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DOCTORAL THESIS

Essays in Health and Income Dynamics

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in the

Department of Economics

Declaration of Authorship

I, Duangsuda SOPCHOKCHAI, declare that this thesis–titled, “Essays in Health and Income Dynamics” and the work presented in it are my own. I confirm that:

- This work was done wholly or mainly while in candidature for a research degree at this university.
- Where any part of this thesis has previously been submitted for a degree or any other qualification at this University or any other institution, this has been clearly stated.
- Where I have consulted the published work of others, this is always clearly attributed.
- Where I have quoted from the work of others, the source is always given. With the exception of such quotations, this thesis is entirely my own work.
- I have acknowledged all main sources of help.
- Where the thesis is based on work done by myself jointly with others, I have made clear exactly what was done by others and what I have contributed myself.

Signed:

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To my father, for his unwavering love, support and patience throughout my life.

To my mother, who has always been there for me.

To both my sisters, for knowing exactly how to make me smile.

General Introduction

This thesis consists of two related chapters and one unrelated chapter. The first focuses on the health of immigrants in Canada, using the most up-to-date Canadian Community Health Surveys (CCHS). I re-investigate the previously well-established Healthy Immigrant Effect (HIE)–the finding that upon arrival, immigrants are relatively healthier than the native-born population; but that this health advantage declines over the years after migration. Measures of health used in this study include self-assessed health status, the likelihood of being overweight or obese, and the incidence of various chronic conditions. The first part of this chapter replicates the heavily cited work of McDonald and Kennedy (2004) by pooling multiple years of CCHS, and estimating a model controlling for immigrants' cohorts of arrival to disentangle the true effect of years-since-migration (YSM) from the cohort effects. The second part of this chapter takes a closer look at the more recent cohorts of arrival of immigrants. Here I use a matching method to compare various measures of health between immigrants who arrived before and after the implementation of the Immigration and Refugee Protection Act (IRPA). It is important to note that this study does not make any direct link between the implementation of IRPA and the health outcomes of immigrants. It merely observes and compares the health of two different cohorts of immigrants, making no assumptions as to whether these changes are a result of IRPA. My main finding is that the initial health advantage is no longer present for more recent cohorts of immigrants to Canada, and that these recent cohorts of immigrants face higher health risks associated with being overweight or obese.

The other two chapter–Chapter 2, Income Processes and Intra-household Risk Sharing, and Chapter 3, Health Shocks and Income Dynamics–deal with different aspects of modelling of labour income risk over the life cycle using the US Medical Expenditure Panel Survey (MEPS).[1] In Chapter 2, I take advantage of MEPS's large sample size (some 40,000 households) to concentrate on estimating income risk-sharing among couples. This refers to an intra-household insurance mechanism that allows couples to diversify labour income risks; for instance, they can and dynamically coordinate labour-supply decisions in response to income shocks. Specifically, this study decomposes income volatility, distinguishing between single and couple household types, and models couples' income risk-sharing as the covariance of the husband and wife's income variance for both transitory and permanent components. I use an innovative identification strategy, assuming the invariability of market price for labour to marital status, to uncover couple-specific risk-sharing parameters by allowing the income profile of singles and non-singles to have partial common structure. I find evidence of risk-sharing between spouses in response to both transitory and permanent income shocks, suggesting that couples' earning capability might be partially insulated from the impact of transitory and permanent income risk.

Chapter 3 is co-authored with two of my supervisors, C. Deri Armstrong and G. Dunbar. The work is done primarily by myself, except for the Introduction, where both co-authors contribute to the writing. G. Dunbar also contributes to parts of the sections on Heterogeneous Health Impacts and Endogeneity, and to the Conclusion. This chapter also uses MEPS data, but focuses on understanding the significance of the negative health shocks in decomposing labour income risk. As in Chapter 2, we break down the cross-sectional variance of residual earnings into transitory and permanent components. We then propose a method to decompose the heterogeneity of health shocks impacts by partitioning the cross-sectional variance of residual earnings into a health and non-health component. We use emergency room (ER) visits as a proxy for negative health shocks,

[1] The MEPS is a panel survey that collects nationally representative data on demographic characteristics, all aspects of health, income, and employment of US citizens. The MEPS is described in detail in Chapter 2.

and we separately consider the impact of these negative health shocks for several groups, such as single men and single women with no child, single mothers, and couples. We also probe the role of health insurance in attenuating the income effects of health shocks, and put forward a creative method to control for misspecification biases in the income regressions–the usual ability bias. Our results suggest that heterogeneity in health shocks is gender-differentiated. We find that health shocks have heterogeneous impacts for single women with no child, as well as single mothers; but no such evidence is found for single men. For couples, we find that having health insurance coverage reduces the impact of negative health shocks on income volatility by roughly 10 percentage points.

Chapter 1

The Healthy Immigrant Effect: New Evidence from the Canadian Community Health Survey

Abstract

Using the most up-to-date Canadian data available, I re-examine the Healthy Immigrant Effect (HIE)—the finding that, on arrival, immigrants are relatively healthier than the native-born population, but that this health advantage declines over time. I find that, after controlling for the usual set of observables and cohort effects, the HIE is no longer found for recent cohorts of immigrants. Immigrants are no longer arriving with an initial health advantage relative to the Canadian-born population, thus making the convergence story irrelevant. In addition, there is evidence supporting a *worsening* in health among male immigrants: their health deteriorates with years-since-migration (YSM), diverging from the average health of native Canadians. This change is particularly interesting given recent policy changes targeting more employable and educated immigrants, which might have been expected to *increase* the initial health advantage. I verify whether more recent cohorts of immigrants have poorer health than earlier cohorts using a semi-parametric matching method. My findings contradict the standard conjecture regarding the relationship between health and socio-economic variables: more recent cohorts of immigrants experience a higher likelihood of being overweight

or obese than their earlier cohort counterparts, even though they have higher educational attainment and better labour-market outcomes. As an extension to this study, I demonstrate how the previous misreporting of height and weight may partly play a role in explaining the HIE found in earlier literature.

1.1 Introduction

Immigrants accounted for 20.6% of the Canadian population in 2011, and each year since 1990, approximately 200,000 individuals have immigrated to Canada (Statistics Canada, 2011b). Canada views immigration, in part, as the solution to many long-term economic problems. It is hoped that immigrants will help to ease the fiscal burden of the growing population of retirees, and will also help to fill labour-market shortages (Friesen, 2012). However, if immigrants' health deteriorates over time in Canada, then their ability to work, as well as their level of productivity, will be negatively affected; and the overall economic contribution of immigration will also be lessened. Poorer health can also translate into costly health-care expenditures. Given that immigrants represent a large proportion of Canada's population, immigrant health is of interest to both researchers and policy makers.

The Healthy Immigrant Effect (HIE) refers to the finding that, on arrival, immigrants are relatively healthier than the native-born population; but that this health advantage diminishes with YSM. The HIE can generally be broken down into two parts (see for example Deri (2005) or McDonald and Kennedy (2004)). First is an initial health advantage: newly arrived immigrants are healthier than their native-born counterparts. Second is a shrinking or disappearance of this health advantage over time. The initial health gap is generally explained by selection: immigrants are likely to come from the upper tail of the health distribution. Selection may arise from a variety of sources, including the non-randomness of the types of individuals who choose to take on the risky process of migration; or the health-screening process of the host country, which precludes entry of immigrants whose health is likely to cause excessive demands on health or social services (Citizenship and Immigration Canada, 2012).[1] The most commonly cited reasons for the subsequent disappearance of the health advantage include: 1) acculturation—over time, immigrants adopt the poorer lifestyle of the native-born population; 2) barriers to health-care access resulting from cultural and/or language differences;[2] 3) selective emigration: relatively healthier immigrants emigrate from their host country, thereby decreasing the average health among those who remain; and 4) immigrants often learn of previously undiagnosed health problems in the host country, whose health-care system is assumed to be superior.[3]

It is well known that properly identifying immigrant health dynamics cannot be done with a single cross-section of data. With only one cross-section, a researcher cannot distinguish a story of declining health following immigration from a story of increasing health quality of immigrants arriving over time. At a minimum, pooling of multiple cross-sections is required so that each specific immigrant cohort can be observed at multiple points in time.[4] Using such methods, a number of studies have found evidence supporting the HIE in a variety of countries and for a variety of measures of health.[5] Specific to Canada, studies documenting the HIE include McDonald and Kennedy (2004), McDonald and Kennedy (2005), Deri (2005), Kennedy, McDonald, and Biddle (2006), and McDonald (2006).

[1] An American study confirms this hypothesis. Jasso et al. (2004) find robust empirical evidence that healthier people are more likely to emigrate because they have relatively higher economic gains from doing so.

[2] Dunlop, Coyte, and Mclsaac (2000) and Stewart (1990) find that inequality in health-care access across various social groups can partially be explained by perceptions of health and illness, awareness of health risks, and attitudes toward the benefits of medical treatment.

[3] For further discussion of these hypotheses, see McDonald and Kennedy (2004), McDonald and Kennedy (2005), Kennedy, McDonald, and Biddle (2006), Antecol and Bedard (2006), and Deri (2005).

[4] This empirical limitation is demonstrated in Borjas (1985), in the context of immigrants' income growth with YSM.

[5] For example, an Australian study—Biddle, Kennedy, and McDonald (2007)—and an American study, Antecol and Bedard (2006), both find support for the HIE using chronic conditions and activity limitation as measures of health. Other American studies, such as Choi (2012) and Corlin et al. (2014), also find evidence of the HIE among immigrants using obesity rates, the incidence of chronic conditions, mortality rates, asthma, body weight, and cholesterol levels.

McDonald and Kennedy (2004) and McDonald and Kennedy (2005) pool the 1996 National Population Health Survey (NPHS) and 2000/01 Canadian Community Health Survey (CCHS), and confirm the presence of the HIE using the incidence of chronic conditions, self-assessed health, and the probability of being overweight or obese as measures of health. In particular, McDonald and Kennedy (2005) explain the decline in immigrants' health post-migration by the concept of "ethnic social network effects"—how residing in a neighbourhood with a relatively large ethnic community negatively affects overweight and obesity rates. Their results suggest that living among one's own ethnic group can mitigate the health convergence process among the immigrant population in Canada. Deri (2005) uses three cycles of NPHS (1994–95, 1996–97, and 1998–99), and finds strong evidence of the HIE—using as measures of poor health respondent self-assessments, activity limitation, excessive Body Mass Index (BMI), and presence of chronic conditions. Moreover, she finds that the HIE is a phenomenon that primarily affects older immigrants.

Using the 2000/01 NPHS and the 2002/03 CCHS, McDonald (2006) examines how health-related behaviours such as regular alcohol consumption, binge drinking, daily smoking, lack of physical activity, and lack of fruit and vegetable consumption change with YSM to help understand immigrants' health dynamics. However, the inclusion of these behaviours as explanatory variables is found to have little effect on the magnitude of the initial health gap, or on the rate of health convergence between immigrants and native-born Canadians. Other studies that pool data—NPHS (1996) and CCHS (2001 and 2003), Kennedy, McDonald, and Biddle (2006)—also find clear evidence of the HIE. The authors show that the effect is stronger for immigrants from developing countries than for immigrants from developed countries. In contrast to these studies, Laroche (2000)—using two cycles of the General Social Survey (1985 and 1991), and using as measures of health self-assessed health status, chronic conditions, activity limitation, and measures of health service utilization—finds no evidence of the HIE. Granted, these Canadian studies only include data up to the early 2000s; while Canada's evolving immigration policy means that newly arrived immigrants continue to change in their demographic characteristics. The current immigrant population is exceptionally diverse.

One of the key factors in this process is immigration policy. In 2002, Canada's immigration system underwent a major reform with the implementation of the Immigration and Refugee Protection Act (IRPA), which changed the conditions under which points were awarded for "economic class" immigrants (Citizenship and Immigration Canada, 2010a).[6,7] Under this new selection system, factors that contribute to long-term economic success—age, education, work experience, and language skills—are given more weight than under the previous points system.[8] As a result, the characteristics of pre- and post-IRPA immigrant are rather distinct—an implication supported by recent studies. Immigrants that arrived under IRPA are, on average, better educated, with a better knowledge of Canada's two official languages. Before IRPA, some 26% of immigrants held an M.A. or a Ph.D. degree; after IRPA, the number rose to 46%. The same is true of individuals knowing English and/or French: 77% pre-IRPA and 96% post-IRPA. More importantly, IRPA immigrants perform, on average, some 21% to 46% better than pre-IRPA immigrants in terms of employment earnings (Citizenship and Immigration Canada, 2010a).

Given that newer cohorts of immigrant are relatively more economically successful than previous cohorts, and given the well-established positive relationship between income and health, one might expect the initial health advantage between the immigrant and the native-born populations to be even larger. So in re-investigating the topic, I use measures of health common in the HIE literature, and follow closely the methodology

[6] Canada admits immigrants through three classes: economic, family, and refugee. Each year, about 50% of new immigrants are admitted through the economic class (Citizenship and Immigration Canada, 2010b). This class is based on a system of points awarded for factors such as education and language skills. For this type of application to be considered, a minimum of 70 out of a possible 112 points was required before IRPA; post IRPA, the requirement is 67 out of a possible 100.

[7] Other recent changes that concern economic class immigrants include the introduction of new programs such as the Canadian Experience Class (2008), the Ministerial Instruction (2008), the Provincial Nominee Program, and the Temporary Foreign Worker program. These all encourage applications from immigrants who possess socio-economic factors associated with labour market success.

[8] Applicants may now receive 25 points for their education, rather than 20 points pre-IRPA (see Citizenship and Immigration Canada (2010a) for a detailed comparison between the two regimes).

from previous studies—particularly the heavily cited McDonald and Kennedy (2004). I consider the incidence of chronic conditions, the likelihood of individuals' self-reporting poor health, and their likelihood of being overweight or obese. My main finding is that the HIE is no longer present. Specifically, I find no initial health advantage for recent immigrant cohorts—a surprising result, given the trend toward higher levels of education and better labour-market outcomes for these cohorts. Moreover, I find evidence that the recent cohorts of male immigrants not only have no initial health advantage; they actually experience a *deterioration* in health with YSM, diverging from the average health of otherwise identical Canadian-born males.[9]

To investigate the disappearance of the initial health gap, I employ a semi-parametric matching technique to compare the health of newly arrived immigrants pre-IRPA to post-IRPA. Specifically, I compare the health of recent immigrants in 2000, before the change in the selection system, to that of immigrants in 2009 and 2011, who arrived after the change.[10] This technique allows me to analyze whether there is any change in observed health between these cohorts of immigrants. It is important to note that this study does not intend to directly identify the effect of IRPA on the health of recent immigrants. In fact, I expected average immigrant health to be higher post-IRPA, since recent cohorts have more human capital and a higher earning potential. However, I find the reverse: later immigrants experience worse health than earlier, as measured by their incidence of being overweight or obese.

As an extension to this study, I also investigate how immigrants' misreporting of their weight and height in earlier research (which translates to underestimating the probability

[9]Note that this finding is similar to the HIE previously found in earlier studies, albeit from a higher initial level.

[10]Note that I do not use the years immediately after the change, from 2002 to 2008, for two reasons. First, immigrants under the pre-IRPA regime continued to arrive in Canada until 2006. Second, I define a recent immigrant as one who arrived in Canada in the previous three years. For example, using 2009 data, I define recent immigrants as those who arrived between 2007 and 2009. The ideal method is to use only those who arrived within a year to precisely represent newly arrived immigrants; however, the small sample sizes preclude this strategy. My intention is to exclude immigrants who arrived during the overlapping of the two regimes.

of being overweight or obese) could play some role in explaining the HIE reported in previous literature. My analysis takes advantage of the Canadian Health Measures Survey (CHMS), which collects information on the health of Canadians through both personal interviews and physical measurements. I find that immigrants are, on average, more likely than the native-born population to misreport both weight and height. This probability, however, decreases with the number of years residing in the new country. My finding suggests that the classic HIE may be partially explained by measurement errors of this sort.

In this chapter, Section 1.2 describes the data set used in this study; Section 1.3 illustrates the empirical model and methodology used in replicating McDonald and Kennedy (2004); Section 1.4 displays results from the replicating model; Section 1.5 shifts the focus of the analysis to recently arrived immigrants; and Section 1.6 presents the extension of this study. My Conclusion, Section 1.7, sums up the findings of Chapter 1.

1.2 Data

The main source of data used in this study is the Canadian Community Health Survey (CCHS), a cross-sectional survey that collects general health information on all Canadians over 12 years of age living in the ten provinces and three territories. Each survey cycle represents approximately 98% of the population. Starting in 2000, the CCHS collected data every two years until a redesign of the survey in 2007, after which data was collected annually. I use the CCHS 2007 and 2011 cycles to replicate the work of McDonald and Kennedy (2004), for two reasons.[11] First, CCHS 2011 is the most recent data available at the time of this study; and second, the interval between the 2007 and 2011 cycles is the same number as the two surveys used by McDonald and Kennedy (2004). I also use other cycles (2000 to 2012) for robustness and sensitivity analyses.[12]

[11] In their study, McDonald and Kennedy (2004) use the 1996 NPHS survey and the 2000–01 CCHS.
[12] Note that CCHS 2012 only became available at a later stage of this study.

I use CCHS 2000, 2009, and 2011 for the propensity score matching exercises. Specifically, I choose recent immigrants who arrived within three years of 2000 as a benchmark, and compare their health both to immigrants who arrived within three years of 2009, and to those who arrived within three years of 2011. I have two main reasons for using the year 2000 as a reference point. First, the most recent data used in McDonald and Kennedy (2004) is CCHS 2000, which makes it a relevant comparison. Second, that date is right before the implementation of IRPA, which brought cohorts of immigrants with very different observed characteristics (Citizenship and Immigration Canada, 2010a).

Following McDonald and Kennedy (2004), I restrict the sample to individuals aged 20 to 65 during the time of the health interviews, and living in the provinces (excluding those in the Northwest Territories, Yukon, and Nunavut). As in McDonald and Kennedy (2005), my study also excludes respondents whose Body Mass Index (BMI) is extreme: both greater than 50 (441 observations, about 0.8 % of my sample), and below 10 (8 observations, about 0.02% of my sample).[13] I also exclude pregnant women from my analyses, to avoid heterogeneity in terms of health issues associated with pregnancy as suggested in Deri (2005); and students, to avoid heterogeneity in terms of lifestyle and associated health.[14] Out of 53,162 observations, there are 421 pregnant women and 4,098 students in CCHS 2007 and 2011. The final sample for the pooled 2007 and 2011 data is therefore 21,048 observations for males, and 24,057 for females.

I consider four measures of health: the incidence of 1) relatively more serious, and 2) less serious chronic conditions (hereafter Type A CC and Type B CC respectively); 3) an

[13]BMI is a reliable indicator of body fatness (World Health Organization, 2015). It is defined as the ratio between a person's weight and height, BMI $= \frac{\text{mass(kg)}}{(\text{height(meter)})^2}$.

[14]I do not include pregnant women in my analyses because the heavier weight reported during pregnancy is likely going to mislead the calculated BMI, and thus the likelihood of being overweight or obese. I also do not include university/college students in my analyses because a large volume of research shows that they are more likely than the average population to engage in various risky health behaviours, including alcohol use, tobacco use, physical inactivity, unhealthy dietary practices, overexposure to the sun, and ignoring preventative measures during sex. See, for example, Ah et al. (2004), Wechsler et al. (1998), Steptoe and Wardleb (2001), Steptoe et al. (2002), Meilman et al. (1989), and Jones et al. (2001).

indicator of poor self-assessed health status (SAHS); and 4) the likelihood of being overweight or obese (OVOB).[15] Type A CC is an indicator variable, which takes a value of one if the respondent reports at least one of the following conditions: asthma, back pain, high blood pressure, allergies, migraines, ulcers, bronchitis, or arthritis. Similarly, Type B CC takes a value of one if the respondent reports at least one of the following conditions: heart disease, cancer, thyroid disease, Crohn's disease, or diabetes.[16] In terms of SAHS, respondents are asked to rate their own health as: excellent, very good, good, fair, or poor. As in McDonald and Kennedy (2004), I re-define SAHS as a binary variable: it takes a value of one if the respondent reports having fair or poor health, and zero otherwise. As an additional objective measure of health, I also consider the likelihood of OVOB, commonly used in studies examining the HIE—see, for example, Kennedy, McDonald, and Biddle (2006), Antecol and Bedard (2006), Cairney and Ostbye (1999), and Averett, Argys, and Kohn (2012). I define this variable as in previous studies: as a binary variable taking a value of one for a BMI greater than or equal to 25. A BMI range of 18.5 to 25 is considered a normal or healthy weight, 25 to 30 is considered overweight, above 30 is considered obese, and above 40 is considered morbidly obese. The BMI variable is calculated from the respondent's self-reported weight and height.

1.3 Replicating the Empirical Model

My analysis must deal with a well-known methodological limitation: when using a single cross-section of data to identify the effect of YSM, it is impossible to disentangle within-cohort effects from across-cohort effects. Borjas (1985) shows that using a single cross-section of data in this type of analysis results in an estimate that is biased by the across-cohort changes in immigrant characteristics. In the context of my study, this means that

[15]The Type A CC, Type B CC, and poor SAHS are measures of health used in McDonald and Kennedy (2004). The likelihood of OVOB is used in McDonald and Kennedy (2005).

[16]These variables are defined exactly as in McDonald and Kennedy (2004).

the declining average post-migration health observed could be biased by two factors: 1) the increasing health quality of later cohorts of immigrants, or 2) the decreasing health quality of immigrants due to relatively healthier immigrants selectively emigrating from the country. Borjas (1985) suggests that when multiple repeated cross-sectional data sets are available, it is possible to extract the true effect of YSM from the cohort effects by pooling such data and controlling for period of arrival in the country, i.e. the cohort effects.

I begin by replicating McDonald and Kennedy (2004) using more recent data (CCHS 2007 and 2011) to estimate the following equation.

$$\begin{aligned}\Pr(Y_{it}=1) = \Phi(&\alpha_0 + \alpha_1 age_{it} + \alpha_2 age_{it}^2 + \sum_{j=1}^{J-1}\delta_0 A_{itj} age_{it} + \sum_{j=1}^{J-1}\delta_1 A_{itj} age_{it}^2 + X_{it}'\delta_2 \\ &+ \beta_0 IMM_{it} + \beta_1 YSM_{it} + \beta_2 YSM_{it}^2 + \sum_{k=1}^{K-1}\lambda_k C_{itk} + \theta_0 cycle_{it} \\ &+ \theta_1 cycle_{it} * age_{it} + \theta_2 cycle_{it} * age_{it}^2). \end{aligned} \tag{1.1}$$

Here, Φ is the Cumulative Distribution Function (CDF) of the standard normal distribution; Y_{it} is one of the health measures mentioned above for individual i from cycle t; A_{itj} is an indicator of age group (20–29, 30–39,..., 60–65) where $j = 1$ is associated with age group 20–29, $j = 2$ is associated with age group 30–39, and so on. The interaction between age and age group indicator allows for a fairly flexible relationship between age and health (McDonald and Kennedy, 2004). The omitted age group is 30–39. IMM_{it} is an indicator of whether the individual was born outside of Canada; YSM_{it} is years-since-migration (set to zero for the native-born population); and C_{itk} is a five-year-interval indicator for cohort-of-arrival. This last is defined as having arrived prior to 1967, or in the date ranges between 1967–1971, 1972–1976, 1977–1981, 1982–1986, 1987–1991, 1992–1996, 1997–2001, 2002–2006, and 2007–2012. $Cycle_{it}$ is a dummy variable equal to 1 if the observation is from CCHS 2007, and equal to zero otherwise. Included in the vector of X_{it} are variables for factors such as education, marital status, province of residence, ethnicity, proxies for permanent income, having official languages as a mother tongue, living in an urban (versus a rural) area, and having a child under 12 in the household. As in McDonald and Kennedy (2004), ethnicity is a categorical variable for respondents—it may be white, black, Asian, aboriginal, or other. The coefficients of interest in measuring the healthy immigrant effect are β_0, β_1, and β_2. The expected signs are negative for β_0 (immigrants are healthier than natives on arrival), and positive for a combined β_1 and β_2 (immigrant's health deteriorates with years spent in Canada).

It is important to emphasize that though these cohort-specific effects allow the identification of both cohort of entry and YSM (Bloom, Grenier, and Gunderson, 1995), it restricts them to being constant over time. This assumption could be violated by selective emigration—that is, the characteristics of immigrants from cohorts in two survey cycles may differ due to non-random out-migration. Year effects, as captured by θ_0, are also assumed to be identical across the native and immigrant populations. Another important implicit assumption in my pooled regression is that all coefficients are temporally stable over the two survey cycles. To test for this, I interact all regressors with the cycle indicator, and run the regression in this form. An F-test of all interaction terms being jointly zero will test the full interacted model against the special case of that model, in which all coefficients are temporally stable. Since the F-test rejects the null, some or all of the coefficients should be allowed to vary over time. Further investigation reveals that the coefficients of age and age^2 are not temporally stable. As a result, I allow the coefficients to vary over the two survey cycles by including the interaction terms $age * cycle$ and $age^2 * cycle$ in the final regression. This is consistent with the model used by McDonald and Kennedy (2004).

The endogeneity between income and health outcomes is well documented (for example, Ettner (1996), Rivera and Currais (1999), and Devlin and Hansen (2001)). Many researchers use Instrumental Variable regressions to address this estimation issue. Unfortunately, convincing instruments are rare; and if the excluded instruments are only weakly correlated with the endogenous variables, the results can be counterproductive. Weak instruments can severely bias the estimates, tests of significance can have incorrect size, and confidence intervals can be wrong (Bound, Jaeger, and Baker, 1993; Bound, Jaeger, and Baker, 1995). I prefer to follow the strategy of McDonald and Kennedy (2004), and include in the regressions proxies for permanent income—which, unlike current income, are unlikely to be endogenous determinants of health status.[17] I use the following variables as proxies for permanent income: a binary variable indicating whether respondents own their residence; a binary variable indicating whether they receive dividend income; the number of persons in the household; the highest level of education of the household; and the number of dependent children living at home.[18] Since all dependent variables are dichotomous, the final estimations are done with a probit model.[19] The list of variables, and their detailed definitions, can be found in Appendix A.1.

So far, my analyses replicate as closely as possible those of McDonald and Kennedy (2004). Where different sets of variables are used to control for socio-economic status, I set in place robustness checks (described below). Since endogeneity does not affect linear and non-linear models in the same way, I explore the effects of income in both linear and non-linear models by estimating three different specifications with a Linear Probability Model (LPM) and a probit model.[20] The three specifications are: 1) no control for income is included; 2) proxies for income are included; and 3) income variable is included (a full analysis can be found in Appendix A.6). I find that the coefficients of interest (immigrant, YSM, and YSM-squared) are robust to various specifications of income in both linear and

[17]This strategy allows me to capture socioeconomic status in terms of income while bypassing the endogeneity issue, as well as bypassing the consequences of weak instruments from Instrumental Variable regression.

[18]This set of proxies for permanent income is chosen to coincide, as much as possible, with those used by McDonald and Kennedy (2004). Two useful proxies they used—the type of dwelling, and the number of bedrooms—are no longer available in the survey cycles available to me.

[19]I have also tried logit and cloglog, but there is no evidence that they fit the data better than a probit model as measured by Akaike Information Criterion (AIC) and Schwarz Criterion (SC).

[20] Instrumental variable techniques are no longer directly applicable in the non-linear model: since the model is not invertible, the error term does not have an explicit expression (see Greene (2002) for a thorough discussion of non-linear models)

non-linear models—in the analysis, using either proxies for income or income variable (versus not controlling for income at all) does not change the direction or the statistical significance of the coefficients of interest across different outcome variables (except for the case of SAHS). The magnitude of these coefficients are similar for specifications 1 and 3, but not for specification 2.

Another assumption that is likely to be violated in empirical work from pooling data sets is the homoscedasticity between the two survey cycles. In the presence of this heteroscedasticity, the t- and F-test based on pooled regression will be invalid (Wooldridge, 2013). The test rejects the null of constant variance. For this reason, I use robust standard error in all regressions to ensure that the tests mentioned above are valid.

1.4 Replicating Results

1.4.1 Descriptive Analysis

Table 1.1 shows the mean and standard deviations of key variables for the native and immigrant populations by gender, for the sample used in the replication analyses (CCHS 2007 and 2011). Immigrant men, on average, do better than the native men in terms of education and employment. Over 70% of male immigrants hold a post-secondary degree, while approximately 63% of male natives hold such credentials. With respect to employment, 81.2% for male immigrants (versus 79.3% for male natives) report holding a full-time job. While the proportion of immigrant women holding post-secondary degrees is similar to that of native women, there appears to be some disadvantage among immigrant women in relation to securing employment—approximately 66.1% of female immigrants are employed as compared to about 70.2% for female natives.

Immigrants also appear to fare better than natives in regards to measures of health, except in the case of SAHS for the female population. For instance, 40.9% of immigrant men and 48.6% of immigrant women report having Type A CC, though this figure is almost

TABLE 1.1: Summary Statistics

Variable	Native				Immigrant			
	Male		Female		Male		Female	
Age	45.599	(12.66)	46.262	(12.71)	46.568	(12.12)	46.852	(12.21)
Married	0.613	(0.49)	0.627	(0.48)	0.714	(0.46)	0.679	(0.47)
Live in urban area	0.696	(0.46)	0.702	(0.46)	0.884	(0.32)	0.882	(0.32)
Employed	0.793	(0.41)	0.699	(0.46)	0.812	(0.39)	0.661	(0.47)
English/French mother tongue	0.968	(0.18)	0.965	(0.17)	0.399	(0.49)	0.412	(0.49)
Own residence	0.78	(0.41)	0.778	(0.42)	0.725	(0.45)	0.734	(0.44)
Household size	2.306	(1.22)	2.346	(1.23)	2.634	(1.47)	2.643	(1.43)
Number of dependent	0.417	(0.85)	0.485	(0.90)	0.589	(1.01)	0.596	(0.97)
Receive dividend	0.216	(0.41)	0.190	(0.39)	0.207	(0.41)	0.198	(0.40)
Education								
Post-secondary	0.617	(0.49)	0.652	(0.48)	0.742	(0.44)	0.722	(0.45)
Some post-secondary	0.054	(0.23)	0.051	(0.22)	0.038	(0.19)	0.037	(0.19)
High school	0.190	(0.39)	0.187	(0.39)	0.130	(0.34)	0.152	(0.36)
Lower than high school	0.139	(0.35)	0.110	(0.31)	0.090	(0.29)	0.089	(0.29)
Race								
White	0.933	(0.25)	0.932	(0.25)	0.526	(0.50)	0.523	(0.50)
Black	0.002	(0.05)	0.003	(0.05)	0.054	(0.23)	0.062	(0.24)
Asian	0.009	(0.09)	0.008	(0.08)	0.315	(0.46)	0.313	(0.46)
Aboriginal	0.049	(0.22)	0.051	(0.23)	0.004	(0.06)	0.003	(0.059)
Other ethnics	0.007	(0.08)	0.006	(0.08)	0.099	(0.30)	0.099	(0.30)
Health								
Type A CC	0.481	(0.50)	0.552	(0.50)	0.409	(0.49)	0.486	(0.50)
Type B CC	0.144	(0.35)	0.166	(0.37)	0.139	(0.35)	0.137	(0.34)
OVOB	0.665	(0.47)	0.510	(0.50)	0.579	(0.49)	0.425	(0.49)
Poor SAHS	0.118	(0.32)	0.109	(0.31)	0.099	(0.30)	0.120	(0.32)
Immigrant-related variables								
YSM					25.359	(16.54)	25.229	(16.89)
N	18,329		20,861		2,719		3,196	

Data source: CCHS 2007 and 2011. Note: The summary statistics are weighted using the CCHS weight. Mean values reported. Standard deviations in parentheses. All summary statistics differences between the native-borns and immigrants reported are statistically significant, with the exception of the proportion of men having Type B CC.

10 percentage points higher for natives. For Type B CC, the difference is not as severe: 13.9% of immigrant men versus 14.4% of native men, and 13.7% of immigrant women versus 16.6% of native women. Similarly, fewer immigrants are overweight or obese than natives (10% lower). Immigrant men are less likely to report poor SAHS compared to native men, but immigrant women are *more* likely: the figure is 12% for immigrant women, and 9.9% for native women.[21]

1.4.2 Regression Analysis

The key coefficients from probit regressions are presented in Table 1.2. The purpose of this demonstration is to display the *general sign* and *significance level* of estimated coefficients of interest, namely *IMM, YSM, and* YSM^2. Complete regression results can be found in Appendix A.2.[22] It is important to note that coefficient estimates from a Probit estimation do not have an intuitive interpretation, and these estimates are displayed as to compare to coefficient estimates (from Probit regressions) in McDonald and Kennedy (2004). I also present marginal probability effects of the key coefficients and discuss them in detail later in the section.

For the male population, I do not find evidence of the HIE in any of the measures of health considered—contradicting most studies in the HIE literature. I find the coefficient estimates of IMM to be statistically insignificant for all measures of health: that is, recent male immigrants do not have an initial health advantage in relation to the native men. The coefficient estimates of YSM and YSM^2 are also statistically insignificant, in all but the case of Type A CC. In that case, even though there is no initial health difference, there is a worsening of the likelihood of reporting Type A CC with YSM.

[21] Differences between native-borns and immigrants reported in Table1.1 are statistically significant, with the exception of the proportion of men having Type B CC.

[22] I provide coefficients from regressions that include and exclude the cohort effects in Appendix A.2.

TABLE 1.2: Coefficients from Probit Regressions

	Type A CC	**Type B CC**	**OVOB**	**SAHS**
Male:				
Immigrant	-1.0200	0.6960	-0.6310	1.6060
	(0.967)	(1.065)	(0.897)	(1.313)
YSM	0.0824*	-0.0558	0.0240	-0.0597
	(0.036)	(0.038)	(0.0332)	(0.051)
YSM-squared	-0.0012**	0.0001	-0.0002	0.0006
	(0.000)	(0.000)	(0.000)	(0.000)
N	21,048	21,048	21,048	21,048
Female:				
Immigrant	1.435 †	0.470	-0.3990	1.834 †
	(0.794)	(1.010)	(0.805)	(0.990)
YSM	-0.0364	0.0173	0.0452	-0.0600
	(0.029)	(0.039)	(0.029)	(0.039)
YSM-squared	0.0002	0.0005	-0.0007	0.0006
	(0.000)	(0.000)	(0.000)	(0.000)
N	24,057	24,057	24,057	24,057

Data source: CCHS 2007 and 2011. Note: Robust standard errors in parentheses. The estimations were done using CCHS weights.
† $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

For the female population, I similarly find no evidence of the HIE. In fact, I find that female immigrants have poorer health relative to native females on arrival into Canada—that is, they have positive and statistically significant coefficient estimates of IMM with Type A CC, and poor SAHS. My findings contrast with those of McDonald and Kennedy (2004) who find significant evidence of the HIE in terms of Type A and Type B CC for the female population. I find that the coefficient estimates of YSM and YSM^2 are statistically insignificant in all measures of health.

Regarding the disappearance of the HIE in the model controlling for cohort dummies, a possible concern might be that this is purely the result of estimating coefficients with large standard errors, thus producing a zero effect. I argue that this is *not* the case, however, because there is no systematic pattern in the change in standard errors across model specifications. That is, the standard errors (and the coefficient estimates) of other variables, such as marital status and education, barely change in terms of their magnitude, sign, and significance level—whether or not I control for the cohort effects. For example, for the coefficient of "being married" in the female Type A CC model (Table A.2-Female-column 1 and 2), the estimates for the two specifications (controlling for, and not controlling for, cohort effects) are -0.098 and -0.099, and the standard errors are 0.0369 and 0.0367. If the standard errors of all other coefficients (education, marital status, etc.) were to change significantly—from very low to very high, or vice versa—between specifications, then the disappearance of the HIE would be a concern. That is not the case here.

One possible explanation for the disappearance of the HIE is related to the changing source countries over time. For example, due to more stringent requirements under the IRPA, the composition of immigrants may have shifted toward individuals from the developed world who are more similar to native Canadians in comparison to previous cohorts of immigrants. To address this possibility, I provide a table (Table A.4) demonstrating immigrants' source regions during the period between 2000 to 2011.[23] I find little changes in source regions over this period. Most immigrants (some 50%) come from Asia and the Pacific; about 15 to 20% are from Europe, UK, Africa, and the Middle East; roughly 10% are from South and Central America; and about 3 to 5% are from the US, confirming that changing source countries are not driving the documented disappearance of the HIE.[24]

To verify that the disappearance of the HIE is not an accidental result of the specific data years chosen (2007, 2011), I also pool various CCHS cycles (results provided in Appendix A.4). First I pool all available cycles (2000 to 2012), and then separately analyze the years 2000–2006 and 2007–2012. This strategy allows me to observe the HIE for the period 2000–2012, but this is clearly driven by earlier immigrant cohorts. I find the HIE for the 2000–2006 period, but not for 2007–2012. The only exception, in the later CCHS cycles, is for Type A CC males—this is consistent with the results discussed in Table 1.2.[25]

Since the parameter estimates from a probit regression are not directly interpretable, I calculate the marginal probability effects, which are the partial effects of each explanatory variable on the probability that individuals report having a health problem, of the key coefficients.[26] I calculate these marginal probability effects for each cohort of immigrants up to 5 years post migration, and present the predicted health profile of a typical immigrant

[23]It is not possible to display immigrants' source countries due to the small sample sizes of immigrants of some source countries and the confidentiality agreement between the researcher and Statistics Canada's Research Data Centre.

[24]Using the National Household Surveys, Figure A.1 in Appendix A.3 (adapted from Statistics Canada (2011b)) shows similar conclusion—no significant changes in immigrants' composition regions of birth of immigrants from 2001 to 2011. In fact, the shift in the composition of immigrants' source regions appear to be from before 1971 to 1990—an increase in the proportion of immigrants from Asia, from about 10% before 1971 to about 60% post 1991, and a decrease in the proportion of immigrants from the US, from about 80% before 1971 to about 20% post 1991.

[25]Other robustness checks include adding access to primary care or physicians as part of the set of covariates, and using the ordered probit (and ordered logit) in analyzing a full self-assessed health variable. The results do not change—that is, I find no indication of the HIE in the latter cycles of CCHS.

[26]Note that I do not present the usual "marginal index effects" as these figures are difficult to interpret when the explanatory variable of interest, *Immigrant* = {0,1}, is a binary variable.

as in McDonald and Kennedy (2004).[27] These results can be found in Tables A.6 to A.13 in Appendix A.5.

Tables A.6 to A.9 show predicted health profiles of each cohort of male immigrants on arrival (YSM = 0) to half a decade post migration for the four measures of health (Type A CC, Type B CC, overweight or obese, and poor SAHS, respectively). Relative to native-born men, the probability of having Type A CC and the probability of being overweight or obese are reduced by about 20 percentage points for immigrant men on arrival. These figures are further reduced with YSM; however, they are statistically insignificant—unsurprisingly as the coefficient estimates from a probit regression are also statistically insignificant. On the contrary, the probability of having Type B CC and the probability of having poor SAHS are about 6 to 10 percentage points higher for immigrants men in comparison to native-born men. These figures do not vary significantly with YSM, but, again, they are statistically insignificant.

Results for female immigrants can be found in Tables A.10 to A.13. Relative to the native-born women, female immigrants are more likely to have Type A CC, Type B CC, and poor SAHS on arrival. There appears to be some evidence suggesting that more recent cohorts of female immigrants are relatively healthier than earlier cohorts. For example, the probability of having Type A CC appears to decline among the more recent cohorts of immigrants—from about 31 percentage points for cohort 1967–1971 to about 18 percentage points for cohort 2007–2011. However, similar to results for immigrant men, these marginal probability effects are statistically insignificant.

