Not in family	0	0.0	0.0	0	0.0	0.0	9455	23.3	100.0	9455	22.8		
Total	400	100.0	1.0	490	100.0	1.2	40525	100.0	97.9	41415	100.0		
Change in FTC amount													
Increase	90	22.8	0.9	260	54.7	2.7	9450	23.3	96.4	9800	23.7	0.35	0.08
Decrease	165	41.8	4.1	40	8.4	1.0	3835	9.5	94.9	4040	9.8	4.13	(0.06 to 0.13)
No change	135	34.2	0.6	170	35.8	0.7	23040	56.9	98.7	23345	56.4		
Not in family	5	1.3	0.1	5	1.1	0.1	4185	10.3	99.8	4195	10.1		
Total	395	100.0	1.0	475	100.0	1.1	40510	100.0	97.9	41380	100.0		
Change in IWTC eligibility													
Increase	65	13.1	2.4	55	14.3	2.0	2580	6.4	95.6	2700	6.5	1.18	1.33
Decrease	40	8.1	2.9	45	11.7	3.3	1290	3.2	93.8	1375	3.3	0.89	(0.77 to 2.32)
No change	390	78.8	1.4	285	74.0	1.0	27195	67.1	97.6	27870	67.3		
Not in family	0	0.0	0.0	0	0.0	0.0	9450	23.3	100.0	9450	22.8		
Total	495	100.0	1.2	385	100.0	0.9	40515	100.0	97.9	41395	100.0		

Chapter 7: Descriptive analyses of time-varying variables

Change in IWTC amount													
Increase	60	12.4	1.8	60	15.4	1.8	3170	7.8	96.4	3290	7.9	1.00	1.00
Decrease	50	10.3	2.5	50	12.8	2.5	1940	4.8	95.1	2040	4.9	1.00	(0.59 to 1.70)
No change	375	77.3	1.4	280	71.8	1.1	25960	64.1	97.5	26615	64.3		
Not in family	0	0.0	0.0	0	0.0	0.0	9455	23.3	100.0	9455	22.8		
Total	485	100.0	1.2	390	100.0	0.9	40525	100.0	97.9	41400	100.0		
Change in the outcome variable													
Change in SRH													
Increase	115	23.7	1.4	80	20.5	1.0	7905	19.5	97.6	8100	19.6	1.44	1.30
Decrease	105	21.6	1.1	95	24.4	1.0	9090	22.4	97.8	9290	22.4	1.11	(0.87 to 1.94)
No change	245	50.5	1.1	190	48.7	0.9	21910	54.0	98.1	22345	53.9		
Missing	20	4.1	1.2	25	6.4	1.5	1650	4.1	97.3	1695	4.1		
Total	485	100.0	1.2	390	100.0	0.9	40555	100.0	97.9	41430	100.0		

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). The presented odds are the row percentage for increase in SRH divided by the row percentage for decrease in SRH, within a category of change in FTC eligibility. These ratios of one proportion divided by another are similar to classical odds of  $p / (1-p)$ . Because participants are included more than once in these analyses, the 95% CI are likely too narrow.

participants lived with no dependent children, but had independent children in their family (**Table 52**). One in four participants had one (24.1%) or two children (26.8%) and one in ten (10.3%) had three dependent children. Having four to ten dependent children in the family was rare, affecting only 3.5% of participants. On average, 7.3% of participants increased and 11.2% decreased the number of dependent children in their family by at least one between consecutive waves (**Table 53**), generating considerable change in the variable over time (**Table 54**).

A strong association of change in the number of dependent children was observed with change in each of the exposure variables, that is change in FTC eligibility (OR 17.33) and FTC amount (OR 0.03), as well as change in IWTC eligibility (OR 11.80) and IWTC amount (OR 1.94, 95% CI 1.60 to 2.36) (**Table 55**). This suggested that changes in FTC and IWTC are driven by change in the number of dependent children in a family. However, no evidence for an association between change in number of dependent children was found with change in SRH (OR 0.98, 95% CI 0.84 to 1.13), suggesting that the number of dependent children did not confound the FTC-SRH and IWTC-SRH relationships.

## Employment status

An average of eight in ten (82.6%) participants were employed, one in six (15.7%) labour market inactive, but only 1.6% were unemployed at each cross-section (**Table 56**). However, some change in employment status occurred, with one in twenty participants becoming employed (4.6%) and unemployed or inactive (4.0%), respectively (**Table 57**). The group experiencing most change in employment status was the small number of participants who were unemployed at wave<sub>t</sub>, half of whom (48.5%) transitioned into employed at wave<sub>t+1</sub> (**Table 58**). In contrast, of the large group of participants who were employed at wave<sub>t</sub>, only 0.8% became unemployed and 4.1% labour market inactive. This suggests little relative movement away from employment, but substantial relative movement away from unemployment and, to a lesser degree, inactive employment status.

Notably strong associations were found between change in employment status and change in IWTC eligibility and amount (OR 2.10, 95% CI 1.46 to 3.01) as well as with change in SRH (OR 0.74, 95% CI 0.61 to 0.90) (**Table 59**), suggesting that changes IWTC eligibility and amount were driven by changes in employment status and raising the expectation that employment status could confound the IWTC-SRH relationship in the fixed effects regression analyses.

Chapter 7: Descriptive analyses of time-varying variables

**Table 52: Cross-sectional distribution of number of dependent children, N=6,900, Waves 1 to 7**

	Wave 1		Wave 2		Wave 3		Wave 4		Wave 5		Wave 6		Wave 7		Total	
	N	%	N	%	N	%	N	%	N	%	N	%	N	%	N	Col %
0	2025	29.3	2250	32.6	2370	34.3	2440	35.4	2510	36.4	2685	38.8	2780	40.3	17060	35.3
1	1785	25.9	1710	24.8	1655	24.0	1655	24.0	1640	23.8	1585	23.0	1600	23.2	11630	24.1
2	1945	28.2	1920	27.8	1890	27.4	1855	26.9	1850	26.8	1775	25.7	1700	24.6	12935	26.8
3	850	12.3	760	11.0	740	10.7	715	10.4	675	9.8	645	9.3	600	8.7	4985	10.3
4 to 10	295	4.3	260	3.8	240	3.5	230	3.3	220	3.2	215	3.1	220	3.2	1680	3.5
Total	6900	100.0	6900	100.0	6895	100.0	6895	100.0	6895	100.0	6905	100.0	6900	100.0	48290	100.0

**Table 53: Change in number of dependent children between wave<sub>t</sub> and wave<sub>t+1</sub>, N=6,900, Waves 1 to 7**

	Increase			Decrease			No change			N
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	
Waves 1 / 2	555	18.5	8.0	985	21.2	14.3	5365	15.9	77.7	6905
Waves 2 / 3	525	17.5	7.6	740	15.9	10.7	5640	16.7	81.7	6905
Waves 3 / 4	535	17.8	7.8	755	16.2	10.9	5610	16.6	81.3	6900
Waves 4 / 5	520	17.3	7.5	720	15.5	10.4	5660	16.8	82.0	6900
Waves 5 / 6	465	15.5	6.7	770	16.5	11.2	5665	16.8	82.1	6900
Waves 6 / 7	405	13.5	5.9	685	14.7	9.9	5810	17.2	84.2	6900
Total	3005	100.0	7.3	4655	100.0	11.2	33750	100.0	81.5	41410

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator).

**Table 54: Transition matrix for number of dependent children, N=6,900, Waves 1 to 7**

Wave <sub>t</sub>	Wave <sub>t+1</sub>					Total
	0	1	2	3	4-10	
0	0.900	0.074	0.019	0.005	0.002	14275
1	0.174	0.733	0.087	0.004	0.001	10035
2	0.028	0.122	0.812	0.036	0.003	11255
3	0.023	0.013	0.154	0.777	0.034	4385
4-10	0.030	0.007	0.020	0.145	0.798	1485
Total	15050	9855	10985	4130	1415	41435

**Table 55: Change in number of dependent children by change in exposure and outcome variables, N=6,900, Waves 1 to 7**

	Change in number of dependent children										Odds increase / decrease	OR (95% CI)	
	Increase			Decrease			No change			Total			
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N			Col %
Change in exposure variables													
Change in FTC eligibility													
Increase	325	19.5	13.6	135	1.0	5.7	1925	7.3	80.7	2385	5.8	2.41	17.33
Decrease	75	4.5	3.8	540	4.0	27.3	1365	5.2	68.9	1980	4.8	0.14	(12.66 to 23.73)
No change	1270	76.0	4.6	3245	24.3	11.8	23070	87.5	83.6	27585	66.6		
Not in family	0	0.0	0.0	9450	70.7	100.0	0	0.0	0.0	9450	22.8		
Total	1670	100.0	4.0	13370	100.0	32.3	26360	100.0	63.7	41400	100.0		
Change in FTC amount													
Increase	95	5.7	2.7	1030	7.7	29.2	2400	9.1	68.1	3525	8.5	0.09	0.03
Decrease	490	29.2	15.2	150	1.1	4.7	2580	9.8	80.1	3220	7.8	3.27	(0.02 to 0.04)
No change	980	58.3	4.1	2870	21.5	12.1	19900	75.5	83.8	23750	57.4		
Not in family	115	6.8	1.1	9305	69.7	85.4	1480	5.6	13.6	10900	26.3		
Total	1680	100.0	4.1	13355	100.0	32.3	26360	100.0	63.7	41395	100.0		
Change in IWTC eligibility													
Increase	300	28.7	10.8	195	2.2	7.1	2270	7.2	82.1	2765	6.7	1.54	11.80
Decrease	45	4.3	3.5	345	3.8	26.7	900	2.9	69.8	1290	3.1	0.13	(8.24 to 16.89)
No change	700	67.0	4.1	1950	21.7	11.4	28205	89.9	91.4	30855	74.5		

Chapter 7: Descriptive analyses of time-varying variables

Not in family	0	0.0	0.0	6485	72.3	100.0	0	0.0	0.0	6485	15.7		
Total	1045	100.0	3.8	8975	100.0	32.5	31375	100.0	75.8	41395	100.0		
Change in IWTC amount													
Increase	295	28.1	4.3	1295	14.4	18.7	5320	17.0	77.0	6910	16.7	0.23	1.94
Decrease	200	19.0	6.0	1705	19.0	51.1	1430	4.6	42.9	3335	8.1	0.12	(1.60 to 2.36)
No change	465	44.3	3.9	1275	14.2	10.7	23935	76.3	93.2	25675	62.0		
Not in family	90	8.6	1.6	4700	52.4	85.8	685	2.2	12.5	5475	13.2		
Total	1050	100.0	3.8	8975	100.0	32.5	31370	100.0	75.8	41395	100.0		
Change in the outcome variable													
Change in SRH													
Increase	510	17.1	6.3	870	18.7	10.7	6735	20.0	83.0	8115	19.6	0.59	0.98
Decrease	600	20.1	6.5	1000	21.5	10.8	7680	22.8	82.8	9280	22.4	0.60	(0.84 to 1.13)
No change	1495	50.1	6.7	2325	50.0	10.4	18535	54.9	82.9	22355	54.0		
Missing	380	12.7	23.4	455	9.8	28.0	790	2.3	48.6	1625	3.9		
Total	2985	100.0	7.2	4650	100.0	11.2	33740	100.0	81.5	41375	100.0		

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). The presented odds are the row percentage for increase in SRH divided by the row percentage for decrease in SRH, within a category of change in FTC eligibility. These ratios of one proportion divided by another are similar to classical odds of  $p / (1-p)$ . Because participants are included more than once in these analyses, the 95% CI are likely too narrow.

**Table 56: Cross-sectional distribution of employment status, N=6,900, Waves 1 to 7**

	Wave 1		Wave 2		Wave 3		Wave 4		Wave 5		Wave 6		Wave 7		Total	
	N	%	N	%	N	%	N	%	N	%	N	%	N	%	N	%
Employed	5520	79.9	5615	81.4	5710	82.8	5805	84.1	5770	83.6	5740	83.2	5730	82.9	39890	82.6
Unemployed	140	2.0	135	2.0	110	1.6	95	1.4	100	1.5	80	1.2	115	1.7	775	1.6
Inactive	1240	18.0	1140	16.5	1075	15.6	995	14.4	1025	14.9	1075	15.6	1055	15.3	7605	15.7
Other	5	0.1	5	0.1	5	0.1	5	0.1	5	0.1	5	0.1	5	0.1	35	0.1
Total	6905	100.0	6895	100.0	6900	100.0	6900	100.0	6900	100.0	6900	100.0	6910	100.0	48310	100.0

Notes: A total of N = 5 (0.0%) participants had missing values for employment status, but were included in the total counts.

Chapter 7: Descriptive analyses of time-varying variables

**Table 57: Change in employment status between wave<sub>t</sub> and wave<sub>t+1</sub>, N=6,900, Waves 1 to 7**

	Became employed			Became unemployed / inactive			No change: Employed			No change: Unemployed / inactive			Missing			N
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	
Waves 1 / 2	400	21.2	5.8	295	17.7	4.3	5215	16.1	75.6	980	18.4	14.2	10	18.2	0.1	6900
Waves 2 / 3	375	19.8	5.4	280	16.8	4.1	5330	16.4	77.3	900	16.9	13.0	15	27.3	0.2	6900
Waves 3 / 4	325	17.2	4.7	230	13.8	3.3	5475	16.9	79.4	860	16.2	12.5	10	18.2	0.1	6900
Waves 4 / 5	250	13.2	3.6	285	17.1	4.1	5515	17.0	79.9	840	15.8	12.2	10	18.2	0.1	6900
Waves 5 / 6	270	14.3	3.9	300	18.0	4.4	5465	16.8	79.3	855	16.1	12.4	5	9.1	0.1	6895
Waves 6 / 7	270	14.3	3.9	280	16.8	4.1	5460	16.8	79.1	885	16.6	12.8	5	9.1	0.1	6900
Total	1890	100.0	4.6	1670	100.0	4.0	32460	100.0	78.4	5320	100.0	12.9	55	100.0	0.1	41395

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator).

**Table 58: Transition matrix for employment status, N=6,900, Waves 1 to 7**

Wave <sub>t</sub>	Wave <sub>t+1</sub>					Total
	Employed	Unemployed	Inactive	Other	Total	
Employed	0.950	0.008	0.041	0.001	34165	
Unemployed	0.485	0.215	0.300	0.000	650	
Inactive	0.239	0.032	0.727	0.002	6570	
Other	0.500	0.167	0.250	0.083	60	
Total	34375	640	6385	45	41445	

**Table 59: Change in employment status by change in exposure and outcome variables, N=6,900, Waves 1 to 7**

	Change in employment status											Odds increase / decrease	OR (95% CI)
	Became employed			Became unemployed / inactive			No change			Total			
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %		
Change in exposure variables													
Change in IWTC eligibility													
Increase	150	13.5	5.5	100	9.2	3.7	2460	6.3	90.8	2710	6.5	1.50	2.10
Decrease	100	9.0	3.8	140	12.8	5.3	2380	6.1	90.8	2620	6.3	0.71	(1.46 to 3.01)
No change	640	57.4	2.2	525	48.2	1.8	27850	71.0	96.0	29015	70.0		
Not in family	225	20.2	3.2	325	29.8	4.6	6535	16.7	92.2	7085	17.1		
Total	1115	100.0	2.7	1090	100.0	2.6	39225	100.0	94.7	41430	100.0		
Change in IWTC amount													
Increase	150	13.5	5.5	100	9.2	3.7	2460	6.3	90.8	2710	6.5	1.50	2.10
Decrease	100	9.0	3.8	140	12.8	5.3	2380	6.1	90.8	2620	6.3	0.71	(1.46 to 3.01)
No change	640	57.4	2.2	525	48.2	1.8	27850	71.0	96.0	29015	70.0		
Not in family	225	20.2	3.2	325	29.8	4.6	6535	16.7	92.2	7085	17.1		
Total	1115	100.0	2.7	1090	100.0	2.6	39225	100.0	94.7	41430	100.0		
Change in the outcome variable													
Change in SRH													
Increase	325	19.5	4.0	425	22.5	5.2	7375	19.5	90.8	8125	19.6	0.77	0.74
Decrease	455	27.3	4.9	440	23.3	4.7	8400	22.2	90.4	9295	22.4	1.03	(0.61 to 0.90)
No change	815	48.9	3.6	950	50.3	4.2	20625	54.4	92.1	22390	54.0		
Missing	70	4.2	4.3	75	4.0	4.6	1495	3.9	91.2	1640	4.0		
Total	1665	100.0	4.0	1890	100.0	4.6	37895	100.0	91.4	41450	100.0		

Notes: Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). The presented odds are the row percentage for increase in SRH divided by the row percentage for decrease in SRH, within a category of change in FTC eligibility. These ratios of one proportion divided by another are similar to classical odds of  $p / (1-p)$ . Becoming employed was defined as a change from *Unemployed* or *Inactive* into *Employed*. Becoming unemployed / inactive was defined as a change from *Employed* into *Unemployed* or *Inactive*. Participants moving into or out of the *Working overseas* employment category were classified as no change. Because participants are included more than once in these analyses, the 95% CI are likely too narrow.

## Conclusions

This chapter described changes in the exposure, outcome and potential confounding variables over the study period of seven waves. Considerable extents of change were found for all four exposure variables, FTC and IWTC eligibility and amount, confirming that these variables were sufficiently time-varying to be used in fixed effects regression analyses. The changes observed in the study population showed the staggered expansion of FTC and the introduction and subsequent expansion of IWTC, which have also been observed for the general population in official statistics.

Substantial change over time was also found for SRH, confirming that the time-variability of this variable was adequate for fixed effects regression analysis. Tabulating change in the exposure variables by change in the outcome variable found, for each of the association of change in the exposure by change in the outcome, a small, positive association, which only reached statistical significance for IWTC amount (OR 1.18, 95% CI 1.01 to 1.38). However, these measures of association carried some risk of bias from misspecification and were not adjusted for potential time-varying confounding (including time itself), both of which issues the fixed effects regression analyses addressed.

All four variables hypothesised to vary over time and potentially confound the FTC-SRH or IWTC-SRH relationship were found to be sufficiently time-varying to warrant inclusion in fixed effects regression analyses. Change in FTC and IWTC eligibility and amount was found to be driven by change in all of these variables. Little evidence was found for confounding of the FTC-SRH or IWTC-SRH relationship by three variable, equivalised gross total annual family income (minus IWTC), family type and number of dependent children. But change in equivalised gross total annual family income (minus FTC) was associated with change in FTC eligibility and amount and with change in SRH, suggesting that the FTC-SRH relationship could be confounded by equivalised gross total annual family income. Furthermore, change in employment status was associated with change in IWTC eligibility and amount, as well as with change in SRH, suggesting that employment could confound the IWTC-SRH relationship in fixed effects regression analyses.

## Chapter 8: Fixed effects regression analyses

This chapter presents the findings from the main crude and fully adjusted fixed effects regression analyses assessing the effect of FTC and IWTC eligibility and amount on SRH in adults at the individual level over the short term, as well as results from subsidiary analyses. The fully adjusted analyses controlled for any time in-variant confounding and adjusted for measured time-varying confounding.

The results find no evidence of an effect of change in FTC and IWTC eligibility or amount on change in SRH. The best estimate of change in SRH associated with becoming eligible for FTC was a 0.013 increase in score (1.4% of one SD in SRH), but with the adjusted 95% confidence interval (CI) including the null (-0.011 to 0.037). A \$1,000 increase in FTC amount also had no discernible association with change in SRH (estimate -0.001, adjusted 95% CI -0.006 to 0.004). This was consistent with no evidence of an association found in the cross-tabular categorical analyses presented in the previous chapter.

The best estimate of change in SRH associated with becoming eligible for IWTC was a 0.007 increase in score (0.8% of one SD in SRH), but with the adjusted 95% confidence interval including the null (-0.018 to 0.032). A \$1,000 increase in IWTC amount also had no discernible association with change in SRH (estimate 0.002, adjusted 95% CI -0.006 to 0.011). The estimate for IWTC eligibility corresponded with no evidence for an association found in the cross-tabular categorical analyses presented in the previous chapter. However, the previous analyses had overestimated the association of change in IWTC amount with change in SRH, possibly due to not controlling for employment status, which, as predicted in the previous chapter, was shown to have a statistically significant effect on SRH.

Thus, using the observed standard error, an increase in FTC amount of \$1,000 would have required a change in SRH of around 0.025 scores for to be statistically significant. I argue that

any meaningful effect of FTC amount on SRH would be at least a 2.7% change of one standard deviation in SRH, suggesting that type II error in these analyses was unlikely.

Subsidiary analyses found no discernible effect of the FTC and IWTC exposures on SRH over the longer term (when the association of change in SRH was studied on change in SRH two to six year lags) and on two other health outcomes, psychological distress and current tobacco smoking. No evidence for any effect modification by ethnicity or income was found, although these analyses may have had limited statistical power.

This chapter presents the crude and fully adjusted main fixed effects regression analyses of this thesis. The fixed effects regression analyses on the FTC eligibility and amount exposure variables are presented first and analyses of IWTC eligibility and amount second. Finally presented are findings from subsidiary analyses that estimated the effect of FTC and IWTC, when the outcomes lagged behind the exposure variables by longer time periods; assess the effect of FTC and IWTC on two other health outcomes, psychological distress and current tobacco smoking; and test for effect modification by Māori / non-Māori ethnicity and by poverty.

The previous chapter presented cross-tabular analyses of change in the tax credit exposures and change in SRH as preliminary analyses for answering the principal research questions of this thesis. These analyses suggested that becoming eligible for FTC or IWTC and increasing FTC amount by \$1,000 was not associated with discernible change in SRH, but that an increase by in IWTC amount by \$1,000 had a small, positive, statistically significant association with change in SRH. However, these preliminary analyses were not adjusted for potential time-varying confounding and may have been biased by misspecification of changes in the exposure and in SRH to a three-level categorical variable, rather than a linear variable. Analyses presented in this chapter overcome these limitations, and provide the opportunity to compare the findings from the simpler analyses presented in the previous chapter with the superior fixed effects regression analytic results.

The previous chapter also found no evidence that three of the four time-varying variables hypothesised to potentially confound the FTC-SRH and IWTC-SRH relationships. However, change in employment status was associated with change in IWTC eligibility and amount as well as with change in SRH, suggesting that this variable indeed constituted a confounding variable. Therefore, it was predicted that the fixed effects regression analyses would not find significant effects for the three potential confounding variables, equivalised gross total annual family income (minus FTC or IWTC), family type and number of dependent children, but would find a statistically significant effect for employment status in IWTC-SRH analyses.

## **The effect of Family Tax Credit on self-rated health**

### **Family Tax Credit eligibility**

The results of the crude (Model 1) and the fully adjusted (Model 2) linear fixed effects models with FTC eligibility as the exposure and SRH as the outcome variable, which analyses the N=6,900 participants of the study sample over all seven waves (Waves 1 to 7), is presented in **Table 60**. Fixed effects models use information on individuals without missing data in any of the variables included in the model. So, if a participant missed, for example, the SRH variable at Wave 1, then the participant did not contribute an observation to the fixed effect regression analysis for this wave. If the participant had no missing values in any of the variables included in the models for the remaining six waves, then the participant contributed six observations to the fixed effects regression analysis. Had no participants had any missing values in any of the variables included in the analyses, then a total of all 48,290 observations would have been available for fixed effects regression analysis. However, both the crude and fully adjusted models of the analyses with FTC exposure variables were based on 39,580 observations. The number of missing values in variables included in the main fixed effects regression analyses were presented in **Table 7** in *Chapter 5*.

**Table 60: Crude (Model 1) and fully adjusted (Model 2) linear fixed effects model with Family Tax Credit eligibility, potential time-varying confounding variables, outcome self-rated health, N=6,900 (39,580 observations), seven waves (Waves 1 to 7)**

Parameter	Model 1				Model 2			
	Estimate	Standard error	95% CI		Estimate	Standard error	95% CI	
FTC eligibility	0.013	0.012	-0.010	0.035	0.013	0.012	-0.011	0.037
Family income <sup>a</sup>					0.060	0.039	-0.016	0.136
Number of children					-0.003	0.007	-0.017	0.011
One-parent family					0.029	0.022	-0.014	0.072
Two-parent family (reference)					0.000	-	-	-
Wave 1	0.257**	0.012	0.233	0.281	0.260**	0.013	0.235	0.285
Wave 2	0.255**	0.012	0.231	0.279	0.258**	0.013	0.233	0.282
Wave 3	0.141**	0.012	0.116	0.165	0.142**	0.013	0.118	0.167
wave 4	0.155**	0.012	0.131	0.179	0.156**	0.012	0.132	0.181
Wave 5	0.100**	0.012	0.076	0.124	0.101**	0.012	0.077	0.125
wave 6	0.056**	0.012	0.032	0.080	0.057**	0.012	0.033	0.081
wave 7 (reference)	0.000				0.000	-	-	-

Note: \*\* significant at 0.1% level. <sup>a</sup> Scaled by \$10,000. Model 1 used 39,580 observations. Model 2 used 39,580 observations.

The crude fixed effects analysis estimated the effect of becoming FTC-eligible on change in SRH over the short term (i.e., the outcome variable lags behind the exposure variable by one year), controlled for all time-invariant confounding variables, but not adjusted for any time-varying confounding variables. More specifically, these analyses can be interpreted as estimating the

effect of change in FTC eligibility for any reason, including changes in the determinants of FTC [equivalised gross total annual family income (without FTC), the family type and the number of dependent children in the family] *and* changes in the eligibility and / or entitlement criteria for FTC. The best crude fixed effects estimate for the effect of FTC eligibility was an increase of 0.013 (1.4% of one SD) in SRH scores, but with the adjusted 95% CI including the null (-0.010 to 0.035). Since the CI includes the null, the crude effect is statistically non-significant, meaning that the null hypothesis of no effect of eligibility for FTC on SRH cannot be rejected. Effect estimates for the baseline covariates included in fixed effects model (Wave 1 to Wave 7) demonstrate the decline in SRH over time that has been described in the previous chapter. The reasons for this decline are complex and may include survey, ceiling, age and cohort effects. In summary, the crude fixed effects model does not provide evidence for any discernible effect of becoming FTC eligible on change in SRH.

As theorised *a priori* in the analytical model of the thesis presented in **Figure 19** in *Chapter 5*, the FTC-SRH relationship could potentially be confounded by three time-varying variables, the equivalised gross total annual family income (without FTC), the family type and the number of dependent children in the family. In addition to controlling for any time-invariant confounding, the fully adjusted fixed effects regression analysis (Model 2 in **Table 60**) adjusted for these three time-varying confounding variables. This analysis can be interpreted as estimating the association of changes in FTC and changes in SRH that occur only due to changes in the eligibility and / or entitlement criteria for FTC. The fully adjusted fixed effects estimate for the effect of becoming eligible for FTC was a 0.013 increase (1.4% of one SD) in the SRH score (95% CI -0.011 to 0.037). For a person with an average SRH score of 4.071 at Wave 4, the estimated SRH score after becoming eligible for FTC was between 4.060 and 4.108. In other words, a change in FTC eligibility had no discernible effect on change in SRH. In summary, this study did not find any evidence that becoming eligible for FTC had a discernible effect on SRH.

The crude effect estimate did not (considerably) differ from the adjusted effect estimate, suggesting little, if any, impact of confounding by any of the three potential confounding variables included in the model. The fully adjusted model provided, for each potential time-varying confounding variable included in the model, an estimate of the variable's effect on SRH, adjusted for all other variables included in the model. In the current model, none of the potential time-varying confounding variables had any considerable and statistically significant effect. As

demonstrated for the SoFIE dataset in a previous study [62, 63], it is noted that equivalised gross total annual family income had a relatively small and statistically non-significant effect. These findings are consistent with those from the previous chapter that change in FTC eligibility was empirically associated with change in all of the three potential time-varying confounding variables, but appeared to not be associated with change in SRH in the study sample, which did not provide any empirical evidence that would have validated the hypothesised confounding effect of the variables.

If a fixed effects model included collinear (or, in other words, very highly correlated) time-varying variables, then the effect estimates may have been biased. The previous chapter found a strong association between FTC eligibility and change in each of the three potential confounding variables, which suggested the potential for collinearity between FTC eligibility and each of these three variables. Progressively adding potential time-varying confounding variable in the model (model-building) provided an additional opportunity to potentially detect collinearity. If adjusting for a time-varying variable substantially (by 50% or more) increased the standard error of the model, then this suggests that the variable may be collinear with one or more other variables included in the model. Considering that the potential time-varying confounding variables were the eligibility criteria for FTC and thus determined the exposure (although only in tandem with the other two potential time-varying confounding variables), the fully adjusted fixed effects analyses may have been biased by collinearity. However, standard errors remained stable during model-building (**Table 61**), suggesting that the analyses were not affected by collinearity bias.

**Table 61: Stepwise adjustment for potential time-varying confounding variables in a linear fixed effects models with Family Tax Credit eligibility, outcome self-rated health, N=6,900 (39,580 observations), seven waves (Waves 1 to 7)**

Model	Confounders adjusted for	Estimate for FTC	Standard error	95% CI	
1	None (crude model)	0.013	0.012	-0.010	0.035
2	Family income	0.014	0.012	-0.009	0.037
3	Number of children	0.016	0.012	-0.008	0.039
4	Family type	0.013	0.012	-0.011	0.037

## Family Tax Credit amount

The results of the crude (Model 1) and fully adjusted (Model 2) linear fixed effects model with FTC amount as the exposure variable and SRH as the outcome variable, which analyses the N=6,900 participants of the study sample over all seven waves (Waves 1 to 7), are presented in **Table 62**. In

the crude model, there was no discernible effect of an increase by \$1,000 in the amount of FTC that a participant's family was eligible for on the participant's SRH (estimate -0.001, 95% CI -0.005 to 0.004). This corresponds with findings from the previous chapter of no evidence for an association between change in FTC amount and change in SRH in cross-tabular analyses. After fully adjusting the fixed effects model for the three potential time-varying confounding variables, an increase by \$1,000 in the FTC amount that a participant's family was eligible for also had no discernible effect on SRH (estimate -0.001, 95% CI -0.006 to 0.004). Again, this is consistent with findings from the cross-tabular analyses presented in the previous chapter of no evidence for confounding by the three time-varying variables hypothesized to potentially confound the FTC-SRH relationship. In summary, a \$1,000 increase in FTC amount did not have a discernible association with change in SRH.

**Table 62: Crude (Model 1) and fully adjusted (Model 2) linear fixed effects model with Family Tax Credit amount, time-varying co-variates, outcome self-rated health, N=6,900 (39,580 observations), seven waves (Waves 1 to 7)**

Parameter	Model 1				Model 2			
	Estimate	Standard error	95% CI		Estimate	Standard error	95% CI	
FTC amount <sup>a</sup>	-0.001	0.002	-0.005	0.004	-0.001	0.002	-0.006	0.004
Family income <sup>b</sup>					0.054	0.040	-0.024	0.132
Number of children					-0.001	0.007	-0.014	0.013
One-parent family					0.034	0.022	-0.009	0.077
Two-parent family (reference)					0.000	-	-	-
Wave 1	0.256**	0.012	0.231	0.280	0.258**	0.013	0.232	0.283
Wave 2	0.253**	0.012	0.229	0.278	0.255**	0.013	0.230	0.280
Wave 3	0.139**	0.012	0.115	0.163	0.140**	0.013	0.115	0.164
wave 4	0.155**	0.012	0.131	0.179	0.156**	0.012	0.131	0.180
Wave 5	0.101**	0.012	0.077	0.125	0.101**	0.012	0.077	0.126
wave 6	0.057**	0.012	0.033	0.081	0.057**	0.012	0.033	0.081
wave 7 (reference)	0.000				0.000			

Note: \*\* significant at p-value < 0.001. <sup>a</sup> Scaled at increments of \$1,000. <sup>b</sup> Derived from gross total annual family income, minus the FTC amount, equalised for family composition and scaled at increments of \$1,000.

It is worth considering the statistical power and, therefore, potential for type II error in these analyses. Taking these latter analyses of FTC amount as an example, based on the observed effect estimate of -0.001 and the standard error of 0.002 reported in **Table 62**, a \$1,000 increase in FTC amount would have been statistically significant, if it increased SRH by around 0.025 scores. Since any meaningful effect of FTC amount on SRH would be at least a 2.7% change of one standard deviation in SRH, it is argued that the risk of type II error in these analyses was low.

## The effect of In-Work Tax Credit on self-rated health

### In-Work Tax Credit eligibility

The results of the crude (Model 1) and fully adjusted (Model 2) fixed effects models with IWTC eligibility as the exposure variable and SRH as the outcome variable, analysing the N = 6,900 participants of the study population over seven waves (Waves 1 to 7) is presented in **Table 63**. The crude model was based on 38,260 observations, while the fully adjusted model used 38,250 observations. Because all participants are set to ineligible for Waves 1-3 before the introduction of IWTC, the models in effect assess changes between Waves 4 and 7, including the initial and major transition of participants into FTC eligibility between Waves 4 and 5, described in *Chapters 2 and 6*. The crude analysis estimated the effect of becoming IWTC-eligible on change in SRH at the individual level over the short term (i.e., SRH lagged behind IWTC eligibility by one year), controlled for all time-invariant confounding. No discernible effect of becoming eligible for IWTC on SRH was found (estimate 0.005, 95% CI -0.019 to 0.028), as predicted in analyses presented in the previous chapter.

