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**Lena Janys**

*University of Bonn, HCM and IZA*

**Bettina Siflinger**

*Tilburg University, Netspar and CESifo*

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## ABSTRACT

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# Mental Health and Abortions among Young Women: Time-Varying Unobserved Heterogeneity, Health Behaviors, and Risky Decisions\*

In this paper, we provide causal evidence on abortions and risky health behaviors as determinants of mental health development among young women. Using administrative in- and outpatient records from Sweden, we apply a novel grouped fixed-effects estimator proposed by Bonhomme and Manresa (2015) to allow for time-varying unobserved heterogeneity. We show that the positive association obtained from standard estimators shrinks to zero once we control for grouped time-varying unobserved heterogeneity. We estimate the group-specific profiles of unobserved heterogeneity, which reflect differences in unobserved risk to be diagnosed with a mental health condition. We then analyze mental health development and risky health behaviors other than unwanted pregnancies across groups. Our results suggest that these are determined by the same type of unobserved heterogeneity, which we attribute to the same unobserved process of decision-making. We develop and estimate a theoretical model of risky choices and mental health, in which mental health disparity across groups is generated by different degrees of self-control problems. Our findings imply that mental health concerns cannot be used to justify restrictive abortion policies. Moreover, potential self-control problems should be targeted as early as possible to combat future mental health consequences.

**JEL Classification:** I12, I10, C23, D91

**Keywords:** mental health, abortions, risky health behaviors, adolescence, time-varying unobserved heterogeneity, grouped fixed-effects

**Corresponding author:**

Lena Janys  
Department of Economics  
University of Bonn  
Adenauerallee 24-42  
53113 Bonn  
Germany  
E-mail: [ljanys@uni-bonn.de](mailto:ljanys@uni-bonn.de)

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# 1 Introduction

In recent years, economists have increasingly paid attention to mental health problems and their consequences, especially when occurring during adolescence and young adulthood (Biasi et al., 2019; Cuddy and Currie, 2020). Mental health problems are often first diagnosed in early adulthood and are very pervasive, in particular among young women (see Eaton et al., 2008). In 2017, about 13-19% of adolescents between 15-25 in the US experienced at least one major depressive episode (NIH, 2019). As pointed out by Currie (2020) mental health problems can reflect deficits in non-cognitive skills that are crucial for human capital development and labor market outcomes in adulthood. Thus, knowing about potential determinants of mental problems is of first-order importance.

One possible determinant that is often discussed in connection with mental health problems is abortion. In the US, abortions for women aged 15-24 years account for almost 40% of all abortions in 2017 (Kortsmitt et al., 2020). The joint occurrence of abortions and the onset of mental health problems in young adulthood suggests an association that has often resulted in claims of a causal relationship. In many countries, this has been used by politicians to justify restrictions on abortion access such as waiting times, mandatory disclosures, or parental consent laws (Guttmacher Institute, 2020).

This paper investigates the impact of having an abortion from an unwanted pregnancy on the incidence of mental health conditions in young women in Sweden. We use individual-level administrative panel data that includes all inpatient and outpatient contacts with the healthcare system. These records contain detailed information on mental health diagnoses and abortions, and they are linked to other administrative records such as the socioeconomic register (LISA), tax-registers, and the intergenerational register. This allows us to investigate the decision to undergo an abortion and abstracts from adverse mental health consequences of medically indicated abortions and miscarriages.

In absence of any policy variation in abortion legislation, identification of a causal effect is challenging. Traditional estimators using within-person variation such as event-study or individual-specific fixed-effects approaches assume that individual unobserved heterogeneity

is time-constant. In our application, this seems too restrictive, as it neglects that selection into abortion is dynamic. To address this issue, we use a grouped fixed-effects estimator, henceforth GFE, proposed by [Bonhomme and Manresa \(2015\)](#). The basic idea of the GFE estimator is that individuals who share similar unobserved characteristics are clustered in groups. Within these groups, unobserved heterogeneity is allowed to vary with age with no further restrictions on the functional form of these unobserved heterogeneity trajectories.

We compare the results from the individual-specific fixed-effects (OLS FE) and the GFE estimator. The estimated OLS FE-coefficient for abortion is positive and highly statistically significant. By contrast, we obtain a precisely estimated zero effect of abortion on diagnoses of mental health problems when using the GFE estimator. The large difference in these estimated coefficients stresses the importance of accounting for time-varying unobserved heterogeneity *in addition* to individual-specific time-constant unobserved heterogeneity.