[27] As in McDonald and Kennedy (2004), a typical immigrant is 45 years old, lives in Ontario outside of a CMA, speaks English or French as a mother tongue, has a high-school education, and does not receive dividend income (average values are used for other explanatory variables).

1.5 A Closer Look at Recent Cohorts

Since I find little evidence of the HIE in recent cohorts of immigrants, I investigate this phenomenon by comparing the initial health of more recent immigrants to that of their earlier counterparts. By non-parametrically matching observable characteristics across cohorts of arrival, I compare various measures of health for these otherwise identical immigrants. The matching method is more flexible than simply interpreting the *IMM* coefficient from the probit regression. The latter is a fully parametric model that requires the normality assumption of the data for the estimates to be valid; whereas a matching method, which requires fewer assumptions about the data, has the advantage in situations where the true distribution is unknown, or when it cannot easily be approximated using a probability distribution.[28]

Using immigrants from the year 2000 as a benchmark, I compare their health to immigrants from the years 2009 and 2011. (As I indicated earlier, it is crucial that immigrants from 2002 and 2006 are excluded from the analysis, since they represent an overlap of pre- and post-IRPA regimes (Citizenship and Immigration Canada, 2010a). One issue is that CCHS 2000—designed to represent Canada's population over a single year—actually contains information collected over two years; so its sample size is double that of the 2009 and 2011 cycles. This is problematic in terms of matching, since the sample size is not well balanced across control and treatment groups. To deal with this, I use two-year files for CCHS 2009, and for 2011. The 2009/10 file consists of samples from 2009 and 2010, and the 2011/12 file consists of samples from 2011 and 2012. This strategy re-weights the data to represent population in the same way.[29]

[28]Since all dependent variables in this study are dichotomous variables, a link function is applied to transform the data into a continuous variable in order to parametrically estimate the model. In the probit case, the link function is the cumulative normal distribution.

[29]The proper re-weighting scheme can be found in the User Guide of a two-year file of CCHS provided by Statistics Canada. See a detailed discussion in Statistics Canada (2011a)

1.5.1 Matching Method

For simplicity's sake, assume that there are two cross-sectional survey cycles, Year 1 and Year 2; and let H_i be the health variable of interest (for instance, incidence of chronic conditions). The potential outcome for each treatment state is.

$$H_i = \begin{cases} H_{1i} & \text{if } D_i = 1 \\ H_{0i} & \text{if } D_i = 0 \end{cases}$$

where D_i is the *treatment variable* defined above. It is easy to see that H_{1i} and H_{0i} cannot be observed at the same time from individual i, and thus constitute the missing data problem. The challenge here is to construct the counter-factual of person i's treatment status: what would happen if i was drawn from Year 2 survey when i actually was not, and vice versa.

The coefficient of interest is the Average Treatment Effect (ATE), which measures the difference in probability of having a health issue between the control and the treatment group. ATE is defined as:

$$\tau = E[H_i|D_i = 1] - E[H_i|D_i = 0] = E[H_1] - E[H_0] = E[H_1 - H_0] \tag{1.2}$$

where $E[H_i|D_i = 1]$ is the mean health of the treatment group, and $E[H_i|D_i = 0]$ is the mean health of the control group. The important assumption here is the exogenous condition, $(H_1, H_0)\perp D$, which allows for $E[H_0] = E[H_0|D = 0] = E[H_0|D = 1]$. Under the assumption of exogeneity, the observable, $E[H_0|D = 0]$, can be used to impute the counter-factual, $E[H_0|D = 1]$. However, the exogenous assumption is most likely violated in social-science studies, where data are rarely generated by randomized experiment, and the estimation is spoiled by selection bias: individuals self-select into treatment (Angrist and Pischke, 2008).

In the case of non-experimental data, the solution to the selection bias is to assume the *Conditional Independence Assumption* (CIA) (Lechner, 1999), $H_j \perp D|X$ for all $j \in \{0, 1\}$.

Simply, after controlling for X, the treatment is somewhat like random assignment and therefore each subgroup of the treatment group and control group is comparable.[30]

There are many possible ways to estimate ATE, including the regression method (OLS) and the matching method. For three reasons, I choose to use a propensity score matching (PSM) method (Rosenbaum and Rubin, 1983). First, I find that the linear model of OLS is restrictive in terms of model specification, while the semi-parametric technique of PSM allows for a more flexible functional form, and avoids potential model misspecification. Second, matching takes into consideration the *support problem* in estimating ATE, whereas OLS uses the entire sample regardless of whether there is a sufficient common support.[31] In essence, matching focuses on comparability in terms of observables—that is, on constructing a suitable comparison group by carefully matching treated and non-treated. Third, matching does not impose any restriction on the heterogeneity of treatment effects. While OLS regression with interactions does allow for heterogeneity, this is still limited by the functional form imposed.[32] Another important advantage of PSM is that it overcomes *the curse of dimensionality*—a phenomenon that arises when analyzing data in high-dimensional spaces (Bellman, 1961).

The PSM estimator imputes the missing potential outcome for each observation with an average of the outcomes of observations in the other treatment group that have similar propensity scores. The pair of estimated potential outcomes is given by:

$$\hat{H}_{0i} = \begin{cases} H_i & \text{if } D_i = 0 \\ \frac{1}{M} \sum_{j \in \mathcal{J}_M(i)} H_j & \text{if } D_i = 1 \end{cases}$$

and

[30] It should be emphasized that this type of matching method does not automatically solve endogeneity issue. The theory suggests that matching "can" solve the endogeneity problem if there are no unobserved variables that systematically influence treatment assignment.

[31] See Abadie et al. (2004) for a thorough discussion of the common support issue.

[32] One can perform an F-test for the presence of heterogenous effects

$$\hat{H}_{1i} = \begin{cases} H_i & \text{if } D_i = 1 \\ \frac{1}{M}\sum_{j \in \mathcal{J}_M(i)} H_j & \text{if } D_i = 0 \end{cases}$$

where $\mathcal{J}_M(i) = \{j_1(i), ..., j_M(i)\}$, and $j_m(i)$ is an index indicating the unit in the opposite treatment group that is m^{th} closest to unit i.

The ATE is then calculated by taking the average of the difference between the observed and potential outcomes for each observation:

$$\begin{aligned} \hat{\tau}_M &= \frac{1}{N}\sum_{i=1}^{N}(\hat{H}_{1i} - \hat{H}_{0i}) \\ &= \frac{1}{N}\sum_{i=1}^{N} D_i \left[H_i - \frac{1}{M}\sum_{j \in \mathcal{J}_M(i)} H_j \right] + (1 - D_i) \left[\frac{1}{M}. \sum_{j \in \mathcal{J}_M(i)} H_j - H_i \right] \end{aligned} \tag{1.3}$$

The propensity score estimation is done using a logit model; the matching algorithm used is the nearest-neighbour, which yields more precise estimates than others. The covariates used in matching analysis are age, ethnicity, education, income adequacy (income adjusted for household size and the area of living), marital status, and gender. However, derivative-based standard-error estimators cannot be used, since the matching estimator is not differentiable. Many researchers resort to using a bootstrap estimator, but this technique does not provide reliable standard errors. This is discussed in Abadie and Imbens (2008), from whose work the standard errors are derived. This also takes into account the fact that propensity scores are estimated in the first stage.

The CIA (Conditional Independence Assumption) is not testable, though one could check the sensitivity of the estimates with respect to their deviations from it. Rosenbaum (2002) suggests that if any unobserved variables affect both the assignment-into-treatment and the outcome variable at the same time, there should be "*hidden bias*" that causes the estimate to become insignificant. In the context of my study, it is possible that immigrants may have pre-existing health conditions prior to their migration that the researcher cannot observe. These conditions may have delayed their decision to migrate, and/or affected their observed health post-migration. For this reason, Rosenbaum (2002) recommends a sensitivity analysis with a bounding approach, which provides the degree to which any significant results hinge on the CIA. More specifically, I calculate statistical bounds for each of the estimates to indicate whether inferences about treatment effects are sensitive to unobserved factors Γ. Different value of Γ represents different level of hidden bias.[33] For example, when $\Gamma = 1$, there is no hidden bias. The value of Γ is then increased to reflect an increasing potential influence from unobserved variables. At some level of $\Gamma = s$, the treatment effect that was statistically significant at $\Gamma = 1$ becomes statistically insignificant. Thus it can be said that the estimate is robust to hidden bias up to $\Gamma = s$.[34] (A Rosenbaum bounds sensitivity analysis is provided at the end of section 1.5.2.)

In addition to the CIA, two other important assumptions are necessary to ensure that the matching estimators identify and consistently estimate the ATE of interest. These are the overlapping assumption (or common support), which states that the probability of assignment is bound away from zero and one; and the balancing property of the estimated propensity score, which requires that the mean of each covariate does not differ between treated and control units after being matched. Overlapping plots of the estimated densities, the discussion of the plots, and the discussion of covariates-balance tests can be found in section 1.5.2.

Lastly, I conduct robustness checks by performing PSM between immigrants and non-immigrants. The result of this matching exercise is unsatisfactory, since it is not possible to achieve a good match between immigrants and non-immigrants. This is due to the common support problem between the two populations: the proportion of non-immigrants in

[33] Rosenbaum bounds are computed using Mantel and Haenszel (MH) statistics (Aakvik, 2001).

[34] See Aakvik (2001) and Becker and Caliendo (2007) for more details on this method and the derivation of test statistics.

the lower income group is very small relative to that of the immigrant group. The complete analysis can be found in Appendix A.7.

1.5.2 Matching Results

Table 1.3 presents summary statistics of recent immigrants across survey cycles, which are consistent with the data of Citizenship and Immigration Canada (2010a). More recent cohorts of female immigrants have slightly improved labour-market outcomes. In 2011, for example, recent female immigrants were situated, on average, around the 41^{th} percentile of the income distribution graph; while back in 2000, their counterparts were only around the 39^{th} percentile. In addition, while 46% of female immigrants in 2000 report holding a job, this increases to approximately 56% for later cohorts. More recent cohorts of women also appear to be better educated, with the proportion holding a post-secondary degree rising from 68% to 81% in more recent years.[35] Likewise, recent cohorts of male immigrants are in a better state in terms of employment. The proportion holding a job during their first three years increases from 79% in 2000 to 84% in 2009, and to 88% in 2011.

For both genders together, there are no clear patterns regarding various measures of health across the data cycles. However, it appears that male immigrants are more likely to be overweight or obese in comparison to females. The gap varies from year to year but, on average, men's likelihood of being OVOB exceeds that of women by about 17 percentage points. Conversely, female immigrants are more likely than their male counterparts to have one or more chronic conditions belonging to Type A. The average gender gap for Type A CC is about 8 percentage points.

Table 1.4 displays ATEs for each health measure from the matched samples. Here, the ATE represents the difference in probability of having a certain health problem between

[35]Since I am not able to identified the specific category of immigrant, it is not possible to determine if any of the observed changes are a direct result of the change in IRPA immigration policy. On average, 67% of immigrants admitted each year were Economic class, 22% were Family class, 9% were Refugee class, and 2% were Other (Citizenship and Immigration Canada, 2010b).

TABLE 1.3: Summary Statistics: Recent Immigrants Across Survey Cycles

	Immigrant with YSM $\leq$ 3					
Variable	**2000**		**2009**		**2011**	
Male						
Age	37.06	(8.81)	36.17	(8.59)	35.23	(8.56)
Income Adequacy Decile	4.43	(3.10)	4.03	(2.90)	4.40	(2.79)
Top 50th percentile of income distribution	0.47	(0.50)	0.49	(0.50)	0.42	(0.49)
Hold a job	0.79	(0.41)	0.84	(0.37)	0.88	(0.33)
Post-secondary education	0.79	(0.41)	0.77	(0.42)	0.85	(0.35)
Ethnicity:						
White	0.38	(0.48)	0.36	(0.48)	0.31	(0.46)
Black	0.02	(0.15)	0.08	(0.27)	0.08	(0.27)
Asian	0.46	(0.50)	0.41	(0.49)	0.46	(0.50)
Age at migration	36.00	(8.85)	34.85	(8.61)	33.82	(8.50)
Type A CC	0.22	(0.41)	0.25	(0.43)	0.22	(0.41)
Type B CC	0.05	(0.21)	0.04	(0.19)	0.03	(0.17)
Overweight/obese	0.42	(0.49)	0.49	(0.50)	0.48	(0.50)
Poor/fair health	0.05	(0.21)	0.05	(0.22)	0.04	(0.19)
N	415		386		339	
Female						
Age	35.84	(9.33)	34.98	(8.87)	35.14	(9.24)
Income Adequacy	3.92	(2.87)	3.92	(2.75)	4.12	(2.70)
Top 50th percentile of income distribution	0.44	(0.50)	0.52	(0.50)	0.39	(0.49)
Hold a job	0.46	(0.50)	0.57	(0.50)	0.55	(0.50)
Post-secondary education	0.68	(0.47)	0.81	(0.39)	0.81	(0.40)
Ethnicity:						
White	0.40	(0.49)	0.27	(0.45)	0.28	(0.45)
Black	0.04	(0.20)	0.06	(0.25)	0.10	(0.30)
Asian	0.43	(0.50)	0.50	(0.50)	0.47	(0.50)
Age at migration	34.62	(9.33)	33.61	(8.91)	33.82	(9.23)
Type A CC	0.31	(0.46)	0.29	(0.45)	0.27	(0.44)
Type B CC	0.03	(0.18)	0.06	(0.23)	0.05	(0.22)
Overweight/obese	0.30	(0.46)	0.30	(0.46)	0.32	(0.47)
Poor/fair health	0.07	(0.25)	0.05	(0.21)	0.05	(0.22)
N	376		452		403	

Data source: CCHS 2000, 2009, and 2011. Note: The summary statistics are weighted using the CCHS weight. Mean values reported. Standard deviations in parentheses.

2000 immigrants and 2009 and 2011 immigrants. A positive ATE means that the later cohorts have poorer health outcomes; a negative ATE figure means the opposite. I find that except for the probability of being OVOB, there are no health differences between previous and recent cohorts—as shown by the statistical insignificance of the ATEs in Table 1.4. Immigrants from 2009 and 2011 are, on average, in worse health than those from 2000—the difference is about 7 percentage points for both groups. This is surprising given the usual negative correlation between BMI and labour-market outcomes (Kpelitse, Devlin, and Sarma, 2014).

TABLE 1.4: Average Treatment Effect

	2009 vs 2000	**2011 vs 2000**
Type A CC	0.0062	-0.0190
	(0.026)	(0.026)
Type B CC	0.0089	-0.0038
	(0.012)	(0.012)
Overweight/obese	0.0709*	0.0740*
	(0.030)	(0.031)
Poor/fair health	-0.0039	-0.0015
	(0.014)	(0.011)
N	1,359	1,486

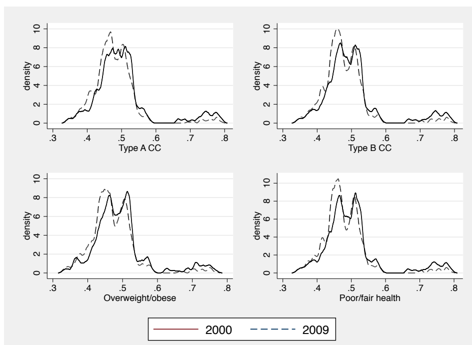
$^{\dagger}p < 0.10$, $^{*}\ p < 0.05$, $^{**}\ p < 0.01$

Data source: CCHS 2000, 2009, and 2011. Note: Robust standard errors in parentheses. The estimations were done using CCHS weights.

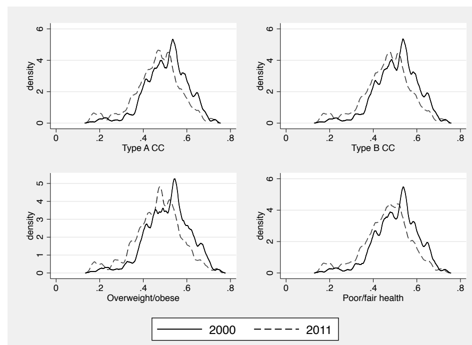
In order to confirm the validity of the estimates obtained from PSM, it is necessary to check the overlap assumption graphically as well as the t-test of balancing covariates. First, I present overlapping plots and discuss a graphical analysis of the overlap assumption. Figure 1.1a shows the estimated densities of the predicted probabilities for all health measures between 2000 and 2009 immigrants. Figure 1.1b shows the same plot for a match between 2000 and 2011 immigrants. When there is a chance of seeing observations in both the control and the treatment groups at each combination of covariate values, it can be inferred that the overlap assumption is satisfied. On the other hand, if the estimated density shows too much mass at 0 or 1, or that there is not much density in regions in which they overlap, the overlap assumption is most likely violated. Figures 1.1a and 1.1b both show well-behaved densities: masses occur in overlapping regions, and have minimal mass at 0 or 1. It can be concluded that there is no evidence of violation of the overlap assumption.

FIGURE 1.1: Estimated Densities

(A) 2000 versus 2009



(B) 2000 versus 2011



Second, I present p-values from t-tests of equality of the mean of each covariates after matching in Table 1.5. For a good balance, the mean of each covariates between the control and the treatment groups should not differ statistically after they are matched. It can be observed that, except for the variable *Post-secondary degree* in the matching sample between 2000 and 2011, all p-values are larger than 0.05. On this evidence, the covariates seem to be well-balanced after the matching.[36]

The semi-parametric approach used in this study eliminates potential bias from the observables (Austin, 2011); however, it is insufficient to eliminate hidden bias from the unobservables. As suggested in Rosenbaum (2002), I therefore check the sensitivity of estimated treatment effects by considering what happens when there are deviations from the underlying identified CIA. In the context of this study, potential bias from the unobservables could result in a positive selection—those immigrating at a later age are more likely

[36]The variable *Post-secondary degree* is kept in the analysis for the purpose of consistency across different matched samples. Omitting this variable does not influence my final results.

TABLE 1.5: Covariates Balance Check: P-Values from t-test

Variable	**2000 vs 2009**	**2000 vs 2011**
Age	0.373	0.662
Ethnic-White	0.667	0.499
Ethnic-Asian	0.958	0.506
Ethnic-Others	1.000	0.218
Post-secondary degree	0.121	0.026
Married	0.093	0.368
Male	0.397	0.474
Household income distribution	0.563	0.942
N	1,359	1,486

Data source: CCHS 2000, 2009, and 2011

to have poor health. Immigrants may have delayed their migration because of their poor health; in that case, the treatment effects will be overestimated. I present the upper-bound test statistics, Q^+, and its corresponding level of significance p^+ for each health outcome in Table 1.6. The test is calibrated for different values of Γ between 1 and 2 at 0.25 increments. Table 1.6 demonstrates some evidence that the ATEs obtained from the matching exercises are reasonably robust to hidden bias. In both matching assignments, the results for various outcomes—Type B chronic condition, overweight/obese, and poor/fair health—are all insensitive to high value of $\Gamma \leq 2$. This indicates insensitivity to any bias that would double the odds of immigrating in later years. The ATEs for the Type A chronic condition are marginally more sensitive. The results could be affected by hidden bias at Γ =1.5. Note that the value of $\Gamma = 1.5$ does not mean that unobserved heterogeneity exists. This test merely indicates that the confidence interval for the calculated effect could change from "including zero" to "not including zero," and vice versa. It is important to stress that this technique cannot directly *test* the CIA; it merely suggests caution when interpreting the results when there are possible deviations from the identifying CIA.

TABLE 1.6: MH Bounds Test Statistics Q^+

	Gamma (Γ)									
	1		1.25		1.5		1.75		2	
2000 VS 2009										
Type A CC	0.819	(0.206)	2.1147	(0.017)	3.180	(0.000)	4.091	(0.000)	4.888	(0.000)
Type B CC	-0.081	(0.532)	0.487	(0.312)	0.956	(0.169)	1.358	(0.087)	1.712	(0.043)
Overweight/obese	1.650	(0.049)	3.080	(0.001)	4.255	(0.000)	5.256	(0.000)	6.131	(0.000)
Poor/fair health	0.130	(0.448)	0.203	(0.419)	0.749	(0.226)	1.215	(0.112)	1.624	(0.052)
2000 VS 2011										
Type A CC	0.708	(0.239)	2.011	(0.022)	3.083	(0.001)	3.998	(0.000)	4.799	(0.000)
Type B CC	-0.147	(0.558)	0.341	(0.366)	0.816	(0.207)	1.223	(0.110)	1.580	(0.056)
Overweight/obese	1.992	(0.023)	3.427	(0.000)	4.608	(0.000)	5.615	(0.000)	6.496	(0.000)
Poor/fair health	0.230	(0.408)	0.801	(0.211)	1.274	(0.101)	1.681	(0.046)	2.040	(0.020)

p^+ p-value in parentheses

1.6 Misreporting of Weight and Height

As an extension of this study, I investigate how errors in the measurement of health, inaccuracy in self-reported weight and height, might explain the HIE phenomenon observed in previous studies. Such survey errors might include confusion, ignorance, or falseness on the part of the respondent; interviewer error; or confusion regarding the wording of the questionnaire (Statistics Canada, 2009). Any such misreporting of weight and height could partially affect the HIE in two ways. First, the initial health gap between the native population and the immigrant population may not be as large as suggested by the data, if newly arrived immigrants are more likely than natives to misreport their physiological measurements. Second, the convergence in health between natives and immigrants may be further distorted, either upward or downward, if there is a relationship between YSM and the likelihood of misreporting.

In this study I examine the particular misreporting of weight and height, which conceivably could translate into an imprecise measure of being overweight or obese. I investigate whether there is any systematic difference between the native and immigrant populations with respect to the likelihood of misrepresentation. Barcellos, Goldman, and

Smith (2012), using US data, attempt to explain part of the HIE in terms of misreporting.[37] The researchers compare clinical to self-reported diagnoses among Americans and recent Mexican immigrants. They find that the immigrants are not as healthy as they believe: about half are unaware that they have diabetes, and a third are unaware that they have high blood pressure. The researchers conclude that these undiagnosed conditions explain one-third to one-fifth of the initial health gap observed between the two populations.

The data set used in this evaluation is Canadian Health Measures Survey (CHMS) cycle 1 (2007 to 2009) and cycle 2 (2009 to 2011). CHMS collects its information in two stages: first through a household interview, and then through a physical examination. This design allows a comparison of self-reports and measured data. However, a major limitation is that CHMS was only begun in 2007, meaning that it is not possible to compare immigrant results pre- and post-IRPA. To the best of my knowledge, mine is the first study to use Canadian data to explain the HIE in terms of measurement errors in weight and height. To be consistent in my previous analysis, I restrict my sample in the same manner—subjects aged 20 to 65, not pregnant, and not a student. I also dropped 19 observations whose BMI is reportedly over 90. The final sample size is 5,043.

Table 1.7 reports the mean of self-reported and measured weight and height, and the gap between the sets of values—calculated simply as the difference between the two. On average, the weight gap is negative while the height gap is positive. This indicates that weight is generally under-reported, while height is over-reported. The table shows that the weight gap is larger for females than for males, but the height gap is larger for males than for females. Females report themselves as some 3.5 pounds less than their actual weight; the gap is only about 1.3 pounds for males. Men over-report their height by about 0.5 inch, but women only by about 0.3 inch. The weight gap is marginally smaller for immigrants: 2.43, compared to 2.51 for natives. However, the height gap is larger. On average,

[37]Note that misreporting is only one of many hypotheses that could partially explain the HIE, and that research in this area is largely ignored due to data unavailability.

immigrants over-report their height by 0.5 inch, while natives over-report by roughly 0.37 inch.

To assess the influence of covariates (age, education, and immigrant status) on the biases for weight and height, I define the outcome variables in terms of relative biases in absolute values, as follows:

$$\begin{aligned} \text{Bias}_w &= |\frac{w_r - w_m}{w_m}| \\ \text{Bias}_h &= |\frac{h_r - h_m}{h_m}| \end{aligned} \tag{1.4}$$

where w_r and w_m are reported measures of weight, and h_r and h_m are reported measures of height. The other relevant variable is the inconsistency between self-reported and clinically measured overweight or obesity, as calculated by the BMI (derived from both the reported or measured weight and height). Since my concern is only the probability of under-reporting, in this latter analysis I do not include 81 observations whose self-reported BMI is in the overweight/obese range but whose measured BMI is in the healthy range. My analysis focuses on assessing a downward bias, as opposed to an upward bias caused by misreporting. Formally, the under-report variable is defined as:

$$\text{Under-report} = \mathcal{I}[obese_m - obese_s = 1] \tag{1.5}$$

where $\mathcal{I}[\bullet]$ is an indicator function taking a value of 1 if [•] is true; and $obese_m$ and $obese_s$ are dichotomous variables taking a value of 1 if an individual actually is overweight or obese (as suggested by the measured and self-reported BMI respectively), and zero otherwise. The model specification is similar to that of Equation 1.1, but simplified. Due to small sample size of the immigrant population, the estimation pools male and female respondents; and I use simple regressions for the weight and height bias analysis. These include measured weight and height in the set of explanatory variables, since any reporting bias

TABLE 1.7: Self-Reported and Measured Mean Weight (lb) and Height (in)

				Education			
	Overall	**Male**	**Female**	**< HS**	**HS**	**some PSE**	**PSE**
Weight (lb):							
Self-reported	171.664	189.107	155.322	173.392	170.832	173.175	171.131
	(39.234)	(34.802)	(35.885)	(37.497)	(37.562)	(38.925)	(39.834)
Measured	174.146	190.437	158.87	175.69	173.149	175.038	173.749
	(40.948)	(36.968)	(38.392)	(40.225)	(39.367)	(40.172)	(41.398)
Gap	-2.482	-1.331	-3.548	-2.298	-2.317	-1.863	-2.618
	(8.088)	(8.166)	(7.815)	(11.192)	(8.518)	(7.559)	(7.416)
Height (in):							
Self-reported	66.907	69.713	64.321	66.191	66.705	67.178	67.014
	(3.854)	(2.797)	(2.723)	(3.949)	(3.804)	(4.102)	(3.818)
Measured	66.514	69.227	64.008	65.647	66.257	66.802	66.652
	(3.727)	(2.768)	(2.584)	(3.783)	(3.672)	(3.822)	(3.714)
Gap	0.393	0.486	0.312	0.544	0.449	0.376	0.362
	(0.974)	(1.005)	(0.937)	(1.157)	(0.914)	(0.945)	(0.959)
N	5,141	2,452	2,689	513	828	320	3,480
			Age group				
	20-29	**30-39**	**40-49**	**50-59**	**60-65**	**Immigrant**	**Native-born**
Weight (lb):							
Self-reported	165.458	170.409	174.138	171.822	172.487	160.394	174.65
	(35.602)	(41.719)	(38.627)	(39.362)	(37.78)	(34.658)	(39.845)
Measured	166.932	172.924	177.048	174.264	175.024	162.825	177.157
	(38.134)	(43.614)	(40.354)	(40.438)	(39.023)	(35.825)	(41.689)
Gap	-1.474	-2.515	-2.910	-2.443	-2.537	-2.43	-2.508
	(9.840)	(8.043)	(8.412)	(7.069)	(6.992)	(7.689)	(8.166)
Height (in):							
Self-reported	67.494	67.059	67.257	66.362	66.188	66.210	67.091
	(3.937)	(3.865)	(3.774)	(3.785)	(3.824)	(3.755)	(3.862)
Measured	67.185	66.758	66.915	65.957	65.515	65.712	66.726
	(3.772)	(3.683)	(3.630)	(3.695)	(3.728)	(3.688)	(3.716)
Gap	0.309	0.300	0.342	0.406	0.673	0.498	0.365
	(1.039)	(1.027)	(0.961)	(0.832)	(0.945)	(1.176)	(0.904)
N	607	1,367	1,389	886	892	1,159	3,982

Data source: CHMS cycle 1 and 2. Note: Standard deviations in parentheses. The summary statistics are weighted using the CHMS weight.

appears to be contingent on respondents' own measured weight and height (Boström and Diderichsen, 1997). A probit model is used for the probability analysis of under-reporting overweight/obesity.

Table 1.8 demonstrates coefficients and standard errors for the weight and height bias from simple regressions. The measured weight appears to be statistically significant, and positively correlated with weight bias. In other words, the heavier a person is, the larger their bias in reporting their own weight. Conversely, the measured height has negative association with height bias—the relatively tall population is less likely to report bias in their height. Nonetheless, these coefficients in measured weight and height are economically insignificant. For instance, an increase of 5 pounds in measured weight increases bias by 0.0005 percent. Similarly, an increase of 1 inch in measured height reduces height bias by 0.0004 percent. As indicated earlier, an interesting aspect of this bias analysis is the difference between men and women: males are less likely to report weight bias, but more likely to report height bias.

Next, Table 1.9 shows coefficients and standard errors from the probit estimation. The coefficient of *Immigrant* is positive, while coefficients of *YSM* and *YSM-squared* are together negative. This suggests that misreporting among immigrants could, to a certain extent, explain the HIE found in terms of overweight or obesity. Immigrants, on their arrival, are more likely to under-report being overweight or obese; but this probability decreases with YSM. Furthermore, immigrants' age appears to be negatively associated with the probability of misreporting: the likelihood of under-reporting seems to decrease with age. Table 1.9 also provides p-values from joint tests of immigrant and cohort dummies, as well as joint tests of all immigrant-related variables. Both of these tests are statistically significant, suggesting that immigrants differ from natives in the probability of their under-reporting overweight or obesity.

Since the coefficients from probit regressions are not directly interpretable, I calculate

TABLE 1.8: Estimates from Simple Regressions: Bias in Weight and Height

	Weight bias		**Height bias**	
Measure weight	0.0001**	(0.000)		
Measure height			-0.0004**	(0.000)
Male	-0.0059**	(0.002)	0.0026**	(0.001)
Age	-0.0008†	(0.000)	-0.0002	(0.000)
Age-squared	0.0000	(0.000)	0.0000	(0.000)
Household income	-0.0010**	(0.000)	-0.0002*	(0.000)
Province:omit Ontario				
Atlantic provinces	-0.0022	(0.002)	0.0002	(0.001)
Quebec	0.0035†	(0.002)	0.0013*	(0.001)
Prairies	0.0019	(0.002)	0.0006	(0.001)
British Columbia	0.0029	(0.002)	-0.0013*	(0.001)
Education: Omit less than high school				
Highschool	0.0015	(0.004)	-0.0006	(0.001)
Some Post-secondary degree	-0.0018	(0.004)	-0.0003	(0.001)
Post-secondary degree	-0.0042	(0.003)	-0.0010	(0.001)
Immigrant	-0.0051	(0.010)	0.0074†	(0.004)
YSM	0.0019 †	(0.001)	-0.0005	(0.000)
YSM-squared	-0.0000	(0.000)	0.0000	(0.000)
Cohort: omit cohort 1997-2001				
Before 1987	-0.0326*	(0.014)	0.0095	(0.006)
Cohort 1987-1991	-0.0162	(0.011)	0.0044	(0.004)
Cohort 1992-1996	-0.0158*	(0.008)	0.0010	(0.003)
Cohort 2002-2006	0.0067	(0.006)	-0.0033	(0.003)
Cohort 2007-2011	0.0117	(0.011)	-0.0052	(0.004)
cycle 1	0.0015	(0.002)	-0.0001	(0.000)
N	5,043		5,043	
F-test immigrant and cohort dummies p-value:				
	0.0083		0.5611	
F-test immigrant, cohorts and YSMs p-value:				
	0.0133		0.0328	

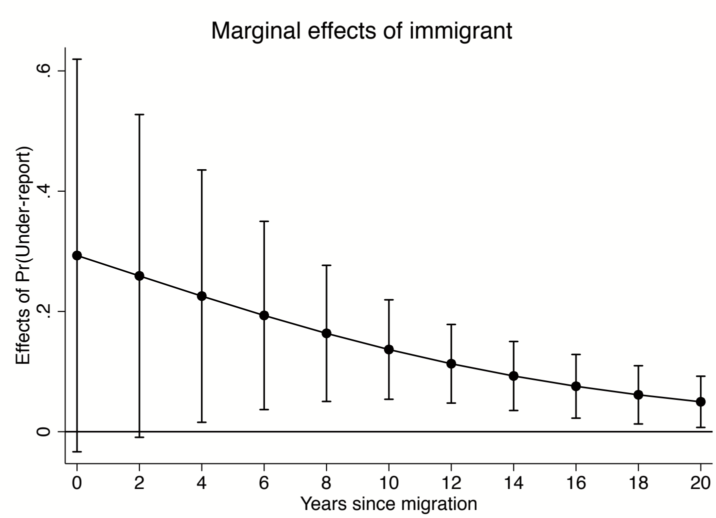
Data source: CHMS cycle 1 and 2.

Note: Robust standard errors in parentheses. The estimations were done using CHMS weights.

† $p < 0.10$ * $p < 0.05$, ** $p < 0.01$

marginal effects of being an immigrant versus being a non-immigrant over YSM. This is illustrated in Figure 1.2, which confirms the findings from the probit regression. On arrival, immigrants are more likely to under-report being overweight or obese. The initial misreport probability gap between the two populations is approximately 30 percentage points; however, the gap shrinks over time spent in Canada, converging with the non-immigrants. Traditionally, the HIE is composed of an initial health gap followed by convergence. Since my findings here suggest that immigrants are more likely to misreport their health on arrival, the initial health gap may not be as strong as previously found.[38] This observed convergence may partially be due to the decreasing probability of misreporting among the immigrant population. Consequently, my findings in this section oppose the classic HIE narrative. Although it is outside the scope of this paper to measure how large a role misreporting plays in explaining the HIE, the subject is of interest for future research.

FIGURE 1.2: Predicted Under Report of Being Overweight/Obese by YSM



[38] In this analysis, it is not my objective to explain the disappearance of the HIE. My results only address the matter of whether the phenomenon should be questioned, when found in other studies using self-reported weight and height data.

TABLE 1.9: Estimates from Probit Regressions: Likelihood of Under Reporting Being Overweight/Obesity

	Coefficient	
Immigrant	1.3831**	(0.526)
YSM	-0.0950 †	(0.057)
YSM-squared	0.0012†	(0.001)
Cohort: omit cohort 1997-2001		
Before 1987	0.5538	(0.725)
Cohort 1987-1991	0.1873	(0.503)
Cohort 1992-1996	-0.059	(0.389)
Cohort 2002-2006	-0.673 †	(0.353)
Cohort 2007-2011	-0.513	(0.502)
Demographic variables		
Male	0.0299	(0.090)
Age	-0.0468†	(0.026)
Age-squared	0.0005†	(0.000)
Proxies for permanent income		
Own dwelling	-0.0392	(0.115)
Dividend	-0.0185	(0.112)
Number of bedroom	0.0653 †	(0.038)
Province:omit Ontario		
Atlantic provinces	0.1039	(0.170)
Quebec	0.2559*	(0.170)
Prairies	-0.0273	(0.132)
British Columbia	-0.1461	(0.139)
Education: Omit less than high school		
Highschool	-0.1205	(0.163)
Some Post-secondary degree	0.3818*	(0.181)
Post-secondary degree	0.0539	(0.138)
cycle 1	0.1345	(0.101)
N	4,962	
F-test immigrant and cohort dummies p-value		0.0254
F-test immigrant, cohorts and YSMs p-value		0.0411

Data source: CHMS cycle 1 and 2.

Robust standard errors in parentheses. The estimation were done using CHMS weights.

† $p < 0.10$ * $p < 0.05$, ** $p < 0.01$

1.7 Conclusion

Health is an important determinant of productivity and ability to work, and hence of immigrants' labour-market outcomes—a topic that receives considerable attention from both academics and policy-makers. Better work prospects improve quality of life for newcomers, make them more productive members of their adopted society, and decrease the potential social burden of immigration—the possibility that the cost of immigrants' use of public services might exceed their tax contributions. For that reason, I feel that the research in this paper is timely.

This study re-examines the previously well-established finding regarding immigrant health: the Healthy Immigrant Effect (HIE), which finds that recent immigrants are, on average, healthier than native-born residents, but lose this health advantage over time. My re-examination was motivated by the 2002 implementation of the Immigration and Refugee Protection Act (IRPA), together with other policies that encourage applications from immigrants possessing characteristics associated with labour market success (such as education, labour-market experience, and ability to speak the official languages).[39] Because of the close relationship between socio-economic status and health, my initial hypothesis was that the initial gap between immigrants and natives should be even greater if calculated with more recent data.

However, the main finding in this paper is that the HIE is no longer present for recent immigrant cohorts. My study considers four measures of health: the incidence of less serious and more serious chronic conditions (Type A CC and Type B CC), the likelihood of poor self-reported health, and the likelihood of being overweight or obese. I find different results for men and women: no initial health advantage for the male sample, and an actual health disadvantage for the female sample. Female immigrants, on average, are more

[39] The Immigration and Refugee Protection Act (2002) pertains to the economic class of immigrants, which represents about half of newly admitted immigrants each year.

likely to report themselves in poor health, including Type A CC, than natives. The convergence in health with YSM, documented in previous studies, is also gone. The one exception is for male immigrants, whose likelihood of reporting Type A CC actually increases with each year in Canada, diverging from the average health of Canadian men.

A separate analysis compares measures of health between immigrants cohorts pre- and post-IRPA. I do matching exercises to directly compare immigrants across their cohorts of arrival, to better understand the disappearance of the HIE. Relative to pre-IRPA arrivals, I find that post-IRPA immigrants have better socio-economic outcomes, as predicted by the policy—but no associated improvement in average health. In fact, the likelihood of being overweight or obese is 7 percentage points higher for post-IRPA immigrants.

These findings raise important questions. Why have the health advantages of previous immigrant cohorts disappeared? Should immigration policy (the points system) be redesigned to specifically attract healthier immigrants? Might there be any economic, social, or labour-market implications associated with the declining quality of new immigrants' health?

In this study I also assess various characteristics that influence measurement error in terms of self-reported weight and height, among both immigrant and non-immigrant populations. The CHMS, a unique data set of health information based on both self-reports and on physical examinations, allows me to identify inconsistencies in weight and height measurements. My analysis concludes that on arrival, immigrants are more likely than natives to under-report their weight; and that the probability of under-reporting health issues declines with increasing years of residence in Canada. This implies an underlying mechanism that contradicts previous findings on the HIE. Since it is plausible that the phenomenon is at least partially driven by misreports among the immigrant population—at least in terms of overweight and obesity. My conclusion is that the HIE may not be as robust as was formerly believed.

Chapter 2

For Better or For Worse: Income Processes and Intra-household Risk Sharing

Abstract

Using data from the Medical Expenditure Panel Survey (MEPS) from 1996 to 2010, this study builds on the work done by Storesletten, Telmer, and Yaron (2004b) on the dynamics of income risk-sharing. My goal is to estimate household income-risk profiles by household composition (singles versus couples). I find that the cross-sectional variation in males' income is the main driving force, accounting for approximately 60% of total household income volatility. Moreover, the covariation of income between spouses accounts for approximately 36%, implying a non-negligible role for the movement of income between spouses. In this chapter, I break down the variances of permanent and transitory income shocks in couple households, incorporating the correlation of both types. Regardless of model specification, however, I find the covariance of transitory income shocks between partners to be both negative and large. This finding implies that couples partially insulate one another from transitory income shocks, and indicates that risk-sharing plays a part in explaining the dynamics of their income-risk profiles. I also find significant evidence of risk-sharing against permanent income shocks.