**Table 63: Crude (Model 1) and fully adjusted (Model 2) fixed effects model with eligibility for In-Work Tax Credit, time-varying covariates, outcome self-rated health, N=6,900 (38,260 observations, and 38,250 observations respectively), seven waves (Waves 1 to 7)**

Parameter	Model 1				Model 2			
	Estimate	Standard error	95% CI		Estimate	Standard error	95% CI	
IWTC amount <sup>a</sup>	0.005	0.012	-0.019	0.028	0.003	0.012	-0.021	0.028
Family income <sup>b</sup>					0.054	0.040	-0.024	0.132
Unemployed					0.078*	0.032	0.016	0.139
Inactive					0.083**	0.015	0.054	0.112
Employed (reference)					0.000			
Number of children					-0.001	0.007	-0.015	0.012
One-parent family					0.036	0.021	-0.006	0.078
Two-parent family (reference)					0.000			
Wave 1	0.257**	0.013	0.232	0.282	0.264**	0.013	0.238	0.290
Wave 2	0.255**	0.013	0.230	0.280	0.260**	0.013	0.234	0.285
Wave 3	0.141**	0.013	0.116	0.166	0.143**	0.013	0.117	0.169
wave 4	0.155**	0.012	0.131	0.179	0.156**	0.012	0.132	0.181
Wave 5	0.100**	0.012	0.076	0.124	0.102**	0.012	0.078	0.126
wave 6	0.056**	0.012	0.032	0.080	0.057**	0.012	0.033	0.081
wave 7 (reference)	0.000				0.000			

## Chapter 8: Fixed effects regression analyses

Note: \*\* Significant at p-value < 0.001. \* Significant at p-value < 0.05. <sup>b</sup> Derived from gross total annual family income, minus the IWTC amount, equivalised for family composition and scaled at increments of \$1,000. The type III p-value for employment status was < 0.001, with the p-value for unemployment being < 0.05 and for inactive < 0.001.

The fully adjusted fixed effects model also found that becoming eligible for IWTC did not have any discernible effect on change in SRH (estimate 0.003, 95% CI -0.021 to 0.028). The crude and fully adjusted estimates were comparable, suggesting no confounding by one or more of the measured time-varying confounding variables. The previous chapter found that change in employment status was associated with both change in IWTC eligibility and SRH, which suggested that employment status may confound the IWTC-SRH relationship. No evidence for such confounding by employment status was observed in the fixed effects models, possibly due to the relatively small amount of change of employment status. However, a statistically significant effect on SRH was observed for employment status (type III p-value < 0.001), where moving from being unemployed to employed moderately increased SRH by 0.078 scores (95% CI 0.016 to 0.139, p-value < 0.05) and moving from inactive to employed also moderately increased SRH by 0.083 scores (95% CI 0.054 to 0.112, p-value < 0.001). This small, positive and statistically significant association was also found in the previous chapter and in a previous study of SoFIE data [62, 63]. In this analysis, when potential confounding variables were added step-wise (model building), standard errors also remained stable, suggesting little risk of bias from collinearity.

### **In-Work Tax Credit amount**

In both the crude and fully adjusted linear fixed effects analyses, the amount of IWTC that family was eligible for also did not have any discernible effect on SRH (**Table 64**), as predicted in the previous chapter. That the fully adjusted did not differ from the crude effect estimate suggested that the potential time-varying confounding variables adjusted for in this model did not confound the IWTC-SRH relationship. Again, while the association between change in IWTC amount and change in SRH provided in the previous chapter suggested some potential for confounding of the fixed effects analyses by employment status, this was not observed in the fixed effects analytic results, possibly due to relatively small amount of change in employment status.

**Table 64: Crude (Model 1) and fully adjusted (Model 2) fixed effects model with In-Work Tax Credit amount, time-varying covariates, outcome self-rated health, N=6,900 (38,260 observations, and 38,250 observations respectively), seven waves (Waves 1 to 7)**

Parameter	Model 1				Model 2			
	Estimate	Standard error	95% CI		Estimate	Standard error	95% CI	
IWTC amount <sup>a</sup>	0.000	0.004	-0.008	0.008	0.000	0.004	-0.008	0.008
Family income <sup>b</sup>					0.053	0.040	-0.025	0.131
Unemployed					0.077*	0.032	0.014	0.140
Inactive					0.083**	0.015	0.054	0.112
Employed (Reference)					0.000		0.000	0.000
Number of children					-0.001	0.007	-0.015	0.013
One-parent family					0.036	0.021	-0.005	0.077
Two-parent family (reference)					0.000		0.000	0.000
Wave 1	0.256**	0.013	0.231	0.281	0.263**	0.013	0.238	0.288
Wave 2	0.254**	0.013	0.229	0.279	0.258**	0.013	0.233	0.283
Wave 3	0.139**	0.013	0.114	0.164	0.142**	0.013	0.117	0.167
wave 4	0.155**	0.012	0.131	0.179	0.156**	0.012	0.132	0.180
Wave 5	0.101**	0.012	0.077	0.125	0.102**	0.012	0.078	0.126
wave 6	0.057**	0.012	0.033	0.080	0.057**	0.012	0.033	0.081
wave 7 (reference)	0.00				0.000			

Note: \*\* Significant at p-value < 0.001. \* Significant at p-value < 0.05. <sup>b</sup> Derived from gross total annual family income, minus the IWTC amount, equalised for family composition and scaled at increments of \$1,000. The type III p-value for employment status was < 0.001, with the p-value for unemployment being < 0.05 and for inactive < 0.001.

## Subsidiary analyses

Two subsidiary questions, hypotheses and analyses are presented in **Table 65**.

**Table 65: Type of other issue, hypothesis and subsidiary analyses**

Subsidiary research question	Hypothesis	Analysis
Was there a different effect of FTC and IWTC on SRH, when SRH lagged behind the exposure variables by longer time periods?	The effect of FTC and IWTC on SRH may not have been immediate (one year lag period between a change in IWTC and a change in SRH). Instead it may have been stronger, when its effect on SRH was measured over a short to medium term (where the effect of a change in IWTC is assessed on a change in SRH two to six years later).	The main fixed effects analyses were repeated with change in the SRH outcome variable lagging behind change in the FTC and IWTC exposure variables by two to six years.
Did FTC and IWTC have an effect on two other relevant health outcomes, psychological distress and tobacco smoking?	FTC and IWTC may increase, decrease or have no effect on psychological distress and tobacco use.	Fixed effects regression analyses were conducted fixed effects with psychological distress and tobacco use as the outcomes.

### Longer-term effects

The main fixed effects regression analyses of this thesis estimated the effect of FTC and IWTC on SRH *over the short term*, namely the effect of change in the FTC and IWTC exposure variables at time<sub>t</sub> on change in SRH at time<sub>t+1</sub> or, in other words, the effect of change in FTC and IWTC on change in SRH in the following year, finding no discernible health effects of these credits over this short period. It is theoretically plausible that change in FTC and IWTC interventions did not have any short-term effect on SRH (one year lag), but that they had an effect on SRH over a longer period, meaning change in the FTC and IWTC was associated with change in SRH after a longer time period. Longer-term effects of financial credits on health in adults have not commonly been studied previously. As described in *Chapter 4*, in previous studies of in-work tax credits, the change in the outcome lagged behind the implementation of the tax credit change by one to eight years.

The fully adjusted linear fixed effects models with FTC and IWTC eligibility and amount as the exposure variables and SRH as the outcome variable, which analysed the N = 6,900 participants from seven waves of the SoFIE (Waves 1 to 7), are presented in **Table 66**. In these subsidiary analyses, change in the outcome variable (SRH) lagged behind change in the exposure variable (FTC or IWTC eligibility or amount) by two, three, four, five and six years for FTC. Because the SoFIE only covered four years of data after IWTC had been introduced (i.e., 2006 to 2009, Waves 4 to 7), change in the outcome could only lag behind change in the exposure by two and three years for subsidiary analyses on IWTC.

At each wave, one dummy variable each was derived for the SRH outcome two, three, four, five and six years after the wave, and the main fixed effects analyses re-run with each of the new dummy variables for SRH (lagged behind the exposure variable) as the outcome. These subsidiary analyses show, for each of the exposures of both tax credits, no effect of the exposure variables on the outcome, when change in the outcome was lagged behind change in the exposure variables by these longer periods. The only exception was that change in FTC eligibility was estimated to have a small, negative and statistically significant (p-value < 0.001) effect on change in SRH five years later. However, no effect was found for the FTC, when other lag times were applied, and the FTC amount did not have a statistically significant effect on SRH lagged behind

FTC by five years suggesting that the statistical significance of this effect may be due to type 1 error.

**Table 66: Fully adjusted linear fixed effects models with Family Tax Credit and In-Work Tax Credit eligibility and amount, outcome self-rated health, N=6,900, seven waves (Waves 1 to 7)**

Model	Estimate	Standard error	95% CI	
FTC eligibility				
1 year lag (main analysis)	0.013	0.012	-0.011	0.037
2 year lag	0.011	0.014	-0.015	0.038
3 year lag	-0.001	0.015	-0.031	0.029
4 year lag	-0.012	0.018	-0.048	0.023
5 year lag	-0.076	0.023	-0.122	-0.030
6 year lag	-0.009	0.034	-0.075	0.058
FTC-amount <sup>a</sup>				
1 year lag (main analysis)	-0.001	0.002	-0.006	0.004
2 year lag	-0.001	0.003	-0.007	0.005
3 year lag	-0.002	0.003	-0.009	0.005
4 year lag	-0.008	0.004	-0.017	0.001
5 year lag	-0.011	0.006	-0.024	0.001
6 year lag	-0.001	0.010	-0.021	0.019
IWTC eligibility				
1 year lag (main analysis)	0.003	0.012	-0.021	0.028
2 year lag	-0.005	0.014	-0.032	0.023
3 year lag	0.017	0.017	-0.015	0.050
IWTC-amount <sup>a</sup>				
1 year lag (main analysis)	0.000	0.004	-0.008	0.008
2 year lag	0.000	0.005	-0.010	0.009
3 year lag	-0.002	0.006	-0.013	0.009

<sup>a</sup> Scaled at increments of \$1,000.

## Associations with other health outcomes

Analyses of the effect of FTC and IWTC on other relevant health outcomes are also informative. The fully adjusted linear fixed effects regression analyses with FTC and IWTC eligibility and amount as the exposure variables and psychological distress (measured by K10) as the outcome variable, analysing the N = 6,900 participants over three waves (Wave 3, 5 and 7), are presented in **Table 67**. No discernible effect on psychological distress was found for either FTC or IWTC. Likewise, the fully adjusted logistic fixed effects estimates (odds ratios) for current tobacco smoking as the outcome variable found that change in none of the FTC and IWTC exposure variables had any discernible association with change in current smoking status (**Table 68**). Because these analyses used less data than the main analyses, namely three waves instead of seven waves, they were relatively less precise, as indicated by the relatively wide CIs.

**Table 67: Fully adjusted linear fixed effects analyses with Family Tax Credit and In-Work Tax Credit eligibility and amount, psychological distress as outcome, time-varying co-variates, N=6,900, three waves (Wave 3, 5 and 7)**

Exposure variable	Estimate	Standard error	95% CI	
FTC eligibility	-0.020	0.107	-0.230	0.190
FTC amount <sup>a</sup>	0.017	0.020	-0.022	0.056
IWTC eligibility	0.135	0.104	-0.069	0.339
IWTC amount <sup>a</sup>	0.056	0.046	-0.034	0.146

<sup>a</sup> Scaled at increments of \$1,000.

**Table 68: Fully adjusted logistic fixed effects estimates (odds ratio) with Family Tax Credit and In-Work Tax Credit eligibility and amount, current tobacco smoking as outcome, time-varying co-variates, N=6,900, three waves (Wave 3, 5 and 7)**

Exposure variable	Estimate	95% CI	
FTC eligibility	0.92	0.63	1.34
FTC amount <sup>a</sup>	1.02	0.95	1.09
IWTC eligibility	0.91	0.65	1.23
IWTC amount <sup>a</sup>	0.98	0.87	1.11

<sup>a</sup> Scaled at increments of \$1,000.

## Effect modification

Even if none of the exposures of both tax credits have any discernible effect on SRH in the group of *all* adults, it is nevertheless theoretically plausible that FTC and IWTC have an effect on SRH in *one (or more) subgroup(s)* (but not others) or that the effect is in opposite directions between subgroups (**Table 69**). Therefore, as long as there is an a priori case for potential heterogeneity of the associations across subgroups, it is warranted to conduct subgroup analyses.

**Table 69: Issue, research question, hypothesis and analysis**

Research question	Hypothesis	Analyses
Do FTC and IWTC have a differential effect on SRH in Māori participants, compared to non-Māori participants?	The FTC and IWTC have a positive effect in non-Māori, but a negative effect in Māori.	The main analyses were repeated with an interaction term, FTC x ethnicity and IWTC x ethnicity respectively, included. The main analyses were repeated on the Māori and non-Māori subsamples.
Do FTC and IWTC have a differential effect on SRH in low-income participants, compared to middle- and high income participants?	The FTC and IWTC have a positive effect in the low-income subgroup, but a negative effect in the middle and high-income subgroup.	The main analyses were repeated with an interaction term, FTC x income and IWTC x income respectively, included. The main analyses were repeated on the low-income subsample and medium- and high-income subsamples.

As outlined earlier in this thesis in *Chapter 3*, there is a rationale that the effect of FTC and IWTC may be modified by two variables, ethnicity and income. First, while a 2013 systematic review seeking to identify intervention-generating inequalities found no evidence that resource- and income-redistributive social interventions (such as FTC and IWTC) increased health inequalities [110], there is some evidence for heterogeneity by ethnicity [84, 85], reviewed in detail in *Chapter 3*. Due to the relatively larger sample size of non-Māori in the study sample, it is not statistically possible for there to be no effect of FTC and IWTC on SRH in Māori, when there is an effect in non-Māori. However, it is theoretically plausible that FTC and IWTC have a positive effect on non-Māori, but have a negative effect on Māori.

Second, Morris *et al.*'s theory of a minimum income for healthy living [4-8] introduced in *Chapter 2* hypothesizes that additional income should have an effect on those people whom it moves above the minimum threshold (low income subgroup), but should not have an effect on those people who already receive income above the minimum threshold (middle- and high-income subgroup). In other words, there may be effect modification of the effect of FTC and IWTC on SRH by low income versus middle and high income.

### ***Effect modification by Māori versus non-Māori ethnic group***

When the main fully adjusted fixed effects analyses with FTC and IWTC eligibility and amount as the exposure variables and SRH as the outcome variable were repeated with *FTC x ethnicity* or *IWTC x ethnicity* interaction terms include, no statistically significant interaction effect was found for any of these analyses, as shown in **Table 70**. Likewise, when the main fixed effects regression analyses were repeated, analysing the Māori subgroup and non-Māori subgroup separately over the seven waves (Waves 1 to 7), no evidence for effect modification by ethnicity was observed. Looking purely at the point estimates, one could conclude that the tax-credit-on-health effects are small in both Māori and non-Māori. However, for each of the exposure variables of both tax credits, CIs for Māori and for non-Māori overlap, indicating no evidence for any Māori / non-Māori differences in the tax-credit-on-health effects.

Furthermore, considering the null-finding estimated in the main analyses, the difference between Māori and non-Māori would need to be very large. So, for the example of the fully adjusted main analysis on FTC eligibility presented in **Formula 5**, the net effect of FTC eligibility on SRH,  $\beta(\text{total})$ , is the sum of the product of the effect size in the Māori sub-group,  $\beta(\text{Māori})$  multiplied by the

sample size of this subgroup,  $N(\text{Māori})$ , plus the effect size in the non-Māori subsample,  $N(\text{non-Māori})$ , multiplied by the sample size of this subgroup,  $N(\text{non-Māori})$ , divided by the total sample size,  $N(\text{total})$ . Therefore, as demonstrated in the **Formula 6** below, knowing that the observed net effect  $\beta(\text{total})$  was 0.013 and assuming an effect  $\beta(\text{Māori})$  of meaningful size of 0.400 in the  $N = 885$  Māori participants, then the effect  $\beta(\text{non-Māori})$  would have to be -0.044 in the  $N = 6,015$  non-Māori participants. It is argued that a difference in effect estimates of 0.444 SRH scores, equating to an 48.3% of one SD in SRH, is unlikely, further weakening the case for effect modification by ethnicity.

$$\beta(\text{total}) = \frac{\beta(\text{Māori}) \times N(\text{Māori}) + \beta(\text{non-Māori}) \times N(\text{non-Māori})}{N(\text{total})} \quad (5)$$

$$\beta(\text{non-Māori}) = \frac{\beta(\text{total}) \times N(\text{total}) - \beta(\text{Māori}) \times N(\text{Māori})}{N(\text{non-Māori})} = \frac{0.013 \times 6900 - 0.400 \times 885}{6015} = -0.044 \quad (6)$$

**Table 70: Fully adjusted linear fixed effects models with Family Tax Credit and In-Work Tax Credit eligibility and amount, outcome self-rated health, Māori subgroup (N = 885) and non-Māori subgroup (N = 6,015), N=6,900, seven waves (Waves 1 to 7)**

Exposure	Ethnic group	Estimate	Standard error	95% CI	
FTC eligibility	Māori	0.065	0.034	-0.003	0.132
	Non-Māori	0.003	0.013	-0.023	0.029
	p-value for interaction effect: 0.95				
FTC amount <sup>a</sup>	Māori	-0.003	0.007	-0.015	0.010
	Non-Māori	0.000	0.003	-0.005	0.005
	p-value for interaction effect: 0.50				
IWTC eligibility	Māori	0.057	0.035	-0.012	0.126
	Non-Māori	-0.004	0.013	-0.029	0.022
	p-value for interaction effect: 0.25				
IWTC amount <sup>a</sup>	Māori	0.013	0.012	-0.010	0.036
	Non-Māori	0.002	0.005	-0.007	0.011
	p-value for interaction effect: 0.14				

a Scaled at increments of \$1,000.

### ***Effect modification by income***

When the main analyses were repeated with interaction terms for income, no significant interaction effects were found, as shown in **Table 71**. Likewise, the analyses were repeated stratified by low income, measured as living in poverty (i.e., an equivalised gross total annual family income (without FTC or IWTC) below 50% of the median such family income) versus middle/high income (i.e., not living in poverty) at baseline (Wave1 for FTC, Wave 4 for IWTC). No discernible effect was found in these stratified analyses either.

**Table 71: Fully adjusted linear fixed effects models with Family Tax Credit and In-Work Tax Credit eligibility and amount, outcome self-rated health, stratified by low versus middle/high income group, N = 6,900, seven waves (Waves 1 to 7)**

Exposure	Ethnic group	Estimate	Standard error	95% CI	
FTC eligibility	Low income	0.031	0.020	-0.008	0.070
	Middle/high income	-0.001	0.016	-0.033	0.031
	p-value for interaction effect: 0.10				
FTC amount <sup>a</sup>	Low income	0.001	0.004	-0.006	0.008
	Middle/high income	-0.001	0.004	-0.008	0.006
	p-value for interaction effect: 0.83				
IWTC eligibility	Low income	0.013	0.024	-0.034	0.061
	Middle/high income	0.004	0.014	-0.025	0.032
	p-value for interaction effect: 0.78				
IWTC amount <sup>a</sup>	Low income	0.008	0.008	-0.007	0.023
	Middle/high income	0.002	0.005	-0.009	0.012
	p-value for interaction effect: 0.99				

<sup>a</sup> Scaled at increments of \$1,000. Low income was defined as being in poverty, defined as an equivalised gross total annual family income (without FTC or IWTC) below 50% of the median. For FTC analyses, the sample sizes were N = 2,630 for the low-income subgroup and N = 4,275 for the middle- and high-income subgroup. For IWTC, the sample sizes were N = 1,800 for the low-income subgroup and N = 5,100 for the middle-income and high-income subgroup.

## Conclusions

This chapter presents the findings from the main crude and fully adjusted fixed effects regression analyses assessing the effect of FTC and IWTC eligibility and amount on SRH in adults at the individual level over the short term (one year lag), as well as results from subsidiary analyses. The fully adjusted analyses controlled for any time in-variant confounding and adjusted for measured time-varying confounding.

The results find no evidence of an effect of change in FTC and IWTC eligibility or amount on change in SRH. The best estimate of change in SRH associated with becoming eligible for FTC was a 0.013 increase in score (1.4% of one SD in SRH), but with the adjusted 95% confidence interval (CI) including the null (-0.011 to 0.037). A \$1,000 increase in FTC amount also had no discernible association with change in SRH (estimate -0.001, adjusted 95% CI -0.006 to 0.004). This was consistent with no evidence of an association found in the cross-tabular categorical analyses presented in the previous chapter.

The best estimate of change in SRH associated with becoming eligible for IWTC was a 0.007 increase in score (0.8% of one SD in SRH), but with the adjusted 95% confidence interval including the null (-0.018 to 0.032). A \$1,000 increase in IWTC amount also had no discernible association with change in SRH (estimate 0.002, adjusted 95% CI -0.006 to 0.011). The estimate for IWTC

eligibility corresponded with no evidence for an association found in the cross-tabular categorical analyses presented in the previous chapter. However, the previous analyses had overestimated the association of change in IWTC amount with change in SRH, possibly due to not controlling for employment status, which, as predicted in the previous chapter, was shown to have a statistically significant effect on SRH.

Thus, using the observed standard error, an increase in FTC amount of \$1,000 would have required a change in SRH of around 0.025 scores for to be statistically significant. I argue that any meaningful effect of FTC amount on SRH would be at least a 2.7% change of one standard deviation in SRH, suggesting that type II error in these analyses was unlikely.

Subsidiary analyses found no discernible effect of the FTC and IWTC exposures on SRH over the longer term (when the association of change in SRH was studied on change in SRH two to six year lags) and on two other health outcomes, psychological distress and current tobacco smoking. No evidence for any effect modification by ethnicity or income was found, although these analyses may have had limited statistical power.

## Chapter 9: Internal validity and precision

This chapter assessed the internal validity of the thesis findings by investigating the risk of bias in and the precision of the main fixed effects regression analyses. Thesis findings were controlled for all time-invariant confounding variables and comprehensively adjusted for time-varying confounding variables. The strong control of confounding considerably strengthens the thesis. Little evidence for the presence of selection bias in this thesis was found. Moreover, the treatment effect in non-responders and participants lost to follow up would need to have been large to have considerably affected the findings, suggesting an overall low risk of selection bias.

Misclassification of the exposure variables presented the largest challenge for this thesis. Misclassification in the measures of income mobility may have suffered from independent or dependent differential or non-differential misclassification, of which neither the size nor the direction could confidently be determined. However, sensitivity analyses that removed participants with extreme income values or eliminated extreme income changes found no evidence for bias from mismeasurement from extreme values in the main fixed effects regression analyses. Mismeasurement of the outcome and the potential confounding variables was judged to pose a low risk of bias.

The outcome variable may have been misspecified as linear, when it should be treated as ordinal. However, sensitivity analyses found no evidence for misspecification bias of the outcome. No evidence of reverse causation was found.

In summary, the internal validity of this thesis was judged good overall, with low risks of bias from selection; misspecification of the outcome; and reverse causation. However, misclassification (or mismeasurement) of the exposure variables of an undeterminable size may have biased findings, likely towards a null finding. Non-differential mismeasurement of the outcome and confounding variables likely also attenuated the results towards a null finding (to an unknown extent). The fixed effects estimates of the main analyses were precise, but the subsidiary analyses may have suffered from random error due to smaller sample sizes.

This chapter assesses the internal validity (absence of confounding, selection and misclassification bias) and statistical precision of the thesis findings. First, the risk of selection bias is assessed. Then bias from misclassification of the exposure is studied. Because of the centrality of measures of income mobility used in this thesis and concerns for the income data, the econometric literature on misclassification in measures of income mobility is reviewed. The risk of bias from mismeasurement of the outcome and the confounding variables is then assessed. Finally, misspecification of the outcome variable is examined.

The overall quality of the evidence presented in an observational, epidemiological study depends on two factors. The first (and perhaps most important) factor is the extent of systematic bias (also called non-random error) in the study. Bias is defined as the “systematic deviation of results or inferences from truth” due to “an error in the conception and design of a study - or in the collection, analysis, interpretation, reporting, publication, or review of data” (p. 18) [1]. Prominent types of bias that are also studied in this chapter include selection and mismeasurement or misclassification biases. (For the remainder of this chapter, I use the term ‘classification’ to refer to categorical variables and ‘measurement’ to continuous variables.) Internal validity refers to the relative lack of bias [1]. The second factor that the overall quality of an observational, epidemiological study depends on is the degree of random error in its findings. Random error is “the portion of variation in measurement that has no apparent connection to any other measurement or variable, generally regarded as due to chance” (p. 85) [1]. Precision refers to “the relative lack of random error” (p. 251) [1] or, more simply, narrow confidence intervals.

Assessing the overall quality of a study therefore requires judging the study’s internal validity. This is done by identifying, describing, evaluating and quantifying the risk of each type of the potential bias in a study, including (if feasible) judging the potential size and direction of each bias, as well as the net effect of all bias. Assessing the overall quality of a study also requires describing, evaluating and quantifying the level of random error in the study in order to judge the precision of the study. Generally speaking, “internal validity must take precedence over precision”, although an estimate that is slightly biased, but highly precise, may at times be preferable to an estimate that is unbiased, yet highly imprecise (p. 251) [1].

Confounding is a central concern in most epidemiological studies. However, a major strength of this thesis is its strong control for confounding. The main fixed effects regression analyses were controlled for all time-invariant confounding variables, both measured variables (e.g., age, gender, ethnicity and education) and unmeasured variables (e.g., intelligence and biological predisposition to diseases). They were also adjusted for the four potential time-

varying confounding variables that determined the exposures, namely equivalised gross total annual family income, employment status, number of dependent children and family type. The fixed effects regression analyses of this thesis comprehensively adjusted for all variables determining the exposure variables. Therefore, I argue there were no unknown time-varying confounding variables. The only exception was potential for residual confounding due to misclassification of the variables that determined the exposure variables. In summary, the risk of bias from confounding in this thesis is judged low.

## **Internal validity**

A sensitivity analysis is “a method to determine the robustness of an assessment by examining the extent to which results are affected by changes in methods, models, values of unmeasured variables or assumptions” (p. 226) [1]. The purpose of sensitivity analyses is “to identify results that are most dependent on questionable or unsupported assumptions” (p. 226) [1]. There is increasing recognition that sensitivity analyses should be conducted to test for the risk of potential bias and, if feasible, to empirically estimate the amount of bias in a study. The type and hypothesised effect, as well as a short description of sensitivity analyses conducted to quantify the risk of each potential source of potential non-random error are presented in **Table 72**. The main fixed effects analyses of this thesis may have principally been affected by selection bias; misclassification bias of the exposure, outcome or confounding variables; and misspecification bias of the outcome. The risk of bias of these biases is investigated over the following sections.

### **Selection bias**

#### ***Theoretical background***

Selection bias occurs when the effect of the exposure variable on the outcome variable is different in the group of participants who were selected into the analysis than in the wider group of persons who were eligible for inclusion in the analysis [1, 208]. In other words, selection bias refers to “systematic differences due to selective loss of subjects” (p. 226) [1]. Selection bias is an umbrella term for a range of different biases that share an underlying causal structure [208]. This underlying structure is “conditioning on a common effect of two variables, one of which is either exposure or a cause of exposure and the other is either the outcome or a cause of the outcome” (p. 615) [208]. A common direct effect of two or more variables is called a collider, and conditioning on the collider may introduce a spurious

**Table 72: Type of bias, hypothesis and sensitivity analysis**

Type of bias	Hypothesis	Sensitivity analysis
Selection bias due to initial survey non-response (sampling bias) and loss to follow-up from the survey (attrition bias)	Persons who did not agree to participate in the survey or were lost to follow up have a different association of change in FTC and IWTC eligibility and amount with SRH than those who agreed to participate or remained in the survey.	<p>The main analyses were repeated on the unbalanced panel – partial test only.</p> <p>The main fixed effects regression analyses were repeated, using longitudinal weights to adjust for attrition by geographic residency, sex and age – partial test only.</p> <p>The hypothetical size of the association of change in FTC and IWTC eligibility and amount with change in SRH in participants who non-responded to or were lost to follow up from the survey that would be required to considerably alter the results of the main analyses was calculated.</p>
Misclassification bias of the exposure	Measurement error in the variables from which the exposure variables were derived, especially income, may have introduced misclassification bias.	<p>The main fixed effects regression analyses were repeated with observations of participants reporting extreme (1% lowest and 1% largest) gross total annual family income values removed.</p> <p>The main fixed effects regression analyses were repeated with extreme <i>changes</i> in gross total annual family income (the 1% largest decreases and 1% largest increases) removed.</p>
Misclassification bias of the outcome	Measurement error in the outcome variable may have introduced misclassification bias.	No additional analyses were conducted (but see simulations below).
Misclassification bias of confounding variables	Measurement error in the only variable found to confound the exposure-outcome relationship, employment status, was considered low and to not introduce considerable measurement error in the fixed effects regression analyses.	No additional analyses were conducted (but see simulations below).
Misclassification bias (all types)	Measurement error, mainly in the exposure variable, but also in the outcome and confounding variables may have introduced misclassification bias in the main fixed effects	Different scenarios of measurement error were simulated to get a sense of the extent of measurement error that would be required to considerably change the effect estimates of the main fixed

## Chapter 9: Internal validity and precision

regression analyses.

effects regression analyses.

---

Misspecification bias of the outcome	Treating the SRH outcome, a categorical (ordinal) variable, as a linear variable in the main fixed effects analyses may have biased the effect estimate towards or away from a null-effect.	Hybrid (fixed effects) proportional odds regression analyses were conducted that treated the SRH outcome as ordinal.
Reverse causation	The association of change in SRH with change in FTC and IWTC eligibility and amount (the health selection pathway) is weak.	Repeated the main fixed effects analyses in a sample restricted to participants with good, very good and excellent SRH at Wave 1 baseline – partial test only.

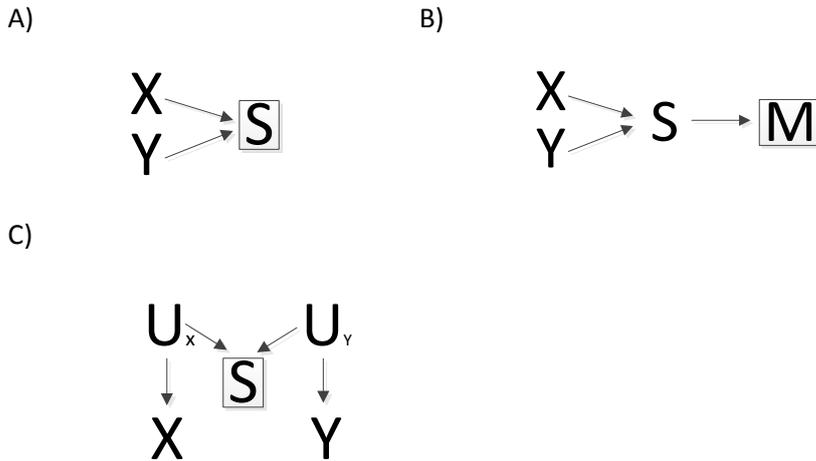
---

association between the two variables, which is referred to as collider bias [1]. Thus, if the exposure and the outcome variable have a common effect, then conditioning on the collider introduces collider bias between the exposure and the outcome [1, 208].

The underlying unifying structure of selection bias has recently been described extensively [208], building on and modifying earlier epidemiological conceptualizations (e.g. [236]). This may explain why different cases of selection bias have been differentiated in the past and the same cases have sometimes been referred to with different terms, rather than simply referring to all of these cases as selection bias. For example, selection bias due to initial survey non-response has been referred to as non-response bias, sample bias or sample selection bias [1]. Selection bias from loss to follow-up has been called attrition bias or follow-up bias [1]. A comprehensive review of the diverse cases of selection bias is beyond the scope of this thesis, but provided elsewhere [208].

The directed graph in Part A of **Figure 24** presents two common scenarios where selection,  $S$ , is a common cause of the exposure variable,  $X$ , and the outcome variable,  $Y$  [208]. Part B of the same figure presents a related scenario, where the exposure variable  $X$  and the outcome variable  $Y$  are common causes of selection  $S$ , and  $S$  is a cause of  $M$ . So, conditioning on  $M$ , the cause of selection, may also introduce selection bias [208]. Thus, conditioning on either selection  $S$  or its effect  $M$  introduces selection bias. In this scenario, selection is a collider. Adjusting for selection  $S$  or its effect  $M$  introduces collider bias on the  $X$ - $S$ - $Y$  pathway. Shown in Part C of **Figure 24** is a special case of selection bias called  $M$  bias. In this scenario,  $U_x$  is a common cause of the exposure variable  $X$  and selection  $S$ , and  $U_y$  is a common cause of both the outcome variable, resulting in an  $M$ -like causal structure [237]. Again, conditioning on selection  $S$ , a collider, creates selection bias [237].

These directed acyclic graphs are fairly intuitive for ‘one off’ measures of exposure and outcome variables in, say, a basic cohort study. However, this thesis has tackled panel data with repeated measures. It is possible to draw directed acyclic graphs with repeated measures data subscripted by time (e.g., see the analytic frameworks of this thesis presented in **Figure 19 and Figure 20** in *Chapter 5*). A useful shortcut, though, is to reconceptualise the exposure ( $X$ ) and outcome ( $Y$ ) variables as *change* between adjacent waves. Selection bias must then arise because both change in the exposure variable and change in the outcome variable dependently effect initial survey response and loss to follow up (i.e. Part A in **Figure 24**), and correspondingly more complex restatements for Parts B and C of the figure.