Since the GFE estimator is a fixed-effects estimator, we perform a within-person comparison to estimate causal effects. This implies that our estimates can answer questions about how much a variable of interest affects the outcome trajectory of an individual. In our case, we estimate the joint event of an unwanted pregnancy followed by an abortion. Because our estimated effect size is very close to zero, we can reasonably conclude that this adverse life event does not change the mental health trajectory of an affected woman.<sup>1</sup> It implies that in the counterfactual where a woman is denied an abortion, we would expect a woman's mental health to deteriorate unless we were willing to assume that continuing the unwanted pregnancy would *improve* her mental health trajectory. Thus, an abortion can make up for the (potentially) negative life event of an unwanted pregnancy as if it had never happened.

The GFE estimator requires the researcher to select the number of groups of time-varying unobserved heterogeneity. We employ several performance measures to select the correct number of groups and choose the GFE estimator with two groups as our main specification.<sup>2</sup> The estimated unobserved mental health profiles differ considerably across groups in both

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<sup>1</sup>In principle it is also possible that an unwanted pregnancy is in itself a neutral event in terms of mental health costs. In that case, abortion restrictions would have no effect on mental health. However, in the context of other costs of abortion denials that have been documented in the literature, abortion restrictions would still have detrimental effects without improving mental health.

<sup>2</sup>However, our main results are robust against including more than two groups.

scale and slope. While a majority of young women share an age profile of unobserved heterogeneity that is practically flat, about 6% exhibit a profile that steeply increases with age. We interpret the profiles as the age-dependent, unobserved risk to develop mental health problems. This implies that the majority of women exhibit a low unobserved risk of getting a mental health diagnosis as they age. By contrast, a small but significant share of women has a mental health risk that is low at age 16 but sharply accumulates as these women get older.

We next address the question of what factors are potentially picked up by the profiles of unobserved mental health risks. Since abortions from unwanted pregnancies are mostly the result of a woman's decision to engage in unprotected sex, we link mental health and abortions to other risky health behaviors observable in our data i.e. chlamydia infections, STD screenings, and alcohol intoxication. The correlation between these observable behaviors and abortion is strong, but controlling for them does not alter the point estimates of abortion. Moreover, estimated coefficients of these other behaviors exhibit a similar pattern as the abortion coefficients across all considered specification. Finally, we show that the estimated unobserved mental health risk profiles are strongly correlated with these behaviors. Overall, these results suggest that risky health behaviors are also outcomes of the same choice process as abortion, rather than omitted control variables.

To understand how dynamic decision-making may lead to diverging unobserved heterogeneity profiles, we propose a model of inter-temporal choices and mental health. As discussed by [O'Donoghue and Rabin \(2001\)](#), adolescents may engage in unprotected sexual activities because they place a much higher weight on the immediate gratification from sex today than on the large costs they may face in the future. We thus model women's time preferences as quasi-hyperbolic to induce self-control problems and link the model to our empirical results by allowing for two types of women who vary by the degree of present bias. This leads to different trade-offs, different decisions, and thus to a different evolution of risky behaviors and mental health across groups as women age. The estimated parameters indicate a large degree of heterogeneity in the present bias across groups, resulting in different mental health trajectories.

A large number of studies has investigated fertility and economic outcomes of abortion (e.g. [Ananat et al., 2007](#); [Ananat et al., 2009](#); [Myers, 2017](#) for women's outcomes; [Currie et al., 1996](#); [Gruber et al., 1999](#); [Pop-Eleches, 2006](#) for child outcomes). Yet, potential mental health consequences have been understudied by economists. The medical literature addresses this topic, finding very mixed conclusions on whether an abortion has negative consequences on mental health or not.<sup>3</sup> To a large extent, these inconclusive results can be attributed to methodological issues related to a difficult-to-study subject. Randomized controlled trials are ethically not feasible. Survey data often suffer from non-classical measurement error, under-reporting, and recall bias in the presence of stigma.<sup>4</sup> Individual-level data is rarely available, even in countries where administrative data is widely used.<sup>5</sup>

An innovative approach to quantify the effect of an abortion denial on women's lives is the Turnaway Study.<sup>6</sup> Using this data, [Biggs et al. \(2017\)](#) find no effect of abortion on depression. There are two potential concerns with this study: First, women who are in time for an abortion might differ substantially in terms of unobservables from women who were late. Second, the sample size is rather small, implying that potential effects would need to be very large to be detected.