2.1 Introduction

Traditional marriage vows often include the phrase "for better or for worse," acknowledging that the institution involves some mutual sharing of risks (Weiss, 1993). One of the most common types of risk involved in everyday life is economic. Both for individuals and society, the costs of income uncertainty can be high. To the extent that asset markets are incomplete, income shocks can lead to large changes in consumption and savings. Some public mechanisms exist to partly mitigate the costs of uncertainty, such as unemployment insurance, bankruptcy codes, medical insurance, and social security programs. Private insurance mechanisms also exist to offset the effects of income uncertainty. To better understand the topic, many studies decompose income variance into components attributable to permanent and temporary (transitory) income shocks.[1] It is important to disentangle the two, since they have different implications for consumption and saving behaviours, and for human capital investments. For instance, increases in permanent income shocks may be attributable to skill-biased technological change (Katz and Autor, 1999; Beaudry and Green, 2005), while increases in transitory income shocks may imply an increasing labour-market volatility—associated with, for example, changes in the rate of job turnovers and business instabilities (Gottschalk et al., 1994). In this study, I extend the variance decomposition strategy proposed by Storesletten, Telmer, and Yaron (2004b) to estimate the variance of permanent and transitory income shocks for single and couple households, and to quantify income risk-sharing within couple households.

Two specifications are typically used to identify permanent and transitory shocks in individual income processes: the Heterogeneous Income Profile (HIP), and the Restricted Income Profile (RIP) (Guvenen, 2009; Hryshko, 2012). The main distinction between the

[1]See, for example, Gottschalk et al. (1994), Gottschalk and Moffitt (2002), Gottschalk and Moffitt (2006), Hyslop (2001), Moffitt and Gottschalk (2011), Meghir and Pistaferri (2004), Blundell, Pistaferri, and Preston (2008), Heathcote, Perri, and Violante (2010), Heathcote, Storesletten, and Violante (2010), Storesletten, Telmer, and Yaron (2004b), Storesletten, Telmer, and Yaron (2004a), Bonhomme and Robin (2010), Guvenen (2009), Karahan and Ozkan (2013), and Hryshko (2012).

two is that HIP assumes that labour-income growth rates exhibit heterogeneity, whereas RIP assumes that labour income growth rates are homogeneous. Studies following HIP usually find that individuals face only modest persistent shocks (for example, see Haider (2001) and Guvenen (2009)); but those following RIP generally conclude that individuals are subject to large and persistent shocks with permanent effects (for example, see MaCurdy (1982), Abowd and Card (1989), and Meghir and Pistaferri (2004)). It is not clear whether either model fits the income data better, and studies show mixed evidence. For instance, using data from the Panel Survey of Income Dynamics (PSID), Guvenen (2009) allows one to find evidence supporting the HIP theory, while Hryshko (2012) argues that it should be rejected in favour of the RIP model.

One limitation of the current income-process literature is that its models commonly treat households as discrete decision-making agents, with the focus solely on the male heads of households (Gottschalk and Zhang, 2010). This assumption seems problematic for a number of reasons. First, such unitary framework models consistently fail to find empirical support in the consumption expenditure data (for example, Browning et al. (1991), Blundell, Pashardes, and Weber (1993), and Browning and Chiappori (1998)). Second, studies have found that labour-market choices tend to reflect *joint* decision-making between individuals in a household, and that more complicated models are required to capture this joint dynamics (for example, Apps and Rees (1996), Apps and Rees (1997), Blundell, Chiappori, and Meghir (2005), Blundell et al. (2007), and Chiappori (1992)). Third, the possibility of risk-sharing and labour-supply adjustments may reduce transitory income dispersion for couples, as compared to singles (Gottschalk and Moffitt, 2002; Blundell, Pistaferri, and Preston, 2008; Blundell, Pistaferri, and Saporta-Eksten, 2016).

My study contributes to the literature by examining income processes and risk-sharing between spouses in two ways. I extend Storesletten, Telmer, and Yaron (2004b)'s study to separately examine income processes by marital status, to analyze how income profiles vary between singles and couples; and I propose an income-process model to identify risk-sharing within couples. To do this, I adapt a general form of the RIP model (the sum of an autoregressive model of order 1 (AR(1)) permanent process and a purely transitory shock) to use with a large panel data set. I allow for dynamics of income shocks between spouses, and derive a generalized method of moments (GMM) estimator from age-dependent moments.

Two aspects of my study merit attention. First, while most studies of the income process employ the Panel Study of Income Dynamics (PSID), I prefer to use the Medical Expenditure Panel Survey (MEPS).[2] The MEPS has a much larger sample size than the PSID, which allows for subsample analyses; and its overlapping (short) panel structure decreases the severity of non-random attrition.[3] Imposing restrictions commonly practiced in PSID literature, Guvenen (2009) has only 1,270 observations; while the MEPS, with similar restrictions, yields some 40,000 observations.[4] Furthermore, Guvenen (2015) points out that practically all income-process analyses that use the PSID conclude that the volatility of income shocks has increased significantly over the past 40 years, and that this increase might be more about the data set than the methodology. Guvenen (2015) also notes that studies that use different data sets, such as administrative ones, generally reach conclusions that conflict with studies using the PSID.[5]

The second aspect of my study worthy of attention is the matter of household identification. Examining risk-sharing between spouses is complicated by the fact that researchers cannot observe how individuals would behave if they were *not* in a couple household (Kooreman and Kapteyn, 1990). For this reason I follow an identification strategy used in

[2] Begun in 1996, the MEPS collects national data on all aspects of the health, income, and employment of US citizens. The survey is described in more detail in Section 2.2.

[3] The PSID has approximately 50% sample loss from cumulative attrition from its initial 1968 membership. Attrition in the PSID is also highly selective, concentrated in individuals of lower socioeconomic status (Fitzgerald, Gottschalk, and Moffitt, 1998).

[4] Common restrictions include limiting observations to male heads of households, between the ages of 20 and 64, reporting positive labour earnings and hours, and working between 520 and 5,110 hours a year.

[5] Guvenen et al. (2014) and Sabelhaus and Song (2010) use administrative data in their studies, and find that income volatility has been declining.

household consumption literature (for example Browning, Chiappori, and Lewbel (2013), Barmby and Smith (2001), and Barmby (1994)), where individual preferences are assumed to be invariant to marital status. Rather than imposing invariance of preference parameters, which may be too stringent, I assume that market prices for labour (wages) are unrelated to marital status.[6] I use parameters governing income processes for singles to estimate the income-risk profile of couples, particularly to estimate the correlations between transitory and permanent incomes of spouses.

This novel identification strategy disentangles transitory and permanent income risk-sharing within couple households. Focusing on permanence in wage inequality using the PSID, Hyslop (2001) finds a positive correlation of spouses' permanent wage shocks, and concludes that this correlation explains about 20% of the overall increase in family earnings inequality. Many studies find that marriage helps couples to manage labour-income risk by adjusting relative labour supply in the household. For example, Blundell, Pistaferri, and Preston (2008) use the PSID and the Consumer Expenditure Survey to find that family labour supply plays an important role in insuring against permanent income shocks. Using a life-cycle model that incorporates household and family labour supply decisions, Blundell, Pistaferri, and Saporta-Eksten (2016) find that 81% of the insured consumption (against husband's permanent wage shocks) is attributable to the family labour supply. Shore (2010) quantifies how the covariance of the changes in couples' income varies over the business cycle: he finds that marriage protects against most of the increased riskiness associated with recessions for individuals. Zhang (2014) uses the Survey of Income and Program Participation and an intra-household insurance structural model to assess risk-sharing between husbands and wives, and finds that it reduces earnings instability by

[6]Wage discrimination in the US is against the Employment Discrimination Law. In particular, the Civil Rights Act 1964, the Civil Service Reform Act of 1978, and individual state statutes prohibit workplace discrimination based on various characteristics, including marital status.

between 2 and 9 %.[7,8] However, these studies generally focus on the magnitude of the permanent income shocks, using specific structural models; while I identify both transitory and permanent income shocks using a variance decomposition.

To demonstrate that my results are not driven by identification restriction, I quantify income covariation among couples using a decomposition (as in Deaton and Paxson (1994)). I find that the male is the main determinant of household income instability, accounting for approximately 60% of income variation; while the covariation of income between spouses accounts for approximately 36% of the total. This sizable difference highlights the importance of not treating all households (single and couples) in the same way; and it also illustrates the potential benefits of risk-sharing between couples.

I find significant evidence of risk-sharing within couples. The covariance of income shocks is both negative and large: -0.25 for transitory income shocks, and -0.03 for permanent income shocks. These findings confirm the presence of income insurance between spouses, indicating that couples respond to labour-market shocks that affect either partner. It is also particularly noteworthy that I find evidence that couples are able to insure against permanent income shocks, though to a lesser degree than against transitory shocks: Blundell, Pistaferri, and Preston (2008) also find that it is extremely difficult to insure against permanent income shocks.[9]

In this chapter, Section 2.2 describes the data set used in this study and presents the results of some descriptive analyses. Section 2.3 presents the model; Section 2.4 demonstrates the empirical estimation, and discusses the estimates; and Section 2.5 concludes.

[7] Other studies that find evidence of intra-household risk sharing include Rosenzweig and Wolpin (1985), Rosenzweig (1988), Rosenzweig and Stark (1989), and Rosenzweig and Wolpin (1994).

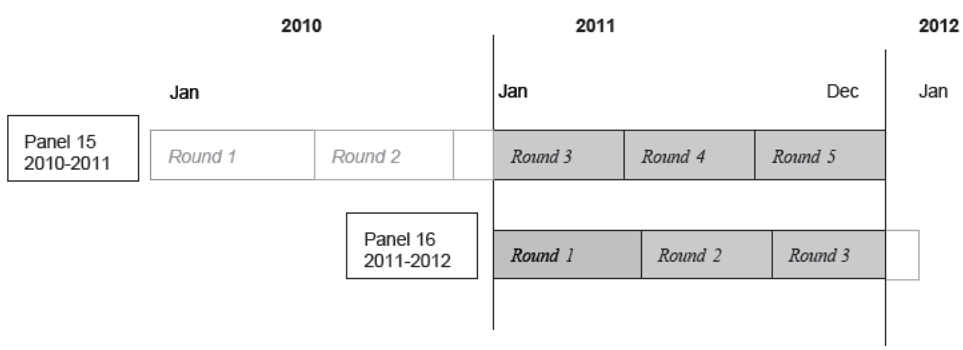
[8] But other studies, such as Altonji, Hayashi, and Kotlikoff (1992), Hayashi, Altonji, and Kotlikoff (1996), and Cochrane (1991), suggest little evidence of risk-sharing within households.

[9] Blundell, Pistaferri, and Preston (2008) find some partial insurance against permanent shocks, and almost complete insurance against transitory shocks.

2.2 The Data

Besides describing the data set used in this study, this section includes three sub-sections: Simple Income Variance Decomposition, Comparison to Existing Studies, and Couples' Income Covariance. The first of these sections presents graphical analyses and decomposes the income variance; the second provides a graphical comparison of the MEPS and the PSID; and the third presents results from quantifying the relative contribution of the covariance of income between spouses in a household. My main purpose here is to highlight two related facts: 1) that income covariation between spouses is empirically relevant, and 2) that empirical relevance is unlikely to be a statistical artifact of the data set.

FIGURE 2.1: Structure of the MEPS, 2011



Note: Image adapted from MEPS 2011User Guide

As mentioned earlier, I use the MEPS household data from 1996 to 2011. Funded by the US Department of Health & Human Services, the MEPS primarily collects health-related information about matters such as health-care use, expenditures, and health insurance coverage. It also collects detailed information on the respondents' demographic and socioeconomic characteristics, employment, and income. The surveys have two major components: the Household Component (HC) and the Insurance Component (IC). The HC provides health data from individual households, and the IC provides health-insurance data retrieved from employers. The data used in this study is from the HC.

For each panel, the MEPS collects five rounds of data from approximately 13,000 households over a two-year period. Figure 2.1 illustrates the survey design. For example, panel 15 starts in January 2010 and ends in December 2011; so the data for 2011 is contained in both panel 15 and panel 16. Each panel provides end-of-year status variables that reflect the information of respondents as of December 31 of that year. I use both those variables in my analysis, creating two observations for each respondent. I pool 15 consolidated files (panels 1 to 15) to form an overlapping data set.

Relative to the PSID, the MEPS has two major strengths: its sample size is much larger, which allows for subsample analyses; and its relatively short panel structure leads to a much smaller attrition rate than the PSID. MEPS surveys, on average, 10,000 families in each panel (150,000 families for 15 panels), which translates to roughly 30,000 individuals in each panel (450,000 individuals for 15 panels). By contrast, the PSID only includes about 10,000 families, or 24,000 individuals by 1997. As for the attrition rate, the MEPS loses about 5% to 8% of its respondents in each panel, while the PSID loses about 50% of its initial 1968 respondents. Moreover, attrition by the PSID respondents is highly selective. The individuals lost are concentrated among those with lower socioeconomic status, and with less stable earnings, marriages, and migration histories.

Family income is derived by aggregating personal income of an individual and their partner.[10] Total personal income is simply the sum of all sources of income: annual earnings from wages, salaries, bonuses, tips, commissions; business and farm gains and losses; unemployment and workers' compensation; interest and dividends; alimony, child support, and other private cash transfers; private pensions, IRA withdrawals, social security, and veterans payments; supplemental security income and cash welfare payments from public assistance, Temporary Assistance for Needy Families, and related programs; gains

[10] To focus on couple dynamics between spouses, I include only the income of the reference person and spouse. I exclude any other family members living in the household, since including them could bias the analysis of risk-sharing between partners—and the direction of that bias is unclear.

or losses from estates, trusts, partnerships, corporations, rent, and royalties; and a small amount of other income. The only exceptions are tax refunds and capital gains. The MEPS questionnaire defines the reference person as the head of the household: the person, aged 16 or older, who owns or rents the home.[11]

As for "family," it is generally defined as two or more persons living in the same household who are related by blood, marriage, or adoption. People living together as a common-law unit are also considered a family. This study uses a family weight to provide estimates at the family level.[12]

Total income is adjusted to common 2010 dollars using the Consumer Price Index (CPI). I select households for the sample based on two criteria: 1) the reference person is aged 23 to 60 at the time of the interview; and 2) the family income is positive. The latter restriction assures that there are no zero or negative incomes among the single population; however, one person of the couple may have a zero (not negative) income, as long as the overall household income is positive. Of some 30,000 couples and singles, 125 spouses have negative income, and 1,570 singles have zero or negative income. In subsequent analyses, I also consider a sample of only couples with both spouses reporting positive income.

I exclude 15 couples from my analyses because of a same-sex partner, since studies such as Berg and Lien (2002), Black et al. (2003), Blandford (2003), and Zavodny (2007) find that same-sex couples tend to experience income inequalities. I also exclude 32 households that report having two spouses, and 516 households where the spouses became deceased or separated during the period of the survey.[13] The final sample size is 30,262 single households, and 31,759 couple households.

Table 2.1 provides mean and standard deviations of some characteristics of the population, by marital status and gender. The ages of married and single women in this sample

[11] If more than one person meets those criteria, the respondents identify who should be considered the reference person.

[12] The family weight is provided with the data files; details can be found in the MEPS user guides.

[13] The low number of MEPS separations do not reflect overall US divorce rates, since the time frame of the data used in this study is relatively short.

TABLE 2.1: Summary Statistics: Mean and Standard Deviations

	Single		Couple	
	Male	Female	Male	Female
Age	38.37	40.01	43.61	41.67
	(0.12)	(0.11)	(0.07)	(0.07)
Income	42,484.31	34,967.73	53,226.97	38,525.08
	(397.16)	(269.11)	(300.529)	(245.05)
Log income	10.32	10.11	10.57	9.96
	(0.01)	(0.01)	(0.01)	(0.01)
Couple's income			91,752	
			(419.59)	
Couple's log income			11.23	
			(0.00)	
Median no. of children	0	1	2	
Highest level of education:				
Lower than high school	0.18	0.18	0.13	0.10
	(0.00)	(0.00)	(0.00)	(0.00)
High school	0.47	0.46	0.45	0.45
	(0.01)	(0.00)	(0.00)	(0.00)
Bachelor degree	0.20	0.20	0.22	0.23
	(0.00)	(0.00)	(0.00)	(0.00)
Graduate degree	0.07	0.09	0.12	0.11
	(0.00)	(0.00)	(0.00)	(0.00)
Other degrees	0.07	0.09	0.09	0.10
	(0.00)	(0.00)	(0.00)	(0.00)
N	12,002	18,260	31,759	31,759

Data source: MEPS 1996 to 2010. Note: The summary statistics are weighted using the MEPS family weight. Mean values reported. Standard deviations in parentheses.

are relatively close, with married women being about $1\frac{1}{2}$ years older than single women. Married men, however, are on average 5 years older than single men; and they report the highest income in comparison to single men and women, and to married women. For instance, on average married men earn about \$11,000 per year more than single men,\$18,000 more than single women, and \$14,000 more than married women. In terms of education, comparing married people of both sexes to single people, the married population has a higher proportion of bachelor and graduate degrees. Roughly 12% of married men hold a graduate degree, while only 7% of single men do. The trend is similar among women, though the gap between married and single is not as large as for men.

2.2.1 Simple Income Variance Decomposition

To help portray my approach, I begin with extracting the "unexplained" component, ϵ_{it}^h, from the following statistical model:[14]

$$y_{it}^h = x_{it}^h \beta + \sum_{t=1}^{T-1} Y_t + \epsilon_{it}^h \tag{2.1}$$

where y_{it}^h is log-income of household i of age h at time t, x_{it}^h is a vector of explanatory variables including age and its polynomials (age^2, age^3); level of education (less than high school, high school, undergraduate degree, graduate degree, and other degrees);[15] number of children; and gender (for the single population). Y_t is a dummy variable for each calendar year,[16] and ϵ_{it}^h is the idiosyncratic random error. Note that h runs from 23 to 60, and t runs from 1996 to 2010. It is assumed that $\mathrm{E}(\epsilon_{it}^h) = 0$ so that $\mathrm{Var}(\epsilon_{it}^h) = \mathrm{E}(\epsilon_{it}^h)^2 = \sigma_{ht}^2$. Consequently, the estimated variance is $\hat{\sigma}_{ht}^2 = \mathrm{E}(\hat{\epsilon}_{it}^h)^2$, where $\hat{\epsilon}_{it}^h = y_{it}^h - \hat{y}_{it}^h$ and $\hat{y}_{it}^h$ is the linear prediction obtained from the regression of equation 2.1.

[14]Some studies also refer to to this ϵ_{it}^h as "earnings inequality."

[15]Other categories include some college, occupational, technical, and vocational degree.

[16]Including year dummy variables ensures that the typical life-cycle pattern of income is not included in measuring income processes.

I begin the analysis by simple regressions of y^h_{it}.[17] Of particular interest are the residuals from simple regressions, which form the unexplained portion of the income regression. The age- and year-specific income variances derived from these residuals are used in the GMM estimation. Since it is known that this type of (Mincerian) regression is plagued by endogeneity, it is possible that the GMM estimation using information derived from the residuals could also be affected. I discuss this issue in Section 2.4.2.

The GMM estimation, which I will discuss in detail in Section 2.3, hinges on the existence of variability in the cross-sectional variance between the different age cohorts. The role that age plays in approximating the income process is described in several studies, notably Storesletten, Telmer, and Yaron (2004b) and Karahan and Ozkan (2013) with the use of PSID. In particular, the income profile increases over time. I demonstrate graphically that this same pattern can be found in the MEPS, and that the income profile of the single population is not identical to that of the married population. Following Deaton and Paxson (1994), I perform a dummy variable regression—decomposing the cross-sectional variance of earnings into cohort and age effects, as follows:

$$\hat{\sigma}^2_{ht} = a_c + b_h + e_{ht}, \tag{2.2}$$

where a_c and b_h are sets of dummy variables indicating birth cohort (defined as birth year, $t - h$) and age cohort, and e_{ht} are residuals. In total there are 37 age cohort dummies, and 52 birth cohort dummies. Because there is no constant in this regression, I am able to obtain all 52 coefficients of birth cohort dummies and 37 coefficients of age dummies. The omitted age cohort is 40, and other age coefficients are interpreted as effects relative to that age.

[17] Regression results can be found in Table B.1 and B.2 in Appendix B for singles and couples respectively. Note that the estimates of the βs obtained from regressions of equation 2.1 are not germane to this study.

2.2.2 Comparison to Existing Studies

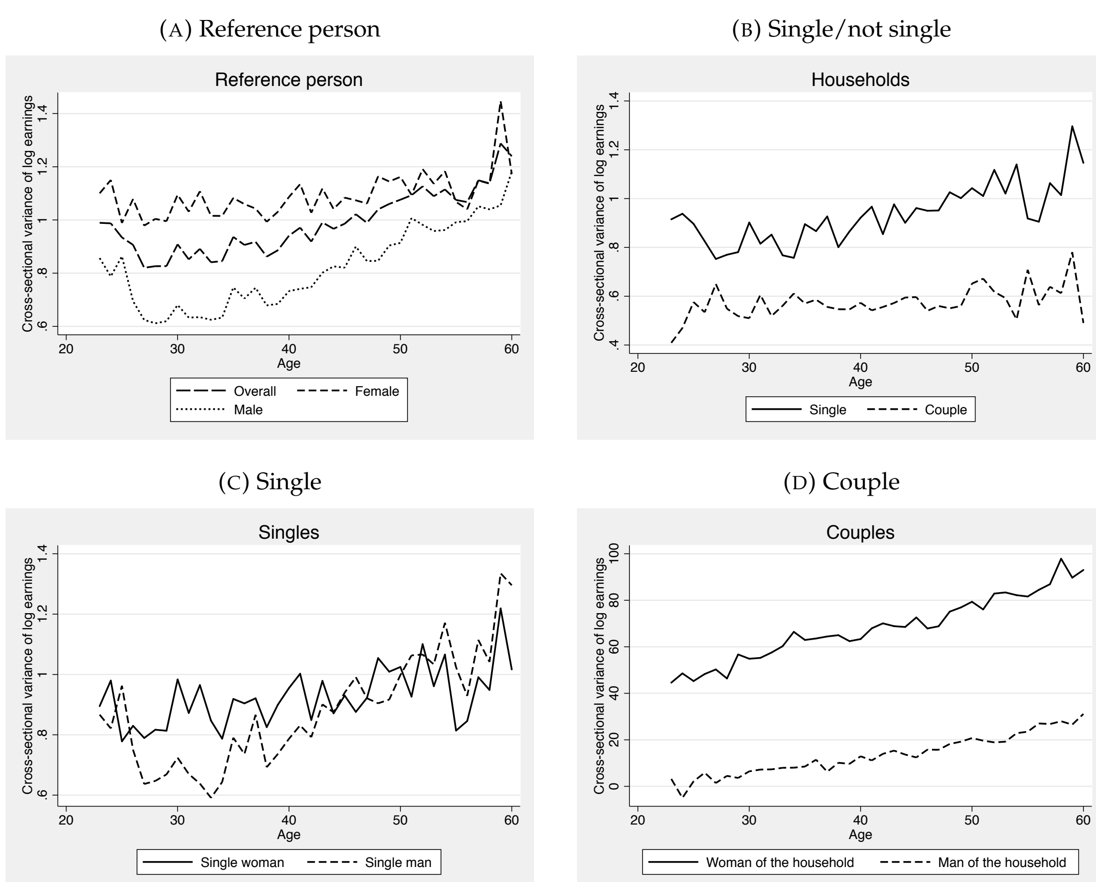
Figure 2.2 reports the cross-sectional variance of the income logarithm, excluding birth cohort effects. It plots the coefficients of age dummies ($\hat{b}_h$) from the regression in equation 2.2. The lines are plotted such that they all go through an unconditional variance of log income at age 40, as in Storesletten, Telmer, and Yaron (2004b). Figure 2.2a reveals that, on average, the cross-sectional income variance increases with age—this fits the stylized US data on income (Storesletten, Telmer, and Yaron, 2004b; Storesletten, Telmer, and Yaron, 2004a; Karahan and Ozkan, 2013). These graphical analyses also encourage the use of age-specific moments in GMM estimation, to approximate the idiosyncratic labour income process.

In Figure 2.2a the cross-sectional variance profile of women, though larger than that of men, is relatively constant. The income variance profile of women also appears to experience a dramatic jump at the end of her work life. At younger ages, the male profile is less volatile than that of women; however, their income volatility increases with age, and catches up with women's at around age 60. The distinctive shapes of the two profiles emphasize the importance of gender-specific analysis in later sections.

Figure 2.2b plots the heterogeneity in household income variation across marital status, and Figure 2.2c (2.2d) plots the heterogeneity in the income variation across genders in the single (couple) population. The former figure makes it clear that the income variance profiles of singles and of couples are not identical: singles' profiles exhibit the rise observed across the general population, but those of couples appear level. This strong difference in the shape of the profiles casts some doubt on the results of previous studies that do not distinguish between households with two and one income earners. In particular, the estimates of the permanent component could be confounded.

Figure 2.2c shows that young single men appear to have low income variance relative to young single women; but the men's variance becomes more dispersed as they age, and exceeds that of single women around age 45. This generally accords with the known fact that younger women may go in and out of the labour market more frequently than men, while they raise their families. Figure 2.2d shows further decomposition between the man and the woman of the household. The woman's cross-sectional income variance profile is higher than the man's, for all ages. This is due to the fact that a substantial number of married women report zero or relatively little income. Figure 2.3 demonstrates a clear drop in woman's cross-sectional income variance profile when I consider only spouses whose earnings are strictly positive. However, the woman's profile is still much higher than that of man's.

FIGURE 2.2: Cross-sectional Variance of Log-income by Household Type and Gender



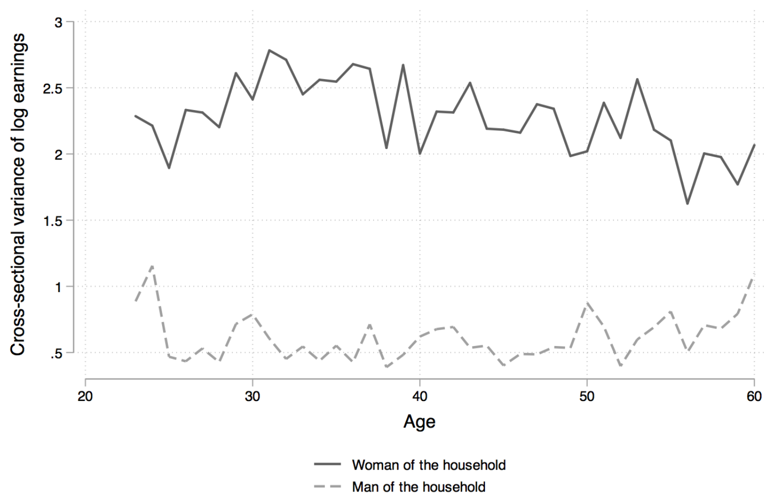
(A) Reference person

(B) Single/not single

(C) Single

(D) Couple

FIGURE 2.3: Cross-sectional Variance of Log-income: Only Couples with Spouses Reporting Positive Earnings



Since mine is the first study to use the MEPS to estimate income processes, I provide some additional analyses to compare it with the more commonly used data set, the PSID. Following Storesletten, Telmer, and Yaron (2004b), Figure 2.4 plots the cohort coefficients from dummy variable regression against the fraction of economic contractionary years each cohort experiences in its working period. I define economic contraction and expansion in the same manner as Storesletten, Telmer, and Yaron (2004b)—an expansion or a contraction is classified according to the growth rate in U.S. gross national product (GNP). If the GNP growth rate is above its sample mean, then the economy is in expansion mode; and vice versa. I obtain the GNP data from 1958 to 2010 from the U.S. Department of Commerce, and convert it to 2010 dollars using the CPI. I also convert the data to per-capita terms, dividing by the total population (obtained from the U.S. Census Bureau).

The graph shows a clear positive relationship between cross-sectional variance and the fraction of contractionary years, which is comparable to the findings in Storesletten, Telmer, and Yaron (2004b) and replicates previous results. The MEPS does not exhibit any irregularities in comparison to the PSID.[18] I provide a detailed comparison of both the PSID and the MEPS to the U.S. Current Population Survey in Section B.2 Appendix B.

FIGURE 2.4: Cohort Effects Versus Contractionary Years

2.2.3 Couple's Income Covariance

In this section, I explain the relative significance of couples' income covariance in terms of the household's income variation. I quantify the relative weighted contribution of each component of total household income variation: the variation of income of the male spouse, the female spouse, and the covariation between the two. For a couple of age h at time t, I assume that household income variance can be characterized by:

$$\mathrm{Var}_t^h(aY_m + bY_w) = a^2\,\mathrm{Var}_t^h(Y_m) + b^2\,\mathrm{Var}_t^h(Y_w) + 2ab\mathrm{Cov}_t^h(Y_m, Y_w) \tag{2.3}$$

[18] In general, it is difficult to compare the PSID and the MEPS, since both the structures and the time frames of the surveys are different. Guner, Kulikova, and Llull (2014) use both data sets in their study, and show that the surveys are in fact similar in many other aspects—such as proportion of married individuals (about 60%), average age of the household head (40), level of education (36% have a college degree), number of children, and income (roughly $36,000).

where Y_m and Y_w are the incomes of a man and a woman. I obtain $\mathrm{Var}_t^h(aY_m + bY_w)$ from calculating the variance of total household income for each age cohort and year t, and I stack them into a vector of 570 elements: 38 age cohorts multiplied by 15 survey cycles. $\mathrm{Var}_t^h(Y_m)$, $\mathrm{Var}_t^h(Y_w)$, and $\mathrm{Cov}_t^h(Y_m, Y_w)$ are obtained in a similar way, using the income variance of a man and a woman. The estimation is done using unconditional and conditional (on the covariates) cross-sectional age-specific variances. The terms a^2, b^2, and $2ab$ can be seen as weighted contributions of the cross-sectional variances from the male spouse, the female spouse, and their covariance.[19] These three terms (a^2, b^2, and $2ab$) are normalized so that the sum is unity. Estimated coefficients can be found in Table 2.2.

TABLE 2.2: Weighted Contribution from Variance of Each Spouse

	Weight contribution from		
	Man	Woman	Covariance
Unconditional variance	0.598	0.051	0.350
	(0.034)	(0.010)	(0.024)
Conditional variance	0.563	0.062	0.374
	(0.032)	(0.011)	(0.021)

Data source: MEPS 1996 to 2010 panel 1-15. Note: Robust standard errors in parentheses. The estimations were done using the MEPS family weights.

The weight contribution from the male is found to be approximately 0.6. This implies that for both types of variances (unconditional and conditional), total household income variation is mainly driven by the volatility of the male spouses' income—about 60% is attributable to them. The covariance of income dispersion between male and female spouses also appears to make an essential contribution to total household income volatility: the covariation of income accounts for approximately 40%. This finding—that the weight contribution from the covariance of spouses' income is over 1/3—demonstrates the possibility of income dynamics between spouses, and prompts a more in-depth research of intra-household risk-sharing in this study. The weight contribution from the man of the

[19] Recall that these terms are different from the actual cross-sectional income variances of the spouses and their covariance.

household is found to be approximately 0.6. This implies that, both unconditional and conditional, total household income variation is mainly propelled by the volatility of income of the male spouses—about 60% is attributable to the man of the household. The covariance of income dispersion between male and female spouses also appears to make essential contribution to the total household income volatility. The covariation of income accounts for approximately 40% of the total household income variation. This finding, that the weight contribution from the covariance of income of spouses is over 1/3, demonstrates the possibility of income dynamics between spouses and motivates a more in-depth research of intra-household risk-sharing in this study.[20]

2.3 Model

Since my goal in this study is to analyze the unexplained component of the income regression, I rely on well-established tools such as the error components model—in which residual labour incomes, or income inequality, are the sum of transitory and persistent shocks.[21] This model describes income well (Abowd and Card, 1989).[22] The unexplained component (the residuals from the simple regression) is obtained from equation 2.1, which

[20]This conclusion, that the weight contribution from the covariance is relatively high, contrasts with findings in Ostrovsky (2012) and Hryshko, Juhn, and McCue (2016). These two studies find that the weight contribution from the covariance of income of spouses is under 20%. Both Ostrovsky (2012) and Hryshko, Juhn, and McCue (2016) calculate the weight contributions of each component of total household income variation by decomposing the squared-coefficient of variation of total household earnings. I replicate their exercises, discuss the findings, and consider a potential issue with their method in Appendix B.6.

[21]Dating back to Lillard and Willis (1978), Lillard and Weiss (1979), MaCurdy (1982), and Abowd and Card (1989), autoregressive-moving-average (ARMA) models are commonly used to fit panel data to understand the labour income risk facing individuals. Many models assume labour income is the sum of a transitory and persistent shock, where often the persistent shock is assumed to follow a random walk.

[22]It is important to bear in mind that, though simple, this structural model has economic implications. A large volume of studies in the labour economic literature conclude that earnings inequality in the US could attribute to two main sources: earnings instability and lifetime earnings inequality. An increase in earnings instability is primarily related to labour market instability such as changes in the returns from job turnover and in firm instability (Gottschalk et al., 1994); this is captured by the transitory component, usually comprises of purely transitory i.i.d. shocks and/or an MA process (serially correlated transitory process). An increase in lifetime earnings inequality, on the contrary, is attributable to changes in how different skills are valued by the labour market—a skill-biased technological change (Katz and Autor, 1999; Beaudry and Green, 2005); this is captured by the permanent component, usually represented by some forms of an AR process.

can be structurally modelled for individual i of age h at time t. I follow Storesletten, Telmer, and Yaron (2004b) in modelling the income process for the single population:

$$
\begin{aligned}
\epsilon_{it}^{h} &= \alpha_i + z_{it}^{h} + \upsilon_{it}, \\
z_{it}^{h} &= \rho z_{i,t-1}^{h-1} + \eta_{it},
\end{aligned}
\tag{2.4}
$$

where

$$
\begin{aligned}
\alpha \sim (0, \sigma_{\alpha}^{2}), \qquad \upsilon \sim (0, \sigma_{\upsilon}^{2}), \qquad \eta \sim (0, \sigma_{\eta}^{2}) \\
\alpha \perp \upsilon \perp \eta, \qquad i.i.d..
\end{aligned}
\tag{2.5}
$$

I extend the model of income process in Storesletten, Telmer, and Yaron (2004b) for the couple population, as follows:

$$
\begin{aligned}
\epsilon_{it}^{h} &= \alpha_i^{m} + \alpha_i^{w} + \alpha^{c} + z_{it}^{hm} + z_{it}^{hw} + \upsilon_{it}^{m} + \upsilon_{it}^{w}, \\
z_{it}^{hm} + z_{it}^{hw} &= \rho_m z_{i,t-1}^{h-1,m} + \rho_w z_{i,t-1}^{h-1,w} + \eta_{it}^{m} + \eta_{it}^{w},
\end{aligned}
\tag{2.6}
$$

where

$$
\begin{aligned}
\alpha^{m} \sim (0, \sigma_{\alpha_m}^{2}), \qquad \upsilon^{m} \sim (0, \sigma_{\upsilon_m}^{2}), \qquad \eta^{m} \sim (0, \sigma_{\eta_m}^{2}) \\
\alpha^{w} \sim (0, \sigma_{\alpha_w}^{2}), \qquad \upsilon^{w} \sim (0, \sigma_{\upsilon_w}^{2}), \qquad \eta^{w} \sim (0, \sigma_{\eta_w}^{2}) \\
\alpha^{c} \sim (0, \sigma_{\alpha_c}^{2}), \quad \alpha \perp \upsilon \perp \eta \quad \text{for both men and women}, \quad i.i.d., \\
\text{and} \quad \mathrm{Cov}(\eta^{m}\eta^{w}) = \sigma_{mw}^{\eta} \quad \mathrm{Cov}(\upsilon^{m}\upsilon^{w}) = \sigma_{mw}^{\upsilon}.
\end{aligned}
\tag{2.7}
$$

The terms α_i, α_i^{m}, α_i^{w}, and α^{c} represent time-invariant individual specific effects for, respectively, singles, the man of the household, the woman of the household, and the couple-specific fixed effect. These individual-specific fixed effects illustrate the individuals' levels of permanent and transitory components at the start of their work careers. The terms υ_{it}, υ_{it}^{m}, and υ_{it}^{w} capture transitory innovations to income, while z_{it}, z_{it}^{hm} and z_{it}^{hw} capture permanent shocks to income. The coefficients ρ, ρ_m and ρ_w determine the degree of persistence of the permanent shocks. The initial condition, z_{it}^{0}, is assumed to be zero. The assumptions used in this study closely follow the majority of studies in this literature (including Storesletten, Telmer, and Yaron (2004b)).

From the equations in 2.4 and 2.5, the cross-sectional variance of the single population for each age h is defined as

$$\begin{aligned} \mathrm{Var}^{sm}(\epsilon_{it}^{h}) &= \sigma_{\alpha_{sm}}^2 + \sigma_{\upsilon_{sm}}^2 + \sum_{j=0}^{h-1} \rho_{sm}^{2j} \sigma_{\eta_{sm}}^2 \\ \mathrm{Var}^{sw}(\epsilon_{it}^{h}) &= \sigma_{\alpha_{sw}}^2 + \sigma_{\upsilon_{sw}}^2 + \sum_{j=0}^{h-1} \rho_{sw}^{2j} \sigma_{\eta_{sw}}^2 \end{aligned} \tag{2.8}$$

for single men and single women, respectively. From the equations in 2.6 and 2.7, the cross-sectional variances of the couple population, for each age h, are defined as:

$$\mathrm{Var}^{c}(\epsilon_{it}^{h}) = \sigma_{\alpha_m}^2 + \sigma_{\alpha_w}^2 + \sigma_{\alpha_c}^2 + \sigma_{\upsilon_m}^2 + \sigma_{\upsilon_w}^2 + 2\sigma_{mw}^{\upsilon} + \sum_{j=0}^{h-1} \rho_m^{2j} \sigma_{\eta_m}^2 + \sum_{j=0}^{h-1} \rho_w^{2j} \sigma_{\eta_w}^2 + 2\sum_{j=0}^{h-1} \rho_m^{j} \rho_w^{j} \sigma_{mw}^{\eta}. \tag{2.9}$$

The autocovariance is defined as:

$$\begin{aligned} \mathrm{E}^{sm}(\epsilon_{it}^{h} \epsilon_{i,t-1}^{h-1}) &= \sigma_{\alpha_{sm}}^2 + \rho_{sm} \sum_{j=1}^{h-1} \rho_{sm}^{2(j-1)} \sigma_{\eta_{sm}}^2 \\ \mathrm{E}^{sw}(\epsilon_{it}^{h} \epsilon_{i,t-1}^{h-1}) &= \sigma_{\alpha_{sw}}^2 + \rho_{sw} \sum_{j=1}^{h-1} \rho_{sw}^{2(j-1)} \sigma_{\eta_{sw}}^2 \end{aligned} \tag{2.10}$$

for the singles, and as:

$$\begin{aligned}\mathrm{E}^c(\epsilon_{it}^h \epsilon_{i,t-1}^{h-1}) =& \sigma_{\alpha_m}^2 + \sigma_{\alpha_w}^2 + \sigma_{\alpha_c}^2 + \rho_m \sum_{j=1}^{h-1} \rho_m^{2(j-1)} \sigma_{\eta_m}^2 + \rho_w \sum_{j=1}^{h-1} \rho_w^{2(j-1)} \sigma_{\eta_w}^2 \\ &+ (\rho_m + \rho_w) \sum_{j=1}^{h-1} \rho_m^{j-1} \rho_w^{j-1} \sigma_{mw}^{\eta} \qquad (2.11)\end{aligned}$$

for the couples, for all h and t. I follow Storesletten, Telmer, and Yaron (2004b) in assuming that individuals, on average, start their work life at age 23. For simplicity's sake, $h = 1$ when the individual is aged 23.