**Figure 24: Directed acyclic graphs of selection bias**

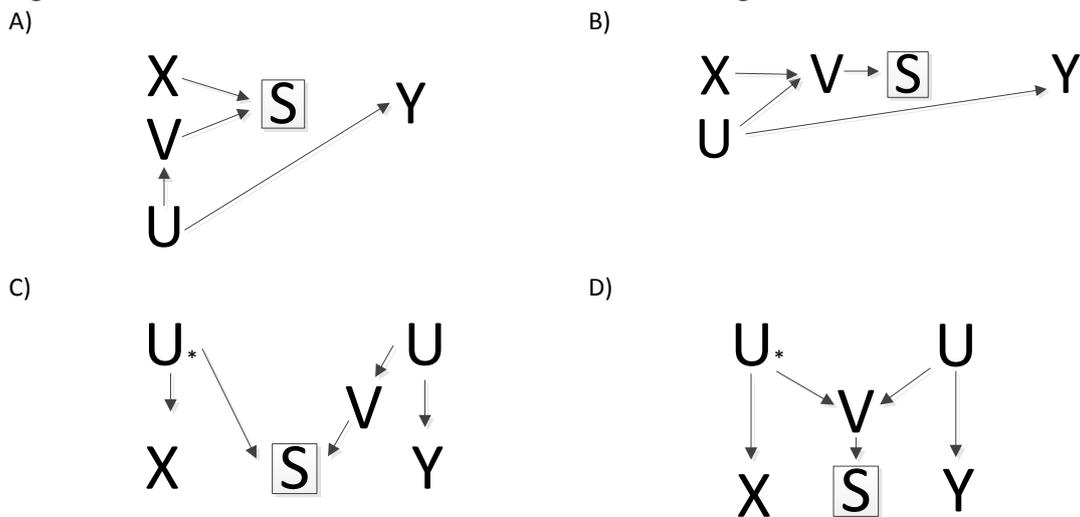
Source: Part A and B are adapted from Hernan *et al.*, 2004, p. 616 [208] and Part C from Liu *et al.*, 2012, p. 939 [237].

Selection bias with the first two (modified) causal structures described above and presented in Part A and B of **Figure 24** could potentially have occurred in this thesis. But I believe the risk of M bias in the main fully adjusted fixed effects regression analyses is low. The reason is that these analyses comprehensively controlled causes of change in IWTC and FTC, as demonstrated in *Chapter 5*. These analyses controlled for the variables determining FTC and IWTC eligibility and amount, as these were considered to be potential time-varying confounding variables, blocking the backdoor path in M-bias opened up by conditioning on 'S'. Although residual confounding could have occurred if the potential confounding variables were measured with error, the majority of the effects of these variables on the exposure should have been adjusted for. Therefore, M bias, if any, should have been adjusted for, even though a residual effect could have occurred due to factors such as errors in the measurement of the common causes of the exposure variable and selection.

Hernan, Hernandez-Diaz and Robins describe four specific scenarios of selection bias in cohort studies [208]. While these scenarios were specifically developed to explain selection bias from differential loss to follow-up, they can be used to explain any missing data, including initial survey non-responses [208]. Directed acyclic graphs describing these different scenarios are presented in **Figure 25**. Part A of the figure shows the scenario where selection (S) is a common effect of the exposure variable (X) and also associated with the outcome variable (Y), through the Y-U-V pathway. U refers to an unmeasured variable that caused both the outcome and a variable V, which could also be either measured or unmeasured. An example is now provided, where this scenario of selection bias is applied to the analysis of the impact of

change in FTC eligibility on change in SRH studied in this thesis. Selection bias could occur if participation or retention in the study (*S* in Part A of **Figure 25**) was caused by both a change in FTC eligibility, the exposure variable (*X*), and a change in the unmeasured variable (*U*), which also caused a change in SRH, the outcome variable (*Y*). This common cause of selection (*S*) and the outcome variable (*Y*) is *U*, usefully conceptualized as the disease, with the ‘symptom’ (*V*). So, if the participant became clinically depressed (*U*), and this caused both deterioration in self-rated health (*Y*) and a drop in motivation to participate in the survey (*V*), which, in turn, resulted in her leaving the survey, then this could have introduced selection bias. If the true average treatment effect of becoming FTC-eligible was an increase in self-rated health, then this selection bias would spuriously increase the size of the observed positive effect, because participants with deteriorating SRH were disproportionately lost to follow up. Nevertheless, many steps are required to achieve such a selection bias, and as shown by Greenland all strengths of association along these paths must be strong for substantive selection bias to arise [118].

**Figure 25: Hernan 2004 classification of selection bias in longitudinal studies**



Source: Adapted from Hernan, Hernandez-Diaz and Robins., 2004, p. 617 [208]

Part B of **Figure 25** is a variation of Part A, where the exposure (*X*) is a cause of selection (*S*) and the outcome variable (*Y*) is also associated with selection through the disease (*U*) and symptom (*V*). So, to stick with the previous example, if becoming FTC-eligible reduced a participant’s motivation to remain in the study and a deterioration in SRH was due to the participant becoming depressed, which also resulted in reduced motivation to remain in the study, then a spurious association between becoming FTC-eligible and change in SRH would be observed. Assuming this time that the true average treatment effect of becoming FTC-eligible

was a decrease in SRH, then the introduced selection bias would reduce the size of the observed effect due to participants with deteriorating SRH selecting out of the study.

The final two scenarios of selection bias presented in Parts C and D are cases of M bias. In Part C of **Figure 25**, the exposure does not itself cause selection, but has a common cause,  $U^*$ , such as personality, lifestyle or educational variables, which affects both the exposure variable,  $X$ , and participation in the study,  $S$ . It is hard to imagine an example for such a scenario for FTC eligibility, but an example can be constructed for receipt of FTC. If a participant developed a more negative attitude towards the government ( $U^*$ ), which caused her to stop claiming FTC ( $X$ ), reduced her willingness to continue participation in the official survey ( $S$ ) and reduced her self-rated health ( $X$ ), for instance because she had less income available, then this would introduce selection bias. Again, though, several steps of tenuous associations are required in this argument; substantial selection bias seems unlikely.

Finally, Part D presents a scenario of selection bias, where the symptom  $V$  causes selection  $S$  and is a common effect of both the unmeasured variables  $U^*$  (associated with the exposure variable  $X$ ) and  $U$  (associated with the outcome variable  $Y$ ). For example, if a drop in the motivation to participate in a study was caused by both a more negative attitude towards the government ( $U^*$ ), which, in turn, caused a change in FTC receipt ( $X$ ), and a change in depression ( $U$ ), which also changed SRH ( $X$ ), then this would introduce such selection bias.

### ***Assessment of risk of selection bias in this thesis***

This discussion of the risk of bias from selection bias in this thesis focuses on two types of selection bias: bias due to initial survey non-response, followed by selection bias from loss to follow up. A common method for assessing the risk of selection bias due to initial survey non-response is to collect information on the exposure, outcome and relevant variables such as potential confounding variables from or about non-responders. However, such information provides hints only about any selection bias; it is not possible to estimate the effect of the exposure on the outcome in the group of non-participants without additional data from another source. Rather, and rarely explicitly stated by researchers, an assessment of the covariate patterns of participants and non-participants allows some assessment of 'm-Bias' potential; if a potential common cause of selection and exposure does not differ between participants and non-participants, then there is no bias through this covariates when conditioning on 'S'. If the researcher is lucky enough to have some data on the exposure-outcome association among the non-participants (e.g. some validation sub-sample), then they can then compare the effect estimate in the group of participants with the effect estimate of

all persons who were eligible to participate in the study. The difference, if any, between the two effect estimates is an estimate of the extent of selection bias due to non-participation in the study. Studies, however, generally do not collect any information on non-participants. The SoFIE study also did not collect any information from or about non-participants. Therefore, this does not present a feasible approach to investigate the risk of selection bias due to initial survey non-participation in this thesis.

This discussion now turns to the risk of selection bias from loss to follow-up. The study sample of this thesis was a balanced panel of adults in one- or two-parent families over two or more successive waves. As demonstrated above for selection bias due to initial survey non-response, the percentage of participants who were lost to follow up does not provide an indication of the direction or size of the net effect of an exposure variable. (Although, of course, a very low loss to follow-up means selection bias is impossible, compared to a high loss to follow-up meaning selection bias is *possible*.) The restriction of the study sample to a balanced panel means that those participants who were lost to follow-up were excluded from the study sample. Therefore, if the treatment effect in participants who were lost to follow up differed from the treatment effect in those who remained in the survey for all seven waves (the balanced panel), then the study may be affected by selection bias from loss to follow-up. It is worth noting that studying a balanced panel carries a higher risk of selection bias due to attrition than studying an unbalanced panel. The reason is that a study of an unbalanced panel uses information from participants up until when they drop out of the survey, rather than excluding all their information. In this study, when the main fixed effects regression analyses were repeated on the unbalanced panel, comparable results were found for all analyses (**Table 73**). This suggests that restriction of the survey sample to a balanced panel did not introduce selection bias in this thesis. Previous studies of the SoFIE have also found comparable effects of change in income on change in SRH in balanced and unbalanced panels of the SoFIE [63], as well as comparable associations of employment status and education with SRH in participants who were and those who were not to follow-up [238].

Weighting the balanced panel to the general population may provide some insights into the level of selection bias, assuming that this weighting produces a study sample with the characteristics of all participants who would have been eligible to participate in the survey and that selection bias arises due to M-bias variants whereby the common causes of exposure/outcome and selection can be included in the weighting algorithm. (Selection that is directly determined by either exposure or outcome cannot be 'adjusted for'.) The study sample of this thesis was weighted longitudinally for gender and age, using weights created by

Statistics New Zealand. The main fixed effects regression analyses were the re-run on the weighted population. These sensitive analyses presented in **Table 74** found comparable effect estimates in the unweighted and the weighted samples for all analyses. However, these analyses are very limited, since age and gender are time-invariant variables that would not be expected to have an effect on the effect between change in FTC, a time-varying variable, and change in SRH, a time-varying outcome, in a fixed effects regression analysis. Even if the weights would be extended to weight the sample for other more relevant variables, as demonstrated in the examples for attrition bias in **Figure 25**, factors that cause selection bias are often unmeasured. In the main example provided above, the participant developed a clinical depression and, as a symptom, reduced her motivation to participate in the survey. Both of these factors were not measured in the SoFIE, so could not be weighted for. Therefore, weighting approaches, while potentially providing some relevant information, are limited in their ability to assess the extent of selection bias.

**Table 73: Fully adjusted fixed effects regression model, with Family Tax Credit and In-Work Tax Credit eligibility and amount, outcome self-rated health, balanced panel (N = 6,900) and unbalanced panel (N = 9,360), seven waves (Waves 1 to 7)**

Exposure	Balanced panel			Unbalanced panel		
	Estimate	Standard error	95% CI	Estimate	Standard error	95% CI
FTC eligibility	0.013	0.012	(-0.011, 0.037)	0.009	0.012	(-0.015, 0.033)
FTC amount <sup>a</sup>	-0.001	0.002	(-0.006, 0.004)	0.000	0.000	(0.000, 0.000)
IWTC eligibility	0.003	0.012	(-0.021, 0.028)	0.005	0.013	(-0.020, 0.030)
IWTC amount <sup>a</sup>	0.000	0.004	(-0.008, 0.008)	0.000	0.000	(0.000, 0.000)

<sup>a</sup> scaled at \$1,000.

**Table 74: Fully adjusted fixed effects regression model, with Family Tax Credit and In-Work Tax Credit eligibility and amount, outcome self-rated health, weighted longitudinally for gender and age, N = 6,900, seven waves (Waves 1 to 7)**

Exposure	Unweighted sample			Weighted sample		
	Estimate	Standard error	95% CI	Estimate	Standard error	95% CI
FTC eligibility	0.013	0.012	(-0.011, 0.037)	0.015	0.012	(-0.009, 0.039)
FTC amount <sup>a</sup>	-0.001	0.002	(-0.006, 0.004)	-0.003	0.002	(-0.007, 0.002)
IWTC eligibility	0.003	0.012	(-0.021, 0.028)	0.003	0.012	(-0.021, 0.027)
IWTC amount <sup>a</sup>	0.000	0.004	(-0.008, 0.008)	0.002	0.004	(-0.006, 0.010)

<sup>a</sup> scaled at \$1,000.

Another approach is to look at previous studies that have estimated the extent of selection bias in studies that are comparable to this thesis. One relevant such assessment of the extent

of selection bias is Carter *et al.*'s [238]. The researchers estimated the extent of selection bias due to attrition in regression analyses that investigated the effect of employment status and education on SRH in the SoFIE [238]. They found comparable associations of employment status and education with SRH in participants who were and were not lost to follow up [238]. These findings may be seen as strengthening the argument for a limited risk of selection due to loss to follow up in this thesis.

Perhaps the most informative approach – even though it is ‘speculative’ – to determining the potential effect of selection bias in this thesis is to calculate the hypothetical size of the treatment effect in participants who were not selected into the study that would be required to considerably alter the results of an analysis. Such a calculation would provide an understanding of the net extent of selection bias from both initial survey non-response and loss to follow-up that would be required to meaningfully bias the findings of this thesis.

The goal is to calculate the hypothetical size of the treatment effect in eligible persons not selected into the study (non-responders and persons lost to follow up) that would considerably alter the findings of this study. The example of the effect of becoming FTC-eligible on change in SRH is further pursued. **Formula 5** shows that the total treatment effect ( $\beta_{total}$ ) is the product of the treatment effect in participants followed over all waves ( $\beta_a$ ) multiplied by the response rate ( $r$ ) plus the treatment effect in persons not selected into the study ( $\beta_b$ ) multiplied with the total survey non-response rate ( $1-r$ ). **Formula 5** can be rewritten to **Formula 6** for calculating the hypothetical effect  $\beta_a$  that would considerably change the findings of the thesis. A meaningful effect of becoming FTC-eligible ( $\beta_{total}$ ) was assumed to be a 0.200 increase in SRH scores, which is an increase by 22% of one standard deviation of SRH. Combining 71.0% of initial survey response [211] and the 72.7% of non-attrition calculated above gives a response rate ( $r$ ) of 51.6% for this study. The treatment effect in participants ( $\beta_b$ ) was observed as a 0.013 increase in SRH scores. Persons who did not initially respond to the survey or were lost to follow up must hypothetically have had increases their SRH by 0.399 scores (or by 44.3% of one standard deviation of SRH) after becoming FTC-eligible in order to mask a considerable average treatment effect of an improvement in SRH of 0.200 scores<sup>12</sup>. This hypothesized treatment effect in persons not selected into the survey was very large – about thirty times larger than that observed. It seems unlikely that the treatment effect would be so much different among the non-participants, especially in light of the other considerations so far in this chapter that also suggest substantial selection bias is unlikely.

---

<sup>12</sup> $\beta_b = \frac{0.200 - 0.516 * 0.013}{1 - 0.516} = 0.399$

$$\beta_{total} = \beta a * r + \beta b * (1 - r) \quad (5)$$

$$\beta b = \frac{\beta_{total} - \beta a * r}{1 - r} \quad (6)$$

In summary, this section sought to assess the risk of bias from selection bias in this thesis, focusing on such bias due to initial survey non-response and loss to follow up. Repeating the fixed effects regression analyses of the balanced panel on the unbalanced panel provided comparable findings. Weighting the balanced panel to the general population by age and gender and repeating the main analyses also returned results that were comparable to those of the main analyses. One previous study has found no evidence in regression analyses for selection bias from loss to follow up in the SoFIE [238]. Finally, a very much larger effect would have to have occurred in non-responders and participants lost to follow up to ‘shift’ the observed null association in this thesis to an association of ‘noteworthy’ magnitude. In conclusion, taken together, this evidence is cautiously interpreted as suggestive of a small risk of selection bias in this thesis.

## **Misclassification bias and mismeasurement error**

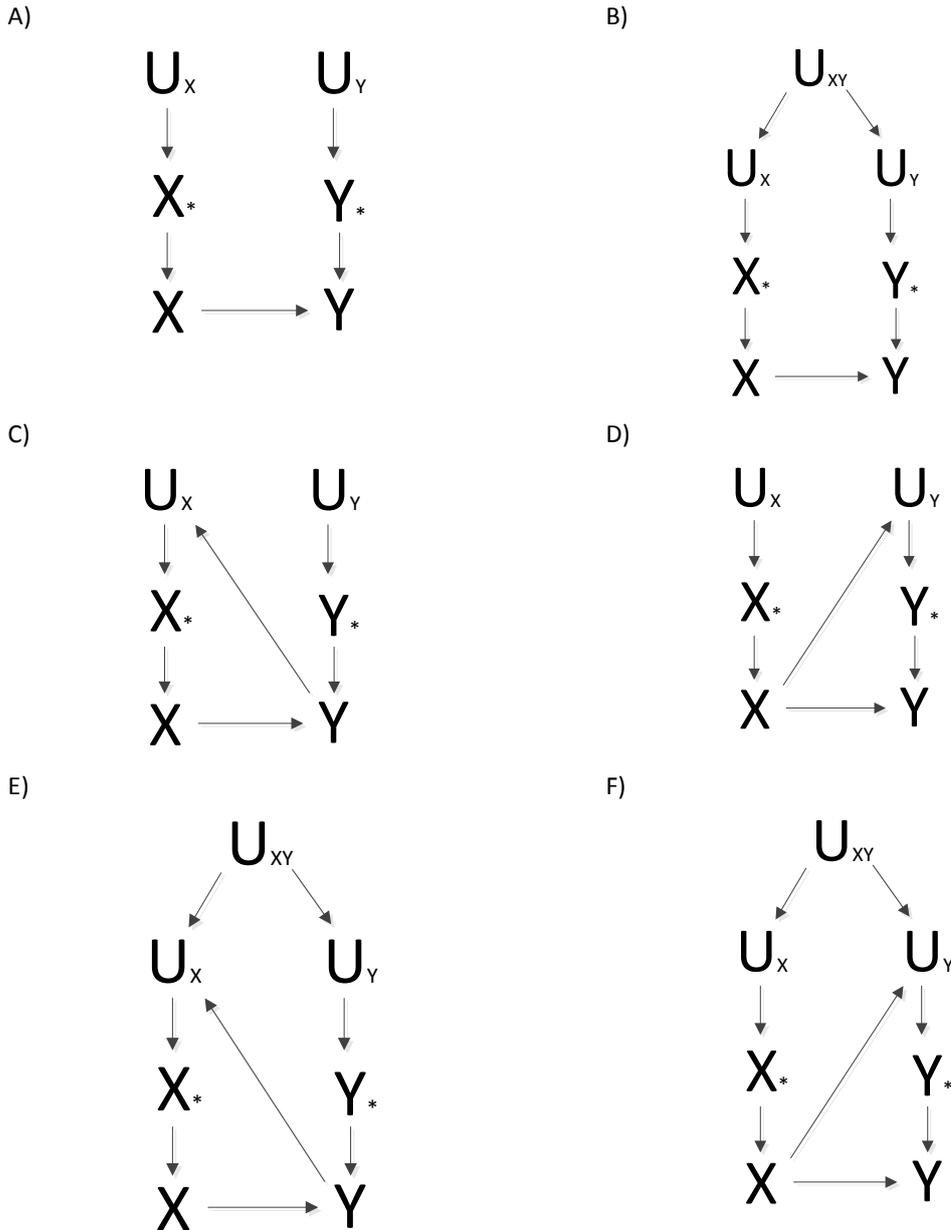
### ***Theoretical background***

Misclassification bias and measurement error is defined as “the erroneous classification of an individual, a value or an attribute into a category other than that to which it should be assigned” (p. 157) [1]. Consider the case of misclassification. Misclassification of the exposure, outcome or other variables included in statistical models can bias an estimate of the casual effect of the exposure on the outcome variable [1, 239]. Assume that we are interested in estimating the effect of an exposure variable  $X$  on an outcome variable  $Y$ , as shown in Part A in Figure 26. The exposure variable  $X$  is not directly observable, but a proxy measure of the exposure variable  $X^*$  can be obtained. The measure  $X^*$  is (strongly) associated with the exposure variable  $X$ , but may capture the exposure variable  $X$  with some inaccuracy, causing misclassification. Because the association between  $X$  and  $Y$  cannot be directly estimated due to  $X$  being unobservable, a study can only estimate the effect of the measure  $X^*$  on  $Y$ . If the exposure variable  $X$  is not fully captured by the measure  $X^*$ , then there is one measurement error process  $U_X$ . The estimate of the effect of  $X^*$  on  $Y$  may be a biased estimate of the effect of  $X$  on  $Y$ . This bias is referred to as misclassification bias. Similarly, the outcome variable  $Y$  may be assessed with measure  $Y^*$ , which could also suffer from misclassification or measurement error  $U_Y$ . This would indicate misclassification bias or mismeasurement error of the outcome.

In the epidemiological literature, four main types of misclassification bias are delineated: independent nondifferential; dependent nondifferential; independent differential; and dependent differential [239]. Hernan and Cole's 2009 structural classification of measurement error presented in **Figure 26** shows directed acyclic graphs for each of these misclassification biases [239]. Part A of **Figure 26** displays a scenario where the misclassification or measurement error process  $U_x$  of the exposure variable  $X$  and the misclassification or measurement error  $U_y$  of the outcome variable  $Y$  do not have a prior common cause and are thus independent of each other. Second, there is no arrow (or other pathway) from  $Y$  to  $U_x$ , meaning that misclassification or measurement error of the exposure is not differential by level of the outcome (and vice versa for  $X$  to  $U_y$ ). Thus Part A also depicts both independent and non-differential misclassification bias or measurement error. Examples of the types of misclassification bias and measurement error tailored to this thesis are provided shortly below. Part B of **Figure 26** shows the case where the misclassification or measurement errors  $U_x$  and  $U_y$  of the exposure  $X$  and outcome  $Y$  variables both are affected by the same variable  $U_{xy}$ , resulting in what is referred to as dependent misclassification bias.

These cases of independent and dependent misclassification bias or measurement error presented in Parts A and B of **Figure 26** are cases of non-differential misclassification bias or measurement error. A misclassification bias (or measurement error) is referred to as non-differential when the misclassification (or measurement error)  $U_x$  of the exposure variable  $X$  is not structurally related to the outcome variable  $Y$ , and when the misclassification (or mismeasurement) error  $U_y$  of the outcome variable  $Y$  is not structurally related to the exposure variable  $X$ . Part C of **Figure 26** shows independent, but differential misclassification (or mismeasurement) bias of the exposure variable by the outcome variable. This is the case where the misclassification (or mismeasurement) of the exposure variable ( $U_x$ ) and that of the outcome variable ( $U_y$ ) do not have a common cause, but the outcome variable  $Y$  affects the misclassification (or mismeasurement) of the exposure variable  $U_x$ . (The classic case of this is 'recall bias' in case control studies, where respondents differentially recall past exposure based on current outcome status.) The equivalent case of independent, differential misclassification (or mismeasurement) bias of the outcome variable  $Y$  by the exposure variable  $X$  is presented in Part D of **Figure 26**. Finally, cases of dependent and differential misclassification (or mismeasurement) bias are presented in Parts E and F of **Figure 26**. So, for the case presented in Part E, the misclassification (or mismeasurement) bias is dependent, because  $U_x$  and  $U_y$  have a prior common cause ( $U_{xy}$ ), and differential, where the true outcome  $Y$  affects the misclassification (or mismeasurement) of the exposure variable  $X$ .

**Figure 26: Hernan 2009 structural classification of measurement error**



Adapted from Hernan and Cole, 2009, p. 960 [239]

The health economists Huber, Lechner and Wunsch [240] have described a type of misclassification bias specific to research on the effect of publicly funded financial credits on health. They have argued that when receipt of publicly funded financial credits is socially stigmatised, welfare recipients could systematically report lower health status to justify receiving a financial credit from the state [240]. The health economists argued that this was a strategy for avoiding the social stigma associated with receiving such credits [240]. This type of bias, called justification bias [240], provides the opportunity to classify it within Hernan and Cole's structural classification of measurement error presented above [239]. Since the

exposure variable (receipt of publicly funded financial credits) determines the level of measurement error of the outcome variable (health status), justification bias is a differential mismeasurement bias of the outcome by the exposure. If a participant misreported her health outcome to justify welfare receipt, but did not also misreport the amount of income from publicly funded financial credits she receives, then this would be a case of independent, differential mismeasurement bias of the outcome by the exposure, as shown in Part D of **Figure 26**. However, if a participant deliberately reported worse health and reported receiving a lower income to justify having received social assistance, then the measurement errors of the exposure and the outcome variable were dependent, resulting in dependent, differential mismeasurement bias of the outcome by the exposure. An example of this bias is presented in Part F of **Figure 26**.

The above framework works well for single or cross-sectional measures. But this thesis requires an application to repeated measures data, and, in the context of fixed effects estimation, error processes in the classification or measurement of *change* between waves. The effect of misclassification and mismeasurement bias in longitudinal studies such as this thesis that estimate the effect of a time-varying exposure variable on a time-varying outcome variable has received little attention to this date and is conceptually challenging. One epidemiological study demonstrated that mismeasurement in a time-varying exposure variable resulted in a large measurement error [241]. This study conducted survival analyses to assess the effect of marital status on survival, using survey data [241]. Then it repeated the same analyses, but with measurement error in the exposure variable reduced by up-dating the information for repeated measures [241]. While the survival model with the (more) mismeasured exposure suggested that marital status had a protective effect, the model that used the exposure variable with reduced measurement error conversely found an adverse effect of marital status on survival [241]. This study suggested that measurement error in a time-varying exposure in longitudinal studies can have a large impact. However, the underlying mechanisms of misclassification and mismeasurement bias in epidemiological longitudinal studies are underresearched and not well understood yet.

### ***Errors in measures of income mobility***

This thesis used several measures of income mobility. The exposure variables of the thesis were change in FTC and IWTC eligibility *and* amount. These exposure variables were in turn due on changes in family income, and family income itself (net of IWTC and FTC) was also a potential time-varying confounding variable. One of the potential effect modifying variables

was change in poverty. All of these three measures were derived from total gross annual family income collected in the SoFIE. Thus, measures of income mobility play a central role in this thesis.

The measures of income mobility used in this thesis are complex. For example, as described in *Chapter 5*, to determine whether a participant had changed their FTC and IWTC eligibility status or amount (exposure variables), income thresholds were applied to the gross total annual family income data. This thesis refers to these measures as income threshold variables. Other prominent examples of measures of mobility in such income threshold variables are measures of poverty dynamics. Moreover, an appraisal of the income data presented in *Chapter 5* raised concerns about the quality of these income data, which included the large percentage of missing values as well as a small number of extreme income values and large income changes. Considering the centrality and complexity of these income measures and the concerns for the quality of the data they produced, an investigation of error in the income mobility measures used in this thesis is warranted. Studies of misclassification in poverty dynamics provide important insights and help advance a thesis focused on this complex task.

One of the simplest cases of error in income measures to consider is mismeasurement in personal income from one source in a cross-sectional survey – I start with this. Several types of mismeasurement in such data in a *cross-sectional* setting can be differentiated. A selection of nine is sketched in **Table 75**. More comprehensive descriptions of these and further types of such mismeasurement fall beyond the scope of this thesis, but are provided in various literature reviews [214, 242]. Even when the simplest case is considered, multiple types of measurement errors can occur, potentially of different size and into the same direction or opposite directions. The level of complexity increases further when mismeasurement in a measure of aggregate income from multiple income types in a cross-sectional survey is considered. This thesis refers to measures of income from multiple sources as ‘aggregate income measures’. Family income from all sources is derived by adding the personal incomes from each family member from each type of income. Therefore, the mismeasurement in family income from all sources is some composite of all types of error in each personal income measure for each income type. For example, the percentage of missing values in the SoFIE differed by the income types, which were combined into personal income (see **Table 6** in *Chapter 5*). This could indicate mismeasurement of different sizes and potentially different directions in these component income types of the aggregate family income measure used in this thesis. However, some of the measures used in this thesis have greater complexity still. As stated above, the exposure variables are income threshold variables derived by applying an

**Table 75: Types of misclassification bias in income measures**

Type of misclassification	Description
Recall bias	The bias from misclassification of income due to a participant not recalling receiving income from a particular source or forgetting or misremembering the amounts that she received [214, 217].
Rounding bias	The bias from misclassification of income due to a participant rounded her reported income either up or down to the next relevant sum [218]. The relevant sum could vary, so that an income amount could be rounded to the next \$100, \$1,000 or \$10,000 [218]. In one study that tested extensively for rounding error, when rounding error was modelled in the same direction as it had occurred, 40% of reported income values matched the incomes from administrative data [218]. This suggested that rounding error may be relatively minor and random, producing “white noise” more so than introducing systematic bias [218].
Bias due to a lack of financial literacy	The bias from misclassification of income due to a lack of ability to differentiate income types or sources [214]. One income type or source may be confused with another income type or source [214]. For example, a participant may find it hard to differentiate a tax credit from a tax rebate.
Bias from inability to determine income amounts	The bias from misclassification of income due to the inability to determine the amount of income that should be reported to provide an accurate account. First, the exact amount of an income type may not be defined. For example, one individual generally received the payment for government transfers from social assistance for families, but the receiver unit of this income is the family. This may leave participants in the family confused about how much income from the family assistance credit they should report as their personal income [214]. Second, for some types of income, such as income from self-employment, it may be difficult to determine the amount of income from this source at a certain point in time [214]. Third, a participant may not know the answer to an income question because the question is irrelevant or not applicable for the participant [214].
Social desirability bias	The bias from misclassification of income due the different extent of social desirability and stigma of certain types of income. Socially undesirable or stigmatized types of income may be underreported or may not be reported altogether [214]. In the case of income from social assistance, this misclassification is likely to disproportionately affect low-income participants, whereas for other income types such as illegal income it may affect participants from all income groups. The mode of survey administration can ease or aggravate this misclassification. For example, a participant may feel less inclined to disclose income from a socially stigmatized income type in a personal face-to-face interview than in a self-administered computer survey, where she may not have to fear the confidentiality of the response from the interviewer, family members or other participants who are in the room when the interview is being conducted.
Justification bias	The bias from misclassification of income due to the desire to justify receiving social assistance from the state (see above) [243].
Bias from reluctance to	The bias from misclassification of income due to a participant’s suspicion or fear that the income information that she discloses in the

## Chapter 9: Internal validity and precision

---

disclose incomes to authorities	survey may be shared with the inland revenue department or other authorities [214]. This suspicion or fear could potentially lead participants to underreport some taxable incomes, especially those they have failed to declare for tax purposes [214]. While this error could occur in surveys conducted by private organisations such as universities or research companies, it is likely to be most pronounced in official surveys.
Bias from missing income values	The bias from misclassification of income due to missing values. First, values on an income type could be missing due to a participant refusing to answer it. Second, it may be missing due to the participant providing a 'don't know' response. The participant may give such a response either pretending to or genuinely not knowing the answer.
Seam bias	The bias from misclassification of income due to seam error in the income spell data. If income histories over a reference period are reported in spell data, as is frequently done in longitudinal studies, then seam error can occur [243]. Seam error becomes apparent when income data that are derived from spell histories encounter a questionably large number of changes in income reported around the seam of the spells [243]. As a result, the lengths of a spell and the amount of an income spell could be misclassified [243].

---

income threshold to an aggregate income variable from all income types, generally resulting in a categorical classification such as FTC-eligible or FTC-ineligible. Therefore, assessing misclassification in these variables additionally also requires attention to income threshold effects.

Attempts have been made to quantify the extent of measurement error in the income data collected in surveys. Validation studies have been conducted that compare self-reported survey data on income from employee earnings with more objective measures of such earnings from administrative records (e.g., employer or tax records) and often in male factory workers. Duncan and Hill were amongst the first to conduct such a validation study in the mid-1980s [244]. They found that more than 80% of the variability observed in survey data corresponded with variation in administrative records [244]. This suggested a moderate level of mismeasurement in personal income data in cross-sectional surveys [244]. These early findings were reproduced in similar validation studies, including such studies that did not assume administrative records were free of measurement error, but did account for mismeasurement in the administrative data (for a review see [214]).

Mismeasurement in income variables in longitudinal surveys is often considered as more complex and problematic than such error in income variables in cross-sectional surveys. The reason is that longitudinal analyses generally use measures of income mobility that are derived from two repeated measures, both of which can be affected by error, possibly of a different size and operating into different directions at different time points [214]. This thesis uses measures of income threshold variables in a longitudinal survey setting. An appraisal of misclassification in these variables may thus ideally not only consider the diverse types of misclassification across persons and income type, as well as the effects of application of income thresholds, but also changes in these multiple types of misclassification between waves over time. This briefs the considerable complexities required to attempt to estimate the size and direction of misclassification in measures of mobility in the income threshold variables. However, and one 'saving grace', some of these errors are likely to be consistent within individuals over time, meaning that they drop out of repeated measures as the error is 'unchanging over time'. Attention in this thesis is thus placed primarily on identifying and assessing those measurement errors that are time-varying.

In the econometrics literature, error in income measures is principally classified into 'classical' versus 'non-classical'. Classical measurement errors are those that introduce random variation or 'white noise' in the income variable, approximately equating with non-differential

mismeasurement in epidemiological terms. Non-classical measurement error is the umbrella term for errors that introduce systematic bias in income data, roughly the equivalent of differential mismeasurement in the epidemiological literature.

Non-differential error in income measures can be understood as random error that is not correlated with past or future income within the individual [214]. Non-differential misclassification in measures of mobility in an income threshold variable depends on the specific threshold amount or level at which the threshold is set and the number of people with borderline incomes in the proximity of the threshold [214]. Jenkins states that the effect of classical measurement error on measures of mobility in income and misclassification in income threshold variables is not currently well understood [214].

The main type of differential mismeasurement of income variables explored in the econometrics literature is reversion to the mean [214, 217, 218, 242]. Mean-reversion in income data occurs when participants with incomes below the mean tend to over-report and participants above the mean tend to underreport their income [214, 217, 218, 242]. Mean-reversion in income data has been well documented, with findings from early studies [242] repeated more recently [214, 217, 218]. Income types may be affected differentially by mean-reversion, which has implications for mismeasurement in aggregate income variables, such as total personal income from all income types. For example, validation studies of income from social assistance financial credits, generally paid to low-income persons, have found that these income types are commonly underreported, although generally only modestly [242]. In contrast, there is little evidence for mismeasurement of income from assets, which are generally more prevalent in higher income persons [242]. This means that the types of income that contribute to an aggregate income measure may influence the total size and direction of mismeasurement in this variable. Furthermore, if aggregate income measures add different income types from two or more persons, then the size and direction of mismeasurement could also vary by person [214]. Jenkins argues that the current lack of knowledge challenges an accurate appraisal of the size or even direction of misclassification in aggregate income measures from several income types and persons [214]. Therefore, with regards to the findings of this thesis, it is difficult to quantify the extent and direction of mismeasurement in the types of income variables used in this thesis in a cross-sectional setting, but it is even more difficult to judge, whether this measurement error is constant or varies over time in longitudinal settings.