In economics, studies analyzing abortion effects typically exploit changes in abortion legislation for identification and mostly focus on the US (see for instance [Ananat et al., 2007](#); [Currie et al., 1996](#); [Fischer et al., 2018](#); [Gruber et al., 1999](#); [Lindo et al., 2020](#); [Miller et al., 2020a](#); [Steingrimsdottir, 2016](#)). [Myers \(2017\)](#) uses state-level variation in the access to the oral contraceptive pill and abortions to estimate the impact on fertility and

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<sup>3</sup>See [Reardon \(2018\)](#) for a comprehensive discussion of the medical literature examining the relationship between abortion and mental health.

<sup>4</sup>[Biggs et al. \(2020\)](#) show in a study in the US that perceived abortion at baseline, is associated with higher self-reported measures of psychological distress five years after an abortion.

<sup>5</sup>Only two studies in the medical literature address some methodological issues using an event-study approach and Danish healthcare registers. [Munk-Olsen et al. \(2011\)](#) find no evidence of an increased risk of mental disorders after a first-trimester induced abortion. [Steinberg et al. \(2018\)](#) show that women who had a first-trimester induced abortion have higher rates of antidepressant use. Event-study approaches have the disadvantage of failing to identify key components of the model ([Borusyak and Jaravel, 2017](#)) and cannot account for time-varying unobserved heterogeneity. Thus, a causal interpretation is unlikely to be valid.

<sup>6</sup>The Turnaway Study collects individual longitudinal information of women who received an abortion and women who were denied an abortion due to ineligibility based on cut-off dates in the US. The study followed women over five years after the initial abortion encounter to collect information about health, well-being, education, and labor market outcomes ([Miller et al., 2020b](#)).

marriage. She shows that while legalizing the pill also for minors does not significantly influence these outcomes, abortion legalization had a considerable impact. Only a handful of studies have studied variation in abortion legislation in countries other than the US (Mølland, 2016; Pop-Eleches, 2006). For instance, Clarke and Mühlrad (2021) examine the effect of abortion on health in Mexico, considering mental health as a secondary outcome. Exploiting both progressive and regressive changes in abortion legislation, the authors show that the original legalization resulted in a sharp decline in maternal morbidity but they do not find an effect on mental health in either direction. However, the study uses inpatient postpartum depression as the only measure of mental health, making their results rather inconclusive. A common limitation of the studies discussed above may be that changes in legislation might be intertwined with changes in stigma, thus potentially violating the identifying assumptions of the differences-in-differences estimation strategy. This may be particularly important when mental health is the outcome of interest (Biggs et al., 2020).

We complement this strand of economic research in several ways. Our analysis utilizes administrative records, covering the universe of women in the Swedish region of Skåne over 10 years. Hence, we observe all individual abortion decisions that emerge from *unwanted* pregnancies as well as individual mental health trajectories. Our identification strategy does not rely on state- or birth-cohort variation in legal abortion access, as the Swedish abortion policy has not changed since the early 1970s. Instead, we deal with unobserved heterogeneity in the abortion decision by applying a novel estimator – the GFE estimator – which allow for time-varying unobserved heterogeneity within groups of individuals (Bonhomme and Manresa, 2015). Our analysis is carried out in Sweden, a Northern-European country with virtually no restrictions on either abortion or contraception. This minimizes the potentially confounding effects of stigmatization on mental health.

Our joint analysis of several other risky health behaviors and abortion highlights the importance of accounting for dynamic unobserved heterogeneity. More specifically, we show that it is not sufficient to control for other behaviors in conventional individual-specific fixed-effects estimation, as these may be driven by a similar underlying process of decision-making.

With our theoretical model we show that heterogeneity in the degree of present bias is sufficient to explain heterogeneity in mental health trajectories. Using non-standard time-preferences is motivated by a large literature in behavioral- and health economics (for comprehensive reviews see [Cawley and Ruhm \(2011\)](#) in health economics; [Gruber \(2000\)](#) and [Frederick et al. \(2002\)](#) in behavioral economics). [Gruber and Köszegi \(2001\)](#) is an early, highly influential paper showing that inconsistent time preferences can generate economic models which rationalize risky health behaviors. Among adolescents, present-biased preferences have been analyzed in the context of smoking or alcohol consumption ([Sutter et al., 2013](#)), and risky sexual behavior ([Chesson et al., 2006](#)). Our theoretical model combines these insights, and links them to results generated by a novel econometric estimation approach to illustrate the evolution of mental health among young women.