These cross-sectional variances and autocovariances for each age h (equations 2.8 to 2.11) underlie the GMM estimator. For single individuals, it is straightforward to assign each to their age cohort (h). For couples, however, this is less clear. Again for the sake of simplicity, I use the age of the reference person (regardless of gender) as the age of the couple.[23]

2.4 Estimation

In studies of household income and consumption, it is widely known that estimating the parameters for married couples poses some identification problems in demand models. Specifically, as indicated earlier, the issue comes from asking the data to establish how individuals in a collective setting might have acted were they *not* in a collective setting (Kooreman and Kapteyn, 1990). Many studies—notably Browning, Chiappori, and Lewbel (2013), Lewbel (1985), Gorman (1976), and Barten (1964)—attempt to tackle this issue by using data from singles to identify some common parameters of couples. Empirically, identification relies on the assumption that some aspects of individual preferences remain the same after marriage, and hence estimates for singles can be used for couples. A similar

[23] I also try using other numbers: the average age of the couple, the age of the male spouse, and the age of the reference person's spouse. The results do not vary significantly.

strategy is employed in studies estimating labour-supply models, for instance in Barmby and Smith (2001), Barmby (1994), and Vermeulen et al. (2006).

Following these studies, I assume that "potential" wages are competitive, and that the labour market does not discriminate against individuals because of their marital status. Employers are assumed to offer certain jobs and wages to all applicants, and workers select from these offers. In other words, spouses can choose to accept the same wage offers as singles, if they wish, and any premium or penalty in pay rate does not come from the employer. As a result, singles and couples share some parameters characterizing their labour-income risk profiles—though for men, admittedly, the literature on marriage earnings typically estimates a 10% premium for married men compared to single men (Dougherty, 2006).

Three theories compete to explain the "marriage premium" phenomenon. Becker (1973) posits that marriage allows one spouse to focus fully on the labour market, while the other specializes in home-making. [24] This makes married men more productive at work than non-married men, and they are rewarded accordingly. The second theory hypothesizes an unobserved fixed effect, "ability bias": married men possess certain qualities that are valued in both the labour market and the marriage market. On the one hand, men who are more economically successful are more likely to get married; on the other hand, the support and the civilizing effect of marriage might cause a man to become more economically successful. A third possibility is that employers may discriminate in favour of married men. There is no consensus on the first two theories (see Dougherty (2006) and Krashinsky (2004)); but the third has been tested by a handful of studies. Results are mixed: Coverman (1983) and Loh (1996) find evidence supporting the discrimination theory, while Hundley (2000) and Hamilton (2000) find evidence that contradicts it.

The assumptions I make here are consistent with the first two theories. I assume that potential wages depend on the labour market, and that employers do not care whether

[24] Traditionally, this means that husbands do paid work, and wives do housework.

workers are single or married. Nevertheless, the hypothesis of employer discrimination causes some concern. If it is true—if the marriage earnings premium is in fact the result of discrimination in favour of married men—this study's assumption could be invalid. However, I argue that such concerns are unlikely to be valid in the United States, since such behaviour is against the Employment Discrimination law. In particular, the Civil Rights Act 1964, the Civil Service Reform Act of 1978, and various State statutes prohibit workplace discrimination based on certain characteristics, including marital status.[25]

My identification strategy relies first on obtaining estimates using information from single individuals, and then on using these estimates to uncover couple-specific parameters. The cross-sectional variances (equations 2.8 and 2.9) represent one variance for each age and time pair, (h, t). For example, the moments associating with variances for single males might be defined as:

$$\mathrm{E}_t^{sm}\left[(\epsilon_{it}^h)^2 - \sigma^2_{\alpha_{sm}} - \sigma^2_{\upsilon_{sm}} - \sum_{j=0}^{2h-1} \rho_{sm}^{2j}\sigma^2_{\eta_{sm}}\right] = 0 \tag{2.12}$$

where h runs from age 23 to 60, and t runs from the year 1996 to 2010. The cross-sectional autocovariances are used in identifying σ_α from σ_υ. The moments associated with autocovariances for single males are:

$$\mathrm{E}_t^{sm}\left[\epsilon_{it}^h\epsilon_{i,t-1}^{h-1} - \sigma^2_{\alpha_{sm}} - \rho_{sm}\sum_{j=1}^{h-1} \rho_{sm}^{2(j-1)}\sigma^2_{\eta_{sm}}\right] = 0 \tag{2.13}$$

[25] Employers are not allowed to ask candidates questions about their country of origin, ethnicity, citizenship, age, sexual orientation, or marital status.

for each age h and time t. Similarly, the moments representing variances and autocovariances for single women are:

$$\mathrm{E}_t^{sw}\left[(\epsilon_{it}^h)^2 - \sigma_{\alpha_{sw}}^2 - \sigma_{\upsilon_{sw}}^2 - \sum_{j=0}^{2h-1}\rho_{sw}^{2j}\sigma_{\eta_{sw}}^2\right] = 0 \tag{2.14}$$

and

$$\mathrm{E}_t^{sw}\left[\epsilon_{it}^h\epsilon_{i,t-1}^{h-1} - \sigma_{\alpha_{sw}}^2 - \rho_{sw}\sum_{j=1}^{h-1}\rho_{sw}^{2(j-1)}\sigma_{\eta_{sw}}^2\right] = 0. \tag{2.15}$$

The parameters of interest are $\theta_{sm} = (\sigma_{\alpha_{sm}}, \sigma_{\upsilon_{sm}}, \rho_{sm}, \sigma_{\eta_{sm}})$ and $\theta_{sw} = (\sigma_{\alpha_{sw}}, \sigma_{\upsilon_{sw}}, \rho_{sw}, \sigma_{\eta_{sw}})$ for single men and single women, respectively.

I use the estimated parameters from the single population ($\hat{\theta}_{sm}$ and $\hat{\theta}_{sw}$) in the estimation for couples. Specifically, I calibrate moment conditions to characterize couples' variance and autocovariance, using estimated parameters obtained from the GMM estimates of the single population. For married couples, I allow a "marriage-specific effect," $\sigma_{\alpha_c}^2$, which captures unobserved heterogeneity in tastes in couple households. The parameters identified in the estimation for couples are $\theta_c = (\sigma_{\alpha_c}, \sigma_{mw}^{\upsilon}, \sigma_{mw}^{\eta})$. The moments representing variances and autocovariances associated with couples are as follows:

$$\begin{aligned}\mathrm{E}_t^{c}\Bigg[(\epsilon_{it}^h)^2 - \hat{\sigma}_{\alpha_m}^2 - \hat{\sigma}_{\alpha_w}^2 - \sigma_{\alpha_c}^2 - \hat{\sigma}_{\upsilon_m}^2 - \hat{\sigma}_{\upsilon_w}^2 - 2\sigma_{mw}^{\upsilon} - \sum_{j=0}^{h-1}\hat{\rho}_m^{2j}\hat{\sigma}_{\eta_m}^2 \\ - \sum_{j=0}^{h-1}\hat{\rho}_w^{2j}\hat{\sigma}_{\eta_w}^2 - 2\sum_{j=0}^{h-1}\hat{\rho}_m^{j}\hat{\rho}_w^{j}\sigma_{mw}^{\eta}\Bigg] = 0\end{aligned} \tag{2.16}$$

and

$$\mathrm{E}_t^c \Bigg[\epsilon_{it}^h \epsilon_{i,t-1}^{h-1} - \hat{\sigma}_{\alpha_m}^2 - \hat{\sigma}_{\alpha_w}^2 - \sigma_{\alpha_c}^2 - \hat{\rho}_m \sum_{j=1}^{h-1} \hat{\rho}_m^{2(j-1)} \hat{\sigma}_{\eta_m}^2 + \hat{\rho}_w \sum_{j=1}^{h-1} \hat{\rho}_w^{2(j-1)} \hat{\sigma}_{\eta_w}^2 - (\hat{\rho}_m + \hat{\rho}_w) \sum_{j=1}^{h-1} \hat{\rho}_m^{j-1} \hat{\rho}_w^{j-1} \sigma_{mw}^{\eta} \Bigg] = 0. \tag{2.17}$$

I follow the conventional method and calibrate $\rho = (0, 1]$ or $\rho = 1$ in all my estimations.[26] The estimated parameters and corresponding standard errors for single men and single women can be found in Table 2.3, and in Table 2.4 for couples. I perform the estimations using a set of 15 moment conditions (variances of age 25, 30, 35, 40, 45, 50, 55, 60, and the first-order autocovariances of age 25, 30,35, 40, 45, 50, and 55). All standard errors reported from GMM estimation are bootstrapped; the number of bootstrap replications is 1,000.[27]

Table 2.3 shows GMM parameter estimates for the single population. In the estimates from calibrating $\rho = (0, 1]$, the fixed effect for single men is $\sigma_{\alpha_{sm}}^2 = 0.4941^2 \approx 0.2441$; for single women, it is $\sigma_{\alpha_{sw}}^2 = 0.4829^2 \approx 0.2331$. These are close to the results found in Storesletten, Telmer, and Yaron (2004a) ($\sigma_{\alpha}^2 = 0.2105$). The transitory shocks are sizable both for single men ($\sigma_{\upsilon_{sm}}^2 = 0.5408^2 \approx 0.2925$) and for single women ($\sigma_{\upsilon_{sw}}^2 = 0.5725^2 \approx 0.3278$); these estimates are also close to those found in Storesletten, Telmer, and Yaron (2004a) ($\sigma_{\upsilon}^2 = 0.3158$). The permanent volatility is smaller than the transitory volatility for both single men and single women, $\sigma_{\eta_{sm}}^2 = 0.1030^2 \approx 0.0101$ and $\sigma_{\eta_{sw}}^2 = 0.1601^2 \approx 0.0256$.[28] For the parameter capturing persistence of permanent shock, the ρ is very close

[26]See, for example, Haider (2001), Guvenen (2007), and Guvenen (2009) for studies that assume $\rho = (0, 1]$; and see, for example, Gottschalk and Smeeding (1997) and Storesletten, Telmer, and Yaron (2004b) for studies that assume $\rho = 1$.

[27]Specifically, I simulate a set of n *i.i.d.* uniform random integers from the range 1,. . .,n with replacement. I construct a bootstrap sample to compute the estimates, and repeat these steps 1,000 times to computer the standard of estimates.

[28]These estimated variances of permanent shocks are very close to those found in Storesletten, Telmer, and Yaron (2004a), where ρ is also calibrated to be between zero and one ($\sigma_{\eta}^2 = 0.0166$).

TABLE 2.3: GMM Parameter Estimates: Singles

	σ_α	σ_υ	ρ	σ_η
Single men (sm)				
$\rho = (0, 1]$	0.4941	0.5408	0.9949	0.1030
	(0.0000)	(0.0001)	(0.0026)	(0.0040)
$\rho = 1$	0.5319	0.5717	–	0.0941
	(0.007)	(0.0005)	–	(0.0000)
Single women (sm)				
$\rho = (0, 1]$	0.4829	0.5725	0.9309	0.1601
	(0.0000)	(0.0001)	(0.0010)	(0.0004)
$\rho = 1$	0.5252	0.5717	–	0.0796
	(0.0003)	(0.0003)	–	(0.0000)

Data source: MEPS panel 1 to 15. Note: Bootstrapped standard errors in parentheses.

The 15 moments include variances of age 25, 30, 35, 40, 45, 50, 55, 60, and the first-order autocovariances of age 25, 30, 35, 40, 45, 50, and 55.

to unity. It is estimated to be 0.9949 for single men, and 0.9309 for single women. These estimates are also very close to those found in existing studies.[29] In the estimates from calibrating $\rho = 1$, the fixed effect and the transitory volatility for both single men and single women are similar to when $\rho = (0, 1]$. The major change in my results is in the variance of permanent shocks: the figures are now 0.0941 for single men ($\sigma^2_{\eta_{sm}} \approx 0.0089$), and 0.0796 for single women ($\sigma^2_{\eta_{sw}} \approx 0.0063$).

Next, Table 2.4 shows parameter estimates for couples from both including and excluding the marriage fixed effect, σ_{α_c}. When the marriage fixed effect is included, the results suggest that this effect is generally zero regardless of the calibrated values of ρ. This confirms the assumption that the job market does not differentiate between workers' marital status. No significant effect specific to being married can be captured in the unexplained portion of the family income. I estimate two equivalent models for couples here, one restricted (no σ_{α_c}) and one unrestricted (with σ_{α_c}). Then I perform a Likelihood Ratio (LR) test on the models to compare their fit. The null hypothesis here is that the restriction

[29]Such studies generally find that ρ is very close to 1, indicating highly persistent shocks. See, for example, Guvenen (2007), Guvenen (2009), Storesletten, Telmer, and Yaron (2004b), and Storesletten, Telmer, and Yaron (2004a).

TABLE 2.4: GMM Parameter Estimates: Couples

	σ_{α_c}	σ^{υ}_{mw}	σ^{η}_{mw}
$\rho = (0, 1]$	0.0000	-0.2375	-0.0289
	(0.0000)	(0.0006)	(0.0000)
	–	-0.2155	-0.0308
	–	(0.0006)	(0.0000)
$\rho = 1$	0.0000	-0.2912	-0.0119
	(0.0000)	(0.0002)	(0.0000)
	–	-0.2577	-0.0128
	–	(0.0001)	(0.0000)

Data source: MEPS panel 1 to 15. Note: Bootstrapped standard errors in parentheses.

The 15 moments include variances of age 25, 30, 35, 40, 45, 50, 55, 60, and the first-order autocovariances of age 25, 30, 35, 40, 45, 50, and 55.

$\sigma_{\alpha_c} = 0$ is true. The calculated LR test statistic is 2.9. With 1 degree of freedom, the associated p-value is between 0.05 and 0.10. This means that the null hypothesis cannot be rejected with the conventional 95% level of confidence. In other words, adding σ_{α_c} to the model does not result in a statistically significant improvement. Table 2.4 also demonstrates evidence of income risk-sharing among couples, particularly with $\sigma^{\upsilon}_{mw} = -0.2155$ when $\rho = (0, 1]$, and $\sigma^{\upsilon}_{mw} = -0.2577$ when $\rho = 1$. This negative and sizable results suggest that couples can be insulated from temporary income shocks. For the permanent shocks, however, σ^{η}_{mw} is relatively small—roughly -0.03 when $\rho = (0, 1]$ and roughly -0.01 when $\rho = 1$. These small figures imply that there is minimum "marriage insurance" against permanent shocks. Couples cannot easily insure each other against long-term labour-market shocks.

To the extent that my results might be driven by spouses whose earnings are zero, I re-estimate the model using only couples with both spouses reported non-zero earnings in the two-year period of the survey.[30] Table 2.5 reveals that the temporary risk-sharing effect is larger with this sample restriction, σ^{υ}_{mw} increases in magnitude from -0.2155 to -0.2619 when $\rho = (0, 1]$, and from -0.2577 to -0.2978 when $\rho = 1$. The permanent risk-sharing

[30]This restriction reduces my sample size from 31,759 couples to 26,391 couples.

TABLE 2.5: GMM Parameter Estimates for Couples: Spouses with Positive Earnings Only

Calibrated ρ	σ^{υ}_{mw}	σ^{η}_{mw}
$\rho = (0, 1]$	-0.2619	-0.0282
	(0.0008)	(0.0001)
$\rho = 1$	-0.2978	-0.0137
	(0.0007)	(0.0000)

Bootstrapped standard errors in parentheses
Data source: MEPS panel 1 to 15

The 15 moments include variances of age 25, 30, 35, 40, 45, 50, 55, 60, and the first-order autocovariances of age 25, 30, 35, 40, 45, 50, and 55.

effect is, however, does not vary significantly from previous estimates.

2.4.1 Sensitivity Analysis and Robustness Check

I perform sensitivity analyses using various sets of covariates in the first-stage regressions.[31] For the single population, I add two more specifications to the original set of covariates. The first specification includes interaction terms between age and education (model 2), and the second includes interaction terms between age and number of children (model 3). I do not find any changes in GMM resulting from the variances and covariances generated from these two new specifications. For the couple population, I augment the original set of covariates with interaction terms between husband's and wife's education, and later with interaction terms between age of spouses. Similarly, I do not find substantial changes to the GMM estimates.

I check the robustness of the estimates by randomly matching 1,000 single males and 1,000 single females (from the same panel year, within the same region) and modelling these "synthetic couples" as if they were true couples.[32] The idea is similar to the synthetic control method proposed by Abadie and Gardeazabal (2003) and Abadie, Diamond, and

[31] Results from GMM estimations using these different sets of covariates can be found in Section B.3 of Appendix B.

[32] I later match single males and single females based on their education and age. Similar results prevail.

Hainmueller (2010), but much simpler. The family income is assumed to be simply the sum of the man's and the woman's incomes. I then run the analysis exactly as I would for the real couples. My expectation is that there should not be any evidence of covariation of shocks; and that is exactly what is shown by their parameter estimates—these synthetic couples have no significant covariations in their shocks, either transitory or permanent, that can be picked up by the model. (The GMM results from the synthetic couples can be found in Section B.4 of Appendix B.) This result, in line with my expectation, suggests that the insurance findings shown previously are not simply correlations with regional shocks. The results are in line with prior expectation.

2.4.2 Endogeneity

Endogeneity in this type of Mincerian wage regression typically involves an omitted variable, such as "unobserved ability" (Griliches, 1977), and its correlation with education and income. The term "ability" is generally understood to be an element of human capital—a time-invariant description of a skill level that is innate to a given individual, susceptible to improvement over time, and a factor in influencing labour-market wages. This endogeneity concern is addressed in Deri Armstrong, Dunbar, and Sopchokchai (2016), where the authors show that the endogeneity bias could affect the variance of the residuals from the simple regressions. This effect could be up to the magnitude of the correlation between the unobserved ability and income, and also the correlation between the unobserved ability and education (scaled by the variance of education). Since the latter factor can be observed, this bias term can be included directly in the GMM estimation to uncover the true variance.

It is widely known that in this specific case, the estimated β coefficient associated with education may be biased by the degree of correlation between education and the omitted ability. That is, $plim\hat{\beta} = \beta + plim(\hat{\delta})\beta_2$, where $plim(\hat{\delta})$ is the coefficient of a regression of ability on education, and β_2 is the coefficient of ability (if it were included in the wage regression). Thus the variance of the regression residual, σ^2_{υ}, defined in equations 2.4 and 2.5, is in fact:

$$\sigma^2_{\upsilon} = \mathrm{Var}(\tilde{\upsilon}_{it} - EDU^h_{it}\hat{\delta}\beta_2) \tag{2.18}$$

where $\tilde{\upsilon}_{it}$ is the true regression residual, EDU is education, and $\tilde{\upsilon}_{it} - EDU^h_{it}\hat{\delta}\beta_2 \lesseqgtr \tilde{\upsilon}_{it}$. Using equation 2.18, the expression for variance in equation 2.8 can be rewritten as:

$$\mathrm{Var}(\epsilon^h_{it}) = \sigma^2_{\alpha} + \sigma^2_{\tilde{\upsilon}} + (\hat{\delta}\beta_2)^2\sigma^2_{edu^h_{it}} + \sum_{j=0}^{h-1}\rho^{2j}\sigma^2_{\eta}. \tag{2.19}$$

Here I drop the subscript sm, since this equation applies to both single men and single women. The terms $\sigma^2_{\tilde{\upsilon}}$ and $\sigma^2_{edu^h_{it}}$ are the true variance of the regression residual, and the variance of education of each age group. Since the variance $\sigma^2_{edu^h_{it}}$ can be observed, it can be included in the GMM estimation.[33] Deri Armstrong, Dunbar, and Sopchokchai (2016) also note that the problem associated with this endogeneity bias largely consists of difficulty in estimating the specific time-invariant term, given that there is little change in the variance of education within a given age cohort. Since the data used in this study is a relatively short two-year panel, for my purposes education is, in fact, time invariant. I perform this exercise as part of my robustness check. The results can be found in Table B.8 in Section B.3 of Appendix B. The estimated bias is very close to zero for both single men and single women. Moreover, other parameter estimates of the model are stable with the inclusion of the endogeneity bias term. The result implies that this simple model of the income process is robust to the omitted variable bias in terms of the endogeneity between ability, education, and income.[34]

[33]The term $\hat{\delta}\beta_2$ is assumed to be constant across all cohorts.

[34]Measurement error could also be another source of endogeneity; for examples, see Wooldridge (2013).

2.4.3 Wage Income

The definition of family income I use in my analyses matches as closely as possible with existing studies: the sum of all sources of income of the household reference person and their spouse. Since this includes various transfers, the covariance terms may reflect the couple's dynamic beyond the scope of the labour market; however, here I investigate only wage income. My results can be found in Section B.5 of Appendix B. Figure B.1 shows a graphical analysis of cross-sectional variance of log wage (similar to those in Section 2.2.2), and Table B.10 provides estimates of the model using only wage income.

Figure B.1 generally resembles the patterns previously found in Figure 2.2. On average, men's wage profile is similar to their income profile, increasing over time. However, the women's wage profile appears to differ from their income profile: it decreases in the early years, then rises quickly at around age 55.

The cross-sectional variance of wages is generally larger for singles than for couples, and the variance profile of the singles increases significantly with age. At early ages, the wage profile of single men is much lower than that of single women. However, the women's wage profile is relatively stable over time; and again, the single men catch up with the single women in later years, at around age 45. Among couples, the woman of the household has a much larger wage variance than the man. Women's wage income variance is larger than the income variance that includes other transfers. This is due to the fact that there are more female spouses with zero wage income than ones with no sources of income at all.

Compared to previous parameter estimates, those for single men do not vary significantly. However, the fixed effect, σ_α for single women increases by about 40%, from some 0.5 to some 0.7. These results are consistent across the two calibration of ρ. For couples, using only wage income show stronger covariance of transitory shocks. The magnitude of the covariances of the transitory shocks, σ^{v}_{mw}, is increased from -0.2155 to -0.3103 for

$\rho = (0, 1]$ and from -0.2577 to 0.3340 for $\rho = 1$; the magnitude of the covariance of permanent shocks, σ^{η}_{mw}, is still approximately zero. This finding suggests that, in mitigating the effect of transitory income shocks, spouses may not rely on one another as much when couples have access to income sources other than wage income. On the contrary, when spouses do not have any other sources of income except wage income, they appear to rely more heavily on one another as suggested by a larger magnitude of the covariance estimate.

2.5 Conclusion

This study extends the decomposition strategy used by Storesletten, Telmer, and Yaron (2004b) to compare the income processes of singles and couples, and to quantify the income-risk insurance mechanism in the couple households. My analyses use the MEPS, a rich data set with several advantages over the PSID, the typical data set used in such studies. The MEPS has a much larger sample size than the PSID, which allows for subsample analyses; its short panel structure also leads to less non-random attrition. In estimating couples' income processes, I follow the identification strategy used in other studies of household consumption, such as Browning, Chiappori, and Lewbel (2013), Barmby and Smith (2001), and Barmby (1994) and Vermeulen et al. (2006), I also posit that employers do not discriminate against workers based on their marital status. Subsequently, I use the same parameter governing the income processes of singles to estimate the income processes of couples, in particular the correlations of income shocks between spouses.

In seeking to understand the nature of uninsurable idiosyncratic labour-income risk, and to provide evidence of potential risk-sharing in couple households, I emphasize the importance of treating single and couple households differently. I find that, on average, income variance of couples is largely driven by the instability of the male spouses—60%

of total variation can be explained by the male spouses' income variation. More importantly, the covariation of income between spouses is not negligible in terms of explaining the total household income variance. The covariation accounts for about 36% of the total household income variation. The magnitude of the covariance of income between spouses suggests that the co-movement of income may play an important role in decomposing income variance amongst couples.

I find robust evidence of risk-sharing between spouses. In the baseline specifications, couples appear to be protected against transitory income shocks—the covariance of the transitory shocks between spouses is estimated to be roughly -0.25. This negative covariance of transitory shocks between spouses suggests that the impact of a transitory shock can be reduced when one spouse experiences an unforeseen disruption of their income stream. I find little evidence of permanent income insurance between spouses—the covariance of permanent shocks is estimated to be very close to zero. In line with findings in Blundell, Pistaferri, and Preston (2008), I find that it is more difficult to insure against permanent shocks than against transitory shocks. I check the robustness of my findings by adopting a "synthetic couple" approach, similar to that used by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010). I randomly match single males and single females, based only on region of residence, panel of the survey, and characteristics such as age and education. Re-estimating the income processes using these synthetic couples yields no evidence of risk-sharing. Furthermore, concentrating on wage income, I find an increase in the magnitude of covariance of transitory shocks between spouses. This implies that, when couples do not have other sources of income aside from wage income, spouses count on one another greatly in attenuating the negative effects of income shocks.

In a modern time of change and growing income instability, I feel my research may potentially be a valuable asset to economic policy-makers. Understanding the differences in the income dynamics between singles and couples could advance knowledge of intra-household insurance as a risk-coping (and consumption smoothing) strategy. This understanding is central to many issues, including household welfare and government insurance policies (as described in Gottschalk and Moffitt (2009)), insurance markets (see Hryshko (2012)); family labour supply (for example, Blundell, Pistaferri, and Saporta-Eksten (2016) and Knowles (2013)); economic gender inequality (see Huggett, Ventura, and Yaron (2011)); modelling consumption, savings, and wealth (outlined in Ortigueira and Siassi (2013)); and the welfare costs of business cycles (see Storesletten, Telmer, and Yaron (2004a) and Storesletten, Telmer, and Yaron (2004b)).

Chapter 3

Health Shocks and Income Dynamics

This chapter is co-authored with Catherine Deri Armstrong and Geoffrey Dunbar

Abstract

In this paper we study the impact of health shocks for the residual variance of labour income, using data from the American Medical Expenditure Panel Survey (MEPS). Following Storesletten, Telmer, and Yaron (2004b), we decompose the cross-sectional variance of residual earnings into a transitory and a permanent component. Our main innovation is to use information on emergency room visits to separate the transitory component into health and non-health components. Another innovation is controlling for any mis-specification biases that might result from an incorrectly specified earnings regression. We compare the effects of health shocks on single individuals and on couples, to examine the extent of household insurance against health shocks (for which we use emergency room visits as proxies). Our preliminary results indicate that such shocks have a sizable effect on income inequality for single women, but exhibit little persistence. We find some evidence that being in a couple mitigates the effects of health care shocks, and that having health insurance coverage can significantly reduce income volatility.

3.1 Introduction

The study of income dynamics—the unexplained portion of individuals' residual income variance—has a rich tradition in economics. It is well understood, for example, that individual income exhibits uncertain transitory and permanent components that tend to rise with increasing age (see studies such as Deaton and Paxson (1994) and Storesletten, Telmer, and Yaron (2004b)). These and other researchers have expended considerable effort on the task of disentangling short-term (transitory) from long-term (permanent) components of income risk.[1] The relative contribution of each component is an important matter for the makers of public policy to understand, since they are responsible for designing insurance mechanisms, setting changes in wage structures, and determining the welfare consequences of rising income inequality. In this paper, we consider the extent to which unforeseen health shocks contribute to both transitory and permanent income dynamics.

It is, of course, well established that health shocks negatively affect income.[2] To the extent that diminished health decreases earning capacity, immediate or very short-term effects on work and wages are predicted. Long-term income effects are far more complicated to predict, since they depend on a number of factors. These include an individual's ability to recover partially or completely from the health shock; their ability to return to the same employment as before, or to retrain for alternative employment (if in a diminished health state); the degree to which the initial health shock led to further deterioration; the availability of private incentives (such as early retirement) or public incentives (such as social assistance programs); and the different circumstances of individuals' participation in the labour force, their financial constraints, and even their household division of labour

[1] See, for instance, Gottschalk et al. (1994), Gottschalk and Moffitt (2002), Gottschalk and Moffitt (2006), Hyslop (2001), Moffitt and Gottschalk (2011), Meghir and Pistaferri (2004), Blundell, Pistaferri, and Preston (2008), Heathcote, Perri, and Violante (2010), Heathcote, Storesletten, and Violante (2010), Storesletten, Telmer, and Yaron (2004b), Storesletten, Telmer, and Yaron (2004a), Shore (2010), Bonhomme and Robin (2010), Guvenen (2009), Karahan and Ozkan (2013), and Hryshko (2012).

[2] More generally, many studies link health and labour-market outcomes. See Jones, Rice, and Zantomio (2016) for an excellent summary of the literature.

(see García-Gómez et al. (2013) and Jones, Rice, and Zantomio (2016)).

A number of studies have estimated these short- and long-term effects, and examined how the mean impact of health shocks varies by risk group. For example, García-Gómez et al. (2013) use six years of Danish health and income data to identify the effects of acute hospitalizations on long-term earnings. These are found to decrease the probability of being employed by seven percentage points, up to two years later. The associated loss in personal income is estimated to be 5%, with poorer individuals experiencing greater losses of income. No recovery is found in either employment or income four years after hospitalization. García-Gómez and Nicolás (2006) find similar results using Spanish data: health shocks negatively affect the likelihood of employment and income. Cai, Mavromaras, and Oguzoglu (2014) use six waves of the Household Income and Labour Dynamics Australia survey to estimate the impact of health on working hours. They find that health shocks lead to a reduction in working hours of 7–9 hours per week; they also find that females are more likely to cease working altogether after a health shock. Lundborg, Nilsson, and Vikström (2015) use 12 years of Swedish administrative data to compare the effects of specific health shocks on earnings for workers of different levels of education. Short-term effects are found to be larger for individuals with less education; and the differences are even more pronounced for older individuals. Jones, Rice, and Zantomio (2016) use British survey data and matching methods to examine control groups as well as the treatment groups: those experiencing recent health shocks, defined as a first diagnosis of cancer, stroke, or heart attack within the past two years. They find that the risk of leaving the labour market doubles as a result of health shocks; effects are smaller for younger workers, and particularly large for older and more educated women.

In this paper we take an agnostic view of identifying groups, considering the **dispersion** of the impact of health shocks over all workers. In particular, we consider the extent of the transitory effects of health shocks for cross-sectional income variances, and the extent to which they persist over time. We might reasonably expect much heterogeneity in health impacts, since two factors—the individual's original health, and the severity of the incident—are likely to vary widely.

To uncover the effects of health shocks on income dynamics, we adapt the generalized method of moments (GMM) estimator proposed by Sopchokchai (2016) and Storesletten, Telmer, and Yaron (2004b). We use income and health data from the 1996–2012 waves of the Medical Expenditure Panel Survey (MEPS) to run earnings regressions designed to identify the mean effects of health shocks on current and future income. We then show how to reveal the second moment of the distribution of those health shocks from the residuals of the earnings regressions. One important consequence of our process is that it allows us to break down the permanent and transitory components of income into health and non-health. We use a wide variety of controls, and (following Sopchokchai (2016)) we disaggregate our data into single childless men, single childless women, single mothers, and couples.[3]

While the MEPS contains many measures of health and health-care utilization that could potentially identify events we define as health shocks, we focus our analysis on emergency room (ER) visits. There are several reasons for this. First, the number of Americans who visit emergency departments is substantial: one estimate put the number at 136.3 million for 2011 (National Center for Health Statistics, 2011). Second, ER visits are, for the most part, unanticipated.[4] Third, while the reasons for ER visits to vary immensely (from a life-threatening heart failure to a broken finger), all identify a range of negative health shocks; hence, any associated income effect is also negative. Finally, ER visits are relatively common: in our sample, roughly 15–20% of single individuals and 30% of couples visit in

[3]Sopchokchai (2016) demonstrates the importance of family composition in determining the effects of income shocks. As show in Chapter 2, she finds evidence of risk sharing—couples insure each other against both transitory and permanent shocks. Her results underline the importance of not treating singles and couples in the same way, as is common in the income dynamics literature.

[4]One problem is that some uninsured Americans may use ER visits as an alternative to primary health care. We address this possibility by considering an alternative definition that excludes visits for which the cost is below $500, the likely maximum for most such non-urgent cases.

any calendar year.[5]

The sheer volume of ER visits suggests a wide range of underlying medical conditions, motivating our focus on heterogeneous impacts.[6] We find that such health shocks, plotted on a graph, have a U-shaped relationship with age. This implies that demographic transitions in the U.S. population have non-trivial implications for aggregate income dynamics.[7]

The estimated mean effects of health shocks (as per ER visits) are similar to those found in the literature. We find current income effects ranging from -0.06 log earning points for single mothers, to -0.12 points for childless women. We also find evidence of heterogeneous income effects for single women (both with and without children); but, somewhat surprisingly, virtually no evidence that health shocks contribute to the transitory or permanent variance components for single men or for couples. Specifically, we find that health shocks for single women exhibit large transitory variance relative to the mean health effect (roughly 70% of the mean), and that 75% of the transitory component can be attributed to health shocks. This finding suggests the existence of household insurance to such shocks. This result is consistent with findings by Sopchokchai (2016), which suggest that being part of a couple provides insurance against both transitory and permanent income shocks.

We might naturally wonder what role health insurance might play in lessening the income effects of health shocks. There are at least two reasons to expect this. First, having insurance might provide the quality and quantity of health care individuals need to recover faster or more completely. Second, having insurance might proxy for other benefits and compensation schemes that will smooth income loss: for instance, salaried workers may have sick-leave benefits. We do find some evidence that health insurance lowers the income risk from health shocks. Specifically, we find that single mothers are affected

[5]Since these numbers include children, we consider alternative definitions for adults only.

[6]It also suggests that health shocks, as proxied by ER visits, may be quantitatively important for the macroeconomy in terms of health care costs.

[7]Jaimovich and Siu (2009) document how the intensive margin of hours worked depends on age and thus, by construction, how demographics affect aggregate dynamics. Although our work does not focus on that particular aspect, we can still associate income dynamics with particular age-cohorts.

worst by health shocks; and that uninsured couples face the most heterogeneity in income effects.[8] Further extrapolating our analysis for couples, our results suggest that health insurance for couples may reduce residual income variance by roughly 10 percentage points.

3.2 Data

We use the consolidated files of the MEPS household component from panels 1 to 16 (1996–2012). The MEPS is a series of large-scale surveys of families and individuals, in selected communities across the United States, drawn from a nationally representative sample of households. The survey, begun in 1996, collects primarily health-related information such as health-care use, health-care expenditures, and health insurance coverage. [9]It also collects detailed information on the respondents' health status, demographic and socio-economic characteristics, employment, access to care, and satisfaction with health care. The panel design of the survey includes five rounds of interviews covering two calendar years. For example, observations in panel 1 are from years 1996 and 1997, and in panel 2 are from years 1997 and 1998. Each calendar year occupies two panels in the survey (except 1996, the initial year). This design provides data that allows us to examine individual changes in selected variables, such as income and health status

The MEPS has two major components: the Household Component (HC), which provides data from individual households and their members, and the Insurance Component (IC), which provides health insurance data retrieved from employers of respondents. The data used in this study is from the HC, which can be retrieved from the Internet.[10] The

[8]Note that our income measure does not include transfers. In particular, any income from disability insurance would not be captured in our analysis.

[9]The MEPS survey, administered by the Agency for Healthcare Research and Quality, examines the civilian non-institutionalized population; the households selected participated in the previous year's National Health Interview Survey.

[10]Note that information on health insurance coverage is available in the HC. The IC collects data from employers (of the respondents) on the number and types of private health insurance plans offered, benefits associated with these plans, annual premiums, annual contributions by employers and employees, eligibility requirements, and employer characteristics.

MEPS defines a family as a household consisting of two or more persons who are related by blood, marriage, or adoption. Two unmarried persons living together in a common-law relationship are also defined as a family. This study follows the MEPS definitions, and identifies one person in each household as a "reference person" who is aged 16 or older, and who owns or rents the home. [11]

The MEPS provides individual data on total personal income, and its separate sources. Family income, the main variable in this study, is derived by aggregating the incomes of the reference person and their spouse or partner. This study defines income as the sum of all sources of income: annual earnings from wages, salaries, bonuses, tips, commissions, etc. (except tax refunds and capital gains). However, we exclude individuals who are self-employed. Note that income is adjusted to common 2010 dollars using the Consumer Price Index, and that family weight is applied to the appropriate analyses.[12]

3.2.1 Health Shocks

In this study, the proxy used for health shocks is ER visits. The MEPS data contains information on such visits, along with other types of hospital utilization (such as outpatient treatment and inpatient stays). The variable ER visits represents the count for each household member reported for the survey year.[13] We consider ER visits to be a proxy for health shocks that require medical examination. Although the MEPS data do not give the particular reasons for these, we argue that ER visits are sufficiently costly (both in dollars and in time) to reflect the individual's perceived need for medical care.

For couples, we identify health shocks as either involving adults (the reference person and their spouse), or adults and children. These analyses estimate the variables separately

[11]If more than one person meets these criteria, the respondents identify who should be considered the reference person; otherwise, the questionnaire defines "the head of the household" as the reference person.

[12]The detail of the family weight construction can be found in the MEPS user guides.

[13]In the MEPS file, this variable is named $ERTOTxx$, where "xx" is the last two digits of the survey year. For instance, in MEPS 2012 the ER-visits variable is named $ERTOT12$.

for these two definitions of health shocks. For single mothers, the definition of health shocks include both parent and child(ren)

3.3 Model

This section outlines our strategy for identifying heterogeneity in the effects of health shocks. As described in the Introduction, our approach seeks to decompose labour earnings into two orthogonal components using the technique of linear projection. Following Storesletten, Telmer, and Yaron (2004b), we posit an empirical model of log labour earnings, $y^h_{i,t}$, for an individual i in period t, who belongs to age cohort h. We assume that earnings can be decomposed as:

$$y^h_{it} = x^h_{it}\beta + \sum_{j=t-1}^{t} E^h_{it}\delta^{t-j}\gamma + \sum_{t=1}^{T} Y_t + \epsilon^h_{it} \tag{3.1}$$

where x^h_{it} is a vector of explanatory variables including age and its polynomials (age^2, age^3), years of education, number of children (1 to 4 or more), type of health insurance (private, public only, and uninsured), gender (for the single population), geographical region, and an indicator for living in Metropolitan Statistical Area (MSA). A further indicator, E^h_{it}, is a dummy variable which equals 1 only if the individual visits an ER in period t.[14] The variable γ is the effect of a health shock on earnings, and δ is a (constant) decay to account for possible persistence; Y_t is a dummy variable for each calendar year; and ϵ^h_{it} is the idiosyncratic random error. In our estimation, age h runs from 23 to 60, and period t runs from 1996 to 2011. For each individual i, we typically have two observations of $y^h_{i,t}$. Estimation of equation 3.1 by ordinary least squares (OLS) yields the orthogonal components.

Some discussion of equation 3.1 is appropriate to illustrate our identification approach. Variable x^h_{it} contains the observables for individual i, and β captures the labour-market

[14] We specifically define health shocks in later section.

price of these observables. The year dummies Y_t capture changes in earnings over the sample period—that is, they account for business cycle effects and/or aggregate wage inflation. Our $E^h_{i,t}$ captures the current labour-income cost of a health shock. We assume that $E^h_{i,0} = 0$.

The sample of interest includes the population of working age (which we define as 23 to 60), who are employed and have positive income (as defined above) for both panel years. The analysis for the single population covers only the childless; a separate analysis is done for single mothers. We exclude same-sex couples since studies such as Berg and Lien (2002), Black et al. (2003), Blandford (2003), and Zavodny (2007) find that same-sex couples tend to experience income inequalities; couples where one spouse became divorced, separated, or deceased during the survey; and couples whose babies are less than one year old.[15]

We assume that the residuals, ϵ^h_{it}, follow an ARMA process:

$$\epsilon^h_{it} = \alpha_i + z^h_{i,t} + \varphi_{i,t} \tag{3.2}$$

$$z^h_{i,t} = \rho z^h_{i,t-1} + \eta_{i,t} \tag{3.3}$$

with $\alpha_i \sim Niid(0, \sigma^2_\alpha)$; $\varphi_{i,t} \sim Niid(0, \sigma^2_\varphi)$ and $\eta_{i,t} \sim Niid(0, \sigma^2_\eta)$. In this specification, α_i represents individual, specific fixed effects; $\varphi_{i,t}$ represents transitory shocks to log earnings; and $z^h_{i,t}$ is the persistent component of residual earnings driven by shocks, $\eta_{i,t}$, to individual earnings with autocorrelation ρ. We note that some specifications in the literature—specifically Hryshko (2012), Blundell, Pistaferri, and Preston (2008), and Meghir and Pistaferri (2004)—calibrate a permanent component for earnings by specifying $\rho = 1$. This typically yields the result that the variance of the permanent component rises over time. In this study, we restrict parameter $\rho = (0, 1]$ as in Sopchokchai (2016).