Few studies have expanded investigations of the effect of mean-reversion into a longitudinal study setting by looking at such error in measures of income mobility [214, 217, 218, 245,

246]. Such studies generally found evidence for negligible differential misclassification due to mean-reversion in income mobility measures in the United States [217, 218, 245] and Denmark [246]. For example, one of the most recent such studies linked data on annual personal income from employee earnings from the US Survey of Income and Program Participation with administrative tax records, finding comparable levels of income mobility in the survey data and in the tax records [217]. The authors conclude that “if earnings and lagged earnings are obtained from the same survey instrument, administered in two different years, mean-reversion is likely to be similar in both years” (pp. 304-5) [217]. This means – at the individual-level relevant for this thesis – that differential errors that are correlated over time may largely cancel out and not greatly bias measures of change within individuals over time. (Different conclusions might be reached at the population level [214].) If such differential or systematic errors in any single cross-sectional measures cancel out over time when estimating change, then one would be (largely) left with the effect of random or non-differential error that randomly varies over time.

Even fewer studies have been conducted that deal with misclassification in measures of mobility in income threshold variables, generally measures of poverty dynamics. Jenkins argues that non-differential misclassification bias should lead to a greater prevalence of spurious change in income recorded as a transition into poverty (entry rate) or out of poverty (exit rate), but that the direction of bias from this spurious change is unclear, because the true number of persons entering and exiting poverty cannot reliably be established [214]. Differential misclassification bias from mean-reversion may result in underreporting of extreme increases and decreases in measures of mobility in aggregate (e.g., family or household) income measures [214]. This bias may then reduce the number of changes into or out of an income threshold. The net size and direction of misclassification should depend on several factors, including the specific relationship between income and error levels; the location of the income threshold; and how many persons are concentrated in proximity to the threshold [214]. Whether the misclassification would be differential or non-differential may depend on whether misclassification affected persons below and above the threshold similarly or not [247]. However, little is known at this point about the relative importance of these factors [214]. Family dynamics add a further level of complexity, as changes in family composition may lead to changes in the size and direction of misclassification bias over time [214]. Thus, the most important issue in cohort studies using fixed effects methods is, whether mismeasurement or misclassification in two measures are correlated over time [214, 217, 218, 245, 246]. If misclassification or mismeasurement is constant within an individual over time,

then it is eliminated from fixed effects regression model analyses. However, due to the relative complexity of misclassification in income threshold variables, even when only the simple cross-sectional context is considered, it is difficult to judge the extent and direction of misclassification bias of the exposure in this thesis that is due to measurement error in income variables.

### ***Assessment of misclassification bias and measurement error of the exposure variables***

In this section, I draw on the above empirical understandings, but apply ‘first principals’ from epidemiology and econometrics to how misclassification and measurement errors should have behaved in this thesis. I consider *misclassification* of eligibility first, and then *mismeasurement* of FTC and IWTC amount second.

#### Misclassification of Family Tax Credit and In-Work Tax Credit eligibility

Any misclassification process has a non-differential component and may also have an additional differential component. All the complexities in how someone may be assigned an income above or below a threshold for FTC or IWTC at one point in time noted, it seems likely that systematic errors over time largely cancel out, leaving non-differential of ‘classical’ measurement error as the predominant bias. The cumulative effect of classical measurement error over two waves will see some ‘white noise’ changes in income over and above any true change, and these changes (on occasion) may be sufficient to make someone appear to become FTC- or IWTC-eligible when they had not, vice versa ineligible when they had not, and also remain (in)eligible when their status had actually changed. It is possible to ‘invent’ scenarios where such errors in change in eligibility status are determined by ‘true’ change in health (i.e. differential misclassification of eligibility; e.g. whereby someone with deteriorating SRH is more likely to report an incorrect deterioration in income and therefore apparent shift to ‘eligible’). However, for the purposes of this thesis it is probably safest (and most plausible) to argue that ‘white noise’ or non-differential misclassification errors of change in eligibility predominated. Therefore, this will have biased towards a null finding any association of eligibility with SRH in the fixed effects analyses in this thesis. By how much is guesswork. But given the very modest (and nearly null) associations observed in this thesis, any such misclassification bias (other biases assumed away for now) would have to be large to have missed an important and sizeable true association of FTC and IWTC eligibility with SRH. Put another way, I strongly suspect misclassification of FTC and IWTC eligibility biased the results in this thesis to the null, but I also strongly suspect that (at most, and considered in isolation)

this bias no more than halved the true association. And I am almost certain that it did not cause (say) a 90% reduction in the observed compared to the true association.

#### Mismeasurement of FTC and IWTC amount

Among those who remained eligible between waves, it is likely that change in the FTC and IWTC amount was mismeasured. Again, I assume that ‘white noise’, non-differential errors or ‘classical’ measurement errors predominated over differential ones. The empirical studies on measurement error in income reviewed above largely focused on cross-sectional measures, whereas this thesis looks at change over time in an exposure variable (income from FTC and IWTC that a family was eligible for) *and* its association with change over time in an outcome. Some empirical and theoretical work [214, 217, 218, 245] and unpublished simulations [personal communication, T. Blakely, 22 June 2013 (as part of supervision of this thesis)] point to a likely moderate underestimation of the true association of FTC and IWTC amount with SRH. Again, though, not to the extent of missing a meaningful and large ‘true’ association. Therefore, as above for misclassification of eligibility, I conclude bias to the null certainly occurred – but probably not to the extent that an important true observation was being missed.

#### Specific considerations and sensitivity analyses in this thesis

Detailed investigation of the income data used in this thesis caused concerns for these data that were centred on the number of missing values, extreme income values and extreme changes in income. Extreme incomes and income changes may be due to mismeasurement, and could thus introduce misclassification in the exposure variables. Sensitivity analyses were conducted for each exposure variable, where participants reporting the 2% most extreme income values were removed and the fixed effects regression analyses repeated (**Table 76**). Equivalent sensitivity analyses were also conducted with the 2% most extreme income changes removed (**Table 77**). These sensitivity analyses found that removing observations with extreme income values and changes, which may have suffered disproportionately from misclassification, did not meaningfully alter findings from the main fixed effects analyses. This may suggest that misclassification of income measures may not have introduced considerable bias in this thesis.

The derivations of FTC eligibility and amount assumed that all children were neither orphans nor unsupported children, since this information was not collected in the SoFIE. Orphans or unsupported children were not considered dependent children for Working For Families purposes. This misclassification of orphans and unsupported children as dependent children

may have caused an overestimation of FTC eligibility and amount. If the effect of FTC and IWTC eligibility and amount was larger in those eligible for the credits or for an increase by \$1,000 in these credits, then this misclassification could have reduced the effect estimates towards a null finding. However, the small percentage of orphans or unsupported children in the general population suggests that this misclassification, if any, was likely small in size.

**Table 76: Fully adjusted fixed effects regression model, with Family Tax Credit and In-Work Tax Credit eligibility and amount, outcome self-rated health, extreme income values removed, N = 6,900, seven waves (Waves 1 to 7)**

Exposure	All income values included			Extreme income values removed		
	Estimate	Standard error	95% CI	Estimate	Standard error	95% CI
FTC eligibility	0.013	0.012	(-0.011, 0.037)	0.013	0.012	(-0.011, 0.037)
FTC amount <sup>a</sup>	-0.001	0.002	(-0.006, 0.004)	-0.001	0.002	(-0.006, 0.004)
IWTC eligibility	0.003	0.012	(-0.021, 0.027)	0.003	0.012	(-0.021, 0.027)
IWTC amount <sup>a</sup>	0.000	0.004	(-0.008, 0.008)	0.000	0.004	(-0.008, 0.008)

<sup>a</sup> scaled at \$1,000.

**Table 77: Fully adjusted fixed effects regression model, with Family Tax Credit and In-Work Tax Credit eligibility and amount, outcome self-rated health, extreme changes in income removed, N = 6,900, seven waves (Waves 1 to 7)**

Exposure	All income values included			Extreme changes in income removed		
	Estimate	Standard error	95% CI	Estimate	Standard error	95% CI
FTC eligibility	0.013	0.012	(-0.011, 0.037)	0.013	0.012	(-0.011, 0.037)
FTC amount <sup>a</sup>	-0.001	0.002	(-0.006, 0.004)	0.000	0.002	(-0.006, 0.004)
IWTC eligibility	0.003	0.012	(-0.021, 0.027)	0.003	0.012	(-0.021, 0.027)
IWTC amount <sup>a</sup>	0.000	0.004	(-0.008, 0.008)	0.000	0.004	(-0.008, 0.008)

<sup>a</sup> scaled at \$1,000.

The derivations of FTC eligibility and amount assumed that all children were neither orphans nor unsupported children, since this information was not collected in the SoFIE. Orphans or unsupported children were not considered dependent children for Working For Families purposes. This misclassification of orphans and unsupported children as dependent children may have caused an overestimation of FTC eligibility and amount. If the effect of FTC and IWTC eligibility and amount was larger in those eligible for the credits or for an increase by \$1,000 in these credits, then this misclassification could have reduced the effect estimates

towards a null finding. However, the small percentage of orphans or unsupported children in the general population suggests that this misclassification, if any, was likely small in size.

The FTC amount was dependent on the age of the dependent children in the family, but was derived from the SoFIE assuming that the eldest child in the family was over 12 years old, because this was the assumption made by the only available entitlement charts used for the derivation. A family received a small additional credit for each child over 12 years, so that the FTC in families with such children was underestimated. The FTC amount may have been underestimated for all families with children aged over 12 years and more so for families with a larger number of children aged over 12 years (differential misclassification bias). A sensitivity analysis was conducted where the main fixed effects regression analyses were repeated with a sample restricted to families with children less than 13 years of age. The effect of an increase in FTC amount by \$1,000 in this sensitivity analysis was associated with no change in SRH (fully adjusted effect estimate 0.000, 95% CI 0.000 to 0.000). This was near-identical with the equivalent effect estimate for the total study sample of a reduction of SRH by 0.001 scores of SRH associated with a \$1,000 increase in FTC (95% CI -0.006, 0.004). Thus, no evidence was found that suggested that the main fixed effects regression analyses on FTC amount were affected by bias due to the FTC amount not taking into account the age of dependent children in the family.

While justification bias has been proposed as one theoretically plausible bias in studies on the health effect of publicly funded financial credits, no empirical evidence for justification bias has been found [240]. This thesis had a low risk of justification bias for three reasons. First, receiving FTC and IWTC in New Zealand carried little, if any, social stigma, considering the country's long history of family assistance presented in **Figure 5** and that these tax credits are designed to have a wide population coverage (including the middle income population) as shown in **Figure 6** in *Chapter 2*. Second, the SoFIE was not designed to provide data for studies of the relationship between publicly funded financial credits and health, meaning that participants were unlikely to misreport their health as a result of their social assistance status. Third, the question on SRH preceded the question on income in the survey, meaning that participants reported their SRH before they were primed by questions on receipt of publicly funded financial credits.

## Summary

In summary, misclassification and mismeasurement in the exposure variables may have occurred due to mismeasurement in the income variable from which they were derived. The

size and even direction of differential misclassification or misclassification bias from income is difficult to establish, but non-differential misclassification (or mismeasurement) may have introduced a small amount of non-differential misclassification (or mismeasurement) bias, likely towards a null finding. However, simulations suggested that the extent of bias from misclassification and mismeasurement of the exposure variables was low, and sensitivity analyses found no evidence for any bias from extreme incomes or extreme income changes, which may be assumed to have a relatively large risk of misclassification (or mismeasurement) bias. No evidence was found that any assumption made in the derivation of these exposure variables carried a sizeable risk of mismeasurement bias.

### **Mismeasurement bias of the outcome**

Non-differential mismeasurement of the outcome likely also attenuated the results towards a null finding (to an unknown extent). However, it is difficult to imagine cases where misclassification of change in the outcome variable (SRH) could have been differential by the change in the exposure variables, suggesting little risk of differential misclassification bias in this thesis.

The almost inevitable existence of bias to the null in the fixed effects associations of FTC and IWTC with SRH, due to mismeasurement of SRH, 'added to' that occurring due to misclassification or mismeasurement of the exposure variables (IWTC and FTC). It is impossible to reliably quantify the joint bias, but I still believe it would not be of such magnitude to miss a large and meaningful association of FTC or IWTC with SRH. Nevertheless, my opinion is perhaps more one of conjecture. Future theoretical, empirical and simulation research on cumulative effects of misclassification and mismeasurement in both exposure and outcome variables in fixed effects regressions is required.

### **Misclassification and mismeasurement bias of time-varying covariates**

Such error undoubtedly occurred. However, given that adjustment of time-varying covariates made virtually no difference to the main effect, and assuming at least reasonable measurement in change in covariates, then it seems highly unlikely that improved measurement of time varying covariates will have any substantive impact of the IWTC and FTC associations with SRH in the fixed effects models.

## Misspecification bias in the outcome variable

A recent study concluded that intervals between self-rated health categories may not be equally spaced [231]. Thus, treatment of SRH as a linear rather than an ordinal variable in the main fixed effects regression analyses of this thesis may have introduced bias from incorrect specification of the statistical model. Hybrid (fixed effects) proportional odds model analyses [62], which treated SRH as ordinal, were conducted to test for this potential misspecification bias of the exposure.

## Reverse causation

Reverse causation (also called health selection) occurs when an outcome precedes and causes the exposure [248]. In this thesis, if changes in health caused changes in one or more of the tax credit exposure variables, then reverse causation could have biased the findings of the main analyses. The risk of reverse causation in this thesis was considered low, because no evidence for an association between change in the exposure variables and change in the outcome variables was found. However, to test for reverse causation, the main analyses were repeated on the study sample restricted to healthy participants at baseline, defined as those reporting excellent, very good or good SRH at Wave 1. **Table 80** presents the findings from these sensitivity analyses. Consistent with the results from the main analyses, the sensitivity analyses found no evidence for any discernible effects, which suggest low risk of reverse causation in the main fixed analyses.

**Table 78** **Table 78** presents the fully adjusted hybrid (fixed effects) proportional odds model with FTC eligibility as the outcome and SRH as the outcome. The fully adjusted hybrid (fixed effects) proportional odds model estimator (0.059, 95% CI -0.019 to 0.137) was larger than the linear fixed effects regression estimator (0.013, 95% CI -0.011 to 0.037), but was also small, positive and statistically not significant. The fully adjusted hybrid (fixed effects) proportional odds model and the linear fixed effects model estimate for the other three main fixed effects analyses presented in **Table 79**. This shows that treating SRH as ordinal produces comparable results to treating it as a linear variable in these analyses, too. Moreover, these findings are consistent with a previous study that used SoFIE data to estimate the effect of total personal income on SRH [62]. In summary, the presented quantitative bias analyses found no evidence for a discernible bias from model misspecification due to treating the outcome variable as linear.

## Reverse causation

Reverse causation (also called health selection) occurs when an outcome precedes and causes the exposure [248]. In this thesis, if changes in health caused changes in one or more of the tax credit exposure variables, then reverse causation could have biased the findings of the main analyses. The risk of reverse causation in this thesis was considered low, because no evidence for an association between change in the exposure variables and change in the outcome variables was found. However, to test for reverse causation, the main analyses were repeated on the study sample restricted to healthy participants at baseline, defined as those reporting excellent, very good or good SRH at Wave 1. **Table 80** presents the findings from these sensitivity analyses. Consistent with the results from the main analyses, the sensitivity analyses found no evidence for any discernible effects, which suggest low risk of reverse causation in the main fixed analyses.

**Table 78: Fully adjusted hybrid (fixed effects) proportional odds model with Family Tax Credit eligibility, outcome SRH, N = 6,900, seven waves (Waves 1 to 7)**

Parameter	Estimate	Standard error	95% CI
FTC eligibility – mean-centred (FE)	0.047	0.042	(-0.036, 0.129)
Family income – mean-centred (FE) <sup>a</sup>	0.000	0.000	( 0.000, 0.001)
Unemployed – mean-centred (FE)	0.015	0.099	(-0.180, 0.210)
Inactive – mean-centred (FE)	-0.200**	0.050	(-0.298, -0.103)
Employed – mean-centred (FE; reference)	0.000		
One-parent family – mean-centred (FE)	0.065	0.074	(-0.080, 0.209)
Two-parent family – mean-centred (FE; reference)	0.000		
Number of children – mean-centred (FE)	-0.020	0.024	(-0.067, 0.026)
FTC eligibility – average	-0.429**	0.118	(-0.660, -0.197)
Family income – average <sup>a</sup>	0.005**	0.000	( 0.004, 0.006)
Unemployed – average	-1.427**	0.402	(-2.215, -0.639)
Inactive – average	-1.406**	0.101	(-1.603, -1.209)
Employed – average (reference)			
One-parent family – average	-0.373**	0.098	(-0.565, -0.181)
Two-parent family – average (reference)	0.000		
Number of children – average	0.297**	0.032	( 0.235, 0.360)
Wave 1	0.855**	0.045	( 0.767, 0.942)
Wave 2	0.857**	0.044	( 0.770, 0.943)
Wave 3	0.457**	0.043	( 0.372, 0.541)
Wave 4	0.491**	0.042	( 0.408, 0.574)
Wave 5	0.326**	0.042	( 0.243, 0.409)
Wave 6	0.199**	0.042	( 0.117, 0.282)
Wave 7 (reference)	0.000		
SD	2.153**	0.027	( 2.100, 2.207)
Intercept 1	-1.610**	0.071	(-1.749, -1.471)
Intercept 2	1.075**	0.071	( 0.937, 1.214)

Intercept 3	3.736**	0.076	( 3.587, 3.884)
Intercept 4	6.015**	0.093	( 5.832, 6.197)

Notes: \*\* significant at 0.1% level FE = Fixed effects. <sup>a</sup> Derived from gross total annual family income (minus FTC amount), equalised to household composition and scaled at \$1,000.

**Table 79: Effect estimate (means-centred parameter), fully adjusted hybrid (fixed effects) proportional odds model, with Family Tax Credit and In-Work Tax Credit eligibility, outcome SRH, N = 6,900, seven waves (Waves 1 to 7)**

Exposure	Linear fixed effects model			Hybrid (fixed effects) proportional odds model		
	Estimate	Standard error	95% CI	Estimate	Standard error	95% CI
FTC amount <sup>a</sup>	-0.001	0.002	(-0.006, 0.004)	0.009	0.009	(-0.008, 0.025)
IWTC eligibility	0.003	0.012	(-0.021, 0.028)	0.013	0.043	(-0.071, 0.025)
IWTC amount <sup>a</sup>	0.000	0.004	(-0.008, 0.008)	0.001	0.015	(-0.027, 0.030)

<sup>a</sup> scaled at \$1,000.

**Table 80: Fully adjusted linear fixed effects models, with Family Tax Credit and In-Work Tax Credit eligibility and amount, outcome self-rated health, N=6,535 (balanced panel of participants reporting excellent, very good or good SRH at Wave 1), seven waves (Waves 1 to 7)**

Exposure	Full balanced panel			Participants reporting excellent, very good or good SRH at Wave 1		
	Estimate	Standard error	95% CI	Estimate	Standard error	95% CI
FTC eligibility	0.013	0.012	(-0.011, 0.037)	0.016	0.012	(-0.008, 0.040)
FTC amount <sup>a</sup>	-0.001	0.002	(-0.006, 0.004)	-0.001	0.002	(-0.006, 0.003)
IWTC eligibility	0.003	0.012	(-0.021, 0.028)	0.007	0.012	(-0.017, 0.031)
IWTC amount <sup>a</sup>	0.000	0.004	(-0.008, 0.008)	0.003	0.004	(-0.005, 0.012)

<sup>a</sup> scaled at \$1,000. The model with the restricted balanced panel of participants reporting excellent, very good or good SRH at Wave 1 was based on 45,745 observations.

## Overall assessment of internal validity

In summary, no evidence was found for the presence of sizeable selection bias in the main thesis findings. Non-differential misclassification of the exposure of an unknown extent may have biased the main thesis findings towards the null. There is little evidence for risk of bias in the exposure variable from non-differential misclassification bias from mean-reversion in income data, but it is difficult to reliably estimate the several and complex underlying biases,

and their dynamics over time. Non-differential mismeasurement or misclassification of the outcome and confounding variables likely also attenuated the results towards a null finding (to an unknown extent). No evidence was found for bias from selection, model misspecification of the outcome variable or reverse causation.

## **Precision**

The 95% CI of the effect estimate for each exposure of both tax credits is narrow. This suggests that the effect estimates of main fixed effects analyses of this study achieved a high degree of precision. Therefore, random error did not present a considerable concern in these main fixed effects analyses. However, the analyses that estimated effect modification by ethnic groups (Māori, non-Māori) and by income group (low, middle and high) were subject to considerable random error, indicated by wide 95% CIs. The reason for the imprecision of these stratified analyses was the relatively smaller sample size of the respective study samples (due to stratification of the study sample by ethnic group and income, respectively). Similarly, analyses of the effect of each exposure of both of the two tax credits on health outcomes other than SRH (psychological distress, smoking) estimated treatment effects imprecisely. The imprecision in these estimates was due to the use of three waves of data in this subsidiary analysis, rather than seven waves of data in the main analyses. However, due to the relatively large sample size, each of these analyses nevertheless produced results that are sufficiently powered to warrant presentation.

## **Conclusions**

This chapter assessed the internal validity of the thesis findings by investigating the risk of bias in and the precision of the main fixed effects regression analyses. Thesis findings were controlled for all time-invariant confounding variables and comprehensively adjusted for time-varying confounding variables. The strong control of confounding considerably strengthens the thesis. Little evidence for the presence of selection bias in this thesis was found. Moreover, the treatment effect in non-responders and participants lost to follow up would need to have been large to have considerably affected the findings, suggesting an overall low risk of selection bias.

Misclassification of the exposure variables presented the largest challenge for this thesis. Misclassification in the measures of income mobility may have suffered from independent or

dependent differential or non-differential misclassification, of which neither the size nor the direction could confidently be determined. However, sensitivity analyses that removed participants with extreme income values or eliminated extreme income changes found no evidence for bias from mismeasurement from extreme values in the main fixed effects regression analyses. Mismeasurement of the outcome and the potential confounding variables was judged to pose a low risk of bias.

The outcome variable may have been misspecified as linear, when it should be treated as ordinal. However, sensitivity analyses found no evidence for misspecification bias of the outcome. No evidence of reverse causation was found.

In summary, the internal validity of this thesis was judged good overall, with low risks of bias from selection; misspecification of the outcome; and reverse causation. However, misclassification (or mismeasurement) of the exposure variables of an undeterminable size may have biased findings, likely towards a null finding. Non-differential mismeasurement of the outcome and confounding variables likely also attenuated the results towards a null finding (to an unknown extent). The fixed effects estimates of the main analyses were precise, but the subsidiary analyses may have suffered from random error due to smaller sample sizes.

## Chapter 10: Discussion

This chapter summarises, discusses and concludes the key findings of this thesis, comparing them to previous evidence, outlining their limitations and strengths and drawing out their implications for policy and future research. The thesis found no evidence of a discernible effect of FTC and IWTC eligibility and amount on SRH in adults at the individual level over the short term in New Zealand. No evidence was further found that would suggest differences in the treatment effects of FTC and IWTC on SRH by ethnicity and poverty status, as well as with a longer follow-up of the change in outcome to the change in the exposure. Also no evidence of an effect of FTC and IWTC on two other health outcomes, psychological distress and current tobacco smoking, was found.

There is limited precedent research that the findings of this thesis could be compared to. The thesis' review of evidence on the effect of anti-poverty tax credits on the health status of parents identified no previous study. It identified five previous studies of the effect of in-work tax credits on health status in adults, all of which studied the EITC in the United States. These studies were generally consistent with the thesis findings, suggesting that the EITC had no discernible effect on health status in parents, albeit inconclusive evidence for smoking in favour of a reduction due to EITC.

It is theoretically puzzling that the improvements to income and employment from FTC and IWTC did not translate into improved health in adults in families in New Zealand. Since FTC and IWTC were aimed to lift people out of poverty, a potential measure of the minimum income for healthy living in New Zealand, the thesis findings may be seen as providing no support for the theory of a minimum income for healthy living. In terms of the FTC's and IWTC's impact on health equity, if these credits had no discernible health effect in their recipients and a positive effect in non-recipients through 'welfare security', then they may paradoxically have *decreased* health equity.

Limitations of the thesis included the quality of the income data used in the thesis; potential violations of the fixed effects regression analyses; and some risk of bias from misclassification of the exposure variables, which may have biased the findings towards a null finding. However, strengths of the thesis include its large, nationally representative

sample with a number of years of follow-up; cohort study design; strong control of confounding; and the precision of the fixed effects estimators. The main fixed effects regression analyses of the thesis were generalizable to the general population of New Zealand, but not necessarily to other health outcomes, country settings, economic contexts and time periods.

The FTC and IWTC and similar tax credits in other high-income countries appear to have an important and positive effect on key SDH and social equity. However, this thesis suggests that these positive effects have not translated into any discernible health improvements in adults in families in New Zealand. The thesis findings thus suggest that policy makers interested in addressing the SDH to improve health equity should exercise caution about health sector investments in anti-poverty and in-work tax credits in New Zealand and comparable high-income countries.

Future research should investigate the effect of a treatment with these credits over longer time periods (rather than change over time in these credits), with randomised control trial and data-linkage study designs, as well as marginal structural model methods carrying particular promise.

This final chapter summarises, discusses and concludes the findings of this thesis. The thesis findings are first summarised and then related to previous empirical and theoretical evidence. The chapter then discusses the strengths and limitations of the thesis findings. Implications of the thesis findings for policy are drawn out. An agenda for future research on the health effects of FTC and IWTC in New Zealand and, more broadly, anti-poverty and in-work tax credits in high-income countries is developed.

The motivation of this thesis was to answer the main research questions: *What was the association of change in FTC eligibility and amount with change in SRH in adults at the individual level over the short term?* And likewise: *What was the association of change in the IWTC eligibility and amount with change in SRH in adults at the individual level over the short term?* These questions were investigated through a cohort study design with individual fixed effects regression methods to control more strongly for confounding than cross-sectional studies can and many previous longitudinal studies have. To address its research questions, the thesis produced a combination of conceptual [9], theoretical [27], systematic review [28, 29] and empirical evidence [30].

## **Summary of findings**

No discernible association of change in FTC and IWTC with change in SRH in adults at the individual level over the short term was found. Becoming eligible for FTC or IWTC did not have an effect on SRH in adults on the individual level over the short term. Furthermore, an increase by \$1,000 in the amount of FTC or IWTC that an eligible family was entitled to also did not have any effect on SRH.

Subsidiary analyses found that none of the (null) effects of FTC and IWTC eligibility and amount on SRH were modified by ethnicity or by poverty. Furthermore, no discernible effects were found when follow-up of the change in outcome to the change in the exposure was longer, with lags of up to six year studied for FTC and up to three years for IWTC. Change in FTC and IWTC eligibility and amount was also not associated with change in two other health outcomes, psychological distress and current tobacco smoking, although these effects were imprecisely estimated.

## **Relationship to previous empirical research**

### **Effect of anti-poverty credits on health status in adults**

The systematic review of this thesis presented in *Chapter 4* did not find any previous evidence on the effect of anti-poverty tax credits for families on health status in parents. A 1999 review had identified no experimental studies of publicly funded financial credits, such as anti-poverty tax credits for families [66]. A 2008 review studied the effect of anti-poverty credits on health status in children, concluding that the small number of eligible studies was of insufficient quality to draw meaningful conclusions [24, 70].

### **Effect of in-work tax credits on health in adults**

Since in-work tax credit interventions are one type of conditional financial credit, evidence on the effect of other conditional financial credits on health status may potentially be relevant for comparison. An important and perhaps the largest body of research on the effects of these credits on health are studies investigating the credits in low- and middle-income countries of South America, including the famous credit schemes *Oportunidades* of Mexico [74] and *Bolsa Familia* of Brazil [75]. A systematic review of the effect of these conditional financial credits on health has found a considerable and positive effect on adults and children [76]. For example, positive effects of *Oportunidades* were documented for body mass index and blood pressure in adults [150], as well as for birth weight [147]; growth [148, 149]; and cognition, language and behaviour [149] in children. However, because these financial credits are conditional on uptake of social, educational and health services, their observed health benefits may be due to their recipients utilizing these services, rather than the additional income the credits provide [76]. This body of evidence on the health effects of conditional financial credits in low- and middle-income countries is therefore not directly comparable with the findings of this thesis on IWTC for two reasons. Firstly, the conditionalities of the credits in low- and middle-income countries and of IWTC in New Zealand substantially differ – the former are conditional on up-take of social, educational and health services, whereas the latter is employment-conditional. Secondly, whereas these previous studies investigated the effect on health of conditional financial credits in low- and middle-income countries, this thesis investigated the effect of a conditional credit in a high-income country. The importance of considering the economic, social, health and other context of a policy intervention when evaluating its health effect has previously been emphasised.

Studies of the effect of conditional credits on parental health in high-income countries may provide better comparisons, but are few. The perhaps first and only such study with a randomised control trial design was the evaluation of *Opportunity NYC – Family Rewards* [152, 153]. This study demonstrated a small, statistically (weakly) significant ( $p < 0.10$ ) improvement

in SRH in adults from this conditional credit for families in New York City in the United States [152, 153]. However, because *Opportunity NYC* was conditional on up-take of social services and not on employment, this study finding may also not be directly comparable with the results of this thesis. Unfortunately, econometric experimental studies of conditional credit interventions in high-income countries and their effects (e.g., Card and Hyslop's 2005 seminal study [154]) have generally not assessed effects on health outcomes [71]. Experts have noted the inconclusiveness of evidence on the effect of conditional credits on health in high-income countries, due to the small number of high-quality studies [23-25].

The most relevant previous research comes from five studies that estimate the effect of in-work tax credits for families on health in adults in high-income countries, which were reviewed systematically in *Chapter 4*. As noted above, these studies were limited to the effects of EITC on women in the United States. Furthermore, when standard systematic review criteria or epidemiological frameworks of critical appraisal were applied, the studies' quality was judged as impaired by bias, mostly from misclassification of the exposure and, to some extent, confounding. It is worth noting that this thesis derived the exposure variables more robustly and applied a cohort study design with individual-level fixed effects, thus carrying a lower risk of bias from misclassification and confounding than previous studies.

However, consistent with this thesis, these previous studies found no evidence for an effect of the EITC on SRH in adults. The only previous study estimating the effect of an in-work tax credit on SRH found that women eligible for a considerable increase in EITC had a 0.5% reduced prevalence of reporting very good or excellent (versus good, fair or poor) SRH after the EITC increase, which was statistically not significant [160]. In addition, a study that used the amount of EITC that individuals received as an instrumental variable for estimating the causal effect of income on SRH found no such effect in participants receiving EITC [78], suggesting that additional income from EITC did not have an effect on SRH (pathway A-B in **Figure 12**). Previous studies' and this thesis' findings on the effect of in-work tax credits for families on SRH are thus consistent.

The only other study, the Evans 2011 study [160], that estimated an in-work tax credit's effect on psychological distress found no effect of the EITC on mental health status, consistent with this thesis finding of no effect of the IWTC on Kessler-10. The subsidiary findings of the thesis of no detectable effect of IWTC on current tobacco smoking are consistent with the Cowan 2011 study that found no evidence of an effect of the EITC on smoking rates [158]. The thesis findings may, perhaps, be inconsistent with the Strully 2010 study [188], which found a large reduction in maternal smoking during pregnancy from EITC. However, the thesis studied

current tobacco smoking, whereas the Strully 2010 study studied maternal smoking during pregnancy, which may not be directly comparable.

In summary, this thesis presented the first evaluation of the health effects of FTC and IWTC in adults in New Zealand. Few studies existed that the thesis findings could be compared to. No previous studies of anti-poverty tax credits for families in high-income countries were identified. All five previous studies that investigated the effect on health of an in-work tax credit estimated the effect of a different in-work tax credit design (EITC); in a different country setting (US); and on a different sample (women only), with quality impairments due to methodological limitations. However, the few previous study findings on the effect of the EITC on SRH and other health status outcomes were generally consistent with the thesis findings. Considering the scarcity and limitations of previous research and this thesis' methodological innovations, this thesis makes an important contribution to knowledge of the health effects accruing from anti-poverty and in-work tax credit interventions, not only for New Zealand, but also globally.

## **Relationship to theories of the effect of income on health**

### **Income and employment as social determinants of health**

An evaluation of the Working For Families tax credit package, including FTC and IWTC, in New Zealand conducted by the Ministry of Social Development found that these credits improved income and employment in low- and middle-income families at risk of poverty and welfare reliance [64]. Similarly, a review of empirical evidence conducted by the Organisation of Economic Co-ordination and Development found that in-work tax credit interventions substantially improved income and employment in their target populations [18]. In addition, the systematic review presented in *Chapter 4* found that in-work tax credits appeared to have increased income and, in some studies, also retained participants in or moved them into employment in studies estimating the health effects of the EITC. Thus, there is mounting evidence suggesting that anti-poverty and in-work tax credits such as FTC and IWTC in New Zealand improve income and, in the case of in-work tax credits, also employment.