Finally, our study adds to a growing literature on the relationship between preferences, non-cognitive skills, and mental health. As pointed out by [Currie \(2020\)](#) mental health issues are an important determinant of human capital development as they can reflect deficits in non-cognitive skills. Using a low-dimensional model of cognitive and non-cognitive skills, [Heckman et al. \(2006\)](#) show that non-cognitive skills play a substantial role in explaining adolescents' decisions to engage in risky behavior, such as marijuana use or illegal activities. Studying the relationship between time-inconsistent preferences, non-cognitive skills, and depression, ([Cobb-Clark et al., 2020](#)) show that self-control problems are strongly correlated with non-cognitive skills such as the internal locus of control and partly explain the depression gap in risky health behaviors among adults.<sup>7</sup> While we cannot incorporate a link between non-cognitive skills and present biased preferences, our theoretical model illustrates how mental health develops as a consequence of dynamic decisions under preference heterogeneity.

Our work has several implications. First, the precisely estimated null-effect of abortion on mental health indicates that an abortion from an unintended pregnancy has no detrimental effect on mental health. Thus, mental health problems should not be used to justify policies that impose restrictions on abortions. Second, restricting access to abortion seems inadvisable:

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<sup>7</sup>[Borghans et al. \(2008\)](#) provide an extensive discussion on how preferences and personality traits can be incorporated in economic models.

there is previous evidence on adverse economic consequences of restrictive abortion policies (see for instance [Felkey and Lybecker, 2018](#); [Lindo and Pineda-Torres, 2019](#); [Miller et al., 2020a,b](#)). Our null results imply that an unrestricted access to abortion does not lead to additional mental health costs. Taken together, restrictive abortion policies are thus unlikely to be welfare-enhancing. Third, the strong differences in the estimated unobserved heterogeneity profiles between and high-risk and the low-risk group of women imply that general mental health screenings are unlikely very effective tools for combating mental illness in adolescents. Instead, interventions should target high-risk women at younger ages, using tools similar as in [Alan and Ertac \(2018\)](#), to reduce self-control problems and the likelihood to develop serious mental illnesses.<sup>8</sup> By doing so, one may not only keep direct medical costs low but also reduce indirect costs of mental health disorders such as lower educational attainment and fewer earnings ([Biasi et al., 2019](#); [Currie et al., 2010](#); [Fletcher, 2010](#)).

The paper is organized as follows. Section 2 outlines the Swedish health care system and the abortion history in Sweden. In Section 3, we describe the data and measures for mental health and abortion. Section 4 introduces our empirical strategy, and Section 5 discusses our results. The theoretical model is presented in Section 6. Section 7 concludes.

## 2 Institutional background

### 2.1 The Swedish health care system

In Sweden, health care is mostly public and organized at the regional level. Within a region (such as Skåne), different municipalities have different health care centers (or primary care units) that house all out-patient care. Here, “out-patient” refers to all contacts with care providers that do not include at least one night’s stay, i.e., it refers to all ambulatory care, such as visits to physicians, dentists, therapists, emergency care units, specialized nurses, and physiotherapists. In addition, it covers consultations by telephone. Typically, a small

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<sup>8</sup>[Aizer \(2017\)](#) discusses different approaches of reducing self-control problems among adolescents. Based on a model of skill formation, she argues that programs to be effective should be implemented in pre-school age as it allows to control the environment interacting with such investments.

rural municipality has only one such health care center. Larger cities have multiple centers. “In-patient” care, as opposed to out-patient care, refers to visits or spells at health centers or hospitals that include at least one night’s stay. These are mostly overnight hospital treatments.

Every individual is assigned to exactly one health care center. This is usually the nearest center. Each center has a team of physicians, first-aid workers, and nurses. In case of a need to see a health care worker, including first-aid and emergency aid, an individual goes to the center and is helped by the next available appropriate health care worker. There is no path dependence in the identity of the health care worker across consecutive contacts. For a given contact reason, on a given day, incoming individuals are dealt with sequentially by the first available health care workers. Workers in the health care sector (from nurses to hospital specialists) are regional civil servants. The health care system is funded through a proportional regional tax on income. Health care usage is free, except for a small deductible which in our observation window is capped at about 80 Euro per adult person per year.