Storesletten, Telmer, and Yaron (2004b) consider the same structure, and investigate

[15] This last restriction is to avoid including women who visited the ER to give birth.

whether being born during an economic expansion or contraction affects the lifetime earnings profile. Similarly, we require that α_i does not depend on h or t, so that the initial distribution of (unskilled) labour earnings potential, detrended, does not depend on age and time. The second assumption we require is that $z^0_{i,t} = 0$, so that individuals have no permanent shocks beginning at birth. Both strong assumptions allow us to systematically decompose the variance of labour earnings across cohorts, since every birth cohort starts from the same distribution of residual earnings.

3.3.1 Heterogeneous Health Impacts

Our interest lies in the residuals, $\varphi_{i,t}$, and the estimated effect of health shocks, γ. Suppose that the effect of the health shock is heterogeneous across individuals so that the true effect is γ_i for individual i. Then:

$$\varphi_{i,t} = \tilde{\varphi}_{i,t} + \sum_{j=t-1}^{t} \delta^{t-j}(\gamma_i - \gamma)E^h_{i,j} = \tilde{\varphi}_{i,t} + \sum_{j=t-1}^{t} \delta^{t-j}\nu_i E^h_{i,j},$$

where $\tilde{\varphi}_{i,t}$ is the "true" residual, and ν_i is the centred γ_i. If we maintain our assumption that the health shock is uncorrelated with $\tilde{\varphi}_{i,t}$, then:

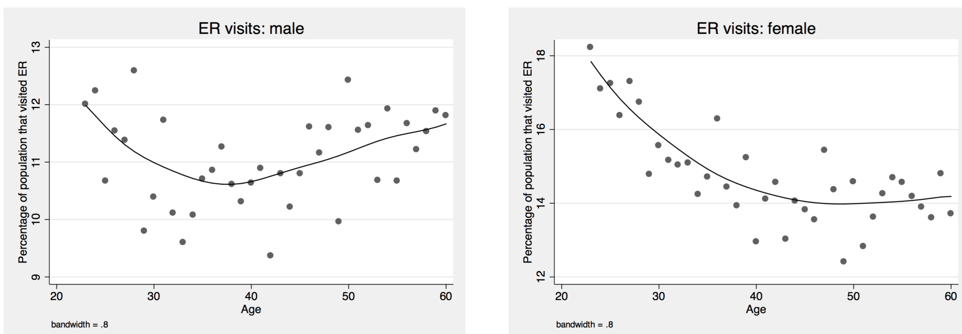
$$\sigma^2_{\varphi} = \sigma^2_{\tilde{\varphi}} + \sum_{j=t-1}^{t} \delta^{2(t-j)}\sigma^2_{\nu}\mathcal{I}(E^h_{i,j} = 1) \tag{3.4}$$

where (generically) σ^2_G is the cross-sectional variance of variable G for cohort h, and $\mathcal{I}$ is an indicator variable. Thus having information on the discrete health event $E^h_{i,t}$, allow us to partition our residuals as the sums of orthogonal components. The second component yields our estimate of the variance of the health shock effect and its decay parameter, δ.

As well, our decomposition clarifies exactly how heterogeneous health shocks translate into the residual variance of income. This suggests that periods of life that are characterized by more health shocks should have higher variances of residual incomes. Since the frequency of health shocks typically follows a U-shape, this suggests that the contribution of health shocks to the residual variance of income is expected to follow demographic profiles.[16] Interestingly, many other studies (for example, Storesletten, Telmer, and Yaron (2004b)) generally find that the residual variance of income also becomes U-shaped with age. Our results may possibly quantify exactly how much of the increasing residual variance of income can be attributed to health shocks.

FIGURE 3.1: Proportion of ER Visits by Age: Male and Female

(A) Male (B) Female



3.3.2 Endogeneity

The model we specify in equation 3.1 is a reduced form and, econometrically, likely suffers from endogeneity biases in the estimated coefficients. A natural concern is that these biases pollute our estimates of σ_γ^2. However, the Frisch-Waugh-Lovell theorem indicates that we do not need to be concerned about the effect of $x_{i,t}^h$ on $E_{i,t}^h$, since some observables explain the incidence of health shocks. We are not, however, sanguine about the effect of omitted variables, such as ability or latent health, for the estimated coefficients.

For expository purposes, assume that $x_{i,t}^h$ is a scalar, that $Y_t = 0$ for all t, and that $E_{i,t}^h$ is uncorrelated with the omitted variable $k_{i,t}^h$. We know that the estimated coefficient β is

[16] See Figure 3.1.

biased by the degree of correlation between $x^h_{i,t}$ and $k^h_{i,t}$, since $plim\hat{\beta} = \beta + plim(\hat{\delta})\beta_2$—where $\hat{\delta}$ is the coefficient of a regression of $k^h_{i,t}$ on $x^h_{i,t}$, and β_2 would be the coefficient on $k^h_{i,t}$ if it were included in the regression. Simple rearrangement implies that the regression residual φ_{it} would, in this case, include additional terms:

$$\sigma^2_{\varphi} = \mathrm{Var}(\tilde{\varphi}_{i,t} - x^h_{i,t}\hat{\delta}\beta_2) + \sum_{j=t-1}^{t} \delta^{2(t-j)}\sigma^2_{\nu}\mathcal{I}(E^h_{i,j} = 1), \tag{3.5}$$

where the term $\tilde{\varphi}_{i,t} - x^h_{i,t}\hat{\delta}\beta_2 \gtrless \tilde{\varphi}_{i,t}$ depends on the sign of $x^h_{i,t}\hat{\delta}\beta_2$ (which is unobserved). Thus, for the case of omitted variables, we know that the residuals from equation 3.1 are likely to be biased, but the direction of this bias is unknown.[17] We assume that $x^h_{i,t}$ is orthogonal to the true residual, $\tilde{\varphi}^h_{i,t}$, so that the cross-sectional variance of unexplained earnings for each cohort h becomes:

$$\mathrm{Var}(\epsilon^h_{it}) = \sigma^2_{\alpha} + \sigma^2_{\tilde{\varphi}} + (\hat{\delta}\beta_2)^2\sigma^2_{x^h_{i,t}} + \sum_{j=t-1}^{t} \delta^{2(t-j)}\sigma^2_{\nu}\mathcal{I}(E^h_{i,j} = 1) + \sum_{j=1}^{h-1} \rho^{2j}\sigma^2_{\eta}. \tag{3.6}$$

The endogeneity bias affects the variance of our errors depending on the magnitude of the correlation of $k^h_{i,t}$ and $y^h_{i,t}$, and the correlation between $k^h_{i,t}$ and $x^h_{i,t}$ scaled by the variance in $x^h_{i,t}$. Since the variance in $x^h_{i,t}$ is observed, we include it in our estimation, assuming that $\hat{\delta}\beta_2$ is constant across cohorts; this allows us to uncover σ^2_{ϵ}. This bias is particularly problematic for our estimates of σ^2_{α} (and, to a lesser extent, $\sigma^2_{\tilde{\varphi}}$). The bias is larger for σ^2_{α} if there is little change in the variance of $x^h_{i,t}$ across h, conditional on a given $\hat{\delta}\beta_2$. This will bias $\sigma^2_{\tilde{\varphi}}$, to the extent that $x^h_{i,t}$ changes over time for a given cohort.

However, since our main concern is to uncover σ^2_{ν}, our decomposition shows that endogeneity in the first stage is unlikely to be a problem for this aspect. Our analysis applies analogously when the observable and/or omitted variables are vectors. Specifically, it is

[17] The case where endogeneity arises from measurement errors in $x^h_{i,t}$ also implies a bias in our estimated residuals. However, in this case we can show that the effect leads to residuals that have unambiguously larger variances.

possible that equation 3.1 also suffers from omitted latent health bias ($H^h_{i,t}$). As pointed out by Gruber (2000), the possession of health insurance coverage may itself be a function of "true" health status, though this is not observable.[18] This omitted latent health variable may affect insurance coverage, as well as educational attainment (see Currie and Madrian (1999)).

Equation 3.1 contains both education and type of insurance coverage, $edu^h_{i,t}$ and $insure^h_{i,t}$, respectively, and thus may possibly also suffer from omitted variable bias in terms of ability and latent health. Following Currie and Madrian (1999), we assume that $edu^h_{i,t}$ is correlated with both $A^h_{i,t}$ (ability) and $H^h_{i,t}$ (latent health), and $insure^h_{i,t}$ is correlated with $H^h_{i,t}$ (latent health). The regression residual φ_{it}, in this circumstance, is:

$$\begin{aligned}\sigma^2_{\varphi} = &\mathrm{Var}(\tilde{\varphi}_{i,t} - edu^h_{i,t}(\hat{\delta}\beta_2 + \hat{\gamma}_1\beta_4) - insure^h_{i,t}(\beta_3 + \hat{\gamma}_2\beta_4)) + \\ &\sum_{j=t-1}^{t} \delta^{2(t-j)}\sigma^2_{\nu}\mathcal{I}(E^h_{i,j} = 1),\end{aligned} \tag{3.7}$$

where $\hat{\delta}$ is the coefficient of a regression of $A^h_{i,t}$ on $edu^h_{i,t}$ and β_2 would be the coefficient on $A^h_{i,t}$ if it were included in the regression of interest, just as before. The coefficients $\hat{\gamma}_1$ and $\hat{\gamma}_2$ come from a regression of $H^h_{i,t}$ on both $edu^h_{i,t}$ and $insure^h_{i,t}$, respectively, and the coefficient β_4 is associated with $H^h_{i,t}$ were it included in a regression of equation 3.1.

3.3.3 Moments

We follow Storesletten, Telmer, and Yaron (2004b) and Sopchokchai (2016) in writing age-specific (h) cross-sectional variances of $\epsilon^h_{i,t}$ as:

$$\mathrm{Var}(\epsilon^h_{it}) = \sigma^2_{\alpha} + \sigma^2_{\tilde{\varphi}} + (\hat{\delta}\beta_2)^2\sigma^2_{edu^h_{i,t}} + \sum_{j=t-1}^{t} \delta^{2(t-j)}\sigma^2_{\nu}\mathcal{I}(E^h_{i,j} = 1) + \sum_{j=1}^{h-1} \rho^{2j}\sigma^2_{\eta}. \tag{3.8}$$

[18] Many empirical studies use self-assessed health status (SAHS) as a measure of health, but researchers are concerned about its validity and reliability. SAHS is generally plagued with inaccuracies and bias (reporting heterogeneity). See, for instance, Crossley and Kennedy (2002) and Pfarr, Schmid, and Schneider (2012).

We also write autocovariances as:

$$\begin{aligned}\mathrm{E}^s(\epsilon_{it}^h \epsilon_{i,t-1}^{h-1}) = \sigma_\alpha^2 + (\hat{\delta}\beta_2)^2 \sigma^2_{edu^h_{i,t}} + \delta\sigma_\nu^2 \mathcal{I}(E^h_{i,t-1} = 1) \\ + \sigma_\nu^2 \mathcal{I}(E^h_{i,t-1} = 1 \& E^h_{i,t} = 1) + \rho \sum_{j=1}^{h-1} \rho^{2(j-1)} \sigma_\eta^2.\end{aligned} \tag{3.9}$$

Note that the highest level of education rarely changes over one period for individuals, for the sake of simplicity we impose $edu^h_{i,t} = edu^h_{i,t-1}$ in the inclusion of the $(\hat{\delta}\beta_2)^2\sigma^2_{edu^h_{i,t}}$ in equation 3.9. We later augment the specification according to equation 3.7, and include the latent health bias term as part of our robustness checks. Specifically, equations 3.8 and 3.9 becomes, respectively:

$$\begin{aligned}\mathrm{Var}(\epsilon_{it}^h) =& \sigma_\alpha^2 + \sigma_{\tilde{\varphi}}^2 + (\hat{\delta}\beta_2 + \hat{\gamma_1}\beta_4)^2 \sigma^2_{edu^h_{i,t}} + (\beta_3 + \hat{\gamma_2}\beta_4)^2 \sigma^2_{insure^h_{i,t}} \\ &+ \sum_{j=t-1}^{t} \delta^{2(t-j)} \sigma_\nu^2 \mathcal{I}(E^h_{i,j} = 1) + \sum_{j=1}^{h-1} \rho^{2j} \sigma_\eta^2,\end{aligned} \tag{3.10}$$

and

$$\begin{aligned}\mathrm{E}^s(\epsilon_{it}^h \epsilon_{i,t-1}^{h-1}) = \sigma_\alpha^2 + (\hat{\delta}\beta_2 + \hat{\gamma_1}\beta_4)^2 \sigma^2_{edu^h_{i,t}} + (\beta_3 + \hat{\gamma_2}\beta_4)^2 \sigma^2_{insure^h_{i,t}} \\ + \delta\sigma_\nu^2 \mathcal{I}(S^h_{i,t-1} = 1) + \sigma_\nu^2 \mathcal{I}(S^h_{i,t-1} = 1 \& S^h_{i,t} = 1) + \rho \sum_{j=1}^{h-1} \rho^{2(j-1)} \sigma_\eta^2,\end{aligned} \tag{3.11}$$

These age-specific variances and autocovariances are the basis of the Generalized Method of Moments (GMM) estimator, described in the next section.

3.4 Estimation

Before the estimation can be done, one other adjustment in the specification of variances and autocovariances is necessary. Because the health shocks indicator, $E_{i,t}$, is equal to 1 for those who experienced health shocks but zero for those who did not, the variance may not be properly scaled. In particular, the variance is σ_ν^2 for individuals with $E_{i,t} = 1$, and zero for individuals with $E_{i,t} = 0$. For simplicity's sake, assume that for individual $i = 1, ..., N$, $e_i = [0, 1]$, and that $\mathrm{Var}(e)$ is the true variance of e_i. Since e_i is a dichotomous variable, the calculated variance is such that:

$$\mathrm{Var}(\tilde{e}) = \frac{1}{N-1}\sum_{i=1}^{N}(0-0)^2 + \frac{1}{N-1}\sum_{i=1}^{N}(e_i - \bar{e})^2 = \frac{1}{N-1}\sum_{i=1}^{N}(e_i - \bar{e})^2 \tag{3.12}$$

where $\mathrm{Var}(\tilde{e})$ is the adjusted health shocks (substituted for $e_i = 0$) and $\bar{e}$ is the mean value of e_i. It is easy to see that the variance in equation 3.12 is not correctly scaled, and thus, does not represent the true variance $var(e)$. Rescaling the variance, we obtain:

$$\mathrm{Var}(e) = \frac{N-1}{N_{S_i=1}-1}\frac{1}{N-1}\sum_{i=1}^{N}(e_i - \bar{e})^2 = \frac{N-1}{N_{S_i=1}-1}\mathrm{Var}(\tilde{e}). \tag{3.13}$$

Applying the rescaling to the moments outlined in equations 3.8 and 3.9 yields:

$$\begin{aligned}\mathrm{Var}(\epsilon_{it}^h) &= \sigma_\alpha^2 + \sigma_{\tilde{\varphi}}^2 + (\hat{\delta}\beta_2)^2\sigma^2_{edu^h_{i,t}} \\ &+ \sum_{j=t-1}^{t}\delta^{2(t-j)}\frac{N^h-1}{N^h_{E_{i,j}=1}-1}\sigma_\nu^2\mathcal{I}(E^h_{i,j}=1) + \sum_{j=1}^{h-1}\rho^{2j}\sigma_\eta^2.\end{aligned} \tag{3.14}$$

and

$$\begin{aligned}\mathrm{E}(\epsilon_{it}^h\epsilon_{i,t-1}^{h-1}) = \sigma_\alpha^2 + (\hat{\delta}\beta_2)^2\sigma^2_{edu^h_{i,t}} + \delta\frac{N^h-1}{N^h_{E_{i,t-1}=1}-1}\sigma_\nu^2\mathcal{I}(E^h_{i,t-1}=1) \\ + \frac{N^h-1}{N^h_{E_{i,t-1,t}=1}-1}\sigma_\nu^2\mathcal{I}(E^h_{i,t-1}=1\&E^h_{i,t}=1) + \rho\sum_{j=1}^{h-1}\rho^{2(j-1)}\sigma_\eta^2,\end{aligned} \tag{3.15}$$

respectively, where N^h is a sample size of age h, $N^h_{E_{i,j}=1}$ is a sample size of age h that experienced health shocks at time $j = (t, t-1)$, and $N^h_{E_{i,t-1,t}=1}$ is a sample size of age h

that experienced health shocks in both t and $t-1$.

TABLE 3.1: Coefficient Estimates from Simple Regressions: Singles

	Childless Male	**Childless Female**	**Single Mom**
ER visits (t-1)	-0.0962	-0.1203	-0.0583
	(0.0234)	(0.0228)	(0.0164)
ER visits (t)	-0.1261	-0.1451	-0.0558
	(0.0234)	(0.0231)	(0.0166)
Age	0.2618	0.1944	0.0310
	(0.0378)	(0.0411)	(0.0412)
Age^2	-0.0058	-0.0041	0.0001
	(0.0010)	(0.0010)	(0.0010)
Age^3	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)
Region *(omit Northeast)*			
Midwest	-0.1014	-0.0713	0.0072
	(0.0261)	(0.0281)	(0.0255)
South	-0.1423	-0.1211	-0.1175
	(0.0242)	(0.0253)	(0.0227)
West	-0.0727	-0.0310	0.0761
	(0.0255)	(0.0273)	(0.0247)
Metropolitan Statistical Area	0.1489	0.1465	0.1302
	(0.0224)	(0.0244)	(0.0213)
Education	0.0640	0.0836	0.0787
	(0.0029)	(0.0034)	(0.0029)
Health Insurance *(omit any private)*			
Public health insurance	-1.0653	-1.0202	-0.8983
	(0.0277)	(0.0262)	(0.0195)
Uninsured	-0.5351	-0.6951	-0.5670
	(0.0196)	(0.0246)	(0.0215)
N	9,655	8,024	9,329

Data source: MEPS panel 1-16 (1996 to 2012). Note: Robust standard errors are provided in parentheses. The estimations were done using the MEPS family weights. The estimates of the year fixed-effects are omitted from this table.

As previously described in Section 3.3, we begin by extracting the idiosyncratic random error, $\hat{\epsilon}^h_{i,t}$, from the simple regressions of equation 3.1, obtaining the age-specific variance, $\mathrm{Var}(\epsilon^h_{i,t}) = \mathrm{E}(\hat{\epsilon}^h_{i,t})^2$. The regressions coefficients of equation 3.1 can be found in Table 3.1 for singles and Table 3.2 for couples. We do not explicitly interpret these coefficients, since they are not of interest to this study. However, we observe that—as expected—the signs of coefficients of all measures of health shocks are negative, and statistically significant. Using the income process model outlined in equations 3.2 and 3.3 (which give rise to the

TABLE 3.2: Coefficient Estimates from Simple Regressions: Couples

	Health Shocks From	
	Adults Only	**Adults and Child**
ER visits (t-1)	-0.0926	-0.0700
	(0.0089)	(0.0078)
ER visits (t)	-0.0867	-0.0650
	(0.0090)	(0.0079)
Age	0.0464	0.0466
	(0.0215)	(0.0216)
Age^2	-0.0008	-0.0008
	(0.0005)	(0.0005)
Age^3	4.29e-06	4.27e-06
	(4.10e-06)	(4.10e-06)
Age of spouse	0.0647	0.0652
	(0.0123)	(0.0123)
Age of $spouse^2$	-0.0011	-0.0011
	(0.0003)	(0.0003)
Age of $spouse^3$	5.19e-06	5.26e-06
	(2.03e-06)	(2.04e-06)
Region *(omit Northeast)*		
Midwest	-0.0258	-0.0260
	(0.0112)	(0.0113)
South	-0.1075	-0.1078
	(0.0105)	(0.0105)
West	-0.0135	-0.0139
	(0.0109)	(0.0109)
Metropolitan Statistical Area	0.1366	0.1364
	(0.0089)	(0.0089)
Education	0.0515	0.0518
	(0.0016)	(0.0016)
Education of spouse	0.0503	0.0506
	(0.0015)	(0.0015)
Number of child	-0.0094	-0.0014
	(0.0031)	(0.0031)
Health Insurance *(omit any private)*		
Public health insurance	-0.8082	-0.8148
	(0.0173)	(0.0173)
Uninsured	-0.4780	-0.4780
	(0.0114)	(0.0114)
N	32,515	32,515

Data source: MEPS panel 1-16 (1996 to 2012). Note: Robust standard errors are provided in parentheses. The estimations were done using the MEPS family weights. The estimates of the year fixed-effects are omitted from this table.

age-specific variances and autocovariances outlined in equations 3.8 and 3.9) for each age and time pair (h, t), we can write the moments associated with these variances and autocovariances as:

$$\mathrm{E}_t\left[(\epsilon_{it}^h)^2 - \sigma_\alpha^2 - \sigma_{\tilde{\varphi}}^2 - (\hat{\delta}\beta_2)^2\sigma^2_{edu^h_{i,t}} - \sum_{j=t-1}^{t}\delta^{2(t-j)}\frac{N^h-1}{N^h_{E_{i,j}=1}-1}\sigma_\nu^2\mathcal{I}(E^h_{i,j}=1) - \sum_{j=1}^{h-1}\rho^{2j}\sigma_\eta^2\right] = 0 \tag{3.16}$$

and

$$\begin{aligned}\mathrm{E}_t\Bigg[&\epsilon_{it}^h\epsilon_{i,t-1}^{h-1} - \sigma_\alpha^2 - (\hat{\delta}\beta_2)^2\sigma^2_{edu^h_{i,t}} - \delta\frac{N^h-1}{N^h_{E_{i,t-1}=1}-1}\sigma_\nu^2\mathcal{I}(E^h_{i,t-1}=1)\\ &-\frac{N^h-1}{N^h_{E_{i,t-1,t}=1}-1}\sigma_\nu^2\mathcal{I}(E^h_{i,t-1}=1\&E^h_{i,t}=1) - \rho\sum_{j=1}^{h-1}\rho^{2(j-1)}\sigma_\eta^2\Bigg] = 0,\end{aligned} \tag{3.17}$$

where h runs from 23 to 60, and t runs from 1996 to 2012. Note that the variances and autocovariances from equations 3.10 and 3.11 yield similar moments as those in 3.16 and 3.17: the only difference is the inclusion of the term $\sigma^2_{edu^h_{i,t}} - (\beta_3 + \hat{\gamma}_2\beta_4)^2\sigma^2_{insure^h_{i,t}}$ instead of the term $(\hat{\delta}\beta_2)^2\sigma^2_{edu^h_{i,t}}$. Because of this, the moments associated with equations 3.10 and 3.11 are omitted from this section.

The estimation of parameters of interest $(\hat{\delta}\beta_2, \sigma_\alpha, \sigma_{\tilde{\varphi}}, \delta, \sigma_\nu, \rho, \sigma_\eta)$ is done using the two-step GMM estimators, and all standard errors reported are bootstrapped.[19] The estimation is performed separately for the following subgroups: childless single males, childless single females, single mothers, couples with health shocks from adults of the household, and couples with health shocks from adults of the household and their children.

[19]Specifically, we simulate a set of n *i.i.d.* uniform random integers from the range 1,. . .,n with replacement. We construct a bootstrap sample to compute the estimates, and repeat these steps 1,000 times to computer the standard of estimates.

3.5 Discussion

This section discusses the results of this study in detail. We begin with a descriptive analysis of the data, then proceed to graphically analyzing the cross-sectional variances of income between those who experienced health shocks, and those who do not. We close this section by discussing all the parameter estimates from the GMM estimation.

3.5.1 Descriptive Analysis

Tables 3.3 and 3.4 respectively show the mean values of variables for singles and couples. The sample size is 9,655 for childless single men; 8,024 for childless single women; 9,329 for single mothers, and 32,515 couples. Table 3.4 also separately shows the mean values of age, and years of education, for male and female household members.

On average, these singles (childless male, childless female, and single moms) are of similar age, approximately 40 years old, and largely live in a Metropolitan Statistical Area (about 85% of the population). Childless single males have a slightly higher number of years of education, 13 years on average, than the childless single female counterparts (about 12.76 years). The single mom population does not fare as well as the childless males and females in terms of years of education—having about 6 to 10 months less education on average.

On average, these singles (childless males, childless females, and single mothers) are of similar age—roughly 40—and mostly (85%) live in a Metropolitan Statistical Area (MSA). Childless single males have very slightly more years of education (13 years on average) than childless single females (about 12.76 years). The population of single mothers, on average, has about 6–10 months less education than the childless males and females. This can be clearly seen when years of education are broken down by the type of degree obtained. Most of the singles hold a high-school diploma, but relative to the childless single males and females, single mothers are heavily concentrated at the lower level: roughly

TABLE 3.3: Summary Statistics: Singles

Variable	**Childless Male**		**Childless Female**		**Single Mom**	
Age	39.46	(0.1122)	40.92	(0.0793)	39.56	(0.0975)
Total household income	42,533	(371.98)	35,367	(224.6638)	29,576.76	(268.5427)
Years of education completed	13.02	(0.0302)	12.76	(0.0211)	12.22	(0.0291)
Proportion in MSA	0.84	(0.0037)	0.86	(0.0031)	0.84	(0.0038)
Education *(proportion)*:						
Less than high school	0.2025	(0.0041)	0.1703	(0.0042)	0.2893	(0.0046)
High school	0.4596	(0.0051)	0.4128	(0.0055)	0.4954	(0.0052)
Bachelor's degree	0.1946	(0.0040)	0.2218	(0.0046)	0.0946	(0.0030)
Graduate degree	0.0713	(0.0026)	0.1067	(0.0034)	0.0348	(0.0018)
Other degree	0.0719	(0.0026)	0.0874	(0.0032)	0.0857	(0.0029)
Region *(proportion)*:						
Northeast	0.16	(0.0037)	0.16	(0.0041)	0.18	(0.0039)
Midwest	0.23	(0.0043)	0.21	(0.0046)	0.20	(0.0041)
South	0.36	(0.0049)	0.38	(0.0054)	0.41	(0.0051)
West	0.26	(0.0044)	0.24	(0.0048)	0.22	(0.0043)
Health Insurance *(proportion)*:						
Any private	0.64	(0.0049)	0.67	(0.0042)	0.51	(0.0052)
Public only	0.11	(0.0032)	0.17	(0.0032)	0.30	(0.0047)
Uninsured	0.25	(0.0044)	0.16	(0.0032)	0.19	(0.0041)
Health Shock *(proportion)*:						
ER visit (t-1)	0.14	(0.0035)	0.19	(0.0036)	0.38	(0.0050)
ER visit (t)	0.13	(0.0034)	0.18	(0.0043)	0.35	(0.0050)
N	9,655		8,024		9.329	

Data source: MEPS panel 1-16 (1996 to 2012), Note: The summary statistics are weighted using the MEPS family weight. Mean values reported. Standard deviations in parentheses. MSA = Metropolitan Statistical Area. Single mom's health shocks include that of herself and her children.

TABLE 3.4: Summary Statistics: Couples

Variable		
Male person of the household		
Age	43.93	(0.0558)
Years of education completed	12.72	(0.0180)
Education *(proportion)*:		
Less than high school	0.2329	(0.0023)
High school	0.4371	(0.0027)
Bachelor's degree	0.1659	(0.0021)
Graduate degree	0.0922	(0.0016)
Other degree	0.0719	(0.0014)
Female person of the household		
Age	41.84	(0.0542)
Years of education completed	12.79	(0.0175)
Education *(proportion)*:		
Less than high school	0.2120	(0.0023)
High school	0.4446	(0.0027)
Bachelor's degree	0.1757	(0.0021)
Graduate degree	0.0802	(0.0015)
Other degree	0.0875	(0.0016)
Total household income	79,387.29	(316.54)
Number of child	1.34	(0.0070)
Proportion in MSA	0.81	(0.0022)
Health Insurance *(proportion)*:		
Any private	0.76	(0.0023)
Public only	0.07	(0.0014)
Uninsured	0.17	(0.0021)
Region *(proportion)*:		
Northeast	0.15	(0.0020)
Midwest	0.21	(0.0023)
South	0.37	(0.0027)
West	0.27	(0.0024)
Health Shock–adults only *(proportion)*:		
ER visit (t-1)	0.21	(0.0025)
ER visit (t)	0.20	(0.0022)
Health Shock–adults and children *(proportion)*:		
ER visit (t-1)	0.30	(0.0025)
ER visit (t)	0.29	(0.0025)
N	32,515	

Data source: MEPS panel 1-16 (1996 to 2012), Note: The summary statistics are weighted using the MEPS family weight. Mean values reported. Standard deviations in parentheses. MSA = Metropolitan Statistical Area.

80% have only a high-school diploma, or never completed high school. This proportion of high-school graduates and non-graduates is substantially lower in the case of childless males (65%) and females (58%).

At the higher end of the educational distribution, childless single females are most likely to hold either a B.A. or a graduate degree—the figure is about 32% for childless females, 26% for childless males, and 12% for single mothers. However, this does not correlate with high income. Childless single males appear to fare better than the females, earning approximately $7,000 more (despite their smaller fraction of population in higher education). Meanwhile, unsurprisingly, university degree holders who are single mothers make up only about 12%; and their income is significantly lower .

In comparison to the single population, couples are slightly older on average: married men are about 44, women about 42. Average years of education for both is slightly under 13 years. Their educational achievement is also very similar to the single population. Roughly 65% of both sexes hold either a high-school diploma or below, while about 25% hold either a B.A. or a graduate degree. In terms of total household income, on average couples earn some $79,000 per year. This is slightly higher than the combined average incomes of childless males and childless females (approximately $78,000).

Both singles and couples mostly have private health insurance, but single mothers depend much more heavily on the public health system (some 30%) than childless single males and females (11% and 17%).[20] The figure for couples that depend solely on public health insurance is significantly lower, about 7%. In all subgroups, the population with no health coverage at all is not insignificant: 16 to 19% of childless single females, single mothers, and couples. This number is markedly larger for the childless single males: about 25%. The finding that this group is more likely to have no health insurance is consistent with the many anecdotal reports that most uninsured individuals in the U.S. are young

[20]The health insurance status of couples is assumed to be that of the household reference person.

men with full-time jobs, or someone else in the household who works full time.[21]

Single mothers (and their children) are more likely than childless single males and females to make ER visits. During the survey years about 37.5% of single mothers visited the ER, compared to only about 13.5% of childless single males and 18.5% of childless single females. Not counting ER visits for the sake of children, roughly 20% of couples visited the ER in each of the two years of the survey. When children are included, this figure rises to roughly 30%.

TABLE 3.5: Health Insurance Categories and ER Visits (Singles)

	Childless Male		**Childless Female**		**Single Mom**	
Emergency Room Visits						
Visited ER:						
Any private	0.5485	(0.0137)	0.5448	(0.0129)	0.4418	(0.0083)
Public only	0.2250	(0.0115)	0.3193	(0.0121)	0.3780	(0.0081)
Uninsured	0.2265	(0.0115)	0.1359	(0.0089)	0.1802	(0.0064)
Did not visit ER						
Any private	0.6531	(0.0052)	0.7078	(0.0056)	0.5543	(0.0066)
Public only	0.0897	(0.0031)	0.1260	(0.0041)	0.2456	(0.0057)
Uninsured	0.2571	(0.0048)	0.1662	(0.0046)	0.2000	(0.0053)
N	9,655		8,024		9,329	

Data source: MEPS panel 1-16 (1996 to 2012), Note: The summary statistics are weighted using the MEPS family weight. Proportion reported. Standard deviations in parentheses. Single mom's health shocks include that of herself and her children.

Table 3.5 shows the proportions of different type of health insurance among the ER visitors. Most of the single population has private health insurance: 54% of childless single males and females, and 44% of single mothers. The latter appear to depend more heavily on public health coverage than childless single males and females: 18% of this group that visited the ER in the past year do not have any health insurance. That figure is even larger for the childless single male population: about 22% of these men do not have any health coverage when they visit the ER.

[21] See the U.S. Department of Health and Human Services study, available online at www.hhs.gov/healthcare/facts/bystate/ny.html.

Like Table 3.5, Table 3.6 presents the same information for couples, both for adults only and from adults and their children. Note that the distribution for the different categories of health insurance does not change considerably. On average, couples that experienced health shocks are largely covered by private health insurance. About 10 to 12 % are covered solely by public insurance, and about 11 to 15% are not insured at all.

TABLE 3.6: Health Insurance Categories and ER Visits (Couples)

	Health Shocks From			
	Adults Only		**Adults and Child**	
Emergency Room Visits				
Visited ER:				
Any private	0.7280	(0.0054)	0.7329	(0.0045)
Public only	0.1164	(0.0039)	0.1035	(0.0031)
Uninsured	0.1557	(0.0044)	0.1636	(0.0037)
Did not visit ER				
Any private	0.7766	(0.0026)	0.7810	(0.0027)
Public only	0.0541	(0.0014)	0.0513	(0.0015)
Uninsured	0.1693	(0.0023)	0.1677	(0.0025)
N	32,515		32,515	

Data source: MEPS panel 1-16 (1996 to 2012), Note: The summary statistics are weighted using the MEPS family weight. Proportion reported. Standard deviations in parentheses.

3.5.2 Graphical Analysis

In this section, we demonstrate the differences in cross-sectional income variance profiles between families that experience health shocks, and families that do not. Following Deaton and Paxson (1994), we decompose the cross-sectional variance of income into (birth) cohort and age effect as follows:

$$(\hat{\epsilon}^h_{i,t})^2 = a_c + b_h + e_{ht}, \tag{3.18}$$

where $(\hat{\epsilon}^h_{i,t})^2$ is obtained from simple regressions of equation 3.1, a_c and b_h are dummy variables respectively indicating birth cohort and age group, and e_{ht} is a residual. The result shows that the cross-sectional variation of income of different age groups is net of the birth cohort-specific dispersion.

Figures 3.2 to 3.4 plot the coefficients of age dummies, $\hat{b}_h$, from regression in equation 3.18. Figure 3.2 demonstrates the usual pattern of income variance similarly found in Storesletten, Telmer, and Yaron (2004b) and Sopchokchai (2016): the profile increases with age. More importantly, Figure 3.2 also shows a notable difference between the profiles of those who experience health shocks (as proxied by ER visits), and those who do not. Families with health shocks have higher income volatility than families without.

FIGURE 3.2: Cross-Sectional Variance of Income: Health Shocks vs. no Health Shocks

(A) Overall population

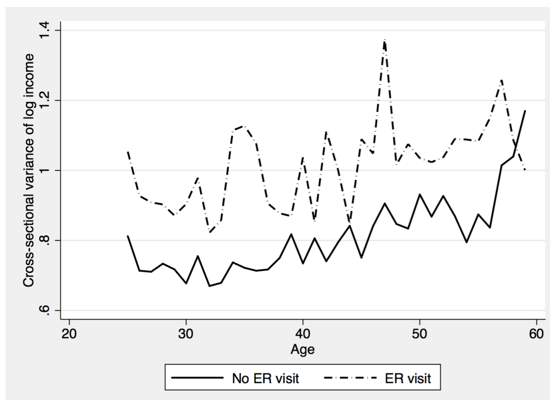


FIGURE 3.3: Cross-Sectional Variance of Income: Insured and Uninsured

(A) Insured (B) Uninsured

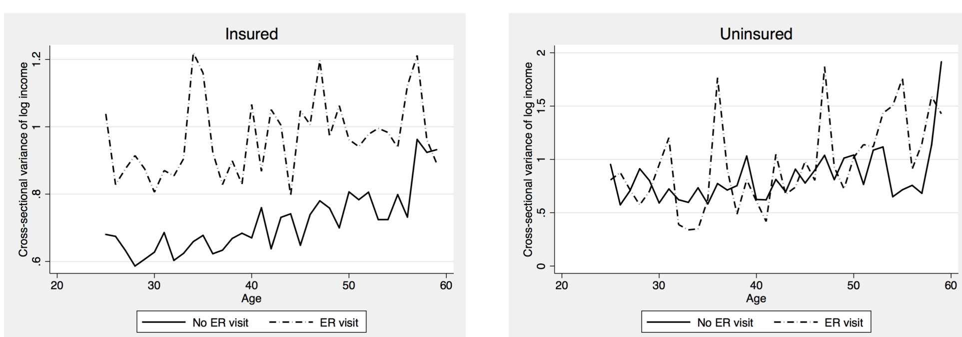
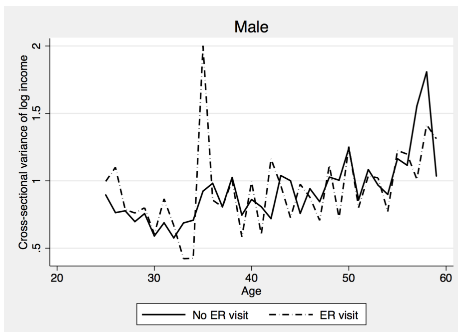
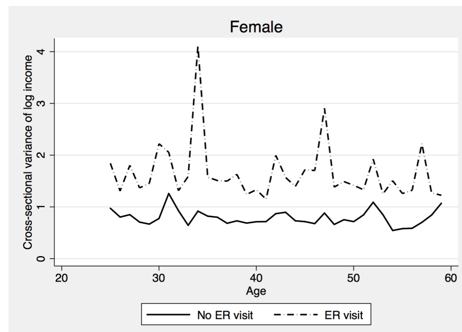


FIGURE 3.4: Cross-Sectional Variance of Income by Different Subgroups

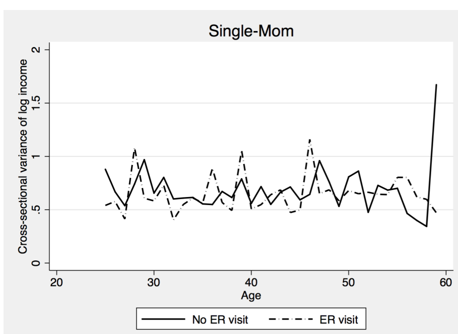
(A) Childless male



(B) Childless female



(C) Single mom



(D) Couple

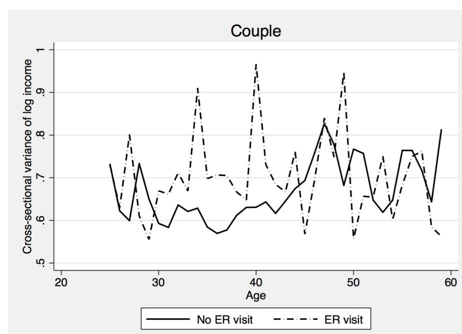


Figure 3.3 illustrates another interesting aspect of health shocks and insurance coverage. The first figure, 3.3a, shows a clear gap between the profiles of families that experienced health shocks and those that did not. However, in the case of uninsured families, the second figure, 3.3b, suggests no significant gap between the two profiles. This may not be surprising, since most uninsured people are from a relatively low socioeconomic background: lower-income families, people who have not earned a high-school diploma, and who work at blue-collar jobs (Institute of Medicine (US) Committee on the Consequences of Uninsurance, 2001). In comparison to insured families, uninsured families exhibit relatively high income volatility regardless of whether the family experiences health shocks. This indicates that health insurance provides some degree of protection for earning ability from negative health shocks.