Theoretical frameworks of the SDH such as the Commission on Social Determinants of Health's [20, 33] presented in **Figure 1** in *Chapter 1* commonly included social policy as a structural SDH, as well as income and employment as key intermediary determinants. The

Lundberg 2010 model of the causal relationship between income (from publicly funded financial credits) and health presented in **Figure 13** also suggested that income affects health, and specified the three causal effects through which increasing income may impact health, namely direct consumption effects, direct status effects and combined consumption-status effects [19]. This framework usefully extended more generic frameworks in that it drew attention to the negative psychological effects that income can have for those in relatively low income positions, such as the majority of recipients of FTC and IWTC, through direct status and consumption-status effects [19]. Theoretical evidence thus suggests that if a financial credit intervention is successful at improving income (e.g., FTC) or income and employment (e.g., IWTC) in socially disadvantaged populations (e.g., low- and middle-income families), then the intervention should theoretically translate into improved individual health status. In turn, improved individual health in disadvantaged groups should increase average health status and health equity in the population. On the basis of this rationale, social protection over the life course, including financial credits for socio-economically disadvantaged groups, have been promoted as interventions for improving health and health equity by key global players, including the World Health Assembly [44], Commission on Social Determinants of Health [20], Social Protection Floor Initiative [55] and World Bank [56].

This thesis found evidence that contradicts these assumptions from prominent theoretical models of the social determinants of health. A detailed review of theoretical and empirical literature on publicly funded financial credits and their effects on consumption behaviours presented in *Chapter 3* found that some studies documented an increased consumption of health-promoting goods and services in recipients of publicly funded financial credits. However, other studies found increased consumption of goods and services detrimental to health, such as alcohol and tobacco. Moreover, the hypothesised health improvements from publicly funded financial credits may differ between members of the recipient unit. For example, there may not be a positive effect in parents, as the additional income may be spent on improving the health of their children, at least over the short term. Similarly, theoretical and empirical evidence suggests that moving into employment may increase, decrease or have no effect on health in adults, depending on whether the employment is stable or insecure and employment conditions positive or negative. As counter-intuitive as it may appear on first sight, publicly funded financial credits could improve, have no effect on or even decrease health status in adults.

However, the findings of this thesis that FTC and IWTC did *not* improve health suggest that addressing income and employment did not translate into short-term improved health

outcomes in adults at the individual level in New Zealand. The thesis is in line with previous study findings from the SoFIE that change in total personal income from all sources was associated with a very small and positive, but statistically insignificant change in SRH [62, 63]. It is also consistent with evidence that employment status-health trajectories were complex and differed between subgroups in the SoFIE [249]. Still, that additional income from publicly financial credits, in the case of FTC of an annual dollar value of up to \$19,410, had no discernible effect on SRH nevertheless presents a considerable puzzle to policy makers and researchers focused on the SDH and health equity.

This puzzle raises two questions. First: *Which underlying causal mechanisms prevented increases in income and movements into employment from translating into better health in a low- to middle-income population?* This thesis has commenced assembling and further developing some of the evidence that challenges the commonly held belief that additional income from social assistance for families and exiting welfare improves health status. However, the causal pathways that explain the null effects on health of some publicly funded financial credits, such as FTC and IWTC in New Zealand and EITC in the United States, require further investigation. Second: *How must publicly funded financial credits in high-income countries be designed to ensure that their improvements to SDH translate into better health?* Policy development is increasingly conducted based on evidence and there is increasing pressure for policy makers to justify their choices. Action on the SDH more and more requires evidence of which specific types of publicly funded financial credits are successful at improving health and health equity. Furthermore, as Westin has also argued [26], the specific policy design features that provide health benefits require further attention.

### **The theory of a minimum income for healthy living**

Morris *et al.*'s theory of a minimum income for healthy living posits an income threshold effect - having an income below the threshold is theorised to be detrimental to health, whereas an income above the threshold is hypothesised to enable healthy living [4-8]. According to the theory, publicly funded financial credits should improve the health of individuals whom they move from below to above the minimum income threshold. They should not have an effect on the health of individuals who remain below the minimum income threshold after receipt of the credit or already had an income above the income threshold before receiving the credit.

This thesis can be regarded as a test of the theory of a minimum income for healthy living. Although the minimum for a healthy living has not been calculated for New Zealand, this

threshold can be assumed to fall around the poverty line, estimated at 50% of median income. The main analyses of this thesis estimated the average treatment effect of FTC and IWTC in the total group of participants, whether they are below or above the poverty line. These analyses thus included low-income participants, whose health FTC and IWTC should have improved by moving them out of poverty. At the same time, they also included middle-income participants already above the poverty line, who would be hypothesised to experience no health improvements from the credits. Therefore, application of the theory of a minimum income for a healthy living to the main thesis could provide a theoretically plausible explanation of the null effects: The positive effect of the credits in participants being lifted out of poverty may have been too small to shift the null-effect in those above the poverty line. However, sensitivity analyses presented in the previous chapter showed that becoming FTC- and IWTC-eligible or increasing the FTC or IWTC amount did not have any effect on health in the subgroup of participants initially below the poverty line, whom FTC and IWTC should have lifted above the poverty line (see **Table 71**). Therefore, the thesis does not provide evidence in support of the theory of minimum income for healthy living.

It may also be possible that this thesis did not find an effect of IWTC and FTC on parental health in New Zealand, because the needs for financial support were already met by other publicly funded financial credits. In New Zealand, many preventive, secondary and tertiary health services, as well as social, housing and education services were public and may have met fundamental health needs regarding health prevention and care, as well as the wider SDH. With this strong social protection floor that may have provided the resources for a healthy living for all, additional income may not have provided additional health. If this was the case, then anti-poverty designs such as FTC and in-work tax credit designs such as the IWTC may be health-advancing in countries without, but have no effect in countries with a strong social protection floor.

## **Effect of publicly funded financial credits on health equity**

A recent review of evidence on interventions for reducing health inequalities concluded that publicly funded financial credits for low-income populations may reduce health inequalities [68]. Furthermore, a systematic review of intervention-generated health inequalities found no evidence that such credits generated health inequalities [110]. While the former conclusion

was based on very few studies and the latter on the absence of negative findings, they do imply that publicly funded financial credits improve and do not decrease health equity.

This thesis focused its empirical study on the health effect of FTC and IWTC at the individual level. However, that the thesis found no discernible effects on SRH, an indicator of morbidity and mortality [222, 226-228, 250], at the individual level suggests no (or, at the most, small) effects of these credits on health and its distribution in the population. Furthermore, in subsidiary analyses, the potential for different individual-level effects in different ethnic and income groups was investigated, finding no evidence for any differences. So, the thesis did not produce any empirical evidence that would suggest that FTC and IWTC impacted health and health equity in their recipients.

It has puzzled researchers for some time that health inequalities have persisted or even widened in the modern welfare states of the Nordic countries [251]. I have theorised that publicly financial credits, which are core welfare state interventions, present one plausible explanatory mechanism for persisting or widening health inequalities in modern welfare states, including New Zealand [27]. In line with other evidence [24], the systematic review and empirical findings presented in the thesis suggests that anti-poverty and in-work tax credits may had no effect on health in persons who receive them (low-income population). However, if these credits had a positive health effect in their non-recipients (middle- and high-income population), then this could increase health inequalities [27]. Sjöberg has recently (in 2010) published first empirical evidence that publicly funded financial credits improved health in their non-recipients [129]. I have theorised that health improvements from publicly funded financial credits in their non-recipients may be caused by 'welfare security', or feeling well due to knowing that one's welfare is secured in case an unmet financial need arises. While my theory requires rigorous testing with empirical data in the future, it provides a plausible explanation for the persistence of health inequalities in modern welfare states.

## **Study strengths and limitations**

### **Study strengths**

This thesis was based on a large, nationally representative panel survey (over 22,000 individuals at baseline) followed for an extensive time period (seven years). The sample studied was a large balanced panel of 6,900 working-age parents in one- or two-parent families, contributing over 48,000 observations for the analysis. This large, nationally

representative sample ensured a high level of precision and generalisability of the thesis findings.

The cohort study design of this thesis was suited to a political epidemiological study that adopts the individual policy approach, as explained in *Chapter 3*. While four of five previous studies of in-work tax credits were longitudinal, they relied on repeated cross-sections [158-160, 188] (see *Chapter 4*). Only one previous study also estimated the health effects of an in-work tax credit intervention on parental health using repeated measures of the same individuals [84, 85]. Thus, the cohort study design of this thesis was innovative and held considerable promise for advancing the global evidence base on how dynamics in social assistance relate to health dynamics.

The study methods were tailored to the study question and, under their assumptions, produced unbiased, consistent and statistically efficient estimates. The reasons for prioritising fixed effects regression over alternative analytic methods were discussed in *Chapter 3*. Under their assumptions, the fixed effects regression estimators presented in this thesis were unbiased, consistent and statistically efficient, providing an estimate of the average treatment effect in participants experiencing change in FTC and IWTC eligibility and amount. Importantly, all of the four exposure variables and the outcome variable varied substantially over time, ensuring the efficiency of the method. Moreover, since fixed effects regression analyses estimate the association of change in the exposure variable and change in the outcome variable, they provide treatment effect estimates that approximate causal estimates.

Confounding is generally a major (and not uncommonly *the*) cause of concern in observational, epidemiological studies. One of the main advantages of this thesis was its strong control of confounding. The fixed effects regression estimators were controlled for all time-invariant confounding. Potential time-invariant confounding variables included age, ethnicity and gender, as well as often unmeasured variable such as intelligence and biological predisposition for disease. Furthermore, fully adjusted analyses also adjusted strongly for time-varying confounding, blocking many back-door paths by including all determinants of FTC and IWTC in the fixed effects models. Thus, risk for confounding principally only existed from residual confounding due to misclassification or –measurement of the potential time-varying confounding variables included in the fully adjusted analyses – this risk was judged to be low, if any.

Furthermore, theoretical and quantitative sensitivity analyses suggested that the thesis was unlikely to be affected by several potential other biases. No evidence was found for the

presence of selection bias, and a very large treatment effect in survey non-participants and those lost to follow-up would have been required for selection to bias the thesis findings. Furthermore, no evidence was also found that would have indicated bias from misspecification of the outcome and from reverse causation. Finally, the main thesis findings' high level of precision suggested that the main analyses were sufficiently powered statistically to answer the research questions. It can be argued with confidence that random error was unlikely to explain the null findings of this thesis.

### **Study limitations**

There was the possibility that two assumptions of the fixed effects regression method may have been violated (an issue of potential endogeneity), which could have introduced bias in the fixed effects estimator. The first potential model violation was the potential of reverse causation (see pathway A in **Figure 15**). However, this violation was judged to be unlikely for two reasons. First, no causal effect was established that could have been 'reversed'. Second, eliminating (some) health selection from the study by repeating the fixed effects regression analyses on a sample restricted to those in good health at baseline (a partial test of the health selection pathway) produced comparable findings to the main analyses. The second potential model violation was that the fully adjusted main fixed effects regression analyses included variables [i.e., equivalised gross total annual family income (minus FTC or IWTC) and employment status] that could potentially simultaneously have been time-varying confounding and mediating variables (see black dashed arrows in **Figure 19** and **Figure 20**). However, including the variables in the regression analytic model did not result in any noteworthy change in the fixed effects estimator, and a recent study has shown that income has no causal effect on SRH in individuals receiving an in-work tax credit [84]. This suggests that income and employment may be inactive (and, therefore, not mediating) pathways. However, a small risk of bias from including potential time-varying mediating variables remained. Therefore, marginal structural modelling analyses may be conducted in the future to produce results that control for this small potential risk of bias.

The thesis findings may have been affected by differential (by the outcome) misclassification or mismeasurement of the exposure variables of an unknown size due to error in the income measures, likely introducing bias towards a null finding. Concerns for the quality of the income data from the SoFIE focused attempts to untangle and quantify misclassification bias in the exposure variables principally on such error in income measures. However, the econometric literature provided limited knowledge for determining the size and direction of

mismeasurement in the income variables used in this thesis. What matters for fixed effects regression analyses is not necessarily the type and extent of measurement error in the exposure (or outcome) variables, but whether this error *changed* over time. If measurement error in the exposure variable remained constant over time, then it was eliminated from the analyses. This thesis conducted two sensitivity analyses to attempt to test for bias from potential misclassification or mismeasurement in the exposure variables. The main analyses were repeated on the study sample removing individuals with the 1% and 99% percentiles of gross total annual family income at each wave, and the 1% most extreme decreases and increases (i.e., changes) in the income variable respectively. These sensitivity and the main analyses produced comparable estimators, providing no evidence for bias from misclassification in the income variables used in this thesis.

Other sources of misclassification of the exposure variables (FTC and IWTC) were also considered. Except income, none of the other variables that were used to derive the exposure variables were considered to carry noteworthy risk of misclassification. However, some assumptions needed to be made in the derivation of the exposure variables due to the unavailability of certain data. Quantitative bias analyses found no evidence for any bias from these assumptions though. There may perhaps have been a small extent of non-differential (by the outcome) misclassification bias of the exposure variables from these assumptions, which may resulted in an underestimate of the treatment effect. In addition, while not tested empirically, misclassification or –measurement of the outcome and confounding variables (likely non-differential by the exposure variables) may have resulted in a small, if any, underestimate of the treatment effect.

In summary, quantitative sensitivity analyses found no evidence for misclassification bias of the exposure variable. However, some doubts about the risk of bias from misclassification of the exposure variable remain, particularly due to the inability to estimate the change in the mismeasurement of the income variables used in this thesis. Some (assumed small) risk of bias from misclassification or mismeasurement in the outcome and confounding variables also existed. So, misclassification bias of an unknown size possibly caused an underestimate of the treatment effect in this thesis.

As Larrimore has argued [78], the health effect of publicly funded financial credits could develop over the longer term, rather than be instant. Subsidiary analyses of this thesis estimated treatment effects with longer follow-up of the change in the outcome to the change in the exposure, finding comparable effect estimates as the main analyses. However, another type of long term change may also need to be considered, namely treatment with FTC and

IWTC *over several years* (without change in treatment status), such as the effect of a regime of ten years of consistent treatment with FTC. Fixed effects models are not suited to study this type of long-term treatment, because participants who do not change their treatment status are eliminated from the analysis. Thus, if the effect of FTC and IWTC on health is larger the longer an individual has consistently received this treatment over time, then the fixed effects regression analyses were not able to estimate this effect. As described in *Chapter 3*, marginal structural modelling methods can be used to study the constant (no change) treatment effects of FTC and IWTC over longer time periods, and it is suggested that such analyses are conducted in the future.

This study did not specifically set out to isolate individual causal pathways, through which FTC and IWTC may affect health. However, such information may be of interest for policy development, e.g., when policy makers aim to design a conditional financial credit intervention to activate a specific SDH, such as employment or up-take of health services. However, estimating the effect of the causal pathways between in-work tax credits and health may be impossible, because eligibility for the tax credits is determined by a complex combination of factors that can independently change over time [28, 29, 70]. Therefore, this thesis concentrated on generating the best estimate of the net health effects of FTC and IWTC through all causal pathways.

Furthermore, this thesis has not pursued any empirical analysis to explain the null-findings. It is theoretically plausible that IWTC interventions enhanced work-related stress due to recipients moving into unstable employment with negative working conditions or experiencing negative psychological effects such as feelings of guilt due to having to give their children into external child care during their working hours. Work-related stress may have cancelled out any potential positive effect from additional income from the tax credits, in line with previous theory [19]. Therefore, if the stress-reducing income effect from purchasing or having the ability to purchase stress-reducing goods and services was equal to or smaller than the stress-enhancing effect from moving into employment with unstable working conditions, then this would explain why IWTC interventions were found to have had no health effect. Therefore, the effect of IWTC on health could be differential by employment status and / or working condition. Because the SoFIE does not provide data on the nature of employment status (e.g., stable, unstable) and working conditions (e.g., exposure to work hazards), the potential differential effect of these factors on the health effect of IWTC cannot be tested empirically, and such analyses would also likely be underpowered. Therefore, this line of inquiry could not be pursued in this thesis, and the thesis raises the question: *Was the effect*

*of IWTC on health differential by employment status and / or working conditions?* However, to answer this question, extremely large sample sizes or more extreme changes in social conditions than those in SoFIE would be required.

The participants of this thesis were adult parents in families. FTC and IWTC arguably aimed to improve the lives and opportunities not only of parents, but also of children in families. The thesis does not provide any direct evidence on the effect of the credits in children. It is plausible that parents spent additional income from the tax credits on goods and services for improving their children's life and opportunities (see *Chapter 2*). However, it could be argued that any effect on children should have a spin-off or follow-on effect on their parents. Therefore, not finding an effect on parental health points towards a small or no effect on children - or a lack of translation of change in health in children to their parents. In addition, although the thesis showed that individual-level effects of FTC and IWTC were very small and not significant, it is nevertheless theoretically possible, albeit unlikely, that FTC and IWTC interventions may have had a larger effect on health at the population level, and this could be tested with population-level analyses.

Finally, some of the subsidiary analyses were imprecisely estimated. This included analyses that estimated the effect of the tax credits on psychological distress and current tobacco smoking; effect modification; and effect of the tax credits on health with long follow up of the outcome to the exposure (e.g., 4-6 year lag in analyses on FTC, 3-4 year lag in analyses on IWTC). Imprecisely estimated effects from the subsidiary analyses must be interpreted with caution. Furthermore, despite its large participant numbers and length of follow up, the thesis had insufficient statistical power to conduct sub-group analyses by important social factors, such as family type and the number of dependent children in the family, preventing the study from testing for treatment heterogeneity in relevant subgroups. Although it is theoretically plausible that FTC and IWTC may have had a large and positive effect on SRH in adults over the short term in specific subgroups, any effect of a large size would need to be concentrated in a small subgroup in order for it to not affect the overall effect estimate.

## **External validity**

This thesis analysed a panel of adults in families extracted from a random survey sample of households in private dwellings in New Zealand. The sampling frame included all permanent, private dwellings in New Zealand, with few exceptions such as dwellings located on off-shore islands other than the Waiheke Island and remote rural areas, as well as institutions such as

hospitals, prisons and foreign and New Zealand military dwellings. This suggests that the studied sample is widely generalisable to the New Zealand generally resident population, with the few noted exceptions.

The aims, design and, importantly, the economic, social and health context of the FTC and IWTC were unique to the country setting of New Zealand. Differences between the design of New Zealand's and other countries' anti-poverty and in-work tax credit for families intervention could explain that the thesis found no effect on SRH, when other studies may find considerable effects on health from equivalent interventions in other countries. For example, whereas New Zealand's IWTC universally covered low- and middle-income groups, the United States' and United Kingdom's in-work tax credit interventions for families exclusively target low-income families – this could result in different effects on health. Furthermore, as noted above, such contextual factors as the strength of a country's social protection floor may explain why no health effects are found in New Zealand, even when a similar financial credit design in another country may have a health benefit. "If the broad welfare state provision of public health and social services fulfil basic health needs for low- and middle-income families in New Zealand, but if these needs are not equally met by the lower safety nets provided to low-income families in the USA/UK, then additional income from IWTC should affect health less in New Zealand than in the USA/UK. For example, whereas low-income families can access several public health services free-of-charge in New Zealand, many low-income families in the United States do not receive health insurance through their employment package and cannot afford private health insurance, even with a substantial income boost from EITC." (p. 5) [30].

While we have tested the effect of the tax credits on SRH, psychological distress and current tobacco smoking, the findings from this thesis cannot necessarily be generalised to the effect of FTC and IWTC on other health outcomes. To be clear, it is thus of course possible that FTC and IWTC have positive effects on health and other social outcomes that have not been studied in this thesis. However, SRH is widely validated as a predictor of morbidity and mortality in a range of population groups [222, 226-228, 250], but the longitudinal validity of the measure has been questioned [224]. However, if it was unrealistic in the first place to expect considerable changes in SRH as the result of increases in IWTC (especially over the short term), then this thesis was arguably an 'unfair test' of the credits' health impact (Professor N. Krieger, personal communication, 23 October 2012). It could further be argued that using as the outcome variable an aspect of health that IWTC is likely to remedy, such as stress from being financially under resourced, would be a more appropriate test (Professor N. Krieger, personal communication, 23 October 2012). While these are valid

points, I note that other publicly funded financial credits, such as the *NYC Opportunity - Family Rewards*, have achieved an effect on SRH, over a two-year period [153], which may provide some validation for the choice of SRH as the main outcome variable for this thesis.

The findings of this thesis were specific to the economic context of the 2002-09 study period. The FTC intervention was less prone to changes to the economic context, because these credits were not dependent on economic variables (except for family income). So, in times of economic crisis, FTC was still paid to families falling within the income bounds defined by the number of children in the family. However, the effectiveness of IWTC is dependent on the degree of labour demand, i.e. the *opportunity* to leave welfare. These credits may have influenced the supply of labour force by moving welfare recipients into the labour force, but they did not have a direct effect on demand for labour. In times of high labour demand, individuals in families seeking employment may be able to secure employment, whereas when labour demand is low, only individuals with a labour advantage (e.g., more highly skilled individuals) may be able to find employment. In other words, in times of low labour demand those who cannot secure employment may incur a loss of income from employment, plus a loss of additional income from the IWTC. The study period of this thesis was characterised by high labour demand - even at the latest wave (Wave 7, 2009), despite the global financial crisis hitting New Zealand in 2008, the demand for labour remained high, indicated by unemployment rates remaining low until after 2009. Therefore, the findings are for FTC and IWTC, as implemented during a time of high labour demand, and these findings cannot be generalised to other time periods with different economic contexts.

In summary, the main analyses of this thesis are generalisable to the general population of New Zealand. However, the findings may not necessarily be generalised to other health outcomes, countries and time periods with different economic contexts.

## **Implications for policy**

Policy makers, politicians and disadvantaged communities share a strong interest in identifying policy tools for addressing the SDH to improve health and health equity [20]. Publicly funded financial credit are social protection policy interventions that have been proposed as such policy tools [21]. The Commission on Social Determinants of Health called for social protection over the life course (see Chapter 8 in [20]). Similarly, the Social Protection Floor Initiative, a coalition of international organisations lead jointly by the International Labour Organisation and World Health Organisation, has recommend “social transfers in cash

or in kind, to ensure income security, food security, adequate nutrition, and access to essential services” [55]. Experts have recently argued the case for “greater health sector involvement in the design, implementation and evaluation of such [financial credit] schemes” (p. 551) [21].

There is considerable and conclusive evidence showing that financial credits conditional on up-take of social and health services improve health status in adults in low- and middle-income countries (predominantly of Central and South America) [76]. However, I argue that even this relatively strong evidence base for applying conditional cash credit interventions in low-income countries does not necessarily imply that these types of credits are effective and cost-effective policy tools. If the health-promoting effect of these conditional financial credits acted (primarily) through the up-take of social and health services (which may be the case [76]), then government investments in financial credits may be less (cost-)effective than initiatives that directly improve coverage with or up-take of social and health services. In my opinion, this casts considerable doubts on the (cost-)effectiveness of financial credits even in low- and middle-income countries, potentially making the case for *less* involvement of the health sector in these credits in low- and middle-income countries, at least until their (cost-)effectiveness is established.

The evidence base for the effect of publicly funded financial credits on health in high-income country is thin [23-25]. Lucas *et al.*'s systematic review concluded that “on the basis of current evidence we cannot state unequivocally whether financial benefits delivered as an intervention are effective at improving child health or well-being in the short term” (p. 1) [24]. Similarly, neither the systematic review, nor the empirical evidence of this thesis found any evidence for any discernible effect of in-work tax credits on health status in parents, whether in the United States or in New Zealand. Researchers have previously drawn attention to the lack of evidence for reliable conclusions about applying financial credits to improve health and health equity in high-income countries and have cautioned policy actors about drawing premature conclusions [23-25]. I echo the call for caution about advocating for and implementing publicly funded financial credits to improve health in high-income countries. To improve the evidence base for financial credit interventions (as the foundation for policy and decision making), I call for additional research on financial credits and their impact on health in high-income countries. Future research priorities are outlined in the next section.

Not only the lack of evidence should caution policy makers about using publicly funded financial credits as tools for improve health equity, but also some of the conceptual evidence presented in this thesis. As noted above, if publicly funded financial credits have no effect on

their recipients and a positive effect on non-recipients, these expensive policy interventions funded by tax payer money could potentially *generate* health inequalities [27].

As the policy agenda of addressing the SDH and health equity competes with other health sector policy agendas, it needs to attract considerable political support to have a chance to succeed [20]. Politicians and the citizenry closely scrutinize the cost-effectiveness of policies tools in an attempt to secure the best possible outcomes for the general and disadvantaged populations from public funds paid by the tax payers. The cost-effectiveness of policy tools is therefore one key criterion for assessing the performance and usefulness of a policy tool in achieving its outcomes. Publicly funded financial credits for assisting families are often costly interventions. During the 2011 tax year alone, the New Zealand government spent an estimated \$2,139 million on FTC and \$567 million on IWTC, totalling 24.9% of all public annual expenditure on social assistance in that year [252]. This thesis suggests that these large public investments may not increase health status in parents.

To be clear, the thesis findings of no health effect do not detract from the success of the FTC and IWTC interventions in increasing income (and thereby reducing income poverty) and improving employment [18]. Thus, other research suggests that they have achieved their goals. However, from a health perspective, these improvements in the SDH appear to not translate into health, at least in parents and over the short term.

Taken together, the evidence presented in this thesis makes the case for *less*, if any, involvement of the health sector in funding, planning, designing, implementing and evaluating these social assistance credits, at least in high-income countries. If the overall goal of the health sector is to improve health (and those SDH that *do* improve health), then health sector workers are perhaps best advised to refocus their efforts and resources on policy tools that have established health benefits.

## **Future research**

The Commission on Social Determinants of Health [20] and the Sixty-Second [44] and Sixty-Fifth [46] World Health Assemblies have called for research into the political context of health and health equity. Political epidemiology is an emerging, but fast developing sub-discipline of epidemiology that occupies this research domain. Political epidemiology has considerable promise and will without doubt expand considerably conceptually and methodologically over the years to come. At the current point, this sub-discipline requires additional definitional, conceptual, theoretical and methodological development [9, 35, 41, 42, 66, 79-81]. Especially

high-quality studies assessing the impact of individual policies are required, if the goal is to provide evidence that policy makers and affected communities can utilise to improve health and health equity.

One important field of political epidemiological scholarship that requires further attention is research assessing the health effects of publicly funded financial credits interventions. Considerable evaluation research has been conducted on publicly funded financial credits conditional on up-take of social and health services in low- and middle-income countries, mostly of Central and South America. Additional research is required on such credits in low- and middle-income countries from other regions, especially Africa and the Eastern Mediterranean, where the World Bank and other international organisations have established several such schemes with local partners. Moreover, empirical and systematic review evidence is currently lacking on unconditional credits in low- and middle-income countries, such as the Government of Pakistan's *Benazir Income Support Programme* [253].

However, most under researched is the impact on health of financial credits in high-income countries [9, 23-25, 27, 70]. Only few studies have been conducted to this date on the effect of conditional financial credits on health, and even fewer on the effect of unconditional financial credits (such as this thesis investigating FTC) in high-income countries. Additional empirical and systematic review evidence is required on both conditional and unconditional financial credits in high-income countries. Several areas require special attention, namely the effects of different: conditionalities (e.g., conditional versus unconditional); policy objectives (e.g., poverty reduction, uptake of employment or increased service utilisation); universalism vs. targeted needs-testing; levels of generosity; and policy implementation and delivery (e.g., paid in cash versus paid as a tax return; delivered by agency responsible for social assistance benefits versus delivered through the tax system). Furthermore, subgroup analyses by such factors as ethnicity, gender, age and education, as well as income level, family type and number of dependent children in the family are required to provide a more nuanced understanding. Furthermore, studies of the effect of publicly funded financial credits over longer time periods and from different country settings; economic, social and health context; and time periods are required.

Some study designs may be particularly helpful in advancing empirical evidence on health effects of publicly funded financial credits in high-income countries. Randomised controlled trials of publicly funded financial credits could be conducted in high-income countries to improve the current evidence base [29, 71, 73]. The ethical, political and practical feasibility of such experiments has been demonstrated [153]. Although it has proven difficult to find

suitable policy experiments to estimate the effect of publicly funded financial credits on health in high-income countries, quasi-experimental study designs are the next best level of evidence. Cohort studies are a feasible and promising study design, as demonstrated in not only this thesis, but also previous studies [84, 85].

Minimising misclassification of the exposure variable may be one of the methodological issues that future research on financial credits may want to tackle. Linkage of health surveys to administrative data on receipt of publicly funded financial credits such as individual tax records could provide one promising research avenue in this regard, especially considering self-reported survey data is likely to be biased by the low knowledge of the receipt of such credits in their recipients [254]. While current data limitations often prohibit studying the long-term impact of publicly funded financial credits, data linkage may create the opportunity to follow participants beyond the end of a panel survey. Such data linkage is currently undertaken by Statistics New Zealand for the SoFIE, providing considerable promise for future analyses of the SoFIE.

Several research avenues can also be pursued in direct follow-up of this thesis study of the effect of FTC and IWTC on SRH in parents in New Zealand on the individual level over the short term. Marginal structural models could be conducted to explore the degree of bias, if any, due to including in the fixed effects regression anal variables that could simultaneously time-varying confounding and mediating variables. Analyses could also be conducted to study the cumulative effect of FTC and IWTC over longer time periods, such as treatment with FTC over the full seven years of the SoFIE.

## **Conclusions**

This thesis found no evidence that becoming eligible for FTC or IWTC and an increase by \$1,000 in FTC or IWTC had any effect on self-rated health in working-age parents in New Zealand, on the individual level and over the short run. These findings withstood rigorous testing through multiple sensitivity analyses. They are of interest to policy makers in New Zealand and in other high-income countries aiming to identifying effective policy tools for addressing the SDH to improve health and health equity. The findings caution against health sector involvement and investment in the types of anti-poverty and in-work tax credits studied in this thesis for the purpose of improving adult general health status and health equity in New Zealand and comparable high-income countries.

## References

1. Porta, M., S. Greenland, and J.M. Last, *A dictionary of epidemiology*. 5th ed. 2008, Oxford, United Kingdom, and New York, NY: Oxford University Press.
2. Black, J., M. Hashimzade, and G. Miles, *A dictionary of economics*. 3rd ed. 2009, New York, NY: Oxford University Press.
3. Whitehead, M., *The concepts and principles of equity in health*. 1992, World Health Organization Regional Office for Europe: Copenhagen, Denmark
4. Morris, J.N., et al., *A minimum income for healthy living*. *Journal of Epidemiology & Community Health*, 2000. **54**(12): p. 885-9.
5. Morris, J.N., *Commentary: Minimum incomes for healthy living: Then, now - and tomorrow?* *International Journal of Epidemiology*, 2003. **32**(4): p. 498-9.
6. Morris, J.K., *Are we promoting health?* *Lancet*, 2002. **359**(9317): p. 1622.
7. Morris, J.N., et al., *Action towards healthy living - for all*. *International Journal of Epidemiology*, 2010. **39**: p. 266-73.
8. Morris, J.N., et al., *Defining a minimum income for healthy living (MIHL): Older age, England*. *International Journal of Epidemiology*, 2007. **36**(6): p. 1300-7.
9. Pega, F., et al., *Politics, policies and population health: A commentary on Mackenbach, Hu and Looman (2013)*. *Social Science & Medicine*, 2013. **93**: p. 176-9..
10. Malcolm Wiener Center for Social Policy. *Malcolm Wiener Center for Social Policy*. n.d. [cited 2011 August 2]; Available from: <http://www.hks.harvard.edu/centers/wiener>.
11. United Nations Research Institute for Social Development, *Combating poverty and inequality: Structural change, social policy and politics*. 2010, United Nations Research Institute for Social Development: Geneva, Switzerland.
12. Esping-Andersen, G., *The three worlds of welfare capitalism*. 1990, Cambridge, United Kingdom: Polity Press.
13. Organization for Economic Co-operation and Development, *Society at a glance 2011: OECD social indicators*. 2011, Organization for Economic Co-operation and Development: Paris, France.
14. The World Bank, *Global monitoring report 2011: Improving the odds of achieving the MDGs*. 2011, The World Bank: Washington, United States.
15. Organization for Economic Co-operation and Development, *Divided we stand: Why inequalities keep rising*. 2011, Organization for Economic Co-operation and Development: Paris, France.
16. United Nations Development Program, *Human development report 2011: Sustainability and equity: A better future for all*. 2011, United Nations Development Program: New York, NY.
17. International Labour Organization, *Global employment trends 2012: Preventing a deeper job crisis*. 2012, International Labour Organization: Geneva, Switzerland.
18. Immervoll, H. and M. Pearson, *A good time for making work pay? Taking stock of in-work benefits and related measures across the OECD*. *OECD Social, Employment and Migration Working Paper No. 81*. 2009, Organization of Economic Co-operation and Development: Paris, France.
19. Lundberg, O., et al., *The potential power of social policy programmes: Income redistribution, economic resources and health*. *International Journal of Social Welfare*, 2010. **19**: p. SUPPL. 1.
20. Commission on Social Determinants of Health, *Closing the gap in a generation: Health equity through action on the social determinants of health*. 2008, World Health Organization: Geneva, Switzerland.