## **2.2 Abortions in Sweden**

In Sweden, abortions were first legalized by the Abortion Act of 1938, guaranteeing access to abortions for restricted cases. The act states that pregnancies may be terminated if the child’s birth results in danger to the mother’s life or health, or if the child is expected to have severe malformations, insanity, or mental deficiencies (Glass, 1938). The act was further amended in 1946 to allow abortions on social medical grounds. The current version of the abortion act took effect in January 1975. It grants access to abortions on request until week 18 without any restrictions. Importantly, minors do not require parental consent to receive an abortion (Socialstyrelsen Sweden, 2010, 2020). Thus, the decision to terminate a pregnancy is solely made by the pregnant woman regardless of her age.

In 1992, Sweden approved the “abortion pill” (mifepristone) for use in medical abortions. This drug allows terminating a pregnancy at an early stage (earlier than 49-56 days after conception) without a hospital stay (Jones and Henshaw, 2002). Between weeks 9–13, abortions are conducted through surgical intervention. After week 13, an overnight stay at

the hospital is required. Since the mid-1990s, the emergency contraceptive pill (ECP) also known as the morning-after pill, is available in Sweden. In 2001, the ECP was approved to over-the-counter (OTC) purchase (Guleria et al., 2020). Figure B.1 in Appendix B illustrates the aggregate time trends in abortions by gestation week and age groups for the region of Skåne and the whole of Sweden. There is a trend to substitute later abortions (week 9-11) with earlier abortions (before week 9) regardless of age. Besides, there is neither a discernible trend nor a discontinuity around the date of OTC availability.

In 1999, 26.3 out of 1,000 women had an abortion in the age group 20–24, and 19.0 out of 1,000 women aged 19 and below. These numbers increased over the years to about 34.7 and 24.4 abortions per 1,000 women in respective age groups. The overall number is just slightly lower in Skåne than in the whole of Sweden but with 33.9 per 1,000 women between age 20–24 and 22.3 abortions per 1,000 women aged 19 and below still very high (Socialstyrelsen Sweden, 2020).

These abortion rates are higher than in other developed countries, especially among young women (Haegle, 2005). Figure 1 compares abortion rates and alternative birth outcomes among adolescents in Sweden to those in the US, a country in which access to abortion is more restrictive, at least in practice. Abortion rates are indeed higher in Sweden. However, teenage birth- and miscarriage rates in Sweden are only about 15% and 20% of those in the US, respectively.

What would we expect from restricting access to abortions in Sweden? According to the literature, abortions could be substituted by increased birth rates, abstinence or higher contraceptive use. Fischer et al. (2018) show that proclivities for risky sexual behavior are not very sensitive to restrictive abortion policies, at least not among adolescents in the US. This is in line with the finding that abstinence-only sexual education programs are not effective in increasing abstinence (Santelli et al., 2017) or reducing birth rates (Kearney and Levine, 2015). Substituting abortions by higher contraceptive use is also unlikely to happen, at least not in Sweden where contraception is widely available and easily accessible. Sydsjö et al. (2014) find no evidence that increased contraceptive use is associated with lower rates of

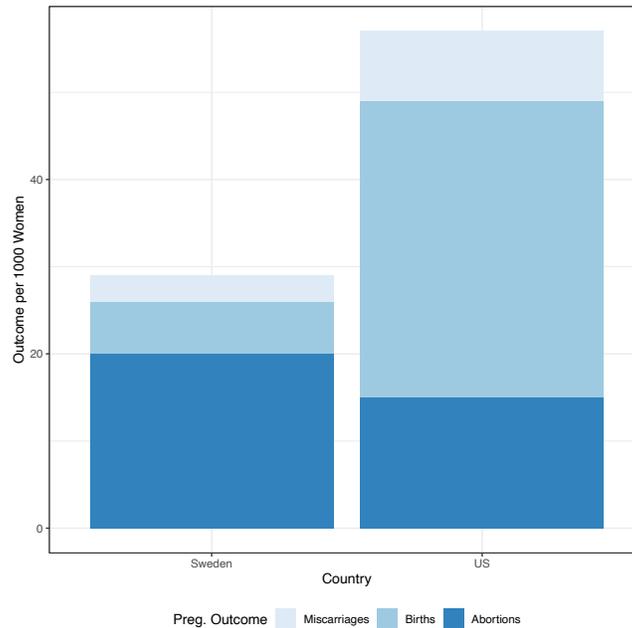


Figure 1. Pregnancy outcomes in Sweden and the US in 2010 for 15-19 year old women. Data from Sedgh et al. (2015).

induced abortions. Thus, introducing abortion restrictions in Sweden would most likely lead to an increase in teenage birthrates, all else being equal.