Figure 3.4 shows cross-sectional income profiles of families that experience health shocks, and those that do not, for four subgroups: childless male, childless female, single mother, and couple. For ER visits (Figure 3.4), a clear distinction can be observed between those who experienced health shocks and those who did not; this is most obvious among childless single females and couples. Again, people who have a family member visit the ER in the past year appear to have relatively higher income volatility than other people. The pattern is less clear in the case of childless single males and single mothers (Figure 3.4a and 3.4c). Except for childless single males of age 35, there is no difference between the profiles of those who visited the ER and those who did not.

3.5.3 GMM Estimation Results

This section discusses parameter estimates from the GMM estimation discussed in Section 3.4. As noted above, and following Sopchokchai (2016), we examine three cases: single men, single women, and couples. For single women we also consider single mothers separately. We consider four different sources of health shocks for the couples: both the family head and spouse, just the head, just the spouse, and anyone else in the household—such as children. Our results can be briefly summarized as follows: we find evidence of heterogeneous income effects for single women with no children, and for single mothers; but virtually no evidence for single men or for couples. We consider various sources of health shocks for couples, but find no evidence of heterogeneous income effects regardless of the source. However, when we condition on health insurance, we find some evidence that uninsured couples face the most heterogeneity in income effects from health shocks. Overall, our results point to some gender disparity in income effects (though our model is silent as to why); we also find some evidence that health insurance lowers income risk from health shocks. Further extrapolating our analysis for couples, our results suggest that health insurance for couples may reduce residual income variance by roughly 10 percentage points.

Table 3.7 presents our estimates for singles, including both the health and non-health components.[22] We do not explicitly discuss the non-health component, though we do note that the estimates are in line with previous studies, particularly with Sopchokchai (2016). The estimates for singles suggest no heterogeneity in income effects related to health shocks for single men—indeed, the estimates are essentially zero. However, we do find evidence of heterogeneity for single women and single mothers. For single women, the estimates suggest that the variance of health shocks is roughly $3/4^{\text{th}}$ of the transitory variance of non-ER shocks, or roughly 70% of the mean impact from the first-stage regression. Thus our results suggest strongly that heterogeneity in health-care shocks is gender-differentiated. We also find modest evidence of persistence, as our estimates of δ are roughly one fifth. We note that single mothers experience a longer impact from health shocks than childless single females, the estimate of δ is close to unity, implying that the impact of such shock is largely permanent.

Turning to insured versus uninsured, the limitations of our data prevent analysis by

[22] Again, our first-stage estimates suggest an income effect of health-care shocks of roughly 0.1 log points; see Table 3.1.

gender. But considering all the singles together, we see little evidence that the uninsured experience different heterogeneous impacts than the insured. However, the impact might have more persistence in the case of uninsured singles: the parameter estimate of δ is approximately 0.24 for the uninsured population, and approximately 0.15 for the insured population. These figures suggest that uninsured singles could experience the effect of health shocks twice as long as insured singles.

TABLE 3.7: GMM Parameter Estimates: Singles

	Health Component		Non-Health Component				
	δ	σ_ν	$\hat{\gamma}\beta_2$	σ_α	$\sigma_{\tilde{\varphi}}$	ρ	σ_η
ER Visits:							
Childless Male	0.5355	0.4e-08	6.4e-09	0.5053	0.5249	0.9504	0.0866
	(0.0001)	(0.0000)	(0.0005)	(0.0001)	(0.0000)	(0.0000)	(0.0000)
Childless Female	0.2210	0.1051	1.3e-09	0.1272	0.1391	0.9642	0.0466
	(0.0002)	(0.0003)	(0.0000)	(0.0000)	(0.0001)	(0.0004)	(0.0000)
Single Mother	0.8458	0.0105	3.4e-09	0.5439	0.4833	0.5003	0.6e-06
	(0.0004)	(0.0000)	(0.0000)	(0.0001)	(0.0000)	(0.0001)	(0.0000)
ER Visits (charges $\geqslant$ \$500):							
Childless Male	0.5219	0.0009	0.4641	0.3152	0.2e-07	0.9647	0.0906
	(0.0003)	(0.0000)	(0.0001)	(0.0002)	(0.0011)	(0.0041)	(0.0000)
Childless Female	0.4232	0.0621	0.0046	0.3900	0.6191	0.5894	0.1296
	(0.0001)	(0.0032)	(0.0000)	(0.0006)	(0.0013)	(0.0086)	(0.0004)
Single Mother	0.8720	0.0742	0.0038	0.3371	0.4765	0.14617	0.3871
	(0.0048)	(0.0005)	(0.0068)	(0.0070)	(0.0017)	(0.0059)	(0.0021)
All singles:							
Insured	0.1509	0.0591	0.7e-08	0.3885	0.5594	0.9985	0.0349
	(0.0001)	(0.0000)	(0.0000)	(0.0007)	(0.0027)	(0.0011)	(0.0000)
Uninsured	0.2357	0.0018	0.0684	0.4639	0.5001	0.9544	0.0166
	(0.0012)	(0.0000)	(0.0000)	(0.0050)	(0.0010)	(0.0002)	(0.0000)

Data source: MEPS panel 1 to 16 (1996 to 2012). Note: Bootstrapped standard errors in parentheses.

Parameter ρ is calibrated: $\rho = (0, 1]$.

Table 3.8 outlines the estimates for couples (for both health and non-health components) from the variance decomposition outlined in equation 3.16, of the residuals from the first-stage regression in equation 3.1.[23] Again, concentrating on the health component estimates, the variance decomposition suggests that there is very little heterogeneity

[23]The first-stage estimates of the income effect from ER visits are presented in Table 3.2; they suggest that ER visits decrease income by roughly 0.1 log points for couples.

among couples in income effects from health shocks. One possibility is that couples provide insurance to health-care shocks, in a manner similar to that found by Sopchokchai (2016). One possible mechanism is that couples coordinate their labour-supply decisions in response to health shocks. This appears to be true in households regardless of the source of the health shocks except for when shocks are from both adults and children. In this case, we find evidence of heterogeneity in health shocks, and the impact of health shocks could potentially persist with time as the parameter estimate δ is close to unity.

Next we differentiate between couple households with insurance, and those without (though we do not differentiate between private and public insurance, because of sample size issues).[24] We find no evidence of heterogeneous health impacts for insured couples, but the effects of health shocks do exhibit heterogeneity for uninsured couples—specifically, we find that the variance of health shocks is roughly one fourth of the transitory variance of non-ER shocks (or roughly equivalent to the mean impact from the first-stage regression). Our results also suggest that the residual variance from health shocks for uninsured couples is persistent. Our estimates of δ is roughly 0.69, implying that the income effects of such shocks have a half-life of around 2 years. In contrast, for insured couples we find no evidence of income effects. This suggests that the lack of heterogeneity in income effects among couples is not due to within-couple insurance.

One concern with our definition of ER visits as health shocks is that such visits may merely reflect predictable visits to a health-care provider, in lieu of a family doctor. To control for such cases, we employ a threshold cost for ER visits, on the assumption that planned events are unlikely to be costly. We choose a threshold of $500, and re-analyze the numbers for both singles and couples. We find little change in our results among single childless men. For single childless women, the variance of health shock declines to roughly 0.6 while the persistence of the variance of health shock doubles. For single

[24]We note that the first-stage regression for this specification includes an interaction term for ER visits and insurance.

TABLE 3.8: GMM Parameter Estimates: couples

	Health Component		Non-Health Component				
	δ	σ_ν	$\hat{\gamma}\beta_2$	σ_α	$\sigma_{\tilde{\varphi}}$	ρ	σ_η
ER Visits:							
Couple (1)	0.4972	0.0003	0.0066	0.3997	0.1310	0.6481	0.1623
	(0.0001)	(0.0000)	(0.0000)	(0.0002)	(0.0003)	(0.0006)	(0.0001)
Couple (2)	0.9999	0.1221	0.0061	0.2413	0.5015	0.9999	0.01891
	(0.0007)	(0.0000)	(0.0000)	(0.0000)	(0.0001)	(0.0008)	(0.0000)
Couple (3)	0.4986	1.8e-05	0.0014	0.2323	0.4294	0.9989	0.0174
	(0.0005)	(0.0000)	(0.0000)	(0.0011)	(0.0021)	(0.0001)	(0.0001)
Couple (4)	0.5050	0.0006	0.0002	0.3560	0.4352	0.6061	0.1111
	(0.0005)	(0.0000)	(0.0001)	(0.0043)	(0.0090)	(0.0001)	(0.0023)
Insured (1)	0.5016	0.0001	0.0001	0.2295	0.4219	0.9999	0.0226
	(0.0002)	(0.0141)	(0.0002)	(0.0042)	(0.0003)	(0.0139)	(0.0121)
Uninsured (1)	0.6895	0.1244	0.0096	0.5059	0.5342	0.9999	0.0152
	(0.0003)	(0.0022)	(0.0103)	(0.0011)	(0.0010)	(0.0009)	(0.0001)
ER Visits (charges $\geqslant$ $500)	0.4823	0.0003	0.0714	0.2919	0.4213	0.9971	0.0575
	(0.0014)	(0.0000)	(0.0000)	(0.0069)	(0.0001)	(0.0056)	(0.0002)

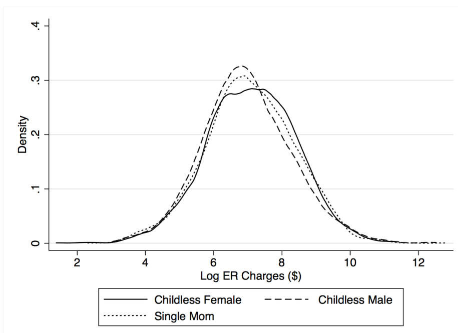
Data source: MEPS panel 1 to 16 (1996 to 2012). Note: Bootstrapped standard errors in parentheses. Specifications: (1) Shocks from adults only, (2) Shocks from adults and children, (3) Shocks from male adults only, (4) Shocks from female adults only.

mother, while the persistence of health shock is unchanged, the variance of health shock rises to about 0.07. One possible reason for this change in the health shock parameter is that single mothers might be more inclined to use the ER for non-urgent care.

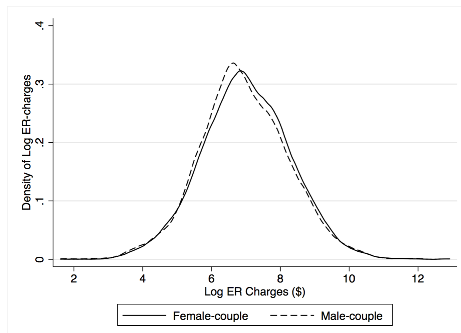
To the extent that the heterogeneity in health shocks is gender-differentiated, we examine what underlying forces might cause this gender disparity. Two possibilities are 1) that men and women receive a different quality of care in the ER; and/or 2) that men and women experience different health shocks. Although the data do not allow us to observe directly the reasons for ER visits, or the treatment received, we can use the billing (as reported by the Emergency Department) as proxies. Figure 3.5 plots the kernel densities of log-ER charges. Among singles, we do not find any major differences between men and women—though the distribution for childless females has a slightly higher mean, and is slightly more dispersed, than that for childless males. Among couples, we also plot the same graph using ER charges from the man and the woman of the household. We find that the distribution is very similar—meaning there is no evidence that the nature of ER visits differs between men and women, either for singles or couples. This implies that the gender-differentiated heterogeneity found in health shocks may not be the result of any gender-specific nature of the ER visit. However, it is possible that women have more medically severe health issues (which we cannot observe), and hence face higher treatment fees.[25]

FIGURE 3.5: Log ER Charges

(A) Singles



(B) Couples



As a second robustness check, we consider the possible confounding of latent health problems with health shock impacts. Table C.1 in Appendix C presents our results for our specification, including variances for health insurance and education to control for unobserved ability and latent health issues (as in equation 3.7). We find little evidence that either latent health or latent ability drive our estimates.

Next, since the measures of income used here include various transfers, we investigate further considering only wage income. In Appendix C, the parameter estimates can be found in Table C.2 for singles, and in Table C.3 for couples. Re-estimating the model using only wage income for the single population yields similar results as before. In general,

[25]We note that there are other channels that could give rise to this gender-differentiated heterogeneity in health shocks, for instance self-selection and discrimination.

we continue to find evidence that heterogeneity in health shocks is gender-differentiated—that single men face virtually no heterogeneity in income effects, while for women the variance of health shocks is roughly one-eighth of the transitory variance of non-ER shocks. For couples, using only wage income in the analyses returns different results. It is no longer the case that there is little heterogeneity in income effects; in fact, the variance of health shocks is approximately one-tenth of the transitory variance of non-ER shocks. This finding suggests that other sources besides wage income may contribute to alleviating the impact of health shocks for couples. When we differentiate the couple households into those with and without insurance, we find similar results as before: there is no evidence of heterogeneous health impacts for insured couples, but the effects of health shocks exhibit heterogeneity for uninsured couples. However, the magnitude of the variance of health shocks is even larger in this case. We find that the variance of health shocks is roughly similar to the transitory variance of non-ER shocks (as opposed to one-fourth when we include the transfers).

Lastly, because individuals with ER visits might be from a selected group of population, perhaps those with more hazardous lifestyles, we include a full set of interaction effects between sociodemographic characteristics and ER visits in extracting income residuals (in simple regressions). The parameter estimates from GMM estimations using these residual variances can be found in Table C.4 of Appendix C. We find no significant changes in health component parameter estimates—single women, with or without children, experience some effect of ER shocks.

3.6 Conclusion

In this chapter, we propose a method for breaking down the heterogeneity of health shock impacts for the second moment of income. Following Storesletten, Telmer, and Yaron

(2004b) and Sopchokchai (2016), we partition the residuals from cross-sectional income regressions into transitory and permanent components, and also partition the residual variances from the health shocks. We specifically focus on the second moment, to examine the extent to which such shocks propagate income inequality. Our results suggest that health shocks have heterogeneous impacts for childless single women and single mothers, but we find no such evidence for single men. This finding seems to highlight gender inequality in terms of heterogeneity of health shocks. Single women appear to encounter higher income risk due to health shocks than single men. We also show that this outcome is not driven by any gender-specific nature of ER visits, as measured by fees charged. Among single women, on average single mothers seem to face a longer impact of health shocks than childless single women. Our findings for the couple population suggest that the effects of health shocks exhibit heterogeneity for uninsured couples, but not for insured couples. A back-of-envelope calculation reveals that having health coverage could potentially reduce the effect of the negative health shocks on income volatility by approximately 10 percentage points for the couple population. These results are robust to various sources of health shocks except when shocks are from both children and adults, to different thresholds of ER charges, and to different model specifications controlling for ability bias.

This study documents the heterogeneous impacts of health shocks, and also highlights the disparity of the heterogeneity between single men and single women, and between insured and uninsured couples. However, we do not address the issue of the driving factors behind our findings; we merely note that future research is necessary to fully understand these observations.

General Discussion and Conclusion

This thesis studies health and income—two important aspects of quality of life—using a variety of econometric techniques. The first chapter, independent from the other two, focuses specifically on the health of immigrants in Canada. Previous literature on this topic finds evidence of a "Healthy Immigrant Effect" (HIE): the idea that new immigrants are healthier than the native-born population when they first arrive, but that this health advantage declines with years-since-migration. However, using a more recent data set from the Canadian Community Health Survey, I find that the HIE is no longer observable among recent arrival cohorts of immigrants. This finding is surprising given that recent immigration policies in Canada—such as the implementation of the Immigration and Refugee Protection Act (IRPA)—have brought about changes in immigrants' characteristics that correlate positively with health and long-term economic success.

For instance, immigrants from the post-IRPA regime are more likely than previous immigrants to be of working age; to have more work experience; to have a greater degree of education; and to speak Canada's official languages. These characteristics imply that recent cohorts of immigrants possess superior labour-market qualifications—which, in theory, means that their health should also be better than earlier cohorts. Given that the average health of native-born Canadians is relatively stable, we might expect the initial health gap between them and recent immigrants to be wider. However, my study finds no initial health gap between the two populations—as measured by their self-assessed health status, and their likelihood of having chronic conditions, or being overweight or obese.

In the second part of Chapter 1, I focus on the health of recent immigrants, comparing them to immigrants from previous years by using a semi-parametric matching method. In terms of their health outcomes, I find that more recent cohorts of immigrants are 7 percentage points more likely to be overweight or obese than their earlier counterparts. These findings raise important questions about the apparent disappearance of the HIE. Moreover, my results also cast doubt on the effectiveness of an immigration policy that focuses solely on education and work experience, but neglects to ask about immigrants' health quality.

In Chapters 2 and 3, I turn to examining different aspects of labour-income risk over the life cycle. Both chapters take advantage of American data from the Medical Expenditure Panel Survey (MEPS), a large and rich study that includes roughly 40,000 U.S. households. Chapter 2 documents the distinct labour-income profiles of both single men and women, and couples. Using a simple accounting tool, I find that covariation of income between spouses accounts for some 36% of total household income variation—a non-negligible figure that indicates potential risk-sharing within couples. To explore further the matter of income risk-sharing, I examine the literature on income processes. Of particular interest is the approach of Storesletten, Telmer, and Yaron (2004b), which I follow in modelling the variance of income residuals. I decompose the variance of income residuals into the usual transitory and permanent components, augmenting this general model to include covariations of permanent and transitory shocks between spouses.

As well, I check the robustness of this model by using various specifications to retrieve income residuals; by using "synthetic couples" to test data matching; and by considering an alternative model that allows for couple-specific persistence in the permanent component. My results find robust evidence of risk-sharing between spouses: the covariance of transitory shocks is approximately -0.25. This negative covariance suggests that household income volatility can be attenuated by the "marriage insurance" effect experienced by couples (both married and common-law). I find little evidence of income risk-sharing for permanent income shocks, the magnitude of that covariance is only about one-tenth of that found for transitory shocks.

In Chapter 3, I turn from income shocks to health shocks, examining their effects on the variance of residual income. The decomposition in this chapter looks not just at transitory and permanent components; it also partitions the variance of residual income into health and non-health components. I take the novel approach of using information on Emergency Room (ER) visits as proxies for the dispersion of health shocks (excluding those that result in fees charged of less than \$500, as likely to be for minor problems). This chapter also proposes an innovative method to control for some possible mis-specification biases often found in income regressions: ability bias, and latent health bias. As in the previous chapter, I separately decompose data for singles (men, women, and single mothers) and for couples. The estimated mean effects of ER visits are negative for both singles and couples, varying from -0.06 log earnings points to -0.12.

These results suggest that for singles, heterogeneity in income effects from health shocks is gender-specific. We find robust evidence of heterogeneous health impacts and income effects for single women, with or without a child; but virtually no evidence for single men. The estimate suggests the variance of health shocks is only about 10% of the transitory variance of non-health shocks for single women; and that the impact of health shocks is fairly persistent. If we extend the analysis to insured and uninsured singles, we do not find any significant differences in heterogeneous impacts between the two groups.

Although we do not explicitly test this against the conclusion from Chapter 2, our results again suggest the possibility of couple-specific insurance against health shocks: couples do not face any heterogeneity in income effects except for when health shocks are allowed to be from both adults and children of the family. We also consider couples with and without health insurance; again, our estimates indicate no heterogeneous health impacts for insured couples, but significant impacts for uninsured couples. A back-of-envelope calculation shows that for couples, having insurance coverage could potentially reduce the impact of health shocks on income volatility by about 10 percentage points. We conduct of variety of robustness checks, and we do not find any major changes in our results.

Appendix A

Chapter 1

A.1 Variable Description

Variable	Description
English or French language	This binary variable indicating individuals who report learning either English or French as their first language, and are still able to communicate in that language. The CCHS codebook terms this the "mother tongue" language.
Income adequacy decile	Total household income, adjusted for household size and area of living. The value ranges from 1 to 10, with 1 being the bottom of the income distribution, and 10 being the top.
Own residence	This binary variable takes the value of 1 if the respondent owns the residence they live in, and zero otherwise.
Household size	Number of individuals living in the household.
Number of dependents	Children under 15 years old who are financially dependent on the respondents.
Dividend	The variable takes value of 1 if the respondent receive dividend income in the last calendar year, and zero otherwise.
Married	A binary variable indicating if one is currently married or has a common-law partner.

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Variable	Description
Province of residence	A categorical variable indicating the Canadian province where the respondent lives (at the time of the interview).
Urban	This binary variable is equal to 1 if the respondent lives in an urban area, and zero otherwise
Job	This binary variable indicates the respondent's work status in the week prior to the interview. It is equal to 1 if the respondent was working at a job or a business (or was employed but absent from work), and zero otherwise.
Education	A categorical variable indicating the highest level of education obtained by the respondent (individuals who are still students, either full-time or part-time, are dropped from the analysis). The four categories are: less than high school, completed high school, some post-secondary, and post-secondary degree.
Ethnicity	This categorical variable describes the racial or cultural group that respondents identify as belonging to. The options are White, Black, Aboriginal, and Asian, defined as Chinese, Filipino, Southeast Asian, Korean, and Japanese. Respondents who identified as South Asian (Pakistani, Sri Lankan, and East Indian), Latin American, Arab, West Asian (Iranian, Afghani), and mixed are coded as "other."
Type A CC	This binary variable is equal to 1 if the respondent reports having at least one of the following conditions: asthma, back pain, high blood pressure, allergies, migraines, ulcers, bronchitis, and arthritis. Otherwise, it is equal to zero.
Type B CC	This binary variable is equal to 1 if the respondent reports having at least one of the following conditions: heart disease, cancer, thyroid disease, Crohn's disease, and diabetes. Otherwise, it is equal to zero.
OVOB	This binary variable refers to whether the respondent is overweight or obese. Derived from the BMI variable, it is equal to 1 if the BMI is greater than or equal to 25, and zero otherwise.

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Variable	**Description**
Poor SAHS	This binary variable measures Self-Assessed Health Status. Respondents are asked to rate their own health as being excellent, very good, good, fair, or poor. The variable takes the value of 1 if the respondent reports having either fair or poor health, and zero otherwise.
Immigrant	This binary variable refers to immigrant status (defined as respondents who were born outside of Canada. Those who answered No to the question "Were you born a Canadian citizen?"" are coded as immigrants.
YSM	Year-since-migration, the number of years immigrants have resided in Canada. This variable (set to equal zero for all native-born respondents) is calculated by subtracting their year of arrival from the current calendar year.

A.2 Full Regression Results: Probit Regressions

TABLE A.2: Males

	Type A CC		Type B CC		OVOB		Poor SAHS	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Province: Omit Ontario								
NFL and Labrador	-0.211**	-0.207**	0.0398	0.0376	0.250**	0.2544**	-0.0504	-0.0613
	(0.0776)	(0.078)	(0.114)	(0.114)	(0.0824)	(0.083)	(0.108)	(0.107)
PEI	0.0145	0.0201	-0.0958	-0.0973	0.0332	0.025	-0.124	-0.127
	(0.109)	(0.109)	(0.120)	(0.120)	(0.111)	(0.112)	(0.113)	(0.113)
Nova Scotia	0.0974	0.1020	0.0917	0.0287	0.0742	0.0797	0.133	0.127
	(0.0763)	(0.076)	(0.0803)	(0.081)	(0.0758)	(0.076)	(0.138)	(0.138)
New Brunswick	0.0110	0.0134	0.207*	0.2090*	-0.0008	-0.0020	0.135	0.119
	(0.0744)	(0.074)	(0.104)	(0.104)	(0.0789)	(0.078)	(0.126)	(0.127)
Quebec	-0.165**	-0.162**	-0.142**	-0.145**	-0.164**	-0.161**	-0.172*	-0.182*
	(0.0471)	(0.047)	(0.0532)	(0.053)	(0.0470)	(0.047)	(0.0725)	(0.0709)
Manitoba	0.0742	0.0707	-0.153	-0.1540	0.0611	0.0596	-0.215**	-0.207*
	(0.0791)	(0.078)	(0.111)	(0.111)	(0.0696)	(0.069)	(0.0810)	(0.0812)
Saskatchewan	0.0296	0.0340	-0.0170	-0.0154	0.161*	0.1642*	0.0177	0.0120
	(0.0742)	(0.074)	(0.0844)	(0.085)	(0.0711)	(0.071)	(0.0906)	(0.0895)
Alberta	0.0275	0.0300	-0.0576	-0.0613	0.0695	0.0730	0.0976	0.0890
	(0.0543)	(0.054)	(0.0645)	(0.065)	(0.0539)	(0.054)	(0.0738)	(0.0728)
British Columbia	-0.0282	-0.0234	-0.131*	-0.1450*	-0.0836	-0.0839†	0.0132	0.0051
	(0.0527)	(0.052)	(0.0612)	(0.061)	(0.0552)	(0.051)	(0.0779)	(0.0753)
Age and age-squared: Omit group 30-39 age group								
Age	0.110	0.1210	0.331	0.3340	0.00179	-0.0031	0.500†	0.483†
	(0.166)	(0.166)	(0.230)	(0.230)	(0.177)	(0.176)	(0.277)	(0.280)
Age-squared	-0.0011	-0.0012	-0.0039	-0.0040	0.0003	0.0004	-0.0070†	-0.0068†
	(0.0024)	(0.002)	(0.0033)	(0.0033)	(0.0025)	(0.0030)	(0.0040)	(0.0040)
Age group(20-29)*age	0.0646	0.0698	0.198*	0.2000*	-0.0596	-0.0638	0.162	0.158
	(0.0688)	(0.069)	(0.0967)	(0.0970)	(0.0686)	(0.0680)	(0.1212)	(0.1221)
Age group(40-49)*age	0.0090	0.0071	-0.0237	-0.0276	0.0195	0.0191	-0.1150†	-0.1050
	(0.0402)	(0.0400)	(0.0550)	(0.0551)	(0.0426)	(0.0412)	(0.0673)	(0.0674)
Age group(50-59)*age	-0.0343	-0.0394	-0.105	-0.1050	0.0308	0.0322	-0.190†	-0.182†
	(0.0632)	(0.063)	(0.0862)	(0.086)	(0.0669)	(0.067)	(0.104)	(0.105)
Age group(60-65)*age	-0.0311	-0.0359	-0.121	-0.1240	0.0281	0.0280	-0.191	-0.179
	(0.0780)	(0.078)	(0.105)	(0.1050)	(0.0828)	(0.083)	(0.127)	(0.128)
Age group(20-29)*age2	-0.0022	-0.0024	-0.0067*	-0.0068*	0.0021	0.0022	-0.00547	-0.0053
	(0.0024)	(0.002)	(0.0033)	(0.0033)	(0.0023)	(0.0023)	(0.0041)	(0.0041)
Age group(40-49)*age2	-0.0002	-0.0001	0.0006	0.0007	-0.00053	-0.0005	0.0029†	0.0027
	(0.0010)	(0.001)	(0.0014)	(0.001)	(0.0011)	(0.001)	(0.0017)	(0.0017)
Age group(50-59)*age2	0.0007	0.0008	0.00231	0.0023	-0.0007	-0.0007	0.0044†	0.0042†
	(0.0015)	(0.001)	(0.0020)	(0.002)	(0.0016)	(0.002)	(0.0024)	(0.0025)
Age group(60-65)*age2	0.0006	0.0007	0.0026	0.0027	-0.0006	-0.0006	0.00447	0.0041
	(0.0017)	(0.002)	(0.0023)	(0.002)	(0.0018)	(0.002)	(0.0028)	(0.0028)
Permanent income proxies								
Household size	0.0038	0.0029	-0.0387	-0.0398	0.0858**	0.0860**	0.0838*	0.0844**
	(0.0227)	(0.023)	(0.0289)	(0.0287)	(0.0229)	(0.023)	(0.0328)	(0.0326)
Number of dependent	-0.0515	-0.0517	0.0097	0.0163	-0.0470	-0.0469	-0.0731	-0.0865
	(0.0326)	(0.033)	(0.0588)	(0.0410)	(0.0335)	(0.033)	(0.0576)	(0.0537)
Highest education in the household: Omit less than high school								
High school	0.0475	0.0548	0.0620	0.0622	0.0594	0.0598	0.0493	0.0352
	(0.0911)	(0.091)	(0.110)	(0.110)	(0.0940)	(0.094)	(0.109)	(0.107)
Some PSE	-0.0520	-0.0422	0.0480	0.0527	0.0727	0.0875	-0.198	-0.231
	(0.132)	(0.131)	(0.142)	(0.140)	(0.129)	(0.127)	(0.186)	(0.186)

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Males (continued from previous page)

	Type A CC		Type B CC		OVOB		Poor SAHS	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
PSE	0.0440	0.0525	0.109	0.1140	0.169†	0.1732†	-0.0498	-0.0767
	(0.0917)	(0.092)	(0.106)	(0.104)	(0.0946)	(0.094)	(0.117)	(0.111)
Dividend	-0.0931*	-0.0897*	-0.163**	-0.164**	-0.137**	-0.136**	-0.242**	-0.246**
	(0.0401)	(0.040)	(0.0468)	(0.047)	(0.0399)	(0.040)	(0.0654)	(0.0629)
Own a house	-0.0865†	-0.0852†	-0.103*	-0.1130*	0.134**	0.1321**	-0.169**	-0.171**
	(0.0442)	(0.0450)	(0.0515)	(0.051)	(0.0436)	(0.043)	(0.0572)	(0.0574)
Socio-demographic								
Urban	-0.0580	-0.0584	0.0139	0.0161	-0.0552	-0.0545	-0.0295	-0.0317
	(0.0376)	(0.038)	(0.0424)	(0.043)	(0.0391)	(0.039)	(0.0625)	(0.0625)
Married	0.00736	0.0070	-0.0272	-0.0323	0.0405	0.0405	-0.296**	-0.284**
	(0.0452)	(0.045)	(0.0542)	(0.054)	(0.0418)	(0.041)	(0.0791)	(0.0723)
Employed	-0.292**	-0.293**	-0.297**	-0.296**	0.0698	0.0746	-0.671**	-0.681**
	(0.0473)	(0.0459)	(0.0466)	(0.0464)	(0.0469)	(0.047)	(0.0523)	(0.0510)
English or French	0.162*	0.1630*	-0.0264	-0.0208	-0.0270	-0.0290	0.206†	0.204*
	(0.0783)	(0.077)	(0.0861)	(0.084)	(0.0748)	(0.074)	(0.105)	(0.101)
Education:Omit post-secondary education (PSE)								
Less than high school	0.160*	0.166*	0.157†	0.1560†	0.147*	0.1530*	0.373**	0.353**
	(0.0698)	(0.070)	(0.0891)	(0.087)	(0.0742)	(0.074)	(0.0983)	(0.0913)
High school	-0.0637	-0.0630	-0.0608	-0.0669	-0.0141	-0.0091	0.0353	0.0356
	(0.0565)	(0.056)	(0.0744)	(0.075)	(0.0593)	(0.059)	(0.0730)	(0.0724)
Some PSE	0.175†	0.1750†	0.283**	0.2780**	-0.00994	-0.0171	0.334†	0.344†
	(0.0990)	(0.099)	(0.106)	(0.104)	(0.0955)	(0.095)	(0.189)	(0.190)
Cycle: Omit 2012								
Cycle	0.0039	0.0192	0.0409	0.0184	-0.0816*	-0.0719*	0.0251	0.0027
	(0.0333)	(0.035)	(0.0416)	(0.042)	(0.0342)	(0.036)	(0.0464)	(0.0460)
Ethnicity: Omit White								
Black	0.0590	0.0666	-0.0631	-0.1260	-0.167	-0.1534	-0.263	-0.210
	(0.177)	(0.180)	(0.204)	(0.199)	(0.175)	(0.175)	(0.280)	(0.263)
Asian	-0.0563	-0.0446	0.176	0.1580	-0.531**	-0.546**	0.132	0.116
	(0.0947)	(0.093)	(0.113)	(0.109)	(0.0883)	(0.087)	(0.129)	(0.132)
Aboriginal	0.271**	0.2740**	0.194†	0.1930†	0.233**	0.2279**	0.361**	0.343**
	(0.0709)	(0.080)	(0.100)	(0.0779)	(0.0747)	(0.077)	(0.114)	(0.105)
Other ethnics	0.0740	0.0775	-0.0622	-0.0998	0.183	0.1766	-0.338†	-0.333†
	(0.121)	(0.120)	(0.156)	(0.150)	(0.131)	(0.130)	(0.178)	(0.181)
Immigrant variables								
Immigrant	-0.372*	-1.0200	-0.543**	0.6960	-0.0932	-0.6313	-0.138	1.606
	(0.153)	(0.967)	(0.161)	(1.065)	(0.136)	(0.897)	(0.199)	(1.313)
YSM	0.0200†	0.0824*	0.0287*	-0.0558	-0.00391	0.0240	0.0267†	-0.0597
	(0.0107)	(0.036)	(0.0116)	(0.039)	(0.00980)	(0.0332)	(0.0146)	(0.0508)
YSM-squared	-0.0003	-0.001**	-0.0004†	0.0008	0.0001	-0.0002	-0.0004†	0.0006
	(0.0002)	(0.000344)	(0.000178)	(0.000)	(0.0002)	(0.000)	(0.000254)	(0.001)
Cohort: omit before 1967								
Cohort 1967-1971		-0.6010*		0.3270		-0.345		-0.0271
		(0.303)		(0.290)		(0.249)		(0.261)
Cohort 1972-1976		-0.4310		0.4010		0.206 9		-0.299
		(0.367)		(0.362)		(0.309)		(0.328)
Cohort 1977-1981		-0.4680		0.2730		-0.1637		-0.268
		(0.442)		(0.494)		(0.392)		(0.445)
Cohort 1982-1986		-0.3370		0.0938		0.1065		0.250
		(0.502)		(0.495)		(0.432)		(0.554)
Cohort 1987-1991		-0.1870		0.3020		0.0470		-0.489
		(0.554)		(0.570)		(0.486)		(0.599)
Cohort 1992-1996		-0.1830		-0.3670		0.0598		-0.473
		(0.640)		(0.666)		(0.574)		(0.738)
Cohort 1997-2001		-0.0027		-0.3430		0.5198		-0.858

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Males (continued from previous page)

	Type A CC		Type B CC		OVOB		Poor SAHS	
	(1)	**(2)**	**(1)**	**(2)**	**(1)**	**(2)**	**(1)**	**(2)**
		(0.715)		(0.779)		(0.648)		(0.920)
Cohort 2002-2006		0.4010		-0.7160		0.4026		-2.041†
		(0.832)		(0.900)		(0.753)		(1.095)
Cohort 2007-2012		0.4980		-1.3720		0.3787		-1.449
		(0.940)		(0.999)		(0.870)		(1.276)
N	21,048	21,048	21,048	21,048	21,048	21,048	21,048	21,048
p-value cohort effects		0.0457		0.0032		0.0663		0.0014
p-value imm variables		0.0710		0.0076		0.0868		0.0021

† $p < 0.10$ * $p < 0.05$, ** $p < 0.01$,

Data source: CCHS 2007 and 2011, Note: (1) Does not include controls for cohort effects; (2) Includes control for cohort effects. Robust standard errors in parentheses. Estimations are done with CCHS weight.