21. Forde, I., K. Rasanathan, and R. Krech, *Cash transfer schemes and the health sector: Making the case for greater involvement*. Bulletin of the World Health Organization, 2012. **90**(7): p. 551-3.
22. Cookson, R. *Should disadvantaged people be paid to take care of their health? Yes*. British Medical Journal, 2008. **337**: p. a589.
23. Lucas, P.J., *Payment to look after health: Family payments: A cautionary tale for policy makers*. British Medical Journal, 2008. **337**: p. a1134.
24. Lucas, P.J., et al. *Financial benefits for child health and well-being in low income or socially disadvantaged families in developed world countries*. Cochrane Database of Systematic Reviews, 2008. **2**: CD006358.
25. Ashcroft, R.E., T.M. Marteau, and A. Oliver, *Payment to look after health: Incentive mechanisms require deeper understanding*. British Medical Journal, 2008. **337**: p. a1135.
26. Westin, S. *Welfare for all - or only for the needy?* Lancet, 2008. **372**(9650): p. 1609-10.
27. Pega, F., et al., *The explanation of a paradox? A commentary on Mackenbach with perspectives from research on financial credits and risk factor trends*. Social Science & Medicine, 2012. **75**(4): p. 770-3.
28. Pega, F., et al., *In-work tax credits for families and their impact on health status in adults [Protocol]*. Cochrane Database of Systematic Reviews, 2012. **7**: CD009963.
29. Pega, F., et al., *In-work tax credits for families and their impact on health status in adults [Review]*. Cochrane Database of Systematic Reviews, 2013. **8**: CD009963.
30. Pega, F., et al., *The impact of in-work tax credit on self-rated health in adults: A cohort study of 6,900 New Zealanders*. Journal of Epidemiology & Community Health, 2013. **67**(8): p. 682-8.
31. World Health Organization. *Social determinants of health: Key concepts*. 2013 [cited 2013 July 12]; Available from: [http://www.who.int/social\\_determinants/en/](http://www.who.int/social_determinants/en/).
32. Krieger, N., *A glossary for social epidemiology*. Journal of Epidemiology & Community Health, 2001. **55**(10): p. 693-700.
33. Solar, O. and A. Irvine, *A conceptual framework for action on the social determinants of health. Discussion paper*. 2007, World Health Organization: Geneva, Switzerland.
34. Kelly, M., et al., *The social determinants of health: Developing an evidence base for political action*. 2007, Measurement and Evidence Knowledge Network, World Health Organization: Geneva, Switzerland.
35. Navarro, V., et al., *Politics and health outcomes*. Lancet, 2006. **368**(9540): p. 1033-7.
36. Bitler, M., J. Gelbach, and H.W. Hoynes, *Welfare reform and health*. Journal of Human Resources, 2005. **40**(2): p. 309-334.
37. Navarro, V. and L. Shi, *The political context of social inequalities and health*. Social Science & Medicine, 2001. **52**(3): p. 481-91.
38. Braveman, P. and S. Gruskin, *Defining equity in health*. Journal of Epidemiology & Community Health, 2003. **57**(4): p. 254-8.
39. Hurrelmann, K., K. Rathmann, and K. Richter, *Health inequalities and welfare state regimes: A research note*. Journal of Public Health, 2011. **19**(1): p. 3-13.
40. Muntaner, C., et al., *A macro-level model of employment relations and health inequalities*. International Journal of Health Services, 2010. **40**(2): p. 215-21.
41. Lundberg, O., *Commentary: Politics and public health - some conceptual considerations concerning welfare state characteristics and public health outcomes*. International Journal of Epidemiology, 2008. **37**(5): p. 1105-8.
42. Lundberg, O., *Politics, public health and pessimism: Should we take studies on welfare states and public health further? A commentary on Tapia Granados*. Social Science & Medicine, 2010. **71**(5): p. 851-2.

43. Marmot, M., *Social determinants of health inequalities*. Lancet, 2005. **365**(9464): p. 1099-104.
44. The Sixty-Second World Health Assembly, *Reducing health inequities through action on the social determinants of health: Resolution WHA62.14*. 2009, World Health Organization: Geneva, Switzerland.
45. World Health Organization, *Rio Political Declaration on Social Determinants of Health*. 2011, World Health Organization: Rio de Janeiro, Brazil.
46. The Sixty-Fifth World Health Assembly, *Outcome of the World Conference on Social Determinants of Health: Resolution WHA65.8*. 2012, World Health Organization: Geneva, Switzerland.
47. Marmot, M., et al., *WHO European review of social determinants of health and the health divide*. Lancet, 2012. **380**(9846): p. 1011-29.
48. Pega, F. and K. Carter, *Social protection policy transition and its effect on child health and health inequalities in a liberal welfare state (New Zealand), 2005-08: Report to the GDP, Taxes, Income and Welfare Task Group of the Review of Social Determinants and the Health Divide in the WHO Euro Region*. 2012, University of Otago: Wellington, New Zealand.
49. Mackenbach, J.P., *Can we reduce health inequalities? An analysis of the English strategy (1997-2010)*. Journal of Epidemiology & Community Health, 2011. **65**(7): p. 568-75.
50. Bambra, C., *Reducing health inequalities: New data suggest that the English strategy was partially successful*. Journal of Epidemiology & Community Health, 2012. **66**(7): p. 662.
51. Matheson, D., K. Rasanathan, and M. Tobias, *Health inequalities: Unfair, measurable and remediable? The case of New Zealand*. 2007, World Health Organization: Geneva, Switzerland.
52. Pega, F., N. Valentine, and D. Matheson, *Monitoring social well-being to support policies on the social determinants of health: The case of New Zealand's "Social Reports / Te Purongo Oranga Tangata"*. Social Determinants of Health Discussion Paper 3. 2010, World Health Organization: Geneva, Switzerland.
53. Pega, F., et al., *Public social monitoring reports and their effect on a policy programme aimed at addressing the social determinants of health to improve health equity in New Zealand*. Social Science & Medicine, In Press.
54. Blakely, T. and K. Carter, *Reflections from the antipodes on the English strategy to reduce health inequalities and Mackenbach's analysis*. Journal of Epidemiology & Community Health, 2011. **65**(7): p. 594-5.
55. The Social Protection Floor. *A social protection floor for all*. n.d. [cited 2013 14 May]; Available from: <http://www.socialprotectionfloor-gateway.org/index.html>.
56. The World Bank. *Social protection & labour*. 2013 [cited 2013 May 26]; Available from: <http://web.worldbank.org/WBSITE/EXTERNAL/TOPICS/EXTSOCIALPROTECTION/0,,menuPK:282642~pagePK:149018~piPK:149093~theSitePK:282637,00.html>.
57. Navarro, V., *The political and social contexts of health*. 2004, Amityville, NY: Baywood Publishing.
58. Lundberg, O., et al., *The Nordic experience: Welfare states and public health. Health Equity Studies No. 12*. 2008, Centre for Health Equity Studies: Stockholm, Sweden.
59. Marmot, M., et al., *Fair society, healthy lives: The Marmot review*. 2010, The Marmot Review.
60. Glennerster, H., et al., *The report of the Social Protection Task Force*. 2010, Institute of Health Equity, University College London: London, United Kingdom.

61. World Health Organization Regional Office for Europe, *Interim second report on social determinants of health and the health divide in the WHO European Region*. 2012, World Health Organization Regional Office for Europe: Copenhagen, Denmark.
62. Imlach Gunasekara, F., *Rich and well, poor and sick? The relationship between income and self-rated health from the New Zealand household panel Survey of Family, Income and Employment (SoFIE)*. 2010, University of Otago: Dunedin, New Zealand.
63. Imlach Gunasekara, F., et al., *The relationship between income and health using longitudinal data from New Zealand*. *Journal of Epidemiology & Community Health*, 2012. **66**(6): p. e12.
64. Dalgety, J., *Changing families' financial support and incentives for working: The summary report of the evaluation of the Working For Families package*. 2010, Ministry of Social Development and Inland Revenue Department: Wellington, New Zealand.
65. Heymann, S.J., *Health and social policy*, in *Social epidemiology*, L.F. Berkman and I. Kawachi, Editors. 2000, Oxford, United Kingdom: Oxford University Press. p. 368-382.
66. Muntaner, C., et al., *Politics or policies vs politics and policies: A comment on Lundberg*. *International Journal of Epidemiology*, 2010. **39** (5): p. 1396-7.
67. Beckfield, J. and N. Krieger, *Epi + demos + cracy: Linking political systems and priorities to the magnitude of health inequities: Evidence, gaps and a research agenda*. *Epidemiologic Reviews*, 2009. **31**: p. 152-77.
68. Bamba, C., et al., *Tackling the wider social determinants of health and health inequalities: Evidence from systematic reviews*. *Journal of Epidemiology & Community Health*, 2010. **64** (4): p. 284-91.
69. Ostlin, P., et al., *Priorities for research on equity and health: Towards an equity-focused health research agenda*. *PLoS Medicine*, 2011. **8**: p. e1001115.
70. Waldfoegel, J., et al., *Commentary on 'Financial benefits for child health and well-being in low-income or socially disadvantaged families in developed world countries' with a response from the review author*. *Evidence-Based Child Health: A Cochrane Review Journal*, 2009. **4**(2): p. 1138-9.
71. Connor, J., A. Rodgers, and P. Priest, *Randomised studies of income supplementation: A lost opportunity to assess health outcomes*. *Journal of Epidemiology & Community Health*, 1999. **53**(11): p. 725-30.
72. Gibson, M., et al. *Welfare to work interventions and their effects on health and well-being of lone parents and their children [Protocol]*. *Cochrane Database of Systematic Reviews*, 2012. **5**: CD009820.
73. Skivington, K., et al., *Challenges in evaluating Welfare to Work policy interventions: Would an RCT design have been the answer to all our problems?* *BMC Public Health*, 2010. **10**: p. 254.
74. Secretaria de Desarrollo Social. *Oportunidades, a human development program*. 2010 [cited 2013 June 19]; Available from: [http://www.oportunidades.gob.mx/Portal/wb/Web/oportunidades\\_a\\_human\\_development\\_program](http://www.oportunidades.gob.mx/Portal/wb/Web/oportunidades_a_human_development_program).
75. International Labour Organization, *Bolsa Familia in Brazil: Context, concept and impacts*. 2009, Social Security Department, International Labour Organization: Geneva, Switzerland.
76. Lagarde, M., A. Haines, and N. Palmer, *The impact of conditional cash transfers on health outcomes and use of health services in low and middle income countries*. *Cochrane Database of Systematic Reviews*, 2009. **4**: CD008137.
77. Popay, J., *Should disadvantaged people be paid to take care of their health? No*. *British Medical Journal*, 2008. **337**: p. a594.

78. Larrimore, J., *Does a higher income have positive health effects? Using the earned income tax credit to explore the income-health gradient*. *Milbank Quarterly*, 2011. **89**(4): p. 694-727
79. Lundberg, O., *Authors' response: Politics and public health - Some conceptual considerations concerning welfare state characteristics and public health outcomes*. *International Journal of Epidemiology*, 2010. **39**(2): p. 632-4.
80. Espelt, A., et al. *Answer to the commentary: Politics and public health--some conceptual considerations concerning welfare state characteristics and public health outcomes*. *International Journal of Epidemiology*, 2010. **39**(2): p. 630-2.
81. Judge, K., *Politics and health: Policy design and implementation are even more neglected than political values?* *European Journal of Public Health*, 2008. **18**(4): p. 355-6.
82. Ministry of Social Development, *The Social Report 2010: Te Purongo Oranga Tangata*. 2010, Ministry of Social Development: Wellington, New Zealand.
83. Tobias, M., et al., *Changing trends in indigenous inequalities in mortality: Lessons from New Zealand*. *International Journal of Epidemiology*, 2009. **38**(6): p. 1711-22.
84. Averett, S. and Y. Wang, *The effect of the EITC payment expansion on maternal smoking*. *Discussion Paper No. 6680*. 2012, Institute for the Study of Labor: Bonn, Germany.
85. Averett, S. and Y. Wang, *The effects of Earned Income Tax Credit payment expansion on maternal smoking*. *Health Economics*, 2012. **22**: p. 1344-59.
86. United Nations Economic and Social Council, *Economic, social and cultural rights: Implementation of existing human rights norms and standards in the context of the fight against extreme poverty*. 2003, United Nations Economic and Social Council: Geneva, Switzerland.
87. Cebulla, A., et al., *Welfare-to-work: New Labour and the US experience*. 2005, Burlington, VT: Ashgate Publishing Limited.
88. Paz-Fuchs, A., *Welfare to work: Conditional rights in social policy*. 2008, Oxford, United Kingdom, and New York, NY: Oxford University Press.
89. Saunders, P., *Welfare to work in practice: Social security and participation in economic and social life*. 2005, Burlington, VT: Ashgate Publishing Limited.
90. Fiszbein, A., et al., *Conditional cash transfers: Reducing present and future poverty*. 2009, The International Bank for Reconstruction and Development / The World Bank: Washington, DC.
91. Holtzblatt, J. and J.B. Liebmann, *The earned income tax credit abroad: Implications of the British working families tax credit for pay-as-you-earn administration*. *Proceedings of the National Tax Association*, 1999: p. 198-207.
92. Nolan, P., *New Zealand's family assistance tax credits: Evolution and operation*. *New Zealand Treasury Working Paper*. 2002, The Treasury: Wellington, New Zealand.
93. Blakely, T., et al., *Tracking disparity: Trends in ethnic and socioeconomic inequalities in mortality, 1981–2004*. *Public Health Intelligence Occasional Bulletin No. 38*. 2007, Ministry of Health: Wellington, New Zealand.
94. Farlex, *The Free Dictionary*. *Rebate*. n.d. [cited 2013 June 20]; Available from: <http://www.thefreedictionary.com/rebate>.
95. Organization for Economic Co-operation and Development, *OECD family database: Child poverty*. 2012, Organization of Economic Co-operation and Development: Paris, France.
96. World Health Organization, *World Health Statistics 2006*. 2006, World Health Organization: Geneva, Switzerland.
97. Ministry of Health and University of Otago, *Decades of disparity III: Ethnic and socioeconomic inequalities in mortality, New Zealand 1981–1999*. *Public Health*

- Intelligence Occasional Bulletin No. 31*. 2006, Ministry of Health: Wellington, New Zealand.
98. *Taxation (Working for Families) Act*, in *New Zealand Statutes*. 2004: New Zealand.
  99. Ministry of Social Development, *Working For Families*. Rise, 2007. **1**: p. 14-5.
  100. Work and Income and Inland Revenue Department. *Working For Families timeline*. n.d. [cited 2013 June 21]; Available from: <http://www.workingforfamilies.govt.nz/timeline/>.
  101. Inland Revenue Department. *Working For Families: Receiving and managing your payments*. 2008 [cited 2013 17 October]; Available from: <http://www.ird.govt.nz/wff-tax-credits/payments/>.
  102. Work and Income. Are you already getting income support? n.d. [cited 2013 October 17]; Available from: <http://www.workandincome.govt.nz/individuals/brochures/help-for-kinship-carers/are-you-already-getting-income-support.html>.
  103. Inland Revenue Department, Working for Families Tax Credits - number of families entitled, 2001 to 2010 (,000). n.d. [cited 2013 June 21]; Available from: [www.ird.govt.nz/aboutird/exteranl-stats/social-policy/wfftc](http://www.ird.govt.nz/aboutird/exteranl-stats/social-policy/wfftc).
  104. Inland Revenue Department. Working For Families tax credits: Family Tax Credit. 2012 [cited 2013 26 April]; Available from: <http://www.ird.govt.nz/wff-tax-credits/entitlement/what-is-wfftc/ftc>.
  105. James, S.R., *A dictionary of taxation*. 2012, Cheltenham, United Kingdom: Edward Elgar Publishing.
  106. Inland Revenue Department. Working For Families tax credits: In-Work Tax Credit. 2011 [cited 2013 May 23]; Available from: <http://www.workingforfamilies.govt.nz/tax-credits/in-work-tax-credit.html>.
  107. Montoya, I.D. and V.L. Brown, *The association between EIC receipt and employment in a sample of drug using and non-drug using TANF recipients*. *American Journal of Drug and Alcohol Abuse*, 2006. **32**(2): p. 189-201.
  108. *Human Rights Act*, in *New Zealand Statutes*. 1993: New Zealand.
  109. Child Poverty Action Group. *Updates on the case and campaign*. n.d. [cited 2013 May 23]; Available from: <http://www.cpag.org.nz/infocus/help-fight-the-injustice-of-discrimination/updates-on-the-case-and-campaign/>.
  110. Lorenc, T., et al., *What types of interventions generate inequalities? Evidence from systematic reviews*. *Journal of Epidemiology & Community Health*, 2013. **67**(2): p. 190-3.
  111. The Treasury. *Core Crown expense tables: New Zealand superannuation and welfare benefit*. n.d. [cited 2013 14 May]; Available from: <http://www.treasury.govt.nz/budget/forecasts/befu2012/080.htm>.
  112. Ludbrook, A. and K. Porter, *Do interventions to increase income improve the health of the poor in developed economies and are such policies cost effective?* *Applied Health Economics and Health Policy*, 2004. **3**(2): 115-20.
  113. Benzeval, M. and K. Judge, *Income and health: The time dimension*. *Social Science & Medicine*, 2001. **52**(9): p. 1371-90.
  114. Imlach Gunasekara, F.I., K. Carter, and T. Blakely, *Change in income and change in self-rated health: Systematic review of studies using repeated measures to control for confounding bias*. *Social Science & Medicine*, 2011. **72**(2): p. 193-201.
  115. Gao, Q., N. Kaushal, and J. Waldfogel, *How have expansions in the earned income tax credit affected family expenditures?*, in *Welfare reform and its long term consequences for America's poor*, J.P. Ziliak, Editor. 2009, Cambridge, United Kingdom: Cambridge University Press. p. 104-139.
  116. Gregg, P., S. Harkness, and S. Smith, *Welfare reform and lone parents in the UK*. *Economic Journal*, 2009. **119**(535): p. F38-F65.

117. Kaushal, M., Q. Gao, and J. Waldfogel, *Welfare reform and family expenditures: How are single mothers adapting to the new welfare and work regime?* *Social Service Review*, 2007. **81**(3): p. 369-96.
118. Greenland, S., *Quantifying biases in causal models: Classical confounding vs collider-stratification bias.* *Epidemiology*, 2003. **14**(3): p. 300-6.
119. Sen, A., *Poverty and famine: An essay on entitlement and deprivation.* 1982, Oxford, United Kingdom: Oxford University Press.
120. Sen, A., *Inequality reexamined.* 1992, Oxford, United Kingdom: Oxford University Press.
121. Gorman, M., *Commentary: Defining a minimum income for healthy living (MIHL): Older age, England - A comment on implications for application in the developing world.* *International Journal of Epidemiology*, 2007. **36**(6): p. 1307-8.
122. Waldfogel, J., *Welfare reform and child wellbeing in the US and the UK.* 2007, Centre for Analysis of Social Exclusion, London School of Economics: London, United Kingdom.
123. Farrell, C. and W. O'Connor, *Low income families and household spending. Department of Work and Pension Research Report No. 192.* 2003, TCO: London, United Kingdom.
124. Borjas, G.J., *Labor economics.* 6th ed. 2013, New York, NY: McGraw-Hill Irwin.
125. Schuring, M., et al., *The effect of re-employment on perceived health.* *Journal of Epidemiology & Community Health*, 2011. **65**(7): p. 639-44.
126. Benach, J., et al., *A micro-level model of employment relations and health inequalities.* *International Journal of Health Services*, 2010. **40**(2): p. 223-7.
127. Benach, J., et al., *Six employment conditions and health inequalities: A descriptive overview.* *International Journal of Health Services*, 2010. **40**(2): p. 269-80.
128. Joyce, K., et al., *Flexible working conditions and their effects on employee health and wellbeing.* *Cochrane Database of Systematic Reviews*, 2010. **2**: CD008009.
129. Sjöberg, O., *Social insurance as a collective resource: Unemployment benefits, job insecurity and subjective well-being in a comparative perspective.* *Social Forces*, 2010. **88**(3): p. 1281-304.
130. Brennenstuhl, S., A. Quesnel-Vallée, and P. McDonough, *Welfare regimes, population health and health inequalities: A research synthesis.* *Journal of Epidemiology & Community Health*, 2012. **66**(5): p. 397-409.
131. Muntaner, C., et al., *Politics, welfare regimes and population health: Controversies and evidence.* *Sociology of Health & Illness*, 2011. **33**(6): p. 946-64.
132. Navarro, V., *Politics and health: A neglected area of research.* *European Journal of Public Health*, 2008. **18**(4): p. 354-5.
133. Lundberg, O., et al., *The role of welfare state principles and generosity in social policy programmes for public health: An international comparative study.* *Lancet*, 2008. **372**(9650): p. 1633-40.
134. Eikemo, T.A. and C. Bambra, *The welfare state: A glossary for public health.* *Journal of Epidemiology & Community Health*, 2008. **62**(1): p. 3-6.
135. Chung, H. and C. Muntaner, *Welfare state matters: A typological multilevel analysis of wealthy countries.* *Health Policy*, 2007. **80**(2): p. 328-39.
136. Chuang, Y.C., et al., *Welfare state regimes, infant mortality and life expectancy: Integrating evidence from East Asia.* *Journal of Epidemiology & Community Health*, 2012. **66**(7): p. e23.
137. Lynch, J., et al., *Income inequality, the psychosocial environment and health: Comparisons of wealthy nations.* *Lancet*, 2001. **358**(9277): p. 194-200.

138. Granados, J.A., *Politics and health in eight European countries: A comparative study of mortality decline under social democracies and right-wing governments*. *Social Science & Medicine*, 2010. **71**(5): p. 841-50.
139. Chen, B. and M. Cammett, *Informal politics and inequity of access to health care in Lebanon*. *International Journal of Equity Health*, 2012. **11**: p. 23.
140. Lin, R.T., et al., *Political and social determinants of life expectancy in less developed countries: A longitudinal study*. *BMC Public Health*, 2012. **12**: p. 85.
141. Mackenbach, J., F.B. Hu, and C.W.N. Looman, *Democratization and life expectancy in Europe, 1960-2008*. *Social Science & Medicine*, 2013. **93**: p. 166-75.
142. Card, D. and A.B. Krueger, *Minimum wages and employment: A case study of the fast-food industry in New Jersey and Pennsylvania*. *American Economic Review*, 1994. **84**(4): p. 774-5.
143. Angrist, J. and A. Krueger, *Does compulsory school attendance affect schooling and earnings?* *The Quarterly Journal of Economics*, 1991. **106**(4): p. 979-1014.
144. Coburn, D., *Beyond the income inequality hypothesis: Class, neo-liberalism, and health inequalities*. *Social Science & Medicine*, 2004. **58**(1): p. 41-56.
145. Webb, P., C. Bain, and S. Pirozzo, eds. *Essential epidemiology*. 2005, Cambridge, United Kingdom: Cambridge University Press.
146. Oakley, A., et al., *Using random allocation to evaluate social interventions: Three recent U.K. examples*. *Annals of the American Academy of Political and Social Science*, 2003. **589**: p. 170-189.
147. Barber, S.L. and P.J. Gertler, *The impact of Mexico's conditional cash transfer programme, Oportunidades, on birthweight*. *Tropical Medicine & International Health*, 2008. **13**(11): p. 1405-14.
148. Leroy, J.L., et al., *The Oportunidades program increases the linear growth of children enrolled at young ages in urban Mexico*. *Journal of Nutrition*, 2008. **138**(4): p. 793-8.
149. Fernald, L.C., P.J. Gertler, and L.M. Neufeld, *10-year effect of Oportunidades, Mexico's conditional cash transfer programme, on child growth, cognition, language, and behaviour: a longitudinal follow-up study*. *Lancet*, 2009. **374**(9706): p. 1997-2005.
150. Fernald, L.C., P.J. Gertler, and X. Hou, *Cash component of conditional cash transfer program is associated with higher body mass index and blood pressure in adults*. *Journal of Nutrition*, 2008. **138**(11): p. 2250-7.
151. The City of New York. *Opportunity NYC*. 2013 [cited 2013 June 19]; Available from: <http://opportunitynyc.org/>.
152. Riccio, J., *Sharing lessons from the first conditional cash transfer program in the United States*. 2010, National Poverty Center, University of Michigan: Ann Arbor, MI.
153. Riccio, J., et al., *Toward reduced poverty accross generations: Early findings from New York City's conditional cash transfer program*. 2010, MDRC: New York, NY.
154. Card, D. and D. Hyslop, *Estimating the effects of a time-limited earnings subsidy for welfare-leavers*. *Econometrica*, 2005. **73**(6): p. 1723-70.
155. Wooldridge, J.M., *Econometric analysis of cross section and panel data*. 2002, Cambridge, MA, and London, United Kingdom: The MIT Press.
156. Angrist, J. and A. Krueger, *Instrumental variables and the search for identification: From supply and demand to natural experiments*. *Journal of Economic Perspectives* 2001. **15**(4): p. 69-85.
157. Rodgers, A., *Income, health and the National Lottery*. *British Medical Journal*, 2001. **323**(7327): p. 1438-9.
158. Cowan, B. and N. Tefft, *The effect of Earned Income Tax Credit expansions on smoking in women*. *Working Paper*. 2011, Washington State University and Bates College: Pullman, WA, and Lewiston, ME.

159. Gomis-Porqueras, P., et al., *The effect of female labor force participation on obesity. Federal Reserve Bank of St Louis Working Paper No. 2011-035A*. 2011, Federal Reserve Bank of St Louis: St Louis, MO.
160. Evans, W.N. and C. Garthwaite, *Giving mom a break: The impact of higher EITC payments on maternal health. Working Paper*. 2011, University of Notre Dame and Northwestern University: Notre Dame, IN, and Evanston, IL.
161. *Omnibus Budget Reconciliation Act, in United States of America Statutes*. 1993: United States of America.
162. Allison, P.D., *Fixed effects regression analysis for longitudinal data using SAS*. 2005, Cary, NC: SAS Institute Inc.
163. Imlach Gunasekara, F., et al., *Fixed effects analysis of repeated measures data*. International Journal of Epidemiology, Accepted.
164. Kaufman, J.S. *Commentary: Why are we biased against bias?* International Journal of Epidemiology, 2008. **37**(3): p. 624-6.
165. Athey, S. and G.W. Imbens, *Identification and inference in nonlinear difference-in-differences models*. Econometrica, 2006. **74**(2): p. 431–497.
166. The Cochrane Collaboration. *Glossary*. 2012 [cited 2013 May 16]; Available from: <http://www.cochrane.org/glossary>.
167. Gunasekara, F.I., K. Carter, and T. Blakely, *Glossary for econometrics and epidemiology*. Journal of Epidemiology & Community Health, 2008. **62**(10): p. 858-61.
168. Athey, S. and G.W. Imbens, *Identification and inference in nonlinear difference-in-differences models*. 2002, National Bureau of Economic Research: Berkeley, CA, and Stanford, CA.
169. Bertrand, M., E. Duflo, and S. Mullainathan, *How much should we trust differences-in-differences estimates?* The Quarterly Journal of Economics, 2004. **119**(1): p. 249-75.
170. Wooldridge, J.M., *Introductory econometrics: A modern approach*. 2012, Mason, OH: South-Western Cengage Learning.
171. Hahn, J., P. Todd, and W. Van der Klaauw, *Identification and estimation of treatment effects with a regression-discontinuity design*. Econometrica, 2001. **69**(1): p. 201-9.
172. Imbens, G.W. and T. Lemieux, *Regression discontinuity designs: A guide to practice*. Journal of Econometrics, 2008. **142**(2): p. 615-35.
173. Lee, D.S. and T. Lemieux, *Regression discontinuity designs in economics*. 2009, National Bureau of Economic Research: Cambridge, MA.
174. Thistlethwaite, D.L. and D.T. Campbell, *Regression-discontinuity analysis: An alternative to the ex post facto experiment*. Journal of Educational Psychology, 1960. **51**(6): p. 309-17.
175. Lalive, R., *How do extended benefits affect unemployment duration? A regression discontinuity approach*. Journal of Econometrics, 2008. **142**(2): p. 785-806.
176. Lee, D.S., *Randomized experiments from non-random selection in U.S. House elections*. Journal of Econometrics, 2008. **142**(2): p. 675-97.
177. Lemieux, T. and K. Milligan, *Incentive effects of social assistance: A regression discontinuity approach*. Journal of Econometrics, 2008. **142**(2): p. 807-28.
178. Robins, J.M., M.A. Hernán, and B. Brumback, *Marginal structural models and causal inference in epidemiology*. Epidemiology, 2000. **11**(5): p. 550-60.
179. Hernán, M.A. and J.M. Robins, *Causal inference*. 2013, Boston, MA: Chapman & Hall / CRC.
180. Westreich, D. and S.R. Cole, *Invited commentary: Positivity in practice*. American Journal of Epidemiology, 2010. **171**(6): p. 674-7; discussion p. 678-81.
181. Petersen, M.L., et al., *Diagnosing and responding to violations in the positivity assumption*. Statistical Methods in Medical Research, 2012. **21**(1): p. 31-54.

182. Higgins, J.P.T. and S. Green, eds. *Cochrane Handbook for Systematic Reviews of Interventions. Version 5.1.0 [updated March 2011]*. 2011, The Cochrane Collaboration.
183. Cochrane Effective Practice and Organization of Care Group, *EPOC-specific resources for review authors*. 2012 [cited 2013 May 23]; Available from: <http://epocoslo.cochrane.org/epoc-specific-resources-review-authors>.
184. Cochrane Public Health Group, *Guide for developing a Cochrane protocol*. 2011 [cited 2013 May 23]; Available from: [http://ph.cochrane.org/sites/ph.cochrane.org/files/uploads/Guide%20for%20PH%20p rotocol\\_Nov%202011\\_final%20for%20website.pdf](http://ph.cochrane.org/sites/ph.cochrane.org/files/uploads/Guide%20for%20PH%20p rotocol_Nov%202011_final%20for%20website.pdf).
185. Institute for Health and Social Policy. *Poverty Reduction Database*. 2011 [cited 2013 May 22]; Available from: <http://www.mcgill.ca/ihsp/research/poverty/database>.
186. The Cochrane Collaboration, *Review Manager (RevMan)*. 2011, The Nordic Cochrane Centre, The Cochrane Collaboration: Copenhagen, Denmark.
187. Cochrane Effective Practice and Organization of Care Group, *Suggested risk of bias criteria for EPOC reviews*. 2012 [cited 2013 May 23]; Available from: <http://epocoslo.cochrane.org/sites/epocoslo.cochrane.org/files/uploads/Suggested% 20risk%20of%20bias%20criteria%20for%20EPOC%20reviews%20%2806%2002%2012 %29.pdf>.
188. Strully, K.W., D.H. Rehkopf, and Z. Xuan, *Effects of prenatal poverty on infant health: State Earned Income Tax Credits and birth weight*. *American Sociological Review*, 2010. **75**(4): p. 534-562.
189. Guyatt, G.H., et al., *GRADE guidelines: 9. Rating up the quality of evidence*. *Journal of Clinical Epidemiology*, 2011. **64**(12): 1311-6.
190. Bureau of Labor Statistics, *Economic news release: County employment and wages summary*. 2013, United States Department of Labor: Washington, DC.
191. United States Department of Treasury. *About EITC*. 2012 [cited 2013 19 June]. Available from: <http://www.eitc.irs.gov/central/abouteitc/>.
192. Ajrouch, K.J., et al., *Situational stressors among African-American women living in low-income urban areas: The role of social support*. *Women & Health*, 2010. **50**(2): p. 159-75.
193. Baker, D. and K. North, *Does employment improve the health of lone mothers?* *Social Science & Medicine*, 1999. **49**(1): p. 121-31.
194. Greenberg, D., *Chapter 12: Welfare-to-work and work-incentive programs*, in *Investing in the disadvantaged: Assessing the benefits and costs of social policies*, D.L. Weimer and A.R. Vining, Editors. 2009, Washington, DC: Georgetown University Press. p. 205-18.
195. Kneipp, S.M., *The health of women in transition from welfare to employment*. *Western Journal of Nursing Research*, 2000. **22**(6): p. 656-74; discussion p. 674-82.
196. Martin, C.T., et al., *Perceptions of self-esteem in a welfare-to-wellness-to-work program*. *Public Health Nursing*, 2012. **29**(1): p. 19-26.
197. Pollack, H.A. and P. Reuter, *Welfare receipt and substance-abuse treatment among low-income mothers: The impact of welfare reform*. *American Journal of Public Health*, 2006. **96**(11): p. 2024-31.
198. Rodriguez, E. and P. Chandra, *Alcohol, employment status, and social benefits: One more piece of the puzzle*. *The American Journal of Drug and Alcohol Abuse*, 2006. **32**(2): p. 237-59.
199. Rodriguez, E., E.A. Frongillo, and P. Chandra, *Do social programmes contribute to mental well-being? The long-term impact of unemployment on depression in the United States*. *International Journal of Epidemiology*, 2001. **30**(1): p. 163-70.
200. Zabkiewicz, D. *The mental health benefits of work: Do they apply to poor single mothers?* *Social Psychiatry and Psychiatric Epidemiology*, 2010. **45**(1): p. 77-87.

201. Kenkel, D.S., M.D. Schmeiser, and C. Urban, *Is smoking inferior? Evidence from variation in the Earned Income Tax Credit*. 2011, Federal Reserve Board of Governors: Washington, DC.
202. Schmeiser, M.D., *Expanding wallets and waistlines: The impact of family income on the BMI of women and men eligible for the Earned Income Tax Credit*. Health Economics, 2009. **18**(11): p. 1277-94.
203. Arno, P.S., et al., *Bringing health and social policy together: The case of the earned income tax credit*. Journal of Public Health Policy, 2009. **30**(2): 198-207.
204. Hoynes, H.W., D.L. Miller, and D. Simon, *Income, the Earned Income Tax Credit and infant health. Working Paper*. 2011, University of California, Davis: Davis, CA.
205. Rehkopf, D., K. Strully, and W. Dow, *The impact of poverty reduction policy on child and adolescent overweight: A quasi-experimental analysis of the Earned Income Tax Credit*. American Journal of Epidemiology, 2011. **173**: p. S238.
206. Gregg, P., S. Harkness, and S. Smith, *Welfare reform and lone parents in the UK. Working Paper No. 07/182*. 2007, The Centre for Market and Public Organisation: Bristol, UK.
207. Alegria, M., D.J. Perez, and S. Williams, *The role of public policies in reducing mental health status disparities for people of color*. Health Affairs (Millwood), 2003. **22**(5): p. 51-64.
208. Hernan, M.A., S. Hernandez-Diaz, and J.M. Robins, *A structural approach to selection bias*. Epidemiology, 2004. **15**(5): p. 615-25.
209. *Statistics Act*, in *New Zealand Statutes*. 1975: New Zealand.
210. Statistics New Zealand. *Official Statistics and the Official Statistics System*. 2012 [cited 2013 July 3]; Available from: [http://www.stats.govt.nz/about\\_us/policies-and-protocols/official-statistics-and-official-statistics-system.aspx](http://www.stats.govt.nz/about_us/policies-and-protocols/official-statistics-and-official-statistics-system.aspx).
211. Carter, K.N., et al., *Cohort Profile: Survey of Families, Income and Employment (SoFIE) and Health Extension (SoFIE-health)*. *International Journal of Epidemiology*, 2010. **39**(3): p. 653-9.
212. Statistics New Zealand, *A longitudinal survey of income, employment and family dynamics*. 2001, Statistics New Zealand: Wellington, New Zealand.
213. Statistics New Zealand. *SoFIE survey objectives and questionnaire flowcharts*. n.d. [cited 2013 May 7]; Available from: [http://www.stats.govt.nz/browse\\_for\\_stats/income-and-work/Income/sofie-objectives-flowcharts.aspx](http://www.stats.govt.nz/browse_for_stats/income-and-work/Income/sofie-objectives-flowcharts.aspx).
214. Jenkins, S.P., *Changing fortunes: Income mobility and poverty dynamics in Britain*. 2011, Oxford, United Kingdom: Oxford University Press.
215. Ryder, A.B., et al., *The advantage of imputation of missing income data to evaluate the association between income and self-reported health status (SRH) in a Mexican American cohort study*. Journal of Immigrant and Minority Health, 2011. **13**(6): p. 1099-109.
216. Schenker, N., et al., *Multiple imputation of missing income data in the National Health Interview Survey*. Journal of the American Statistical Association, 2006. **101**(475): p. 924-33.
217. Gottschalk, P. and M. Huynh, *Are earnings inequality and mobility overstated?: The impact of nonclassical measurement error*. The Review of Economics and Statistics 2010. **92**(2): p. 302-15.
218. Pischke, J., *Measurement error and earnings dynamics: Some estimates from the PSID Validation Study*. Journal of Business and Economic Statistics, 1995. **13**(3): p. 305-14.
219. Statistics New Zealand, *SoFIE user guide: Waves 1-8*. 2012, Statistics New Zealand: Wellington, New Zealand.

220. Inland Revenue Department, *IR 271: Family Assistance 2007 (1 April 2007 to 31 March 2008)*. 2007, Inland Revenue Department: Wellington, New Zealand.
221. Au, N. and D. Johnston, *Self-assessed general health: What does it mean and what is it hiding?*, in *9th World Congress of Health Economics: Celebrating Health Economics*. 2013: Sydney, Australia.
222. Quesnel-Vallee, A., *Self-rated health: Caught in the crossfire of the quest for 'true' health?* *International Journal of Epidemiology*, 2007. **36**(6): p. 1161-4.
223. Singh-Manoux, A., et al., *The association between self-rated health and mortality in different socioeconomic groups in the GAZEL cohort study*. *International Journal of Epidemiology*, 2007. **36**(6): 1222-8.
224. Imlach Gunasekara, F.I., K. Carter, and T. Blakely, *Comparing self-rated health and self-assessed change in health in a longitudinal survey: Which is more valid?* *Social Science & Medicine*, 2012. **74**(7): p. 1117-24.
225. Huisman, M., F. van Lenthe, and J. Mackenbach, *The predictive ability of self-assessed health for mortality in different educational groups*. *International Journal of Epidemiology*, 2007. **36**(6): p. 1207-13.
226. Dowd, J.B. and A. Zajacova, *Does the predictive power of self-rated health for subsequent mortality risk vary by socioeconomic status in the US?* *International Journal of Epidemiology*, 2007. **36**(6): 1214-21.
227. Subramanian, S.V. and K. Ertel, *Is the use of self-rated health measures to assess health inequalities misleading?* *International Journal of Epidemiology*, 2008. **37**(6): p. 1436-7; author reply p. 1437-40.
228. Subramanian, S.V. and K. Ertel, *Self-rated health may be adequate for broad assessments of social inequalities in health*. *International Journal of Epidemiology*, 2009. **38**(1): p. 319-20.
229. Crossley, T.F. and S. Kennedy, *The reliability of self-assessed health status*. *Journal of Health Economics*, 2002. **21**(4): p. 643-58.
230. Zajacova, A. and J.B. Dowd, *Reliability of self-rated health in US adults*. *American Journal of Epidemiology*, 2011. **174**(8): p. 977-83.
231. Perneger, T.V., et al., *Self-rated health: Analysis of distances and transitions between response options*. *Quality of Life Research*, 2013. doi: 10.1007/s11136-013-0418-5.
232. Inland Revenue Department and Ministry of Social Development, *Receipt of the Working For Families package: 2007 Update*. 2007, Inland Revenue Department and Ministry of Social Development: Wellington, New Zealand.
233. Inland Revenue Department. *Working for Families Tax Credits: Number of families entitled, 2001 to 2010*. n.d. [cited 2013 May 7]; Available from: [www.ird.govt.nz/aboutird/exteranl-stats/social-policy/wfftc/](http://www.ird.govt.nz/aboutird/exteranl-stats/social-policy/wfftc/).
234. Hauck, K. and N. Rice, *A longitudinal analysis of mental health mobility in Britain*. *Health Economics*, 2004. **13**(10): p. 981-1001.
235. Jensen, J., *Income equivalences and the estimation of family expenditures on children*. 1988, Department of Social Welfare: Wellington, New Zealand.
236. Kleinbaum, D.G., H. Morgenstern, and L.L. Kupper, *Selection bias in epidemiologic studies*. *American Journal of Epidemiology*, 1981. **113**(4): p. 452-63.
237. Liu, W., et al. *Implications of M bias in epidemiologic studies: A simulation study*. *American Journal of Epidemiology*, 2012. **176**(10): p. 938-48.
238. Carter, K.N., et al., *Differential loss of participants does not necessarily cause selection bias*. *Australian and New Zealand Journal of Public Health*, 2012. **36**(3): p. 218-22.
239. Hernan, M.A. and S.R. Cole, *Invited commentary: Causal diagrams and measurement bias*. *American Journal of Epidemiology*, 2009. **170**(8): p. 959-62; discussion p. 963-4.
240. Huber, M., M. Lechner, and C. Wunsch, *Does leaving welfare improve health? Evidence for Germany*. *Health Economics*, 2011. **20**(4): p. 484-504.

241. Korenman, S., N. Goldman, and H. Fu, *Misclassification bias in estimates of bereavement effects*. *American Journal of Epidemiology*, 1997. **145**(11): p. 995-1002.
242. Bound, J., C. Brown, and N. Mathiowitz, *Measurement error in survey data*, in *Handbook of Econometrics*, J. Heckman and E. Learner, Editors. 2001, New York, NY: Elsevier Science. p. 3707-45.
243. Jäckle, A., *Measurement error and data collection methods: Effects on estimates from event history data*. *ISER Working Paper No. 2008-13*. 2008, Institute for Social and Economic Research, University of Essex: Essex, United Kingdom.
244. Duncan, G.J. and N. Mathiowitz, *A validation study of economic survey data*. 1985, Institute for Social Research, University of Michigan: Ann Arbor, MI.
245. Dragoset, L.M. and G.S. Fields, *U.S. earnings mobility: Comparing survey-based and administrative-based estimates*. *ECINEQ Working Paper Series 2006 – 55*. 2006, Society for the Study of Economic Inequality: Verona, Italy.
246. Kristensen, N. and N. Westergaard-Nielsen, *A large-scale validation study of measurement errors in longitudinal survey data*. *IZA Discussion Paper No. 2329*. 2006, Institute for the Study of Labor: Bonn, Germany.
247. Breen, R. and P. Moiso, *Poverty dynamics corrected for measurement error*. *Journal of Economic Inequality*, 2004. **2**(3): p. 171-91.
248. Rothman, K.J., S. Greenland, and T.L. Lash, eds. *Modern epidemiology*. 3rd ed. 2008, Philadelphia, PA: Wolters Kluwer Health/Lippincott Williams & Wilkins.
249. Richardson, K., D. Harte, and K. Carter, *Understanding health and labour force transitions: Applying Markov models to SoFIE longitudinal data*. *Official Statistics Research Series*, 2011. **2**. Available from [www.statisphere.govt.nz/osresearch](http://www.statisphere.govt.nz/osresearch).
250. Idler, E.L. and Y. Benyamini, *Self-rated health and mortality: A review of twenty-seven community studies*. *Journal of Health & Social Behavior*, 1997. **38**(1): p. 21-37.
251. Mackenbach, J.P. *The persistence of health inequalities in modern welfare states: The explanation of a paradox*. *Social Science & Medicine*, 2012. **75**(4): p. 761-9.
252. The Treasury. *Budget Economic and Fiscal Update 2012: Core Crown Expense Tables*. 2013 [cited 2013 May 19]; Available from: <http://www.treasury.govt.nz/budget/forecasts>.
253. Government of Pakistan. *Benazir Income Support Program*. 2011 [cited 2013 14 May]; Available from: <http://www.bisp.gov.pk/Default.aspx>.
254. Hjollund, N.H., F.B. Larsen, and J.H. Andersen, *Register-based follow-up of social benefits and other transfer payments: Accuracy and degree of completeness in a Danish interdepartmental administrative database compared with a population-based survey*. *Scandinavian Journal of Public Health*, 2007. **35**(5): p. 497-502.

## Appendix 1: Tables of total SoFIE sample at baseline

**Table 81: Gender by age, N=9,360 (unbalanced panel), Wave 1**

Age	Gender								
	Women			Men			Total		
	N	Col %	Row %	N	Col %	Row %	N	Col %	
19-24	350	6.6	68.0	165	4.0	32.0	515	5.5	
25-34	1550	29.4	61.1	985	24.1	38.9	2535	27.1	
35-44	2065	39.1	56.9	1565	38.3	43.1	3630	38.8	
45-54	1135	21.5	49.6	1155	28.3	50.4	2290	24.5	
55-64	175	3.3	44.9	215	5.3	55.1	390	4.2	
Total	5275	100.0	56.4	4085	100.0	43.6	9360	100.0	

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator).

**Table 82: Ethnicity by gender and age, N=9,360 (unbalanced panel), Wave 1**

	Ethnicity														
	Māori			NZ European			Pacific			Asian			Total		
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	
<b>Gender</b>															
Male	310	42.5	7.6	2760	44.9	67.5	305	43.9	7.5	135	52.9	3.3	4090	43.6	
Female	420	57.5	8.0	3385	55.1	64.1	390	56.1	7.4	120	47.1	2.3	5280	56.4	
Total	730	100.0	7.8	6145	100.0	65.6	695	100.0	7.4	255	100.0	2.7	9370	100.0	
<b>Age</b>															
19-24	55	7.5	10.8	255	4.2	50.0	20	2.9	3.9	10	4.0	2.0	510	5.4	
25-34	215	29.3	8.4	1590	25.9	62.5	165	23.7	6.5	55	22.0	2.2	2545	27.2	
35-44	245	33.3	6.7	2395	39.0	65.9	310	44.6	8.5	110	44.0	3.0	3635	38.8	
45-54	180	24.5	7.9	1635	26.6	71.4	170	24.5	7.4	70	28.0	3.1	2290	24.4	
55-64	40	5.4	10.3	265	4.3	67.9	30	4.3	7.7	5	2.0	1.3	390	4.2	
Total	735	100.0	7.8	6140	100.0	65.5	695	100.0	7.4	250	100.0	2.7	9370	100.0	

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). N = 10 (0.0%) participants with missing values and N = 255 (0.0%) reporting an Other ethnicity are not shown in the table, but are counted in the totals.

**Table 83: Highest qualification by gender, age and ethnicity N=9,360 (unbalanced panel), Wave 1**

	Highest qualification													
	No qualification			School qualification			Post-school qualification			Degree or higher			Total	
	N	Col %	Row %	N	Col %	Row %	N	N	Row %	Col %	N	Row %	N	
<b>Gender</b>														
Male	895	36.5	21.9	1660	48.5	40.6	750	46.9	18.4	780	41.4	19.1	4085	43.6
Female	1555	63.5	29.5	1765	51.5	33.5	850	53.1	16.1	1105	58.6	20.9	5275	56.4
Total	2450	100.0	26.2	3425	100.0	36.6	1600	100.0	17.1	1885	100.0	20.1	9360	100.0
<b>Age</b>														
19-24	180	7.3	35.0	185	5.4	35.9	30	1.9	5.8	120	6.3	23.3	515	5.5
25-34	715	29.2	28.2	915	26.7	36.1	435	27.2	17.2	470	24.9	18.5	2535	27.1
35-44	955	39.0	26.3	1320	38.5	36.4	670	41.9	18.5	685	36.2	18.9	3630	38.8
45-54	525	21.4	22.9	860	25.1	37.5	415	25.9	18.1	495	26.2	21.6	2295	24.5
55-64	75	3.1	19.2	145	4.2	37.2	50	3.1	12.8	120	6.3	30.8	390	4.2
Total	2450	100.0	26.2	3425	100.0	36.6	1600	100.0	17.1	1890	100.0	20.2	9365	100.0
<b>Ethnicity</b>														
Māori	310	12.7	20.2	540	15.7	35.2	110	6.9	7.2	575	30.4	37.5	1535	16.4
NZ European	1645	67.3	26.8	2465	71.9	40.1	1060	66.3	17.3	970	51.3	15.8	6140	65.6
Pacific	280	11.5	38.4	175	5.1	24.0	40	2.5	5.5	235	12.4	32.2	730	7.8
Asian	155	6.3	22.5	145	4.2	21.0	310	19.4	44.9	80	4.2	11.6	690	7.4
Other	55	2.2	21.6	100	2.9	39.2	75	4.7	29.4	25	1.3	9.8	255	2.7
Total	2445	100.0	26.1	3430	100.0	36.6	1600	100.0	17.1	1890	100.0	20.2	9365	100.0

Notes: Col % = column percentage (where the column total is the denominator). <sup>b</sup> Row % = row percentage (where the row total is the denominator). NZ European = New Zealand European. N = 15 (0.0%) participants with missing values for ethnicity are not shown in the table, but are counted in the totals.

**Table 84: Family Tax Credit eligibility and amount by time-invariant and time-varying variables, N=9,360 (unbalanced panel), Wave 1**

	FTC eligibility															FTC amount						Not in a family			Total	
	Eligible			Not eligible			Q1-Q2			Q3-Q5																
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %						

		%		%		%		%		%		%		%		%		%
Time-invariant variables																		
Gender																		
Male	640	31.5	15.7	2830	46.2	69.3	440	63.3	8.3	950	71.4	18.0	615	51.3	15.1	4085	43.6	
Female	1390	68.5	26.4	3300	53.8	62.6	255	36.7	6.3	380	28.6	9.3	585	48.8	11.1	5275	56.4	
Total	2030	100.0	21.7	6130	100.0	65.5	695	100.0	7.4	1330	100.0	14.2	1200	100.0	12.8	9360	100.0	
Age																		
19-24	185	9.1	35.9	150	2.4	29.1	60	8.6	11.5	130	9.8	25.0	180	15.0	35.0	515	5.5	
25-34	745	36.7	29.4	1250	20.4	49.3	265	37.9	10.5	480	36.1	18.9	540	45.0	21.3	2535	27.1	
35-44	810	39.9	22.3	2610	42.6	71.9	265	37.9	7.3	540	40.6	14.9	210	17.5	5.8	3630	38.8	
45-54	260	12.8	11.4	1835	29.9	80.1	95	13.6	4.1	165	12.4	7.2	195	16.3	8.5	2290	24.5	
55-64	30	1.5	7.7	285	4.6	73.1	15	2.1	3.8	15	1.1	3.8	75	6.3	19.2	390	4.2	
Total	2030	100.0	21.7	6130	100.0	65.5	700	100.0	7.5	1330	100.0	14.2	1200	100.0	12.8	9360	100.0	
Ethnicity																		
Māori	875	43.2	14.2	4365	71.2	71.0	155	22.1	10.1	415	31.2	27.0	905	75.1	14.7	6145	65.7	
NZ European	570	28.1	37.1	830	13.5	54.1	370	52.9	6.0	510	38.3	8.3	135	11.2	8.8	1535	16.4	
Pacific	305	15.1	41.8	370	6.0	50.7	105	15.0	14.4	200	15.0	27.4	55	4.6	7.5	730	7.8	
Asian	225	11.1	32.4	395	6.4	56.8	55	7.9	7.9	170	12.8	24.5	75	6.2	10.8	695	7.4	
Other	50	2.5	20.0	165	2.7	66.0	15	2.1	6.0	35	2.6	14.0	35	2.9	14.0	250	2.7	
Total	2025	100.0	21.6	6130	100.0	65.5	700	100.0	7.5	1330	100.0	14.2	1205	100.0	12.9	9360	100.0	
Highest qualification																		
No qualification	660	32.6	35.0	1070	17.5	56.8	200	28.4	10.6	465	35.0	24.6	155	12.9	8.2	1885	20.1	
School qualification	545	26.9	22.2	1615	26.3	65.9	215	30.5	8.8	330	24.8	13.5	290	24.1	11.8	2450	26.2	
Post-school qualification	635	31.4	18.5	2310	37.7	67.4	225	31.9	6.6	410	30.8	12.0	480	39.8	14.0	3425	36.6	
Degree or higher	185	9.1	11.6	1135	18.5	70.9	60	8.5	3.8	125	9.4	7.8	280	23.2	17.5	1600	17.1	
Total	2025	100.0	21.6	6130	100.0	65.5	705	100.0	7.5	1330	100.0	14.2	1205	100.0	12.9	9360	100.0	
Time-varying variables																		
Family income																		
Q1 (lowest)	1665	82.2	70.0	435	7.1	18.3	435	62.1	18.2	1235	92.9	51.8	280	23.4	11.8	2380	25.4	
Q2	355	17.5	18.8	1300	21.2	68.8	260	37.1	13.8	95	7.1	5.0	235	19.7	12.4	1890	20.2	
Q3	5	0.2	0.3	1535	25.0	87.7	5	0.7	0.3	0	0.0	0.0	210	17.6	12.0	1750	18.7	
Q4	0	0.0	0.0	1420	23.1	84.5	0	0.0	0.0	0	0.0	0.0	260	21.8	15.5	1680	18.0	

Q5 (highest)	0	0.0	0.0	1445	23.6	87.3	0	0.0	0.0	0	0.0	0.0	210	17.6	12.7	1655	17.7
Total	2025	100.0	21.6	6135	100.0	65.6	700	100.0	7.5	1330	100.0	14.2	1195	100.0	12.8	9355	100.0
Equivalised family income (minus FTC)																	
Q1 (lowest)	1770	87.4	73.6	500	8.2	20.8	445	63.6	18.5	1330	99.6	55.2	135	11.3	5.6	2405	25.7
Q2	255	12.6	13.0	1580	25.8	80.8	255	36.4	13.0	5	0.4	0.3	120	10.0	6.1	1955	20.9
Q3	0	0.0	0.0	1515	24.7	90.2	0	0.0	0.0	0	0.0	0.0	165	13.8	9.8	1680	18.0
Q4	0	0.0	0.0	1420	23.2	83.3	0	0.0	0.0	0	0.0	0.0	285	23.8	16.7	1705	18.2
Q5 (highest)	0	0.0	0.0	1110	18.1	69.2	0	0.0	0.0	0	0.0	0.0	495	41.3	30.8	1605	17.2
Total	2025	100.0	21.7	6125	100.0	65.5	700	100.0	7.5	1335	100.0	14.3	1200	100.0	12.8	9350	100.0
Family type																	
Single, couple only	0	0.0	0.0	0	0.0	0.0	230	32.9	16.2	595	44.7	41.9	1200	100.0	100.0	1200	12.8
One-parent	825	40.6	58.1	595	9.7	41.9	470	67.1	7.0	735	55.3	10.9	0	0.0	0.0	1420	15.2
Two-parent	1205	59.4	17.9	5535	90.3	82.1	0	0.0	0.0	0	0.0	0.0	0	0.0	0.0	6740	72.0
Total	2030	100.0	21.7	6130	100.0	65.5	700	100.0	7.5	1330	100.0	14.2	1200	100.0	12.8	9360	100.0
Number of children																	
0	0	0.0	0.0	1295	21.1	51.9	0	0.0	0.0	0	0.0	0.0	1200	100.0	48.1	2495	26.7
1	700	34.5	27.2	1875	30.6	72.8	305	43.6	11.8	395	29.7	15.3	0	0.0	0.0	2575	27.5
2	690	34.0	26.4	1925	31.4	73.6	260	37.1	9.9	430	32.3	16.4	0	0.0	0.0	2615	27.9
3	385	19.0	32.1	815	13.3	67.9	90	12.9	7.5	295	22.2	24.6	0	0.0	0.0	1200	12.8
5-10	255	12.6	53.7	220	3.6	46.3	45	6.4	9.5	210	15.8	44.2	0	0.0	0.0	475	5.1
Total	2030	100.0	21.7	6130	100.0	65.5	700	100.0	7.5	1330	100.0	14.2	1200	100.0	12.8	9360	100.0

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). Q1-Q2 = quintiles 1-2. Q3-Q5 = quintiles 3-5. NZ European = New Zealand European.

**Table 85: In-Work Tax Credit eligibility and amount by time-invariant and time-varying variables, N=9,360 (unbalanced panel), Wave 4**

	IWTC eligibility			IWTC amount									Not in a family			Total	
	Eligible			Not eligible			Q1-Q2			Q3-5							
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %
Time-invariant variables																	
Gender																	
Male	565	46.7	13.8	2245	42.4	55.0	190	51.4	3.6	455	54.2	8.6	1270	44.6	31.1	4080	43.6

Female	645	53.3	12.2	3055	57.6	57.9	180	48.6	4.4	385	45.8	9.4	1580	55.4	29.9	5280	56.4
Total	1210	100.0	12.9	5300	100.0	56.6	370	100.0	4.0	840	100.0	9.0	2850	100.0	30.4	9360	100.0
Age																	
19-24	15	1.2	9.7	60	1.1	38.7	5	1.3	3.2	10	1.2	6.5	80	2.8	51.6	155	1.7
25-34	355	29.2	18.3	920	17.4	47.4	95	25.0	4.9	260	31.1	13.4	665	23.4	34.3	1940	20.7
35-44	600	49.4	17.2	2100	39.6	60.2	180	47.4	5.2	420	50.3	12.0	790	27.8	22.6	3490	37.3
45-54	225	18.5	7.7	1790	33.8	61.5	90	23.7	3.1	135	16.2	4.6	895	31.5	30.8	2910	31.1
55-64	20	1.6	2.3	430	8.1	49.7	10	2.6	1.2	10	1.2	1.2	415	14.6	48.0	865	9.2
Total	1215	100.0	13.0	5300	100.0	56.6	380	100.0	4.1	835	100.0	8.9	2845	100.0	30.4	9360	100.0
Ethnicity																	
Māori	800	66.1	13.0	3720	70.2	60.5	50	13.3	3.3	130	15.5	8.5	1625	57.1	26.4	6145	65.7
NZ European	180	14.9	11.7	770	14.5	50.2	265	70.7	4.3	535	63.7	8.7	585	20.6	38.1	1535	16.4
Pacific	105	8.7	14.4	290	5.5	39.7	25	6.7	3.4	80	9.5	11.0	335	11.8	45.9	730	7.8
Asian	100	8.3	14.5	365	6.9	52.9	20	5.3	2.9	80	9.5	11.6	225	7.9	32.6	690	7.4
Other	25	2.1	9.8	155	2.9	60.8	15	4.0	5.8	15	1.8	5.8	75	2.6	29.4	255	2.7
Total	1210	100.0	12.9	5300	100.0	56.7	375	100.0	4.0	840	100.0	9.0	2845	100.0	30.4	9355	100.0
Highest qualification																	
Missing	395	21.4	29.6	685	14.7	51.3	50	13.3	3.7	140	16.7	10.5	255	8.9	19.1	1335	14.3
No qualification	495	26.8	26.3	1090	23.4	58.0	100	26.7	5.3	250	29.8	13.3	295	10.4	15.7	1880	20.1
School qualification	720	39.0	23.1	1790	38.4	57.5	155	41.3	5.0	330	39.3	10.6	605	21.2	19.4	3115	33.3
Post-school qualification	205	11.1	13.6	1065	22.8	70.8	70	18.7	4.7	115	13.7	7.7	235	8.2	15.6	1505	16.1
Degree or higher	30	1.6	2.0	35	0.8	2.3	0	0.0	0.0	0	0.0	0.0	1460	51.2	95.7	1525	16.3
Total	1845	100.0	19.7	4665	100.0	49.8	375	100.0	4.0	840	100.0	9.0	2850	100.0	30.4	9360	100.0
Time-varying variables																	
Family income																	
Q1 (lowest)	115	9.5	4.2	815	15.4	29.6	0	0.0	0.0	215	25.7	6.7	1820	63.7	66.2	2750	29.4
Q2	350	29.0	24.7	780	14.7	55.1	55	14.9	3.5	450	53.9	28.5	285	10.0	20.1	1415	15.1
Q3	510	42.3	37.9	655	12.4	48.7	275	74.3	18.0	150	18.0	9.8	180	6.3	13.4	1345	14.4
Q4	225	18.7	13.0	1245	23.5	71.8	40	10.8	2.6	20	2.4	1.3	265	9.3	15.3	1735	18.5
Q5 (highest)	5	0.4	0.2	1800	34.0	85.3	0	0.0	0.0	0	0.0	0.0	305	10.7	14.5	2110	22.6
Total	1205	100.0	12.9	5295	100.0	56.6	370	100.0	4.0	835	100.0	8.9	2855	100.0	30.5	9355	100.0
Equivalised family income																	

(minus IWTC)																	
Q1 (lowest)	325	26.9	10.0	1160	21.9	35.8	5	1.3	0.2	325	38.7	10.0	1755	61.6	54.2	3240	34.6
Q2	510	42.1	32.5	915	17.3	58.3	75	20.0	4.8	440	52.4	28.0	145	5.1	9.2	1570	16.8
Q3	365	30.2	23.8	990	18.7	64.5	285	76.0	18.6	75	8.9	4.9	180	6.3	11.7	1535	16.4
Q4	10	0.8	0.7	1210	22.8	79.3	10	2.7	0.7	0	0.0	0.0	305	10.7	20.0	1525	16.3
Q5 (highest)	0	0.0	0.0	1025	19.3	68.8	0	0.0	0.0	0	0.0	0.0	465	16.3	31.2	1490	15.9
Total	1210	100.0	12.9	5300	100.0	56.6	375	100.0	4.0	840	100.0	9.0	2850	100.0	30.4	9360	100.0
Family type																	
Single, couple only	0	0.0	0.0	0	0.0	0.0	20	5.3	2.0	95	11.4	9.5	2850	100.0	100.0	2850	30.4
One-parent	115	9.5	11.5	885	16.7	88.5	355	94.7	6.4	740	88.6	13.4	0	0.0	0.0	1000	10.7
Two-parent	1095	90.5	19.9	4415	83.3	80.1	0	0.0	0.0	0	0.0	0.0	0	0.0	0.0	5510	58.9
Total	1210	100.0	12.9	5300	100.0	56.6	375	100.0	4.0	835	100.0	8.9	2850	100.0	30.4	9360	100.0
Number of children																	
0	0	0.0	0.0	1385	26.1	32.7	0	0.0	0.0	0	0.0	0.0	2850	100.0	67.3	4235	45.2
1	340	28.1	17.8	1565	29.5	82.2	150	40.5	7.9	190	22.6	10.0	0	0.0	0.0	1905	20.4
2	500	41.3	24.0	1585	29.9	76.0	165	44.6	7.9	335	39.9	16.1	0	0.0	0.0	2085	22.3
3	255	21.1	30.5	580	10.9	69.5	50	13.5	6.0	205	24.4	24.6	0	0.0	0.0	835	8.9
5-10	115	9.5	38.3	185	3.5	61.7	5	1.4	1.7	110	13.1	36.7	0	0.0	0.0	300	3.2
Total	1210	100.0	12.9	5300	100.0	56.6	370	100.0	4.0	840	100.0	9.0	2850	100.0	30.4	9360	100.0

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). Q1-Q2 = quintiles 1-2. Q3-Q5 = quintiles 3-5. NZ European = New Zealand European.

**Table 86: Self-rated health by time-invariant and time-varying variables, N=9,360 (unbalanced panel), Wave 1**

	SRH										
	Excellent, very good, good			Poor			Fair, poor			Total	
	N	Col	Row	N	Col	Row	N	Col	Row	N	Col
Time-invariant variables											
Gender											
Male	3150	43.9	77.1	695	43.4	17.0	235	40.5	5.8	4085	43.6
Female	4025	56.1	76.3	905	56.6	17.2	345	59.5	6.5	5275	56.4
Total	7175	100.0	76.7	1600	100.0	17.1	580	100.0	6.2	9360	100.0

Age											
19-24	385	5.4	74.0	110	6.9	21.2	25	4.3	4.8	520	5.5
25-34	2050	28.6	80.7	365	22.8	14.4	125	21.4	4.9	2540	27.1
35-44	2805	39.1	77.3	615	38.4	16.9	205	35.0	5.6	3630	38.7
45-54	1690	23.5	73.8	420	26.3	18.3	180	30.8	7.9	2290	24.4
55-64	250	3.5	64.1	90	5.6	23.1	50	8.5	12.8	390	4.2
Total	7180	100.0	76.6	1600	100.0	17.1	585	100.0	6.2	9370	100.0
Ethnicity											
Māori	1020	14.2	66.7	360	22.6	23.5	150	25.6	9.8	1530	16.4
NZ European	4945	69.0	80.5	890	55.8	14.5	305	52.1	5.0	6145	65.7
Pacific	515	7.2	70.5	145	9.1	19.9	70	12.0	9.6	730	7.8
Asian	495	6.9	71.7	155	9.7	22.5	40	6.8	5.8	690	7.4
Total	7165	100.0	76.6	1595	100.0	17.1	585	100.0	6.3	9350	100.0
Highest qualification											
No qualification	1215	16.9	64.5	460	28.8	24.4	210	36.5	11.1	1885	20.1
School qualification	1925	26.8	78.7	395	24.7	16.2	125	21.7	5.1	2445	26.1
Post-school qualification	2665	37.1	77.7	565	35.3	16.5	195	33.9	5.7	3430	36.7
Degree or higher	1370	19.1	85.9	180	11.3	11.3	45	7.8	2.8	1595	17.0
Total	7175	100.0	76.7	1600	100.0	17.1	575	100.0	6.1	9355	100.0
Gross total annual family income											
Q1 (lowest)	1565	21.8	65.5	555	34.7	23.2	270	45.8	11.3	2390	25.5
Q2	1395	19.4	73.8	355	22.2	18.8	140	23.7	7.4	1890	20.2
Q3	1385	19.3	79.1	280	17.5	16.0	85	14.4	4.9	1750	18.7
Q4	1390	19.4	82.7	230	14.4	13.7	60	10.2	3.6	1680	17.9
Q5 (highest)	1440	20.1	87.0	180	11.3	10.9	35	5.9	2.1	1655	17.7
Total	7175	100.0	76.6	1600	100.0	17.1	590	100.0	6.3	9365	100.0
Equivalentised gross total annual family income (minus FTC)											
Q1 (lowest)	1550	21.6	64.3	565	35.3	23.4	295	50.9	12.2	2410	25.8
Q2	1445	20.2	73.9	380	23.8	19.4	130	22.4	6.6	1955	20.9
Q3	1345	18.8	80.1	265	16.6	15.8	70	12.1	4.2	1680	18.0
Q4	1420	19.8	83.0	230	14.4	13.5	55	9.5	3.2	1710	18.3
Q5 (highest)	1410	19.7	88.1	160	10.0	10.0	30	5.2	1.9	1600	17.1
Total	7170	100.0	76.6	1600	100.0	17.1	580	100.0	6.2	9355	100.0
Equivalentised gross total annual family income (minus IWTC)											

Q1 (lowest)	1015	17.7	31.3	385	26.7	11.9	195	40.2	6.0	3245	34.6
Q2	1090	19.0	69.2	335	23.3	21.3	110	22.7	7.0	1575	16.8
Q3	1145	20.0	74.6	285	19.8	18.6	90	18.6	5.9	1535	16.4
Q4	1210	21.1	79.1	255	17.7	16.7	55	11.3	3.6	1530	16.3
Q5 (highest)	1265	22.1	84.9	180	12.5	12.1	35	7.2	2.3	1490	15.9
Total	5725	100.0	61.1	1440	100.0	15.4	485	100.0	5.2	9375	100.0
Family type											
Single, couple only	995	13.9	82.9	155	9.7	12.9	50	8.5	4.2	1200	12.8
One-parent	940	13.1	66.0	335	20.9	23.5	150	25.6	10.5	1425	15.2
Two-parent	5240	73.0	77.7	1115	69.5	16.5	385	65.8	5.7	6745	72.0
Total	7175	100.0	76.6	1605	100.0	17.1	585	100.0	6.2	9370	100.0
0	1920	26.8	76.8	415	25.9	16.6	165	28.4	6.6	2500	26.7
1	1915	26.7	74.4	490	30.5	19.0	170	29.3	6.6	2575	27.5
2	2080	29.0	79.7	400	24.9	15.3	130	22.4	5.0	2610	27.9
3	915	12.8	76.3	215	13.4	17.9	70	12.1	5.8	1200	12.8
4 to ten	340	4.7	72.3	85	5.3	18.1	45	7.8	9.6	470	5.0
Total	7170	100.0	76.6	1605	100.0	17.2	580	100.0	6.2	9355	100.0
Employment status											
Unemployed	145	2.0	60.4	65	4.1	27.1	30	5.1	12.5	240	2.6
Employed	5755	80.2	80.5	1110	69.4	15.5	275	47.0	3.8	7145	76.3
Inactive	1275	17.8	64.6	420	26.3	21.3	280	47.9	14.2	1975	21.1
Total	7180	100.0	76.6	1600	100.0	17.1	585	100.0	6.2	9370	100.0

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). NZ European = New Zealand European. N = 5 (0.0%) participants had missing values for SRH and N = 10 (0.0%) participants had missing values for employment status, but are included in total counts. N = 255 participants reporting an *Other* ethnicity are not shown in the table, but are counted in the totals.