TABLE A.3: Females

	Type A CC (1)	Type A CC (2)	Type B CC (1)	Type B CC (2)	OVOB (1)	OVOB (2)	Poor SAHS (1)	Poor SAHS (2)
Province: Omit Ontario								
NFL and Labrador	0.0773	0.0802	0.114	0.1160	0.314**	0.3175**	-0.0595	-0.0556
	(0.070)	(0.070)	(0.0800)	(0.080)	(0.0705)	(0.071)	(0.100)	(0.100)
PEI	-0.129	-0.1260	0.0130	0.0144	0.0644	0.0679	-0.141	-0.137
	(0.098)	(0.098)	(0.110)	(0.110)	(0.0953)	(0.095)	(0.125)	(0.125)
Nova Scotia	0.134*	0.1360*	-0.0084	-0.0066	0.183**	0.1843**	0.0043	0.00646
	(0.0667)	(0.067)	(0.0714)	(0.071)	(0.0649)	(0.065)	(0.0883)	(0.0884)
New Brunswick	0.0803	0.0841	-0.0881	-0.0854	0.159*	0.1600*	0.00531	0.00901
	(0.0651)	(0.065)	(0.0697)	(0.070)	(0.0680)	(0.068)	(0.0774)	(0.0775)
Quebec	-0.131**	-0.127**	-0.308**	-0.307**	-0.134**	-0.132**	-0.207**	-0.204**
	(0.0418)	(0.042)	(0.0544)	(0.054)	(0.0420)	(0.042)	(0.0639)	(0.0640)
Manitoba	0.0241	0.0315	0.0506	0.0583	0.200**	0.2100**	0.0220	0.0268
	(0.0748)	(0.075)	(0.104)	(0.105)	(0.0723)	(0.072)	(0.107)	(0.107)
Saskatchewan	-0.0524	-0.0497	-0.106	-0.1030	0.0808	0.0844	0.0153	0.0166
	(0.0665)	(0.067)	(0.0851)	(0.085)	(0.0692)	(0.069)	(0.0884)	(0.0884)
Alberta	-0.0276	-0.0251	-0.0878	-0.0889	0.00706	0.0146	-0.0334	-0.0303
	(0.0563)	(0.056)	(0.0569)	(0.057)	(0.0560)	(0.056)	(0.0724)	(0.0719)
British Columbia	-0.0211	-0.0174	-0.183**	-0.181**	-0.193**	-0.192**	0.0356	0.0363
	(0.0487)	(0.049)	(0.0547)	(0.055)	(0.0472)	(0.047)	(0.0749)	(0.0745)
Age and age-squared: Omit group 30-39 age group								
Age	0.173	0.1780	-0.213	-0.2080	0.173	0.1925	0.0942	0.106
	(0.148)	(0.148)	(0.203)	(0.203)	(0.146)	(0.146)	(0.227)	(0.225)
Age-squared	-0.0023	-0.0024	0.00339	0.0033	-0.00239	-0.0027	-0.00140	-0.00157
	(0.0022)	(0.0022)	(0.003)	(0.003)	(0.00213)	(0.002)	(0.00330)	(0.00328)
Age group(20-29)*age	0.0505	0.0515	-0.107	-0.1040	0.00515	0.0131	-0.0271	-0.0254
	(0.0613)	(0.061)	(0.0771)	(0.077)	(0.0617)	(0.062)	(0.0886)	(0.0877)
Age group(40-49)*age	-0.0465	-0.0474	0.0464	0.0466	-0.0341	-0.0378	-0.0316	-0.0341
	(0.0367)	(0.037)	(0.0499)	(0.050)	(0.0366)	(0.036)	(0.0555)	(0.0555)
Age group(50-59)*age	-0.0569	-0.0596	0.0760	0.0740	-0.0816	-0.0882	-0.0233	-0.0282
	(0.0568)	(0.057)	(0.0782)	(0.078)	(0.0557)	(0.056)	(0.0869)	(0.0865)
Age group(60-65)*age	-0.0709	-0.0714	0.0964	0.0969	-0.0944	-0.1027	0.0364	0.0330
	(0.0703)	(0.070)	(0.0946)	(0.094)	(0.0693)	(0.069)	(0.109)	(0.109)
Age group(20-29)*age2	-0.0018	-0.0018	0.0035	0.0035	-0.0003	-0.0005	0.0007	0.0006
	(0.0021)	(0.002)	(0.0026)	(0.003)	(0.0021)	(0.002)	(0.0030)	(0.0030)
Age group(40-49)*age2	0.0012	0.0012	-0.0013	-0.0013	0.0008	0.0010	0.0009	0.0010
	(0.0009)	(0.001)	(0.0013)	(0.001)	(0.0009)	(0.001)	(0.0014)	(0.00141)
Age group(50-59)*age2	0.0014	0.0015	-0.0018	-0.0018	0.0018	0.0020	0.0007	0.0008
	(0.0013)	(0.001)	(0.0018)	(0.0018)	(0.0013)	(0.001)	(0.0020)	(0.0023)
Age group(60-65)*age2	0.0017	0.0017	-0.0022	-0.0022	0.0020	0.0022	-0.0002	-0.0002
	(0.0016)	(0.002)	(0.0021)	(0.002)	(0.0015)	(0.002)	(0.0024)	(0.0026)
Permanent income proxies								
Household size	0.0003	-0.0004	0.0477†	0.0488†	0.0622**	0.0657**	0.0691*	0.0639*
	(0.0198)	(0.020)	(0.0268)	(0.0307)	(0.0202)	(0.020)	(0.0307)	(0.0301)
Number of dependent	-0.0529†	-0.0521†	-0.0658†	-0.0669†	-0.0528†	-0.0588*	-0.179**	-0.174**
	(0.0286)	(0.0286)	(0.0388)	(0.039)	(0.0296)	(0.029)	(0.0457)	(0.0460)
Highest education in the household: Omit less than high school								
High school	0.0969	0.1010	-0.0502	-0.0383	0.105	0.0971	0.0463	0.0502
	(0.0956)	(0.096)	(0.106)	(0.105)	(0.0943)	(0.093)	(0.107)	(0.105)
Some PSE	0.167	0.1770	-0.0289	-0.0070	0.0528	0.0444	-0.106	-0.101
	(0.127)	(0.127)	(0.153)	(0.152)	(0.123)	(0.122)	(0.150)	(0.149)
PSE	0.0945	0.1040	-0.100	-0.0869	0.0305	0.0195	0.0877	0.101
	(0.100)	(0.095)	(0.102)	(0.101)	(0.0926)	(0.091)	(0.107)	(0.105)
Dividend	-0.0353	-0.0370	-0.0650	-0.146**	-0.230**	-0.231**	-0.267**	-0.264**
	(0.0406)	(0.041)	(0.0454)	(0.045)	(0.0386)	(0.0402)	(0.0757)	(0.0729)
Own a house	-0.172**	-0.176**	-0.185**	-0.187**	-0.157**	-0.165**	-0.384**	-0.386**

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Females (continued from previous page)

	Type A CC		Type B CC		OVOB		Poor SAHS	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
	(0.0394)	(0.040)	(0.0498)	(0.0498)	(0.0452)	(0.0398)	(0.0584)	(0.0588)
Socio-demographic								
Urban	-0.0193	-0.0197	-0.0187	-0.0191	-0.0245	-0.0278	0.0006	0.0004
	(0.0333)	(0.033)	(0.0387)	(0.039)	(0.0328)	(0.033)	(0.0444)	(0.0441)
Married	-0.098**	-0.099**	-0.0412	-0.0442	0.0191	0.0178	-0.249**	-0.247**
	(0.0369)	(0.0367)	(0.0469)	(0.047)	(0.0377)	(0.038)	(0.0557)	(0.0551)
Employed	-0.166**	-0.167**	-0.252**	-0.250**	-0.0303	-0.0283	-0.613**	-0.615**
	(0.0354)	(0.035)	(0.0403)	(0.040)	(0.0346)	(0.035)	(0.0481)	(0.0480)
English or French	0.0924	0.0917	0.130	0.1290	-0.0053	0.0021	-0.0745	-0.0775
	(0.0648)	(0.065)	(0.0916)	(0.090)	(0.0698)	(0.069)	(0.107)	(0.107)
Education:Omit post-secondary education (PSE)								
Less than high school	0.241**	0.2410**	0.0703	0.0732	0.254**	0.2480**	0.467**	0.472**
	(0.0745)	(0.0750)	(0.0814)	(0.081)	(0.0739)	(0.074)	(0.0870)	(0.0859)
High school	-0.00519	-0.0013	-0.0455	-0.0420	0.0607	0.0600	0.167*	0.174*
	(0.0549)	(0.055)	(0.0662)	(0.066)	(0.0563)	(0.056)	(0.0802)	(0.0793)
Some PSE	0.0258	0.0210	0.0926	0.0842	0.199*	0.2010*	0.365**	0.364**
	(0.0916)	(0.091)	(0.120)	(0.120)	(0.0873)	(0.087)	(0.118)	(0.118)
Cycle: Omit 2012								
Cycle	0.0363	0.0022	-0.0322	-0.0431	-0.0572†	-0.0634*	-0.0399	-0.0797†
	(0.0313)	(0.032)	(0.0377)	(0.038)	(0.0308)	(0.031)	(0.0450)	(0.0463)
Ethnicity: Omit White								
Black	0.0897	0.1020	-0.166	-0.1680	0.398*	0.4290**	-0.00908	0.0127
	(0.154)	(0.148)	(0.165)	(0.156)	(0.162)	(0.152)	(0.163)	(0.159)
Asian	-0.174*	-0.1670*	-0.263**	-0.2500*	-0.311**	-0.315**	-0.0452	-0.0335
	(0.0813)	(0.082)	(0.101)	(0.101)	(0.0867)	(0.086)	(0.128)	(0.128)
Aboriginal	0.219**	0.2180**	0.209**	0.2100**	0.157*	0.155*	0.268**	0.267**
	(0.0760)	(0.076)	(0.0744)	(0.074)	(0.0695)	(0.069)	(0.0809)	(0.0808)
Other ethnics	0.251*	0.2680*	0.0691	0.0886	0.162	0.174	0.217	0.237
	(0.110)	(0.109)	(0.143)	(0.145)	(0.109)	(0.108)	(0.202)	(0.205)
Immigrant variables								
Immigrant	-0.331**	1.4350†	-0.107	0.470	-0.233†	-0.454**	0.0246	1.834†
	(0.122)	(0.7940)	(0.179)	(1.010)	(0.120)	(0.805)	(0.207)	(0.990)
YSM	0.0100	-0.0364	0.00604	0.0173	0.0257**	0.0452	-0.00261	-0.0600
	(0.00912)	(0.030)	(0.0111)	(0.039)	(0.00861)	(0.0294)	(0.0152)	(0.0385)
YSM-squared	-0.0001	0.0002	-0.0001	-0.0005	-0.0003*	-0.001*	0.0001	0.0006
	(0.0002)	(0.000)	(0.0002)	(0.0004)	(0.0002)	(0.000)	(0.0003)	(0.0050)
Cohort: omit before 1967								
Cohort 1967-1971		-0.2220		-0.1960		-0.132		-0.146
		(0.256)		(0.220)		(0.251)		(0.238)
Cohort 1972-1976		-0.4410		-0.5360†		-0.264		-0.277
		(0.279)		(0.292)		(0.266)		(0.305)
Cohort 1977-1981		-0.3780		-0.3411		-0.530		-0.300
		(0.336)		(0.331)		(0.326)		(0.370)
Cohort 1982-1986		-0.9340*		-0.8832*		-0.201		-1.091*
		(0.383)		(0.390)		(0.380)		(0.436)
Cohort 1987-1991		-0.8780*		-0.6291		-0.287		-0.794
		(0.436)		(0.469)		(0.437)		(0.499)
Cohort 1992-1996		-1.2640*		-0.8052		-0.491		-1.081†
		(0.507)		(0.563)		(0.512)		(0.599)
Cohort 1997-2001		-1.2270*		-0.7470		-0.0538		-1.140
		(0.582)		(0.694)		(0.576)		(0.728)
Cohort 2002-2006		-1.4570*		-0.4898		0.0683		-1.517†
		(0.678)		(0.871)		(0.675)		(0.852)
Cohort 2007-2012		-1.7830*		-0.6014		-0.371		-1.813*
		(0.750)		(0.942)		(0.753)		(0.891)
N	24,057	24,057	24,057	24,057	24,057	24,057	24,057	24,057

Continued on next page

Females (continued from previous page)

	Type A CC		Type B CC		OVOB		Poor SAHS	
	(1)	**(2)**	**(1)**	**(2)**	**(1)**	**(2)**	**(1)**	**(2)**
p-value cohort effects		0.0181		0.1926		0.0001		0.1459
p-value imm variables		0.0710		0.2445		0.0002		0.0743

† $p < 0.10$ * $p < 0.05$, ** $p < 0.01$

Data source: CCHS 2007 and 2011, Note: (1) Does not include controls for cohort effects; (2) Includes control for cohort effects. Robust standard errors in parentheses. Estimations are done with CCHS weight.

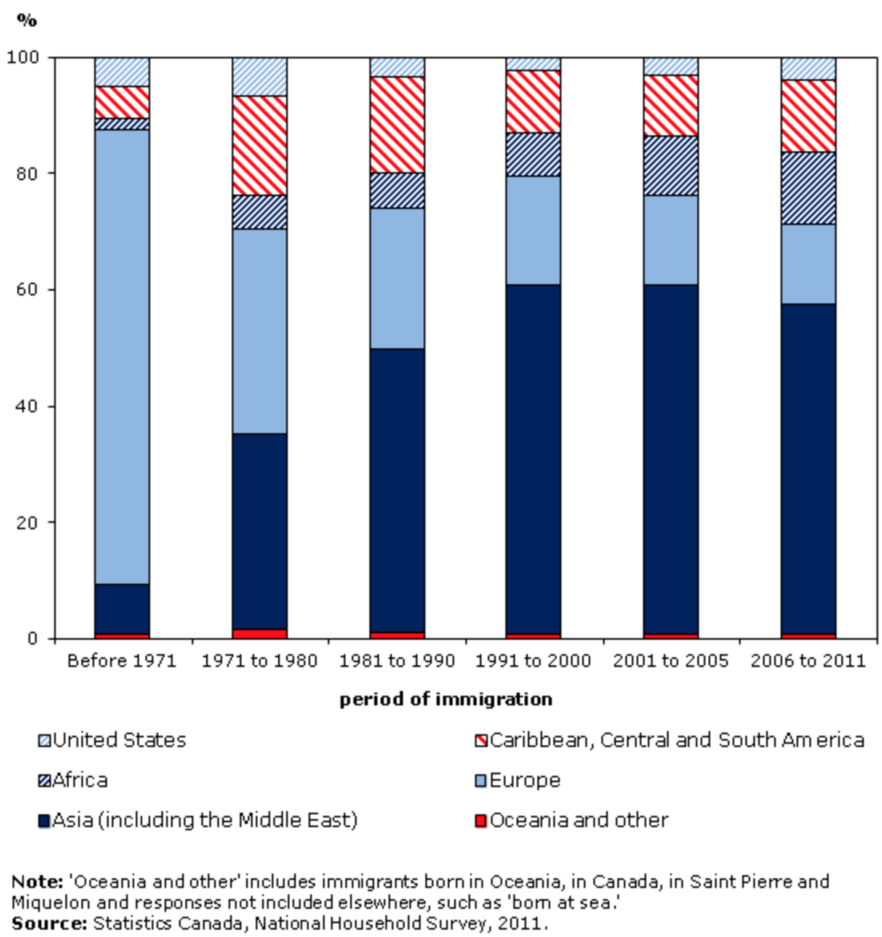
A.3 Immigrants and Source Regions

TABLE A.4: Immigrants by Source Regions, in %

	Year											
Region	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
Africa and Middle East	17.96	19.2	20.13	19.7	21	17.6	19.5	19.8	20	21.8	23.1	23.3
Asia and Pacific	53.03	53.01	51.9	51.4	48.4	54	51.8	49.1	49.6	48.6	48.6	50.3
S. and C. America*	7.45	8.04	8..48	9.2	9.4	9.4	9.8	11	10.7	10.7	10.3	11.2
U.S.	2.56	2.35	2.31	2.7	3.2	3.2	3.8	4	4.1	3.6	2.9	3.1
Europe and UK	18.87	17.26	16.95	17	17.8	15.5	14.7	15.8	15.2	15.2	13.1	11.9
others	0.14	0.13	0.23	0	0.2	0.3	0.4	0.3	0.3	0.2	0.3	0.2
Total	100	100	100	100	100	100	100	100	100	100	100	100

Data source: CCHS 2000/01 - 2011, * South and Central America.

FIGURE A.1: Region of Birth of Immigrants by Period of Immigration, Canada, 2011



Note: Image adapted from Statistics Canada (2011b).

A.4 Probit Regressions using Various Cycles of CCHS

TABLE A.5: Selected Coefficients from Various CCHS Cyles, Controlling for Cohort Effects

CCHS	2000-12		2000-06		2007-12	
	Males	Females	Males	Females	Males	Females
Type A CC						
Immigrant	-0.783**	-0.471*	-0.841*	-0.729*	-1.233*	0.341
	(0.194)	(0.185)	(0.332)	(0.308)	(0.549)	(0.544)
YSM	0.039**	0.027**	0.0359*	0.0368**	0.0644**	-0.001
	(0.008)	(0.007)	(0.015)	(0.013)	(0.020)	(0.020)
YSM-squared	-0.000**	-0.000**	-0.000*	-0.000**	-0.001**	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Type B CC						
Immigrant	-0.838**	-0.395	-0.243	-0.161	-0.0421	-0.494
	(0.284)	(0.283)	(0.502)	(0.467)	(0.714)	(0.739)
YSM	0.0352***	0.016	0.008	0.005	-0.007	0.020
	(0.012)	(0.012)	(0.022)	(0.020)	(0.027)	(0.028)
YSM-squared	-0.000*	-0.000	-0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
OVOB						
Immigrant	-0.333	-0.455*	-0.584†	0.026	0.107	-0.895
	(0.184)	(0.194)	(0.313)	(0.317)	(0.531)	(0.562)
YSM	0.014	0.027**	0.032*	0.007	0.007	0.036
	(0.008)	(0.008)	(0.014)	(0.013)	(0.020)	(0.020)
YSM-squared	-0.000	-0.000**	-0.000**	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Poor SAHS						
Immigrant	-0.661*	0.181	-1.107*	-0.737†	-0.047	0.783
	(0.289)	(0.238)	(0.504)	(0.404)	(0.856)	(0.767)
YSM	0.049**	0.003	0.068**	0.043*	0.019	-0.025
	(0.012)	(0.011)	(0.022)	(0.017)	(0.031)	(0.030)
YSM-squared	-0.000**	-0.000	-0.001**	-0.001**	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	171,080	186,265	105,310	111,550	65,770	74,715

† $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Data source: CCHS 2000 and 2012, Note: All estimations include controls for cohort effects. Robust standard errors in parentheses. Estimations are done with CCHS weight.

A.5 Predicted Health Status from Probit Regressions

TABLE A.6: Predicted Health Status of Type A Chronic Condition (Type A CC) for Immigrant Men by YSM and Period of Arrival in Canada

Incidence of Type A CC	**Year-Since-Migration**					
Cohort	0	1	2	3	4	5
1967 - 1971	-0.199	-0.205	-0.212	-0.218	-0.223	-0.228
	(0.244)	(0.254)	(0.264)	(0.272)	(0.278)	(0.283)
1972- 1976	-0.212	-0.218	-0.224	-0.228	-0.233	-0.236
	(0.266)	(0.273)	(0.280)	(0.284)	(0.286)	(0.286)
1977 - 1981	-0.210	-0.216	-0.221	-0.227	-0.231	-0.235
	(0.267)	(0.275)	(0.282)	(0.286)	(0.289)	(0.290)
1982 - 1986	-0.219	-0.224	-0.229	-0.233	-0.237	-0.239
	(0.282)	(0.287)	(0.290)	(0.291)	(0.290)	(0.287)
1987 - 1991	-0.229	-0.233	-0.236	-0.239	-0.241	-0.242
	(0.291)	(0.292)	(0.291)	(0.288)	(0.282)	(0.274)
1992- 1996	-0.229	-0.233	-0.236	-0.239	-0.241	-0.242
	(0.296)	(0.296)	(0.295)	(0.290)	(0.284)	(0.275)
1997- 2001	-0.236	-0.239	-0.241	-0.242	-0.242	-0.241
	(0.294)	(0.289)	(0.282)	(0.272)	(0.261)	(0.247)
2002 - 2006	-0.241	-0.239	-0.237	-0.234	-0.230	-0.225
	(0.249)	(0.234)	(0.217)	(0.199)	(0.180)	(0.161)
2007 - 2011	-0.239	-0.237	-0.233	-0.229	-0.224	-0.218
	(0.230)	(0.213)	(0.194)	(0.175)	(0.155)	(0.134)

Predicted health status of Type A CC for base-line native-born men is 0.600 with robust standard errors (0.071).

Data source: CCHS 2000/01 - 2011. Robust standard errors are in parentheses.
As in McDonald and Kennedy (2004), predictions are for a baseline individual who is 45-years-old, lives in Ontario outside of a CMA, speaks English or French as a mother tongue, has a high-school education, and does not receive dividend income.

TABLE A.7: Predicted Health Status of Type B Chronic Condition (Type B CC) for Immigrant Men by YSM and Period of Arrival in Canada

Incidence of Type B CC	Year-Since-Migration					
Cohort	0	1	2	3	4	5
1967 - 1971	0.067	0.066	0.066	0.065	0.064	0.064
	(0.115)	(0.114)	(0.113)	(0.112)	(0.111)	(0.110)
1972- 1976	0.067	0.067	0.066	0.066	0.065	0.064
	(0.115)	(0.114)	(0.113)	(0.112)	(0.111)	(0.110)
1977 - 1981	0.066	0.066	0.065	0.065	0.064	0.063
	(0.113)	(0.112)	(0.111)	(0.110)	(0.109)	(0.108)
1982 - 1986	0.064	0.064	0.063	0.063	0.062	0.062
	(0.110)	(0.110)	(0.109)	(0.108)	(0.107)	(0.106)
1987 - 1991	0.066	0.066	0.065	0.065	0.064	0.064
	(0.111)	(0.110)	(0.109)	(0.108)	(0.108)	(0.107)
1992- 1996	0.061	0.060	0.060	0.060	0.059	0.059
	(0.105)	(0.104)	(0.103)	(0.102)	(0.101)	(0.100)
1997- 2001	0.060	0.060	0.060	0.059	0.059	0.059
	(0.102)	(0.102)	(0.101)	(0.100)	(0.100)	(0.099)
2002 - 2006	0.058	0.057	0.057	0.057	0.056	0.056
	(0.097)	(0.096)	(0.096)	(0.095)	(0.094)	(0.093)
2007 - 2011	0.054	0.054	0.054	0.054	0.053	0.053
	(0.087)	(0.086)	(0.084)	(0.083)	(0.081)	(0.079)

Predicted health status of Type B CC for base-line native-born men is 0.384 with robust standard errors (0.084).

Data source: CCHS 2000/01 - 2011. Robust standard errors are in parentheses.
As in McDonald and Kennedy (2004), predictions are for a baseline individual who is 45-years-old, lives in Ontario outside of a CMA, speaks English or French as a mother tongue, has a high-school education, and does not receive dividend income.

TABLE A.8: Predicted Health Status of Overweight/Obese for Immigrant Men by YSM and Period of Arrival in Canada

Incidence of Overweight/Obese	**Year-Since-Migration**					
Cohort	0	1	2	3	4	5
1967 - 1971	-0.206	-0.206	-0.207	-0.207	-0.208	-0.208
	(0.285)	(0.286)	(0.288)	(0.289)	(0.289)	(0.289)
1972- 1976	-0.197	-0.196	-0.195	-0.193	-0.192	-0.190
	(0.275)	(0.273)	(0.269)	(0.266)	(0.263)	(0.259)
1977 - 1981	-0.208	-0.208	-0.208	-0.207	-0.207	-0.207
	(0.288)	(0.288)	(0.287)	(0.286)	(0.284)	(0.283)
1982 - 1986	-0.202	-0.201	-0.200	-0.199	-0.198	-0.196
	(0.275)	(0.273)	(0.270)	(0.267)	(0.264)	(0.260)
1987 - 1991	-0.204	-0.203	-0.202	-0.201	-0.200	-0.199
	(0.276)	(0.273)	(0.270)	(0.267)	(0.264)	(0.261)
1992- 1996	-0.203	-0.203	-0.202	-0.201	-0.200	-0.199
	(0.271)	(0.268)	(0.265)	(0.262)	(0.258)	(0.254)
1997- 2001	-0.177	-0.175	-0.173	-0.171	-0.170	-0.168
	(0.220)	(0.215)	(0.210)	(0.204)	(0.199)	(0.194)
2002 - 2006	-0.186	-0.184	-0.182	-0.181	-0.179	-0.177
	(0.226)	(0.222)	(0.217)	(0.212)	(0.207)	(0.202)
2007 - 2011	-0.187	-0.185	-0.184	-0.182	-0.180	-0.178
	(0.221)	(0.217)	(0.212)	(0.207)	(0.201)	(0.196)

Predicted health status of Overweight/Obese for base-line native-born men is 0.595 with robust standard errors (0.135).

Data source: CCHS 2000/01 - 2011. Robust standard errors are in parentheses.
As in McDonald and Kennedy (2004), predictions are for a baseline individual who is 45-years-old, lives in Ontario outside of a CMA, speaks English or French as a mother tongue, has a high-school education, and does not receive dividend income.

TABLE A.9: Predicted Health Status of Poor/Fair Health for Immigrant Men by YSM and Period of Arrival in Canada

Incidence of Poor/Fair Health	Year-Since-Migration					
Cohort	0	1	2	3	4	5
1967 - 1971	0.102	0.101	0.100	0.098	0.097	0.096
	(0.101)	(0.099)	(0.097)	(0.095)	(0.093)	(0.091)
1972- 1976	0.097	0.095	0.094	0.093	0.092	0.091
	(0.095)	(0.093)	(0.091)	(0.089)	(0.088)	(0.086)
1977 - 1981	0.097	0.096	0.095	0.093	0.092	0.091
	(0.093)	(0.091)	(0.089)	(0.088)	(0.086)	(0.085)
1982 - 1986	0.109	0.107	0.106	0.105	0.103	0.102
	(0.102)	(0.100)	(0.097)	(0.095)	(0.093)	(0.091)
1987 - 1991	0.093	0.092	0.090	0.089	0.088	0.087
	(0.086)	(0.085)	(0.084)	(0.082)	(0.081)	(0.080)
1992- 1996	0.094	0.093	0.091	0.090	0.089	0.088
	(0.085)	(0.084)	(0.082)	(0.081)	(0.080)	(0.080)
1997- 2001	0.087	0.086	0.085	0.084	0.083	0.083
	(0.079)	(0.078)	(0.077)	(0.077)	(0.077)	(0.077)
2002 - 2006	0.075	0.074	0.074	0.074	0.074	0.074
	(0.080)	(0.080)	(0.080)	(0.080)	(0.080)	(0.080)
2007 - 2011	0.079	0.078	0.078	0.077	0.077	0.076
	(0.076)	(0.076)	(0.076)	(0.076)	(0.077)	(0.077)

Predicted health status of Poor/Fair Health for base-line native-born men is 0.403 with robust standard errors (0.052).

Data source: CCHS 2000/01 - 2011. Robust standard errors are in parentheses.
As in McDonald and Kennedy (2004), predictions are for a baseline individual who is 45-years-old, lives in Ontario outside of a CMA, speaks English or French as a mother tongue, has a high-school education, and does not receive dividend income.

TABLE A.10: Predicted Health Status of Type A Chronic Condition (Type A CC) for Immigrant Women by YSM and Period of Arrival in Canada

Incidence of Type A CC	Year-Since-Migration					
Cohort	0	1	2	3	4	5
1967 - 1971	0.311	0.310	0.308	0.306	0.304	0.301
	(0.235)	(0.235)	(0.235)	(0.234)	(0.233)	(0.232)
1972- 1976	0.298	0.296	0.294	0.291	0.288	0.286
	(0.232)	(0.231)	(0.229)	(0.227)	(0.225)	(0.222)
1977 - 1981	0.302	0.300	0.298	0.295	0.293	0.290
	(0.232)	(0.231)	(0.229)	(0.228)	(0.226)	(0.223)
1982 - 1986	0.259	0.256	0.253	0.250	0.246	0.243
	(0.206)	(0.202)	(0.197)	(0.193)	(0.189)	(0.184)
1987 - 1991	0.264	0.261	0.258	0.255	0.252	0.248
	(0.203)	(0.200)	(0.196)	(0.192)	(0.188)	(0.183)
1992- 1996	0.230	0.227	0.224	0.220	0.217	0.214
	(0.169)	(0.164)	(0.159)	(0.155)	(0.150)	(0.145)
1997- 2001	0.232	0.229	0.226	0.222	0.219	0.216
	(0.166)	(0.162)	(0.157)	(0.153)	(0.148)	(0.144)
2002 - 2006	0.209	0.206	0.203	0.200	0.197	0.193
	(0.138)	(0.134)	(0.129)	(0.125)	(0.120)	(0.116)
2007 - 2011	0.182	0.179	0.176	0.173	0.170	0.167
	(0.107)	(0.103)	(0.099)	(0.095)	(0.092)	(0.088)

Predicted health status of type A CC for base-line native-born women is 0.485 with robust standard errors (0.034).

Data source: CCHS 2000/01 - 2011. Robust standard errors are in parentheses.
As in McDonald and Kennedy (2004), predictions are for a baseline individual who is 45-years-old, lives in Ontario outside of a CMA, speaks English or French as a mother tongue, has a high-school education, and does not receive dividend income.

TABLE A.11: Predicted Health Status of Type B Chronic Condition (Type B CC) for Immigrant Women by YSM and Period of Arrival in Canada

Incidence of Type B CC	**Year-Since-Migration**					
Cohort	0	1	2	3	4	5
1967 - 1971	0.075	0.075	0.076	0.076	0.076	0.076
	(0.171)	(0.171)	(0.171)	(0.171)	(0.171)	(0.171)
1972- 1976	0.073	0.073	0.073	0.074	0.074	0.074
	(0.163)	(0.163)	(0.163)	(0.164)	(0.164)	(0.164)
1977 - 1981	0.075	0.075	0.075	0.075	0.075	0.075
	(0.168)	(0.168)	(0.168)	(0.168)	(0.169)	(0.169)
1982 - 1986	0.067	0.068	0.068	0.068	0.069	0.069
	(0.147)	(0.147)	(0.147)	(0.147)	(0.148)	(0.148)
1987 - 1991	0.072	0.072	0.072	0.073	0.073	0.073
	(0.157)	(0.157)	(0.158)	(0.158)	(0.158)	(0.159)
1992- 1996	0.069	0.069	0.070	0.070	0.070	0.071
	(0.147)	(0.147)	(0.147)	(0.148)	(0.148)	(0.148)
1997- 2001	0.070	0.070	0.070	0.071	0.071	0.071
	(0.147)	(0.147)	(0.147)	(0.148)	(0.148)	(0.149)
2002 - 2006	0.074	0.074	0.074	0.074	0.074	0.074
	(0.157)	(0.158)	(0.158)	(0.159)	(0.159)	(0.160)
2007 - 2011	0.072	0.072	0.073	0.073	0.073	0.073
	(0.151)	(0.151)	(0.152)	(0.152)	(0.153)	(0.153)

Predicted health status of type B CC for base-line native-born women is 0.310 with robust standard errors (0.157).

Data source: CCHS 2000/01 - 2011. Robust standard errors are in parentheses.
As in McDonald and Kennedy (2004), predictions are for a baseline individual who is 45-years-old, lives in Ontario outside of a CMA, speaks English or French as a mother tongue, has a high-school education, and does not receive dividend income.

TABLE A.12: Predicted Health Status of Overweight/Obese for Immigrant Women by YSM and Period of Arrival in Canada

Incidence of Overweight/Obese	Year-Since-Migration					
Cohort	0	1	2	3	4	5
1967 - 1971	-0.072	-0.073	-0.074	-0.075	-0.076	-0.077
	(0.154)	(0.156)	(0.159)	(0.161)	(0.163)	(0.164)
1972- 1976	-0.069	-0.070	-0.071	-0.073	-0.074	-0.075
	(0.150)	(0.152)	(0.155)	(0.157)	(0.159)	(0.161)
1977 - 1981	-0.063	-0.064	-0.065	-0.066	-0.067	-0.068
	(0.139)	(0.142)	(0.144)	(0.147)	(0.150)	(0.152)
1982 - 1986	-0.071	-0.072	-0.073	-0.074	-0.075	-0.076
	(0.155)	(0.158)	(0.160)	(0.162)	(0.164)	(0.166)
1987 - 1991	-0.069	-0.070	-0.071	-0.072	-0.073	-0.074
	(0.154)	(0.156)	(0.159)	(0.161)	(0.163)	(0.165)
1992- 1996	-0.064	-0.065	-0.066	-0.067	-0.068	-0.069
	(0.146)	(0.149)	(0.152)	(0.155)	(0.157)	(0.160)
1997- 2001	-0.074	-0.075	-0.076	-0.077	-0.078	-0.079
	(0.166)	(0.168)	(0.170)	(0.171)	(0.173)	(0.174)
2002 - 2006	-0.077	-0.078	-0.078	-0.079	-0.080	-0.080
	(0.172)	(0.173)	(0.174)	(0.175)	(0.176)	(0.176)
2007 - 2011	-0.067	-0.068	-0.069	-0.070	-0.071	-0.072
	(0.158)	(0.161)	(0.163)	(0.166)	(0.168)	(0.170)

Predicted health status of being Overweigh/Obese for base-line native-born women is 0.564 with robust standard errors (0.087).

Data source: CCHS 2000/01 - 2011. Robust standard errors are in parentheses.
As in McDonald and Kennedy (2004), predictions are for a baseline individual who is 45-years-old, lives in Ontario outside of a CMA, speaks English or French as a mother tongue, has a high-school education, and does not receive dividend income.

TABLE A.13: Predicted Health Status of Poor/Fair Health for Immigrant Women by YSM and Period of Arrival in Canada

Incidence of Poor/Fair health	**Year-Since-Migration**					
Cohort	0	1	2	3	4	5
1967 - 1971	0.376	0.366	0.356	0.345	0.334	0.322
	(0.188)	(0.194)	(0.208)	(0.228)	(0.251)	(0.276)
1972- 1976	0.353	0.342	0.331	0.320	0.309	0.297
	(0.212)	(0.232)	(0.256)	(0.282)	(0.307)	(0.331)
1977 - 1981	0.349	0.338	0.327	0.315	0.304	0.292
	(0.210)	(0.234)	(0.259)	(0.285)	(0.311)	(0.335)
1982 - 1986	0.203	0.192	0.182	0.172	0.162	0.153
	(0.447)	(0.446)	(0.443)	(0.437)	(0.430)	(0.420)
1987 - 1991	0.257	0.246	0.235	0.224	0.213	0.203
	(0.395)	(0.408)	(0.418)	(0.426)	(0.430)	(0.433)
1992- 1996	0.207	0.197	0.187	0.177	0.167	0.158
	(0.430)	(0.430)	(0.428)	(0.423)	(0.417)	(0.409)
1997- 2001	0.197	0.187	0.177	0.168	0.158	0.149
	(0.425)	(0.423)	(0.419)	(0.413)	(0.406)	(0.396)
2002 - 2006	0.141	0.132	0.125	0.117	0.110	0.103
	(0.361)	(0.351)	(0.339)	(0.327)	(0.314)	(0.301)
2007 - 2011	0.102	0.095	0.089	0.083	0.077	0.072
	(0.307)	(0.293)	(0.279)	(0.264)	(0.250)	(0.236)

Predicted health status of being Overweigh/Obese for base-line native-born women is 0.153 with robust standard errors (0.416).

Data source: CCHS 2000/01 - 2011. Robust standard errors are in parentheses.
As in McDonald and Kennedy (2004), predictions are for a baseline individual who is 45-years-old, lives in Ontario outside of a CMA, speaks English or French as a mother tongue, has a high-school education, and does not receive dividend income.

A.6 Income Analysis

Tables A.14 and A.15 show selected coefficients from linear and non-linear estimations for males and females, respectively. The three specifications are: 1) include no income variable; 2) include only proxies for income; and 3) include only income variables.

All estimations in this section are performed solely to explore the effect of income on the three coefficients of interest: immigrant, YSM, and YSM-squared. These estimates should not be used to interpret or compare with other results from elsewhere in this study. Moreover, the coefficients from specification (2) are slightly different from those in Table A.2 and Table A.3. This is due to the fact that Stata only allows the use of frequency weight in calculating many measures of model fit—that is, percent correctly predicted, and the receiver operating characteristic (ROC) curves. I convert probability weight to frequency weight, as suggested by Samuel.[1]. The weight provided in CCHS is a probability weight, and thus all other estimations are done in the same way.

Males

Type A CC: The immigrant coefficient appears to be sensitive to income specification in the linear model—the sign of the estimate is negative in specification 2, but is positive in specifications 1 and 3. Nonetheless, theses estimates are all statistically not different from zero. The estimates for YSM and YSM2 are consistent across model specifications. For the non-linear model, estimates of immigrant, YSM, and YSM2 are consistent across the three specifications.

Type B CC: For the linear model, there is some discrepancy in the direction of YSM and YSM2. However, they are all statistically not different from zero. For the non-linear model, the estimates of immigrant, YSM, and YSM2 are consistent across model specifications.

[1] http://www.stata.com/statalist/archive/2009-01/msg00842.html

Overweight/obese: The results are similar across specifications in the linear model. The coefficients are statistically significant, except for YSM2 which is significant at 90% confidence level in specification 2. The non-linear models yield the same result as the linear models.

Poor/fair health: There is very little change across specifications for the linear model, except that the sign of YSM2 changes to positive in specification 2. Nonetheless, they are all not statistically significant. For the non-linear model, the sign of YSM2 changes; but, again, the coefficient is statistically insignificant. Another important change is that the immigrant coefficient becomes significant at 90% confidence level in specification 2, while insignificant in the rest.

Females

The results are consistent across specifications for both linear and non-linear models in all four health outcomes. I also calculated the percent that was correctly predicted across specifications of non-linear models, for each outcome and each gender. There is not much variation in that percent across model specifications, any changes observed being less than 1 percentage point. Lastly, I plot ROC curves for each health outcome for both males and females, to assess the model's predictive power across specifications.[2] I do not find any significant changes.

Next, I calculate the percent-correctly-predicted across specifications of non-linear model. There is only small variation in percent-correctly-predicted across these specifications— less than 1 percentage point differences. Lastly, I plot ROC curves for each health outcome for males and females to assess predictive power across specifications.[3] I do not find significant changes in the predictive power across model specifications.

[2]Results available upon request.
[3]Results available upon request.

TABLE A.14: Males (Selected Coefficients)

	LPM			Probit Model		
	(1)	(2)	(3)	(1)	(2)	(3)
Type A CC						
Immigrant	0.0515	-0.2421	-0.0140	-0.1473	-0.8511	-0.1308
YSM	0.0161†	0.0225 *	0.0157†	0.0495 †	0.0692 *	0.0492†
YSM-squared	-0.0003**	-0.0003**	-0.0003**	-0.0001**	-0.0001**	-0.0001**
Household size		0.0063			0.0162	
Number of dependent		-0.0083			-0.0237	
Household education: Omit less than high school						
High school		-0.0173			-0.0541	
Some PSE		-0.0088			-0.0238	
PSE		0.0131			0.0372	
Dividend		-0.0551**			-0.1503**	
Own a house		-0.0497**			-0.1390**	
Household income			-0.0000			-0.0000
Type B CC						
Immigrant	-0.0992	-0.0261	-0.0384	-0.6932	-0.1058	-0.2671
YSM	0 .0031	-0.0017	0.0005	0.0202	-0.0125	0.0031
YSM-squared	-0.0001	0.0000	-0.0000	-0.0002	0.0002	-0.0001
Household size		0 .0012			0.0048	
Number of dependent		-0.0050			-0.0520	
Household education: Omit less than high school						
High school		0.0324			0.1460	
Some PSE		0.0566			0.2659	
PSE		0 .0429			0.1907	
Dividend		-0.0284**			-0.1515**	
Own a house		-0.0232**			-0.1452**	
Household income			-0.0000*			-0.0000
Overweight/obese						
Immigrant	0.0919	0.1414	0.0060	0.2732	0.4096	0.0338
YSM	-0.0062	-0.01238	-0.0059	-0.0186	-0.0375	-0.0186
YSM-squared	0.0001	0.0002	0.0001	0.0003	0.0006	0.0004
Household size		0.0009			0.0035	
Number of dependent		-0.0133			-0.0380	
Household education: Omit less than high school						
High school		-0.0061			-0.0247	
Some PSE		0.0072			0.0105	
PSE		0.0460			0 .1262	
Dividend		-0.0623**			-0.1749**	
Own a house		0.0543**			0.1467**	
Household income			-0.0000**			-0.0000†
Poor/fair health						
Immigrant	0.1265	0.2319	0.1040	0.9334	1.8175†	0.4865
YSM	-0.0004	-0.0063	-0.0001	-0.0064	-0.0547	0.0021
YSM-squared	-0.0001	0.0000	-0.0001	-0.0003	0.0003	-0.0003
Household size		-0.0004			0.0004	
Number of dependent		-0.0017			-0.0464	
Household education: Omit less than high school						
High school		-0.0107			0.0519	
Some PSE		-0.0219			-0.0442	
PSE		-0.0283			-0.0743	
Dividend		-0.0438**			-0.3718**	
Own a house		-0.0510**			-0.3273**	
Household income			-0.0000**			-0.0000**

$^{\dagger}\ p < 0.10$, $^{*}\ p < 0.05$, $^{**}\ p < 0.01$

Data source: CCHS 2007 and 2012, Note: All estimations include controls for cohort effects. Robust standard errors in parentheses. Estimations are done with CCHS weight. ousehold education omitted category: Less than high school

TABLE A.15: Females (Selected Coefficients)

	LPM			Probit model		
	(1)	(2)	(3)	(1)	(2)	(3)
Type A CC						
Immigrant	-0.2271	-0.2485	-0.1961	-0.6447	-0.6964	-0.5450
YSM	0.0065	0.0071	0.0081	0.0196	0.0207	0.0234
YSM-squared	-0.0001	-0.0001	-0.0001	-0.0002	-0.0002	-0.0003
Household size		0.0029			0.0100	
Number of dependent		-0.0269*			-0.0738	
Household education: Omit less than high school						
High school		-0.0451			-0.1308	
Some PSE		-0.0428			-0.1191	
PSE		-0.0504			-0.1482	
Dividend		-0.0421**			-0.1120**	
Own a house		-0.0879 **			-0.2379**	
Household income			-0.0000**			-0.0000*
Type B CC						
Immigrant	0.0616	0.0184	0.0640	0.3911	0.2385	0.4208
YSM	-0.0041	-0.0020	-0.0036	-0.0237	-0.0162	-0.0221
YSM-squared	0.0000	0.0000	0.0000	0.0003	0.0002	0.0002
Household size		0.0087			0.0376	
Number of dependent		-0.0191†			-0.0927†	
Household education: Omit less than high school						
High school		-0.0045			0.0222	
Some PSE		0.0120			0.0518	
PSE		0.0016			0.0486	
Dividend		-0.0176†			-0.0737	
Own a house		-0.0563 **			-0.2672**	
Household income			-0.0000**			-0.0000**
Overweight/obese						
Immigrant	-0.0445	-0.1754	-0.1190	-0.2059	-0.5802	-0.4083
YSM	0.0037	0.0090	0.0057	0.0130	0.0280	0.0186
YSM-squared	-0.0001	-0.0001	-0.0001	-0.0001	-0.0003	-0.0002
Household size		0.0196			0.0514	
Number of dependent		-0.0210			-0.0538	
Household education: Omit less than high school						
High school		-0.0372			-0.0955	
Some PSE		-0.0445			-0.1113	
PSE		-0.0407			-0.1047	
Dividend		-0.0585**			-0.1553**	
Own a house		-0.0475**			-0.1268**	
Household income			-0.0000**			-0.0000**
Poor/fair health						
Immigrant	0.2093	0.2388	0.1673	3.0489*	2.6898*	2.6018†
YSM	-0.0032	-0.0029	-0.0028	-0.0953†	-0.0785	-0.0709
YSM-squared	-0.0000	-0.0000	-0.0000	0.0007	0.0005	0.0004
Household size		0.0055			-0.0671*	
Number of dependent		-0.0205			-0.2362**	
Household education: Omit less than high school						
High school		0.0139			0.2020*	
Some PSE		-0.0030			-0.0329	
PSE		0.0207			0.0648	
Dividend		-0.0391**			-0.3105**	
Own a house		-0.0841**			-0.3081**	
Household income			-0.0000**			-0.0000**

† $p < 0.10$, * $p < 0.05$, ** $p < 0.01$

Data source: CCHS 2007 and 2012, Note: All estimations include controls for cohort effects. Robust standard errors in parentheses. Estimations are done with CCHS weight. ousehold education omitted category: Less than high school

A.7 Matching Across Groups: Immigrants vs. Non-immigrants

I conduct robustness checks by matching the propensity scores between immigrants and non-immigrants for the years 2000, 2009, and 2011. I use the same set of observables as in my main matching analysis in Section 1.5.1; however, it is not possible to achieve a match without violating the overlap assumption. However (with much trials and error), it is possible to attain a good matching analysis. Some adjustments to the procedure are necessary: the match cannot be performed using the ethnicity variable, since this violates the covariates balance test. As well, the match must be done separately for the upper-income and lower-income quantiles.