**Table 87: Family Tax Credit eligibility and amount by self-rated health, N=9,360 (unbalanced panel), Wave 1**

SRH	FTC eligibility			FTC amount						Not in a family			Total				
	Eligible			Not eligible		Q1-Q3			Q4-Q5								
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %
Excellent	695	34.2	17.3	2735	44.6	68.0	260	37.4	6.5	435	32.7	10.8	590	49.2	14.7	4020	695
Very Good	650	32.0	20.6	2100	34.3	66.6	220	31.7	7.0	430	32.3	13.6	405	33.8	12.8	3155	650

Good	480	23.6	30.0	965	15.7	60.3	155	22.3	9.7	325	24.4	20.3	155	12.9	9.7	1600	480
Fair	165	8.1	36.3	250	4.1	54.9	50	7.2	11.0	115	8.6	25.3	40	3.3	8.8	455	165
Poor	40	2.0	32.0	75	1.2	60.0	10	1.4	8.0	25	1.9	20.0	10	0.8	8.0	125	40
Total	2030	100.0	21.7	6130	100.0	65.5	695	100.0	7.4	1330	100.0	14.2	1200	100.0	12.8	9360	2030

Notes: Col % = column percentage (where the column total is the denominator).<sup>b</sup> Row % = row percentage (where the row total is the denominator). Q1-Q3 = quintiles 1-3. Q4-Q5 = quintiles 4-5. N = 5 (0.0%) participants had missing values for SRH, but were included in total counts.

**Table 88: In-Work Tax Credit eligibility and amount by self-rated health, N=9,360 (unbalanced panel), Wave 4**

SRH	IWTC eligibility			IWTC amount						Not in a family			Total				
	Eligible	Not eligible		Q1-Q3			Q4-5										
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %
Excellent	465	38.4	15.9	2005	37.8	68.7	165	43.4	5.7	300	35.7	10.3	450	15.8	15.4	2920	31.2
Very Good	445	36.8	15.9	1875	35.4	67.1	125	32.9	4.5	320	38.1	11.4	475	16.7	17.0	2795	29.9
Good	230	19.0	16.0	990	18.7	69.0	60	15.8	4.2	170	20.2	11.8	215	7.6	15.0	1435	15.3
Fair	50	4.1	13.5	245	4.6	66.2	20	5.3	5.4	35	4.2	9.5	75	2.6	20.3	370	4.0
Poor	10	0.8	9.5	80	1.5	76.2	5	1.3	4.8	5	0.6	4.8	15	0.5	14.3	105	1.1
Missing	10	0.8	0.6	100	1.9	5.8	5	1.3	0.3	10	1.2	0.6	1615	56.8	93.6	1725	18.4
Total	1210	100.0	12.9	5300	100.0	56.7	380	100.0	4.1	840	100.0	9.0	2845	100.0	30.4	9355	100.0

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). Q1-Q3 = quintiles 1-3. Q4-Q5 = quintiles 4-5. N = 10 (0.0%) participants had missing values for SRH, but were included in total counts.

**Table 89: Gross total annual family income by time-invariant variables, N=9,360 (unbalanced panel), Wave 1**

	Total annual family income (quintiles)																	
	Q1 (lowest)			Q2			Q3			Q4			Q5 (highest)			Total		
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	
Sex																		
Male	755	31.7	18.5	850	45.0	20.8	845	48.3	20.7	825	49.1	20.2	810	48.9	19.8	4085	43.6	
Female	1630	68.3	30.9	1040	55.0	19.7	905	51.7	17.2	855	50.9	16.2	845	51.1	16.0	5275	56.4	
Total	2385	100.0	25.5	1890	100.0	20.2	1750	100.0	18.7	1680	100.0	17.9	1655	100.0	17.7	9360	100.0	
Age																		

19-24	250	10.5	48.1	130	6.9	25.0	65	3.7	12.5	55	3.3	10.6	20	1.2	3.8	520	5.6
25-34	765	32.1	30.2	630	33.4	24.9	510	29.1	20.1	380	22.6	15.0	250	15.1	9.9	2535	27.1
35-44	840	35.2	23.2	710	37.7	19.6	745	42.6	20.6	685	40.7	18.9	645	38.9	17.8	3625	38.7
45-54	435	18.2	19.0	360	19.1	15.7	370	21.1	16.1	495	29.4	21.6	635	38.3	27.7	2295	24.5
55-64	95	4.0	24.4	55	2.9	14.1	60	3.4	15.4	70	4.2	17.9	110	6.6	28.2	390	4.2
Total	2385	100.0	25.5	1885	100.0	20.1	1750	100.0	18.7	1685	100.0	18.0	1660	100.0	17.7	9365	100.0
Ethnicity																	
Māori	625	26.2	40.7	325	17.2	21.2	255	14.5	16.6	215	12.8	14.0	115	6.9	7.5	1535	16.4
NZ European	1095	45.9	17.8	1170	61.7	19.0	1245	70.9	20.2	1270	75.4	20.7	1370	82.8	22.3	6150	65.6
Pacific	290	12.2	39.5	195	10.3	26.5	120	6.8	16.3	90	5.3	12.2	40	2.4	5.4	735	7.8
Asian	310	13.0	44.6	140	7.4	20.1	95	5.4	13.7	75	4.5	10.8	75	4.5	10.8	695	7.4
Other	60	2.5	24.0	60	3.2	24.0	40	2.3	16.0	35	2.1	14.0	55	3.3	22.0	250	2.7
Total	2385	100.0	25.4	1895	100.0	20.2	1755	100.0	18.7	1685	100.0	18.0	1655	100.0	17.7	9375	100.0
Highest qualification																	
No qualification	740	31.0	39.2	460	24.3	24.3	320	18.3	16.9	260	15.5	13.8	110	6.6	5.8	1890	20.2
School qualification	575	24.1	23.5	550	29.1	22.4	500	28.6	20.4	445	26.5	18.2	380	22.9	15.5	2450	26.2
Post-school qualification	805	33.8	23.5	665	35.2	19.4	685	39.1	20.0	675	40.2	19.7	595	35.8	17.4	3425	36.6
Degree or higher	265	11.1	16.6	215	11.4	13.4	245	14.0	15.3	300	17.9	18.8	575	34.6	35.9	1600	17.1
Total	2385	100.0	25.5	1890	100.0	20.2	1750	100.0	18.7	1680	100.0	17.9	1660	100.0	17.7	9365	100.0

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). NZ European = New Zealand European. Note that the income quintile boundaries calculated for the study sample were applied to ensure comparability with **Table 16**.

**Table 90: Equivalised total annual family income (minus Family Tax Credit) by time-invariant and time-varying variables, N=9,360 (unbalanced panel), Wave 1**

	Equivalised total annual family income (minus FTC) (quintiles)																	
	Q1 (lowest)			Q2			Q3			Q4			Q5 (highest)			Total		
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	
Time-invariant variables																		
Gender																		
Male	795	33.0	19.5	855	43.7	20.9	805	47.9	19.7	840	49.3	20.6	790	49.2	19.3	4085	43.7	
Female	1615	67.0	30.6	1100	56.3	20.9	875	52.1	16.6	865	50.7	16.4	815	50.8	15.5	5270	56.3	
Total	2410	100.0	25.8	1955	100.0	20.9	1680	100.0	18.0	1705	100.0	18.2	1605	100.0	17.2	9355	100.0	

<hr/>																	
Age																	
19-24	210	8.7	41.2	125	6.4	24.5	70	4.2	13.7	70	4.1	13.7	35	2.2	6.9	510	5.4
25-34	720	29.8	28.4	565	28.8	22.3	460	27.4	18.1	405	23.8	16.0	385	24.0	15.2	2535	27.1
35-44	925	38.3	25.5	790	40.3	21.8	675	40.2	18.6	660	38.7	18.2	580	36.1	16.0	3630	38.8
45-54	465	19.3	20.3	415	21.2	18.1	420	25.0	18.3	495	29.0	21.6	500	31.2	21.8	2295	24.5
55-64	95	3.9	24.1	65	3.3	16.5	55	3.3	13.9	75	4.4	19.0	105	6.5	26.6	395	4.2
Total	2415	100.0	25.8	1960	100.0	20.9	1680	100.0	17.9	1705	100.0	18.2	1605	100.0	17.1	9365	100.0
Ethnicity																	
Māori	1035	43.0	16.8	1210	61.7	19.7	1235	73.5	20.1	1325	77.5	21.6	1340	83.5	21.8	6145	65.7
NZ European	650	27.0	42.3	365	18.6	23.8	210	12.5	13.7	195	11.4	12.7	115	7.2	7.5	1535	16.4
Pacific	320	13.3	43.8	200	10.2	27.4	120	7.1	16.4	65	3.8	8.9	25	1.6	3.4	730	7.8
Asian	325	13.5	46.8	135	6.9	19.4	85	5.1	12.2	85	5.0	12.2	65	4.0	9.4	695	7.4
Other	75	3.1	29.4	50	2.6	19.6	30	1.8	11.8	40	2.3	15.7	60	3.7	23.5	255	2.7
Total	2405	100.0	25.7	1960	100.0	20.9	1680	100.0	17.9	1710	100.0	18.3	1605	100.0	17.1	9360	100.0
Highest qualification																	
No qualification	785	32.6	41.6	470	24.1	24.9	315	18.8	16.7	220	12.9	11.7	95	5.9	5.0	1885	20.1
School qualification	595	24.7	24.3	560	28.7	22.9	495	29.5	20.2	435	25.4	17.8	365	22.7	14.9	2450	26.2
Post-school qualification	780	32.4	22.8	705	36.2	20.6	645	38.4	18.8	720	42.1	21.0	575	35.7	16.8	3425	36.6
Degree or higher	250	10.4	15.6	215	11.0	13.4	225	13.4	14.1	335	19.6	20.9	575	35.7	35.9	1600	17.1
Total	2410	100.0	25.7	1950	100.0	20.8	1680	100.0	17.9	1710	100.0	18.3	1610	100.0	17.2	9360	100.0
Time-varying variables																	
Family income																	
Q1 (lowest)	2045	84.9	85.7	290	14.8	12.2	50	3.0	2.1	0	0.0	0.0	0	0.0	0.0	2385	25.5
Q2	360	14.9	19.0	1125	57.5	59.5	295	17.6	15.6	100	5.8	5.3	10	0.6	0.5	1890	20.2
Q3	5	0.2	0.3	505	25.8	28.9	875	52.1	50.0	325	19.0	18.6	40	2.5	2.3	1750	18.7
Q4	0	0.0	0.0	35	1.8	2.1	445	26.5	26.5	890	52.0	53.0	310	19.3	18.5	1680	17.9
Q5 (highest)	0	0.0	0.0	0	0.0	0.0	15	0.9	0.9	395	23.1	23.9	1245	77.6	75.2	1655	17.7
Total	2410	100.0	25.7	1955	100.0	20.9	1680	100.0	17.9	1710	100.0	18.3	1605	100.0	17.1	9360	100.0
<hr/>																	

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). NZ European = New Zealand European. N = 10 (10.0%) of participants had missing values for ethnicity, which were included in total counts. Note that the income quintile boundaries calculated for the study sample were applied to ensure comparability with **Table 17**.

**Table 91: Equivalised total annual family income (minus In-Work Tax Credit) by time-invariant and time-varying variables, N=9,360 (unbalanced panel), Wave 4**

	Equivalised total annual income (minus IWTC) (quintiles)																	
	Q1 (lowest)			Q2			Q3			Q4			Q5 (highest)			Total		
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	
Time-invariant variables																		
Gender																		
Male	1195	36.9	29.3	680	43.2	16.6	730	47.6	17.9	745	48.9	18.2	735	49.3	18.0	4085	43.6	
Female	2045	63.1	38.7	895	56.8	17.0	805	52.4	15.2	780	51.1	14.8	755	50.7	14.3	5280	56.4	
Total	3240	100.0	34.6	1575	100.0	16.8	1535	100.0	16.4	1525	100.0	16.3	1490	100.0	15.9	9365	100.0	
Age																		
19-24	80	2.5	50.0	20	1.3	12.5	30	1.9	18.8	20	1.3	12.5	10	0.7	6.3	160	1.7	
25-34	795	24.5	41.0	385	24.5	19.8	295	19.2	15.2	260	17.0	13.4	205	13.8	10.6	1940	20.7	
35-44	1230	38.0	35.3	630	40.1	18.1	625	40.6	17.9	515	33.8	14.8	485	32.7	13.9	3485	37.2	
45-54	870	26.9	29.9	425	27.1	14.6	465	30.2	16.0	575	37.7	19.8	575	38.7	19.8	2910	31.1	
55-64	265	8.2	30.6	110	7.0	12.7	125	8.1	14.5	155	10.2	17.9	210	14.1	24.3	865	9.2	
Total	3240	100.0	34.6	1570	100.0	16.8	1540	100.0	16.5	1525	100.0	16.3	1485	100.0	15.9	9360	100.0	
Ethnicity																		
Māori	815	25.2	53.3	250	15.9	16.3	210	13.7	13.7	165	10.9	10.8	90	6.0	5.9	1530	16.4	
NZ European	1510	46.7	24.6	1060	67.3	17.2	1120	73.0	18.2	1180	77.6	19.2	1275	85.6	20.7	6145	65.7	
Pacific	465	14.4	63.3	125	7.9	17.0	75	4.9	10.2	45	3.0	6.1	25	1.7	3.4	735	7.9	
Asian	355	11.0	51.4	110	7.0	15.9	95	6.2	13.8	80	5.3	11.6	50	3.4	7.2	690	7.4	
Total	3235	100.0	34.6	1575	100.0	16.8	1535	100.0	16.4	1520	100.0	16.2	1490	100.0	15.9	9355	100.0	
Highest qualification																		
Missing	1515	46.8	98.7	10	0.6	0.7	5	0.3	0.3	5	0.3	0.3	0	0.0	0.0	1535	16.4	
No qualification	490	15.1	36.8	320	20.3	24.1	235	15.3	17.7	200	13.1	15.0	85	5.7	6.4	1330	14.2	
School qualification	405	12.5	21.5	430	27.3	22.9	405	26.4	21.5	350	23.0	18.6	290	19.5	15.4	1880	20.1	
Post-school qualification	655	20.2	21.0	630	40.0	20.2	640	41.7	20.5	620	40.7	19.9	570	38.4	18.3	3115	33.3	
Degree or higher	175	5.4	11.7	185	11.7	12.3	250	16.3	16.7	350	23.0	23.3	540	36.4	36.0	1500	16.0	
Total	3240	100.0	34.6	1575	100.0	16.8	1535	100.0	16.4	1525	100.0	16.3	1485	100.0	15.9	9360	100.0	
Time-varying variables																		
Family income																		
Q1 (lowest)	2885	89.0	89.5	265	16.8	8.2	70	4.6	2.2	5	0.3	0.2	0	0.0	0.0	3225	34.5	
Q2	345	10.6	21.8	895	56.8	56.5	230	15.0	14.5	100	6.6	6.3	15	1.0	0.9	1585	16.9	

Q3	10	0.3	0.7	375	23.8	24.6	805	52.4	52.8	290	19.0	19.0	45	3.0	3.0	1525	16.3
Q4	0	0.0	0.0	40	2.5	2.6	420	27.4	27.5	770	50.5	50.5	295	19.9	19.3	1525	16.3
Q5 (highest)	0	0.0	0.0	0	0.0	0.0	10	0.7	0.7	360	23.6	24.0	1130	76.1	75.3	1500	16.0
Total	3240	100.0	34.6	1575	100.0	16.8	1535	100.0	16.4	1525	100.0	16.3	1485	100.0	15.9	9360	100.0

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). NZ European = New Zealand European. N = 5 participants with missing values for ethnicity are not shown in the table, but were included in total counts. Note that the family income quintile boundaries calculated for the study sample were applied to ensure comparability with **Table 18**.

**Table 92: Family type by time-invariant and time-varying variables, N=9,360 (unbalanced panel), Wave 1**

	Family type										
	One-parent			Two-parent			Single, couple only			Total	
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %
Time-invariant variables											
Gender											
Male	210	14.7	5.1	3260	48.4	79.8	615	51.3	15.1	4085	43.6
Female	1215	85.3	23.0	3475	51.6	65.9	585	48.8	11.1	5275	56.4
Total	1425	100.0	15.2	6735	100.0	72.0	1200	100.0	12.8	9360	100.0
Age											
19-24	120	8.4	23.3	215	3.2	41.7	180	15.0	35.0	515	5.5
25-34	405	28.4	16.0	1590	23.6	62.7	540	45.0	21.3	2535	27.1
35-44	540	37.9	14.9	2880	42.8	79.3	210	17.5	5.8	3630	38.8
45-54	295	20.7	12.9	1800	26.7	78.6	195	16.3	8.5	2290	24.5
55-64	65	4.6	16.7	250	3.7	64.1	75	6.3	19.2	390	4.2
Total	1425	100.0	15.2	6735	100.0	72.0	1200	100.0	12.8	9360	100.0
Ethnicity											
Māori	745	52.5	12.1	4495	66.7	73.1	905	75.1	14.7	6145	65.7
NZ European	460	32.4	30.0	940	14.0	61.2	135	11.2	8.8	1535	16.4
Pacific	135	9.5	18.5	540	8.0	74.0	55	4.6	7.5	730	7.8
Asian	60	4.2	8.6	560	8.3	80.6	75	6.2	10.8	695	7.4
Other	20	1.4	8.0	195	2.9	78.0	35	2.9	14.0	250	2.7
Total	1420	100.0	15.2	6735	100.0	72.0	1205	100.0	12.9	9360	100.0
Highest qualification											
No qualification	445	31.3	23.5	1290	19.1	68.3	155	12.9	8.2	1890	20.2

Secondary school qualification	330	23.2	13.5	1830	27.2	74.7	290	24.1	11.8	2450	26.2
Post-school qualification	505	35.6	14.7	2440	36.2	71.2	480	39.8	14.0	3425	36.6
Degree or higher	140	9.9	8.8	1180	17.5	73.8	280	23.2	17.5	1600	17.1
Total	1420	100.0	15.2	6740	100.0	72.0	1205	100.0	12.9	9365	100.0
Time-varying variables											
Family income											
Q1 (lowest)	925	64.9	38.9	1175	17.4	49.4	280	23.4	11.8	2380	25.4
Q2	285	20.0	15.1	1370	20.3	72.5	235	19.7	12.4	1890	20.2
Q3	120	8.4	6.9	1420	21.1	81.1	210	17.6	12.0	1750	18.7
Q4	60	4.2	3.6	1360	20.2	81.0	260	21.8	15.5	1680	18.0
Q5 (highest)	35	2.5	2.1	1410	20.9	85.2	210	17.6	12.7	1655	17.7
Total	1425	100.0	15.2	6735	100.0	72.0	1195	100.0	12.8	9355	100.0
Equivalentised family income (minus FTC)											
Q1 (lowest)	830	58.5	34.5	1440	21.4	59.9	135	11.3	5.6	2405	25.7
Q2	325	22.9	16.6	1510	22.4	77.2	120	10.0	6.1	1955	20.9
Q3	125	8.8	7.4	1390	20.6	82.7	165	13.8	9.8	1680	18.0
Q4	85	6.0	5.0	1335	19.8	78.3	285	23.8	16.7	1705	18.2
Q5 (highest)	55	3.9	3.4	1060	15.7	65.8	495	41.3	30.7	1610	17.2
Total	1420	100.0	15.2	6735	100.0	72.0	1200	100.0	12.8	9355	100.0
Equivalentised family income (minus IWTC) (Wave 4)											
Q1 (lowest)	545	54.8	16.8	940	17.1	29.0	1755	61.6	54.2	3240	34.7
Q2	240	24.1	15.3	1185	21.5	75.5	145	5.1	9.2	1570	16.8
Q3	110	11.1	7.2	1240	22.5	81.0	180	6.3	11.8	1530	16.4
Q4	65	6.5	4.3	1150	20.9	75.7	305	10.7	20.1	1520	16.3
Q5 (highest)	35	3.5	2.3	990	18.0	66.4	465	16.3	31.2	1490	15.9
Total	995	100.0	10.6	5505	100.0	58.9	2850	100.0	30.5	9350	100.0

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). NZ European = New Zealand European.

**Table 93: Number of dependent children by time-invariant and time-varying variables, N=9,360 (unbalanced panel), Wave 1**

		Number of dependent children																
		0			1			2			3			4 to ten			Total	
		N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %

Time-invariant variables																	
Gender																	
Male	1160	46.5	28.4	1065	41.4	26.1	1130	43.3	27.7	530	44.0	13.0	200	42.1	4.9	4085	43.6
Female	1335	53.5	25.3	1510	58.6	28.6	1480	56.7	28.1	675	56.0	12.8	275	57.9	5.2	5275	56.4
Total	2495	100.0	26.7	2575	100.0	27.5	2610	100.0	27.9	1205	100.0	12.9	475	100.0	5.1	9360	100.0
Age																	
19-24	220	8.8	42.7	180	7.0	35.0	85	3.3	16.5	25	2.1	4.9	5	1.0	1.0	515	5.5
25-34	580	23.2	22.9	705	27.4	27.8	730	28.0	28.8	370	30.7	14.6	150	31.3	5.9	2535	27.1
35-44	405	16.2	11.2	925	35.9	25.5	1365	52.3	37.6	660	54.8	18.2	275	57.3	7.6	3630	38.8
45-54	985	39.5	42.9	710	27.6	30.9	415	15.9	18.1	140	11.6	6.1	45	9.4	2.0	2295	24.5
55-64	305	12.2	78.2	55	2.1	14.1	15	0.6	3.8	10	0.8	2.6	5	1.0	1.3	390	4.2
Total	2495	100.0	26.6	2575	100.0	27.5	2610	100.0	27.9	1205	100.0	12.9	480	100.0	5.1	9365	100.0
Ethnicity																	
Māori	1775	70.9	28.9	1565	60.7	25.5	1845	70.7	30.0	740	61.4	12.0	220	46.3	3.6	6145	65.5
NZ European	290	11.6	18.9	475	18.4	30.9	365	14.0	23.8	255	21.2	16.6	150	31.6	9.8	1535	16.4
Pacific	160	6.4	21.8	230	8.9	31.3	135	5.2	18.4	120	10.0	16.3	90	18.9	12.2	735	7.8
Asian	205	8.2	29.5	240	9.3	34.5	185	7.1	26.6	55	4.6	7.9	10	2.1	1.4	695	7.4
Total	2505	100.0	26.7	2580	100.0	27.5	2610	100.0	27.8	1205	100.0	12.9	475	100.0	5.1	9375	100.0
Highest qualification																	
No qualification	495	19.8	26.1	530	20.6	28.0	425	16.3	22.4	285	23.7	15.0	160	33.7	8.4	1895	20.2
School qualification	615	24.6	25.1	665	25.8	27.1	705	27.0	28.8	330	27.4	13.5	135	28.4	5.5	2450	26.2
Post-school qualification	955	38.2	27.9	915	35.5	26.7	1005	38.5	29.3	400	33.2	11.7	150	31.6	4.4	3425	36.6
Degree or higher	435	17.4	27.3	465	18.1	29.2	475	18.2	29.8	190	15.8	11.9	30	6.3	1.9	1595	17.0
Total	2500	100.0	26.7	2575	100.0	27.5	2610	100.0	27.9	1205	100.0	12.9	475	100.0	5.1	9365	100.0
Time-varying variables																	
Family income																	
Q1 (lowest)	495	19.9	20.8	840	32.7	35.2	605	23.1	25.4	300	24.9	12.6	145	30.5	6.1	2385	25.5
Q2	435	17.5	23.1	495	19.3	26.3	530	20.3	28.1	285	23.7	15.1	140	29.5	7.4	1885	20.1
Q3	420	16.9	24.0	445	17.3	25.4	540	20.7	30.9	245	20.3	14.0	100	21.1	5.7	1750	18.7
Q4	545	21.9	32.5	395	15.4	23.6	480	18.4	28.7	205	17.0	12.2	50	10.5	3.0	1675	17.9
Q5 (highest)	595	23.9	35.8	395	15.4	23.8	460	17.6	27.7	170	14.1	10.2	40	8.4	2.4	1660	17.7
Total	2490	100.0	26.6	2570	100.0	27.5	2615	100.0	28.0	1205	100.0	12.9	475	100.0	5.1	9355	100.0
Equivalentised family income (minus FTC)																	
Q1 (lowest)	230	17.8	9.5	745	28.9	30.9	650	24.9	27.0	395	32.8	16.4	255	53.7	10.6	135	11.3

Q2	255	19.7	13.0	525	20.4	26.7	600	23.0	30.5	335	27.8	17.0	130	27.4	6.6	120	10.0
Q3	245	18.9	14.6	470	18.3	28.1	535	20.5	31.9	210	17.4	12.5	50	10.5	3.0	165	13.8
Q4	305	23.6	17.9	465	18.1	27.3	460	17.6	27.0	160	13.3	9.4	30	6.3	1.8	285	23.8
Q5 (highest)	260	20.1	16.2	370	14.4	23.1	365	14.0	22.7	105	8.7	6.5	10	2.1	0.6	495	41.3
Total	1295	100.0	13.8	2575	100.0	27.5	2610	100.0	27.9	1205	100.0	12.9	475	100.0	5.1	1200	100.0
Equivalised family income (minus IWTC)																	
Q1 (lowest)	230	16.6	7.1	395	20.8	12.2	475	22.8	14.7	240	28.7	7.4	145	48.3	4.5	1755	61.6
Q2	215	15.5	13.7	390	20.5	24.8	490	23.6	31.2	240	28.7	15.3	90	30.0	5.7	145	5.1
Q3	285	20.6	18.6	415	21.8	27.0	460	22.1	30.0	160	19.2	10.4	35	11.7	2.3	180	6.3
Q4	360	26.0	23.7	380	20.0	25.0	355	17.1	23.4	105	12.6	6.9	15	5.0	1.0	305	10.7
Q5 (highest)	295	21.3	19.9	320	16.8	21.5	300	14.4	20.2	90	10.8	6.1	15	5.0	1.0	465	16.3
Total	1385	100.0	14.8	1900	100.0	20.3	2080	100.0	22.2	835	100.0	8.9	300	100.0	3.2	2850	100.0
Family type																	
Single, couple only	1200	48.1	100.0	0	0.0	0.0	0	0.0	0.0	0	0.0	0.0	0	0.0	0.0	1200	12.8
One-parent family	280	11.2	19.6	555	21.6	38.9	375	14.4	26.3	140	11.6	9.8	75	15.8	5.3	1425	15.2
Two-parent family	1015	40.7	15.1	2020	78.4	30.0	2235	85.6	33.2	1065	88.4	15.8	400	84.2	5.9	6735	72.0
Total	2495	100.0	26.7	2575	100.0	27.5	2610	100.0	27.9	1205	100.0	12.9	475	100.0	5.1	9360	100.0

Note: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). NZ European = New Zealand European. N = 255 (3.7%) participants with an *Other* ethnicity are not shown in the table but are included in total counts.

**Table 94: Employment status by time-invariant and time-varying variables, N=9,360 (unbalanced panel), Wave 4**

	Employment status														
	Unemployed			Employed			Inactive			Missing			Total		
	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	Row %	N	Col %	
Time-invariant variables															
Gender															
Female	75	62.5	1.4	3305	50.9	62.6	1020	84.0	19.3	880	57.3	16.7	5280	56.4	
Male	45	37.5	1.1	3185	49.1	78.1	195	16.0	4.8	655	42.7	16.1	4080	43.6	
Total	120	100.0	1.3	6490	100.0	69.3	1215	100.0	13.0	1535	100.0	16.4	9360	100.0	
Age															
19-24	5	4.2	3.2	85	1.3	54.8	30	2.5	19.4	35	2.3	22.6	155	1.7	
25-34	20	16.7	1.0	1180	18.2	60.8	345	28.4	17.8	395	25.7	20.4	1940	20.7	
35-44	50	41.7	1.4	2460	37.9	70.5	425	35.0	12.2	555	36.2	15.9	3490	37.3	

45-54	30	25.0	1.0	2185	33.7	75.1	270	22.2	9.3	425	27.7	14.6	2910	31.1
55-64	15	12.5	1.7	580	8.9	67.1	145	11.9	16.8	125	8.1	14.5	865	9.2
Total	120	100.0	1.3	6490	100.0	69.3	1215	100.0	13.0	1535	100.0	16.4	9360	100.0
Ethnicity														
Māori	40	32.0	2.6	830	12.8	54.1	240	19.8	15.6	425	27.7	27.7	1535	16.4
NZ European	55	44.0	0.9	4770	73.5	77.6	710	58.7	11.6	610	39.7	9.9	6145	65.7
Pacific	15	12.0	2.1	320	4.9	43.8	115	9.5	15.8	280	18.2	38.4	730	7.8
Asian	10	8.0	1.4	395	6.1	56.8	110	9.1	15.8	180	11.7	25.9	695	7.4
Total	125	100.0	1.3	6490	100.0	69.3	1210	100.0	12.9	1535	100.0	16.4	9360	100.0
Highest qualification														
Missing	0	0.0	0.0	0	0.0	0.0	0	0.0	0.0	1530	99.4	100.0	1530	16.3
No qualification	35	29.2	2.6	955	14.7	71.8	340	28.0	25.6	0	0.0	0.0	1330	14.2
Secondary school qualification	15	12.5	0.8	1570	24.2	83.3	295	24.3	15.6	5	0.3	0.3	1885	20.1
Post-school qualification	55	45.8	1.8	2620	40.3	84.1	435	35.8	14.0	5	0.3	0.2	3115	33.2
Degree or higher	15	12.5	1.0	1345	20.7	89.4	145	11.9	9.6	0	0.0	0.0	1505	16.1
Total	120	100.0	1.3	6495	100.0	69.3	1215	100.0	13.0	1540	100.0	16.4	9370	100.0
Time-varying variables														
Family income														
Q1 (lowest)	75	60.0	2.3	1055	16.2	32.7	580	47.7	18.0	1515	98.1	47.0	3225	34.4
Q2	25	20.0	1.6	1275	19.6	80.4	270	22.2	17.0	15	1.0	0.9	1585	16.9
Q3	15	12.0	1.0	1360	20.9	88.9	150	12.3	9.8	5	0.3	0.3	1530	16.3
Q4	5	4.0	0.3	1435	22.1	93.5	90	7.4	5.9	5	0.3	0.3	1535	16.4
Q5 (highest)	5	4.0	0.3	1370	21.1	91.0	125	10.3	8.3	5	0.3	0.3	1505	16.0
Total	125	100.0	1.3	6495	100.0	69.2	1215	100.0	13.0	1545	100.0	16.5	9380	100.0
Q1 (lowest)	70	58.3	2.2	1030	15.8	31.8	620	51.0	19.1	1520	98.4	46.9	3240	34.5
Q2	30	25.0	1.9	1280	19.7	81.0	260	21.4	16.5	10	0.7	0.6	1580	16.8
Q3	10	8.3	0.6	1385	21.3	89.9	140	11.5	9.1	5	0.3	0.3	1540	16.4
Q4	5	4.2	0.3	1435	22.1	94.1	80	6.6	5.2	5	0.3	0.3	1525	16.3
Q5 (highest)	5	4.2	0.3	1370	21.1	91.6	115	9.5	7.7	5	0.3	0.3	1495	15.9
Total	120	100.0	1.3	6500	100.0	69.3	1215	100.0	13.0	1545	100.0	16.5	9380	100.0
Family type														
Single, couple only	35	29.2	3.5	660	10.2	65.7	300	24.7	29.9	10	0.7	1.0	1005	10.7
One-parent	65	54.2	1.2	4635	71.4	84.1	750	61.7	13.6	60	3.9	1.1	5510	58.8

Two-parent	20	16.7	0.7	1200	18.5	42.1	165	13.6	5.8	1465	95.4	51.4	2850	30.4
Total	120	100.0	1.3	6495	100.0	69.4	1215	100.0	13.0	1535	100.0	16.4	9365	100.0
Number of children														
0	45	37.5	1.1	2355	36.3	55.5	355	29.3	8.4	1485	96.4	35.0	4240	45.3
1	35	29.2	1.8	1570	24.2	82.4	285	23.6	15.0	15	1.0	0.8	1905	20.3
2	30	25.0	1.4	1725	26.6	82.7	305	25.2	14.6	25	1.6	1.2	2085	22.3
3	5	4.2	0.6	655	10.1	78.4	165	13.6	19.8	10	0.6	1.2	835	8.9
4 to ten	5	4.2	1.7	190	2.9	63.3	100	8.3	33.3	5	0.3	1.7	300	3.2
Total	120	100.0	1.3	6495	100.0	69.4	1210	100.0	12.9	1540	100.0	16.4	9365	100.0

Notes: Col % = column percentage (where the column total is the denominator). Row % = row percentage (where the row total is the denominator). NZ European = New Zealand European. N = 10 (0.0%) participants had missing values for employment status, but were included in total counts. N = 255 (0.0%) participants with an *Other* ethnicity are not shown in the tables, but are counted in totals.