Again, with respect to the lower-income group, it is impossible to perform a propensity-score matching that does not violate the overlap assumption. Figures **??** and **??** use CCHS 2009 to illustrate the estimated densities for the upper-income and the lower-income group, respectively. It is obvious that the lower-income densities are not well-behaved, while the upper-income densities show no signs of violating the overlap assumption. The densities for the other years (2000 and 2011) show the same pattern as for 2009.[4]

Consequently, I present the Average Treatement Effects (ATEs) only for the upper income group. The control group is the non-immigrant population, and the treatment group is the immigrant population. A positive ATE implies that immigrants are, on average, more likely to experience health issues than non-immigrants. On the other hands, a negative ATE implies that immigrants are less likely than native-born Canadians to have the health problems.

The results (across three calendar years, 2000, 2009, 2011) are presented in Table A.16. No clear pattern emerges in terms of health differences between immigrants and natives, except for the SAHS factor. With respect to reporting poor or fair health, immigrants and

[4] Results omitted here.

FIGURE A.2: Estimated Density: Upper Income Group

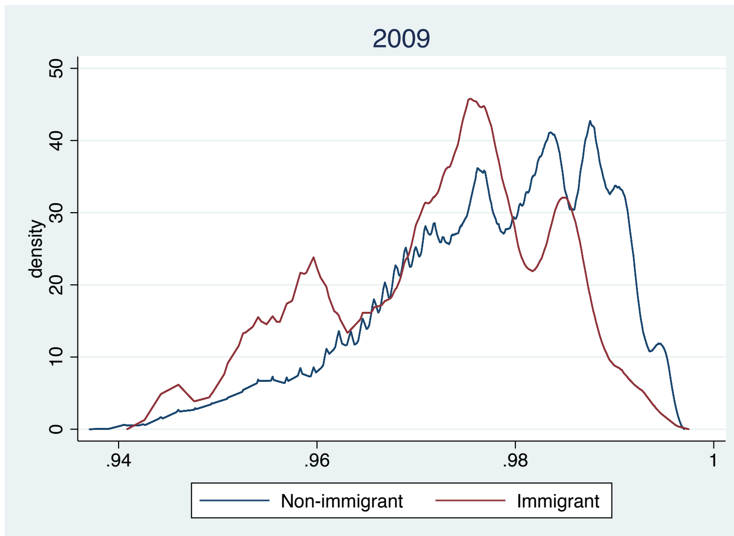
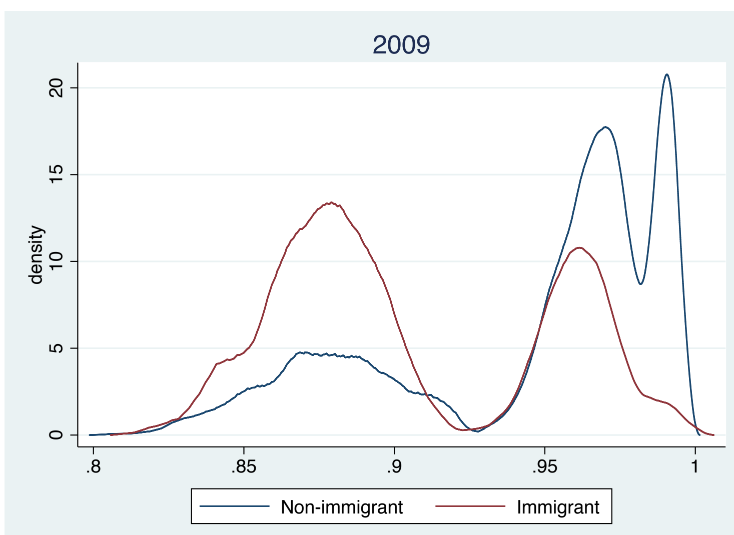


FIGURE A.3: Estimated Density: Lower Income Group



non-immigrants are the same. Note that because I cannot use the entire sample in this analysis, the results from this table cannot serve as a robustness check for a matching analysis between different cohorts of immigrants.

TABLE A.16: Average Treatment Effects: Matching Between Immigrants and Non-Immigrants, Upper Income Group

Year	**Type A CC**	**Type B CC**	**Overweight/obese**	**Poor/fair health**	**Observations**
2000	-0.117*	-0.008	-0.130*	0.002	29,310
	(0 .033)	(0.016)	(0.035)	(0.017)	
2009	-0.023	-0.045*	-0.102*	0.045	20,471
	(0.034)	(0.016)	(0.042)	(0 .033)	
2011	-0.180*	-0.025	-0.059	-0.038	19,634
	(0.030)	(0.017)	(0.082)	(0.006)	

Data source: CCHS 2000, 2009, and 2011, $*p < 0.05$, standard errors in parentheses

Appendix B

Chapter 2

B.1 Table

TABLE B.1: Coefficient Estimates from Simple Regressions: Singles

Log income	**Male**		**Female**	
Age	0.2191	(0.0327)	0.1500	(0.0269)
Age^2	-0.0046	(0.0008)	-0.0028	(0.0007)
Age^3	0.0000	(0.0000)	0.0000	(0.0000)
Education (omit high school):				
Lower than high school	-0.4365	(0.0198)	-0.5271	(0.0158)
Bachelor degree	0.5061	(0.0217)	0.6341	(0.0190)
Graduate degree	0.7791	(0.0326)	0.8999	(0.0264)
Other	0.2004	(0.0316)	0.2668	(0.0234)
Number of children (omit no children):				
1 child	0.1562	(0.0267)	-0.0271	(0.0162)
2 children	0.1387	(0.0381)	-0.0228	(0.0184)
3 children	0.0471	(0.0670)	-0.1035	(0.0248)
4 or more children	-0.1101	(0.1061)	-0.2321	(0.0335)
Constant	7.0774	(0.4127)	7.6210	(0.3429)
N	12,002		18,260	

The data source is the MEPS panel 1-15. Robust standard errors are provided in parentheses. The estimations were done using the MEPS family weights. The estimates of the year fixed-effects are omitted from this table.

TABLE B.2: Coefficient Estimates from Simple Regressions: Couples

Log income		
Husband's		
Age	0.0691	(0.0248)
Age^2	-0.0012	(0.0006)
Age^3	0.0000	(0.0000)
Education (omit high school):		
Lower than high school	-0.3047	(0.0110)
Bachelor degree	0.2789	(0.0114)
Graduate degree	0.3947	(0.0147)
Other	0.1401	(0.0151)
Wife's		
Age	0.0564	(0.0236)
Age^2	-0.0008	(0.0006)
Age^3	0.0000	(0.0000)
Education (omit high school):		
Lower than high school	-0.3508	(0.0114)
Bachelor degree	0.2775	(0.0113)
Graduate degree	0.3988	(0.0155)
Other	0.1452	(0.0140)
Number of children (omit no children):		
1 child	-0.0318	(0.0108)
2 children	-0.0325	(0.0107)
3 children	-0.0769	(0.0132)
4 or more children	-0.1528	(0.0173)
Constant	8.7177	(0.2886)
N	31,759	

The data source is the MEPS panel 1-15. Robust standard errors are provided in parentheses. The estimations were done using the MEPS family weights. The estimates of the year fixed-effects are omitted from this table.

B.2 Data Comparison

Since the Current Population Survey (CPS) is the official source of estimates of income (and poverty) in the US, I use the CPS as a benchmark in making comparison between the PSID and the MEPS. I focus the analysis on the income data as it is the main variable used in this study. Because the surveys are not static in nature, I choose 2002 calendar year as my reference point in making comparison of income across surveys. I have two major findings.

First, the aggregate income in the MEPS is very close to that of the CPS—$6.47 trillion in the CPS and $6.26 trillion in the MEPS. The PSID, though has a weighted population of 21 million persons fewer than the CPS, has higher aggregate income of $6.72 trillion.

Second, relative to the PSID, the average income per capita in the MEPS is closer to the figure found in the CPS than. In fact, Banthin and Selden (2006) assess the quality of income measurement in the MEPS, comparing poverty status distributions overall and by important subgroups between the CPS and the MEPS, and find that the MEPS income data benchmark closely to estimates from the CPS. I provide the weighted income per capita in Table B.3.

TABLE B.3: Average Income Per Capita by Quintile of Family Income

Quintile	CPS	MEPS	PSID
All persons	22,893	22,089	25,710
Lowest	6,513	6,352	7,178
Second	13,789	14,269	15,261
Third	19,293	20,052	21,132
Fourth	25,604	25,976	28,785
Highest	49,316	43,855	56,220

There are several major differences across these three surveys. I summarize these differences in Table B.4. In Table B.5, I provide average age, average years of education , and proportion of married individuals from the MEPS and the PSID.

TABLE B.4: Major Differences and Similarities Across Surveys: Income Data

	CPS 2002	MEPS 2002	PSID 2002
Persons Covered	All persons age 15 or over	All persons age 16 or over	Head an wife but not other family member
Income	Excludes non-periodic or lump sum withdrawals from tax-advantaged retirement accounts	Income questions use Internal Revenue Service definitions, since the questions reference specific lines on the personal income tax return.	Same as CPS but Value-Added benefit variable includes military retirement
Imputation	Statistical match (hot deck) and logical imputations, including Supplement refusals	Statistical match (hot deck) and logical imputations	Statistical match (hot deck) and logical imputations, plus use of prior wave data
Earnings Top-Codes	Wages and salary from longest job top-coded at $200,000. Other wage and salary, self-employment and farm earnings top-coded at $35,000, $50,000 and $25,000 respectively	Wages and salary and self-employment each top-coded at 99th percentile, amount not documented	Maximum values of $9,999,999 are de facto top-codes
Person and Family Totals	Not separately top-coded	Person totals separately top-coded at 99th percentile	Maximum values of $9,999,999 are de facto top-codes
Sample Size	78,300 households	14,700 households	7,822 households

Continued on next page

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	CPS	MEPS	PSID
Family Definition	Persons related by blood, marriage or adoption	Persons related by blood, marriage or adoption, including foster relationships and unmarried (opposite or same sex) partners	Persons related by blood, marriage or adoption, including foster relationships, unmarried (opposite sex) partners, and unrelated persons (may be same-sex partners) identified as part of family
Family Head	Person who owns or rents housing unit is household reference person (householder) and may be either husband or wife	Person who owns or rents housing unit is household reference person (householder) and may be either husband or wife	Family head is always male spouse or partner when present
Marriage	Legal marriage, legal spouse identified if present for all persons, cohabiting (unrelated) partner of reference person identified	Legal marriage or self-identified cohabitation, legal spouse identified if present for all persons, cohabiting partner of reference person or of reference person's parents, and children of partner, identified in relationship codes	Legal marriage or self-identified (opposite sex) partner of a year or more, cohabiting partner of reference person of any duration, and children, siblings or parents of partner identified in relationship codes

TABLE B.5: Comparing Variables Across Surveys

	Survey	2003	2005	2007	2009
Age	MEPS	33.42	33.69	34.34	34.73
	PSID	31.25	31.42	31.60	31.78
Education (years)	MEPS	10.41	9.20	9.53	9.58
	PSID	17.00	12.82	12.90	13.12
Married (%)	MEPS	50.40	49.92	51.99	47.21
	PSID	50.63	50.34	49.20	47.54

All mean values retrieved from each survey's official websites. All values are weighted to represent the US population.

B.3 Sensitivity Analysis

TABLE B.6: GMM Parameter Estimates from Various Specifications: Singles

	σ_α	σ_υ	ρ	σ_η
Single men (sm)				
Model 1	0.4941	0.5408	0.9949	0.1030
	(0.0000)	(0.0001)	(0.0026)	(0.0040)
Model 2	0.4892	0.5412	0.9945	0.1047
	(0.0000)	(0.0000)	(0.0033)	(0.0014)
Model 3	0.4935	0.5404	0.9952	0.1027
	(0.0003)	(0.0004)	(0.0066)	(0.0014)
Single women (sw)				
Model 1	0.4829	0.5725	0.9309	0.1601
	(0.0000)	(0.0001)	(0.0010)	(0.0000)
Model 2	0.4869	0.5726	0.9350	0.1542
	(0.0001)	(0.0000)	(0.0007)	(0.0026)
Model 3	0.5185	0.5682	0.9933	0.0854
	(0.0000)	(0.0005)	(0.0015)	(0.0031)

Data source: MEPS panel 1 to 15. Note: Bootstrapped standard errors in parentheses.

The 15 moments used are variances of age 25, 30, 35, 40, 45, 50, 55, 60, and the first-order autocovariances of age 25, 30, 35, 40, 45, 50, and 55.

Model 1 uses the original set of covariates. Model 2 and 3 add interaction terms between age and education, and age and number of children, respectively.

Parameter ρ is restricted: $\rho = [0, 1]$

TABLE B.7: GMM Parameter Estimates from Various Specifications: Couples

	σ^{υ}_{mw}	σ^{η}_{mw}
Model 1	-0.2155	-0.0308
	(0.0006)	(0.0000)
Model 2	-0.2450	-0.0161
	(0.0006)	(0.0000)
Model 3	-0.2641	-0.0153
	(0.0003)	(0.0000)

Data source: MEPS panel 1 to 15. Note: Bootstrapped standard errors in parentheses.

The 15 moments used are variances of age 25, 30, 35, 40, 45, 50, 55, 60, and the first-order autocovariances of age 25, 30, 35, 40, 45, 50, and 55.

Model 1 uses the original set of covariates. Model 2 add husband and wife's education interactions. Model 3 is model 2 plus husband and wife's age interaction

Parameter ρ is restricted: $\rho = [0, 1]$

TABLE B.8: GMM Parameter Estimates: Allowing for Endogeneity Bias

	σ_α	σ_υ	ρ	σ_η	$\hat{\delta}\beta_2$
Single men	0.4941	0.5408	0.9949	0.1030	0.0003
	(0.0000)	(0.0000)	(0.0007)	(0.0016)	(0.0007)
Single women	0.5279	0.5694	0.9986	0.0767	0.0025
	(0.0000)	(0.0002)	(0.0003)	(0.0027)	(0.0003)

Data source: MEPS panel 1 to 15. Note: Bootstrapped standard errors in parentheses.

The 15 moments used are variances of age 25, 30, 35, 40, 45, 50, 55, 60, and the first-order autocovariances of age 25, 30, 35, 40, 45, 50, and 55.

Parameter ρ is restricted: $\rho = [0, 1]$

B.4 Synthetic Controls

TABLE B.9: GMM Parameter Estimates: Synthetic Couples

	σ_{α_c}	σ_{mw}^{υ}	σ_{mw}^{η}
Synthetic couples:			
15 moments	$3e^{-7}$	-0.0005	-0.0001
	(0.0000)	(0.0005)	(0.0001)

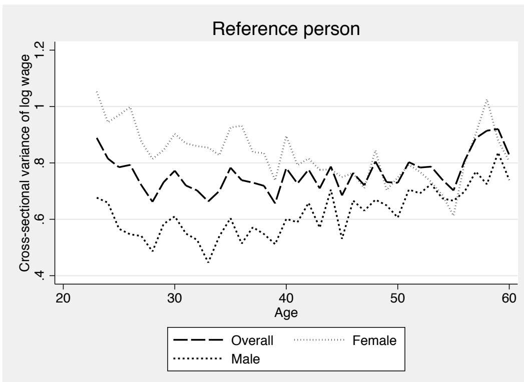
Data source: MEPS panel 1 to 15. Note: Bootstrapped standard errors in parentheses.

The 15 moments used are variances of age 25, 30, 35, 40, 45, 50, 55, 60, and the first-order autocovariances of age 25, 30, 35, 40, 45, 50, and 55.

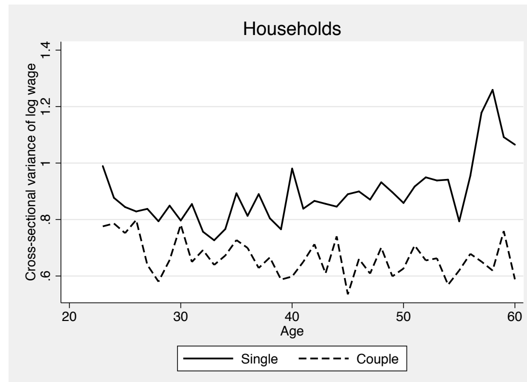
B.5 Using Only Wage Income

FIGURE B.1: Cross-Sectional Variance of Log-Wage

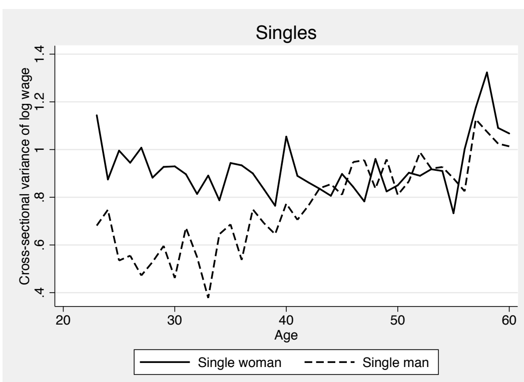
(A) Reference person



(B) Single/not single



(C) Single



(D) Couple

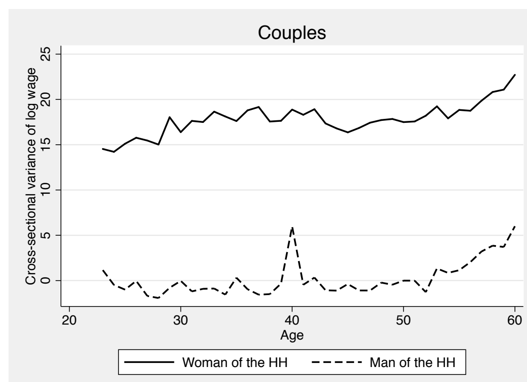


TABLE B.10: GMM Parameter Estimates: Singles and Couples Using Only Wage Data

	σ_α	σ_υ	ρ	σ_η
Single men (sm)				
$\rho = (0, 1]$	0.5226	0.5690	0.9941	0.0834
	(0.0000)	(0.0001)	(0.0008)	(0.0010)
$\rho = 1$	0.5988	0.5628	–	0.1076
	(0.0005)	(0.0008)	–	(0.0000)
Single women (sw)				
$\rho = (0, 1]$	0.7413	0.5608	0.9848	0.0401
	(0.0004)	(0.0002)	(0.0079)	(0.0042)
$\rho = 1$	0.7308	0.5567	–	0.0395
	(0.0006)	(0.0004)	–	(0.0000)
	σ^{υ}_{mw}	σ^{η}_{mw}		
Couples				
$\rho = (0, 1]$	-0.3103	-0.0162		
	(0.0021)	(0.0000)		
$\rho = 1$	-0.3340	-0.0156		
	(0.0013)	(0.0000)		

Data source: MEPS panel 1 to 15. Note: Bootstrapped standard errors in parentheses.

The 15 moments used are variances of age 25, 30, 35, 40, 45, 50, 55, 60, and the first-order autocovariances of age 25, 30, 35, 40, 45, 50, and 55.

B.6 Coefficient of Variation

Ostrovsky (2012) and Hryshko, Juhn, and McCue (2016) decompose the squared-coefficient of variation, CV^2, in the following manner:

$$
\begin{aligned}
\text{CV}^2(Y) = \frac{\text{var}(Y)}{(\bar{Y})^2} &= \frac{\text{var}(Y_m) + \text{var}(Y_w) + 2\text{cov}(Y_m, Y_w)}{(\bar{Y}_m + \bar{Y}_w)^2} \\
&= \frac{(\bar{Y}_m)^2}{(\bar{Y}_m + \bar{Y}_w)^2}\text{CV}^2(Y_m) + \frac{(\bar{Y}_w)^2}{(\bar{Y}_m + \bar{Y}_w)^2}\text{CV}^2(Y_w) + 2\frac{\text{cov}(Y_m, Y_w)}{(\bar{Y}_m + \bar{Y}_w)^2} \\
&= M_m^2\text{CV}^2(Y_m) + M_w^2\text{CV}^2(Y_w) + 2\frac{\text{cov}(Y_m, Y_w)}{(\bar{Y}_m + \bar{Y}_w)^2} \qquad \text{(B.1)}
\end{aligned}
$$

where Y is total family earnings, Y_m and Y_w are men's and women's earnings; $\bar{Y}_m$ and $\bar{Y}_w$ are mean earnings of men and of women; M_m^2 and M_w^2 are ratio of the men's and women's average income to the average couples income.

TABLE B.11: Coefficient of variation of family earnings and it's components, 1996-2010

Year	$\text{CV}^2(Y)$ families	$\text{CV}^2(Y_m)$ husbands	$\text{CV}^2(Y_w)$ wives	M_m^2	M_w^2	$\text{Cov}(Y_m, Y_w)/(\bar{Y})^2$	$\bar{Y}_m$	$\bar{Y}_w$	$\bar{Y}$	N
1996	0.453	0.652	0.964	0.353	0.164	0.032	45,299	30,892	76,190	3,306
1997	0.485	0.623	1.159	0.404	0.133	0.040	45,482	26,054	71,537	1,794
1998	0.499	0.610	1.142	0.361	0.160	0.048	48,738	32,408	81,146	1,646
1999	0.439	0.600	0.970	0.366	0.156	0.034	48,171	31,430	79,600	2,094
2000	0.456	0.587	1.012	0.361	0.159	0.041	47,104	31,261	78,365	1,628
2001	0.527	0.721	1.043	0.368	0.155	0.050	48,398	31,345	79,742	3,301
2002	0.571	0.764	1.143	0.367	0.155	0.056	45,958	29,915	75,873	2,315
2003	0.536	0.707	1.109	0.352	0.165	0.052	46,052	31,514	77,567	2,275
2004	0.564	0.758	1.196	0.367	0.155	0.050	45,666	29,664	75,330	2,395
2005	0.581	0.703	1.284	0.372	0.152	0.062	45,025	28,793	73,818	2,274
2006	0.576	0.715	1.145	0.367	0.156	0.068	46,798	30,504	77,302	2,354
2007	0.565	0.738	1.170	0.381	0.146	0.056	47,113	29,193	76,306	1,857
2008	0.593	0.735	1.252	0.371	0.153	0.064	44,990	28,876	73,866	2,516
2009	0.550	0.729	1.120	0.371	0.153	0.054	43,732	28,109	71,841	2,127
2010	0.554	0.722	1.192	0.377	0.149	0.052	45,382	28,524	73,906	1,863

Data source: MEPS panel 1 to 16. $M_j^2 = \bar{Y}_j/\bar{Y}$ where $j = m, w$.
Either spouses are allowed to have zero earnings.

Table B.11 shows similar finding as those from Ostrovsky (2012), who uses Canadian Longitudinal Administrative data from 1986 to 2008. In particular, I find:

- A general trend in the dispersion of family earnings from 1996 to 2010 was upward. In 2010, the CV^2 was about 22% higher than in 1996.
- While there is a clear pattern in an increase in the dispersion of husbands' earnings, it is not obvious if there is a particular trend in the dynamics of CV^2 of the wives.
  - For the husbands, the CV^2 in 2010 is about 11% higher than in 1996. The results suggest that the main driving force behind the increase in the dispersion of family earnings was the increase in the dispersion of husbands' earnings.
- Although $\text{CV}^2(Y_w)$ is larger than $\text{CV}^2(Y_m)$, the overall effect of $\text{CV}^2(Y_w)$ on $\text{CV}^2(Y)$ is relatively small because wives' earnings still constituted a much smaller fraction of the family earnings than husbands' earnings.
  - For instance, in 2010, column M_m^2 and M_w^2 shows that wives' earnings is only about 40% of husbands.
- The $\text{Cov}(Y_m, Y_w)$ adjusted by $(\bar{Y})^2$ almost double during the period 1996-2010.

Figure B.2 shows fractions of the CV^2 of family earnings from each term in Equation B.1. The men of the household is the main force driving the CV^2, roughly 50% of the total variation. The women of the household accounts for some 30%, and their earnings covariances accounts for some 20%. I also re-estimate these relative contributions using different sub-samples: including spouses with zero earnings, include only wives with zero earnings, and excluding all couples whose at least one spouse have zero earnings. I plot the covariance contribution from these re-estimation in Figure B.3. I do not find significant differences in the contribution of the covariance of income across these sample restrictions.

One concern with this decomposition of CV^2 is that the variance terms, $\text{var}(Y_m)$ and $\text{var}(Y_w)$, may capture "unconditional" variances as opposed to variances of earnings conditional on spouses' earnings. To further clarify my point, assume that $\text{Var}(Y_m)$ is in fact

FIGURE B.2: Fractions of the Coefficient of Variation of Family Earnings from Each Term in Equation B.1

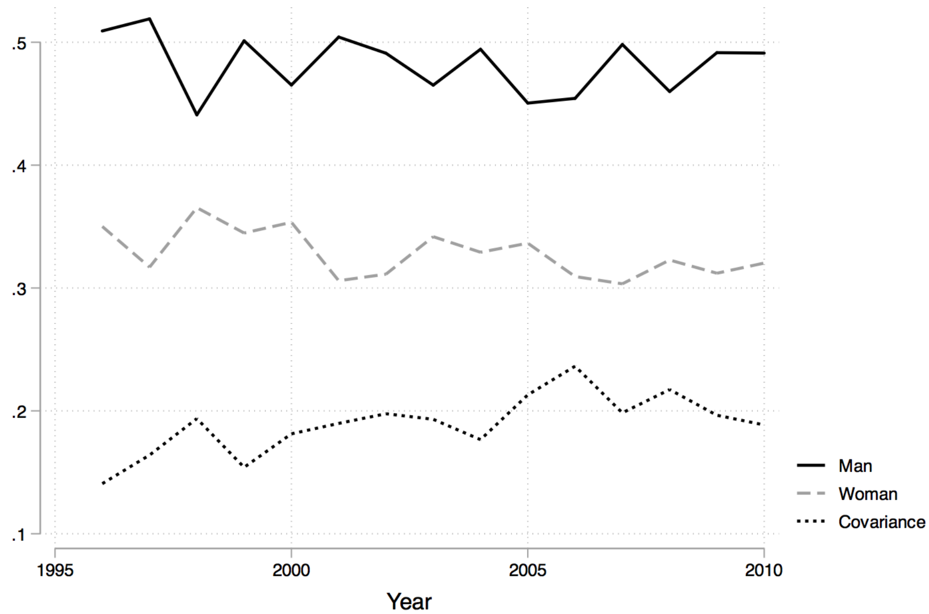
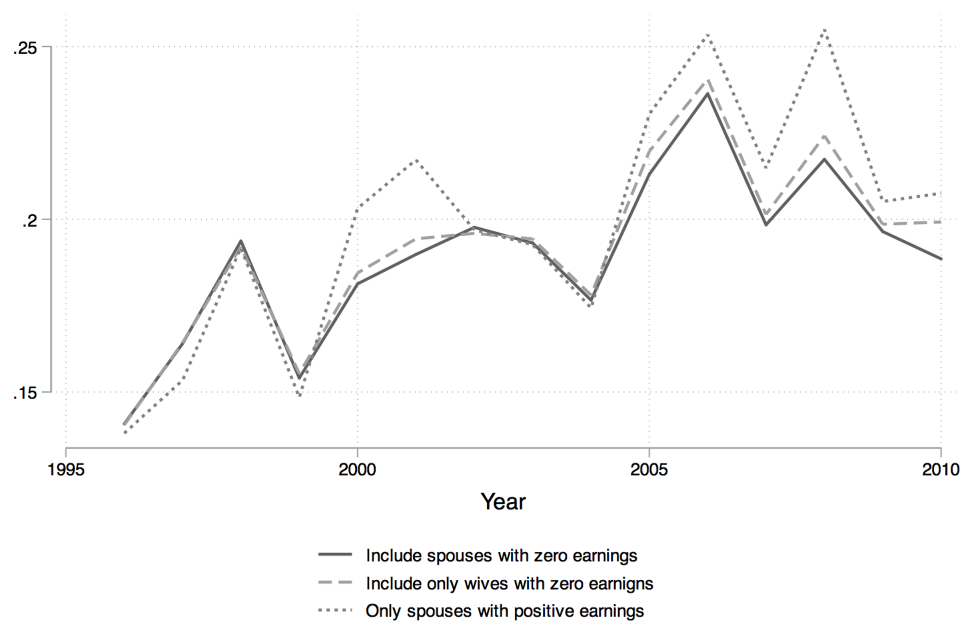


FIGURE B.3: The Contributions of Covariance to the Coefficient of Variation of Family Earnings



$\mathrm{Var}(Y_m|Y_w) = (1-\rho^2)\mathrm{Var}(Y_m)$, where ρ is a correlation between Y_m and Y_w.[1] Then Equation B.1 can be written as:

$$\mathrm{CV}^2(Y) = \frac{(1-\rho^2)\mathrm{Var}(Y_m) + (1-\rho^2)\mathrm{Var}(Y_w) + 2\mathrm{Cov}(Y_m, Y_w)}{(\bar{Y}_m + \bar{Y}_w)^2} \tag{B.2}$$

Using the definition, $\mathrm{CV}^2(Y) = \frac{\mathrm{Var}(Y)}{(\bar{Y})^2}$, to substitute the two variance terms, equation B.2 can be written as:

$$\begin{aligned}\mathrm{CV}^2(Y) &= \frac{(\bar{Y}_m)^2}{(\bar{Y}_m + \bar{Y}_w)^2}\mathrm{CV}^2(Y_m)(1-\rho^2) + \frac{(\bar{Y}_w)^2}{(\bar{Y}_m + \bar{Y}_w)^2}\mathrm{CV}^2(Y_w)(1-\rho^2) + 2\frac{\mathrm{cov}(Y_m, Y_w)}{(\bar{Y}_m + \bar{Y}_w)^2} \\ &= M_m^2\mathrm{CV}^2(Y_m)(1-\rho^2) + M_w^2\mathrm{CV}^2(Y_w)(1-\rho^2) + 2\frac{\mathrm{cov}(Y_m, Y_w)}{(\bar{Y}_m + \bar{Y}_w)^2} \end{aligned} \tag{B.3}$$

where M_m and M_w are ratio of the men's and women's average income to the average couples' income as before.

If the true variances are "conditional" on each partner's earnings, then the weight contributions of the men and of the women calculated from Equation B.1 would be overestimated. Since $\rho < 1$ by definition, the weight contributions would be overestimate by the amount proportionate to ρ^2.

[1]Similarly, $\mathrm{Var}(Y_w)$ is $\mathrm{Var}(Y_w|Y_m) = (1-\rho^2)\mathrm{Var}(Y_w)$.

Appendix C

Chapter 3

TABLE C.1: GMM Parameter Estimates: Unobserved Ability and Latent Health)

	Health Component		Non-Health Component					
	δ	σ_ν	$(\hat{\delta}\beta_2 + \hat{\gamma_1}\beta_4)^2$	$(\beta_3 + \hat{\gamma_2}\beta_4)^2$	σ_α	$\sigma_{\tilde{\varphi}}$	ρ	σ_η
Emergency Room Visits								
Childless Male	0.6883	1.4e-04	1.3e-05	0.0001	0.5112	0.5688	0.8789	0.0110
	(0.0027)	(0.0000)	(0.0000)	(0.0000)	(0.0010)	(0.0098)	(0.0034)	(0.0000)
Childless Female	0.3129	0.0711	0.0001	0.0002	0.2531	0.1231	0.9956	0.0001
	(0.0011)	(0.0000)	(0.0000)	(0.0000)	(0.0011)	(0.0016)	(0.0013)	(0.0000)
Single Mother	0.7988	0.0854	0.0091	0.0001	0.4000	0.3937	0.5628	0.0003
	(0.0017)	(0.0001)	(0.0000)	(0.0050)	(0.0011)	(0.0011)	(0.0013)	(0.0000)
Couple (1)	0.4412	0.1e-05	0.0002	0.0004	0.3819	0.5281	0.8791	0.0233
	(0.0001)	(0.0000)	(0.0000)	(0.0079)	(0.0015)	(0.0000)	(0.0089)	(0.0032)
Couple (2)	0.7557	0.0991	0.0071	0.0002	0.3745	0.4353	0.9981	0.0003
	(0.0000)	(0.0000)	(0.0001)	(0.0000)	(0.0008)	(0.0009)	(0.0035)	(0.0000)

Data source: MEPS panel 1 to 16. Note: Bootstrapped standard errors in parentheses. Specifications: (1) Shocks from adults only; (2) Shocks from adults and children.

TABLE C.2: GMM Parameter Estimates: Only Wage Income (Singles)

	Health Component		Non-Health Component				
	δ	σ_ν	$\hat{\gamma}\beta_2$	σ_α	$\sigma_{\tilde{\varphi}}$	ρ	σ_η
ER Visits:							
Childless Male	0.5351	0.2e-03	1.2e-05	0.5151	0.6375	0.8926	0.0012
	(0.0002)	(0.0000)	(0.0005)	(0.0001)	(0.0000)	(0.0001)	(0.0000)
Childless Female	0.1521	0.0947	1.1e-05	0.2739	0.2217	0.8801	0.0121
	(0.0002)	(0.0004)	(0.0021)	(0.0080)	(0.0001)	(0.0003)	(0.0001)
Single Mother	0.6478	0.0210	1.2e-06	0.6183	0.5630	0.6671	0.3e-07
	(0.0004)	(0.0000)	(0.0000)	(0.0001)	(0.0000)	(0.0001)	(0.0000)
ER Visits (charges $\geqslant$ \$500):							
Childless Male	0.6212	0.0008	0.3415	0.3145	0.2e-07	0.8269	0.0012
	(0.0003)	(0.0000)	(0.0007)	(0.0012)	(0.0090)	(0.0001)	(0.0000)
Childless Female	0.5691	0.0810	0.0019	0.5013	0.8910	0.6671	0.0019
	(0.0001)	(0.0002)	(0.0000)	(0.0019)	(0.0038)	(0.0001)	(0.0004)
Single Mother	0.8182	0.0012	0.0002	0.5441	0.6651	0.2910	0.3841
	(0.0002)	(0.0000)	(0.0000)	(0.0005)	(0.0004)	(0.0009)	(0.0000)
All singles:							
Insured	0.1221	0.0481	0.0003	0.6676	0.4668	0.8192	0.0041
	(0.0004)	(0.0000)	(0.0000)	(0.0011)	(0.0008)	(0.0011)	(0.0001)
Uninsured	0.2281	0.0019	0.0718	0.5541	0.5611	0.8182	0.0082
	(0.0011)	(0.0000)	(0.0000)	(0.0023)	(0.0005)	(0.0009)	(0.0000)

Data source: MEPS panel 1 to 16. Note: Bootstrapped standard errors in parentheses.

TABLE C.3: GMM Parameter Estimates: Only Wage Income (Couples)

	Health Component		Non-Health Component				
	δ	σ_ν	$\hat{\gamma}\beta_2$	σ_α	$\sigma_{\tilde{\varphi}}$	ρ	σ_η
ER Visits:							
Couple (1)	0.6123	0.0226	0.0002	0.3949	0.2211	0.8828	0.0126
	(0.0001)	(0.0000)	(0.0000)	(0.0001)	(0.0000)	(0.0009)	(0.0000)
Couple (2)	0.5519	0.0236	0.0109	0.3624	0.2169	0.7867	0.0219
	(0.0000)	(0.0000)	(0.0000)	(0.0001)	(0.0001)	(0.0012)	(0.0000)
Couple (3)	0.5536	0.0318	0.0013	0.5065	0.4256	0.8698	0.0155
	(0.0060)	(0.0000)	(0.0000)	(0.0007)	(0.0018)	(0.0025)	(0.0000)
Couple (4)	0.6302	0.0211	0.0002	0.5007	0.2110	0.6221	0.0211
	(0.0003)	(0.0000)	(0.0000)	(0.0001)	(0.0006)	(0.0008)	(0.0000)
Insured (1)	0.6030	1.1e-05	1.1e-07	0.5529	0.3128	0.9568	0.0001
	(0.0000)	(0.0000)	(0.0000)	(0.0001)	(0.0001)	(0.0003)	(0.0000)
Uninsured (1)	1.2875	0.2229	0.0002	0.2178	0.2221	0.8978	0.0005
	(0.0000)	(0.0000)	(0.0000)	(0.0010)	(0.0003)	(0.0007)	(0.0000)
ER Visits (charges $\geqslant$ \$500)	0.4762	0.0094	0.0054	0.3672	0.5071	0.0586	0.0146
	(0.0000)	(0.0000)	(0.0000)	(0.0001)	(0.0002)	(0.0009)	(0.0000)

Data source: MEPS panel 1 to 16. Notes: Bootstrapped standard errors in parentheses. Specifications: (1) Shocks from adults only; (2) Shocks from adults and children,; (3) Shocks from male adults only; (4) Shocks from female adults only.

TABLE C.4: GMM Parameter Estimates: Interaction Effects

	Health Component		Non-Health Component				
	δ	σ_ν	$\hat{\gamma}\beta_2$	σ_α	$\sigma_{\tilde{\varphi}}$	ρ	σ_η
ER Visits:							
Childless Single Male	0.4222	0.0000	0.0000	0.5889	0.5609	0.8422	0.0311
	(0.0000)	(0.0019)	(0.0000)	(0.0036)	(0.0020)	(0.0067)	(0.0000)
Childless Single Female	0.7110	0.0984	0.0041	0.2521	0.1782	0.9311	0.0000
	(0.0031)	(0.0011)	(0.0000)	(0.0000)	(0.0001)	(0.0002)	(0.0000)
Single Single Mother	0.8221	0.0199	0.0023	0.5718	0.5872	0.4897	0.0000
	(0.0101)	(0.0000)	(0.0000)	(0.0002)	(0.0002)	(0.0058)	(0.0000)
Couple (1)	0.4857	0.0001	0.0077	0.3880	0.4486	0.7277	0.1121
	(0.0051)	(0.0000)	(0.0000)	(0.0001)	(0.0001)	(0.0011)	(0.0000)
ER Visits (charges $\geqslant$ \$500):							
Childless Single Male	0.6121	0.0002	0.0000	0.4891	0.5893	0.9241	0.0122
	(0.0018)	(0.0000)	(0.0000)	(0.0011)	(0.0011)	(0.0201)	(0.0000)
Childless Single Female	0.3815	0.0126	0.0000	0.5197	0.2355	0.8891	0.0062
	(0.0011)	(0.0000)	(0.0000)	(0.0001)	(0.0037)	(0.0058)	(0.0000)
Single Mother	0.7195	0.0477	0.0007	0.5596	0.5384	0.9825	0.0024
	(0.0028)	(0.0000)	(0.0000)	(0.0066)	(0.0001)	(0.0001)	(0.0001)
Couple (1)	0.5435	0.0001	0.0000	0.3299	0.5293	0.8988	0.0012
	(0.0001)	(0.0000)	(0.0002)	(0.0028)	(0.0001)	(0.0121)	(0.0000)

Data source: MEPS panel 1 to 16. Notes: Bootstrapped standard errors in parentheses. Specifications: (1) Shocks from adults only.

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