### 3 Data

#### 3.1 Description of different data registers

Our empirical analysis is based on a unique set of combined register data for the region of Skåne, the third most populous and southernmost region in Sweden. It consists of individual-level longitudinal records from the intergenerational register, the Skåne inhabitant register, the income tax register, the medical birth register, the in-patient register, and the out-patient register. The in-patient and out-patient registers are from the “patient administrative register systems” from Skåne, administrated by the Regional Council of Skåne. A unique feature of our health care data is that they contain detailed records of *all* occurrences of in-patient *and*

out-patient care for all inhabitants of the region.<sup>9</sup> The registers have previously been used in the literature (Nilsson and Alexander, 2018; Tertilt and van den Berg, 2015; van den Berg and Siflinger, 2018). The health care registers are collected at the regional level because they determine the monetary streams from the region to the various health care centers and hospitals. Moreover, these register data are collected on the national level as part of the so-called “National eHealth” endeavor to improve efficiency in health care.

In Sweden, each individual has a unique identifier that is used to record all in-patient and out-patient contacts with the health care system as well as the general public administration, tax boards, employment offices, and so on. We use this to merge the health care registers to a dataset containing information from several different national registers. This dataset covers all persons born in Sweden between 1940 and 1985, their parents, and all their children (Meghir and Palme, 2005). For all individuals aged 16 and above the data provide a rich set of annual socio-economic information, such as employment status, incomes by type, level of education, or marital status.<sup>10</sup> Further, the intergenerational register allows linking individuals to their children and their parents. The merged dataset contains about 1 million individuals, which is the vast majority of inhabitants of Skåne in 1999–2008.

The focus of our empirical analysis is young women. To this end, we construct an annual panel data set which comprises all women born between 1983 and 1985, and living in the region of Skåne between 1999–2008. We chose to select these birth cohorts because this guarantees that we observe women aged 16 to 23 years in all periods.

### **3.2 Diagnosis variables & abortions**

We define individual measures for mental health and abortions using ICD-10 diagnosis codes. Chapter five of the ICD-10 catalog comprises codes that are used to diagnose mental and

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<sup>9</sup>A small number of health care providers (notably dentists) are private. The patient registers are organized by the public/private distinction. PASiS register contains all publicly provided in-patient and out-patient care, whereas PRIVA contains all privately provided care. The information in PASiS and PRIVA includes dates of admission and discharges, as well as detailed diagnoses and DRG-based costs.

<sup>10</sup>The LISA registers for the years 2007 and 2008 were not available at the time at which we applied for and received the data. Variables from the LISA register for the year 2003 are not provided to us. See *Statistics Sweden (2016)* for a detailed description of the LISA register.

behavioral disorders. The chapter is divided into 11 sub-chapters which classify diagnoses into forms of organic mental disorders, schizophrenia, affective, somatoform disorders, behavioral or developmental mental disorders. Our main outcome of interest is the diagnosis of mental health conditions which we define using the codes F30-F39. These cover affective mood disorders (AMD), which is the most common psychiatric diagnosis in young adults. Besides depression, these codes also cover diagnoses of manic episodes, bipolar affective disorders, and persistent mood disorders.

Figure 2(a) shows the incidence of mental health diagnoses according to our definition per 1,000 women by age and birth cohort. Diagnoses on mental health issues are relatively low at age 16 with about 2-4 diagnoses per 1,000 women from these birth cohorts. From age 17 the numbers steadily increase to about 30 diagnoses in 1,000 women at age 23. Trends are similar across the three birth cohorts.

In the subsequent analysis, we define mental health problems as an absorbing state (cumulative): once a woman is diagnosed with a mental health condition, she is classified as ill for the remaining observation period. The definition is motivated by the medical literature which as shown that an episode of mood disorder, e.g. a depressive or manic episode, can last between a few months and up to several years (Eaton et al., 2008). While short-term recovery rates among adolescents are high, recurrence rates start to increase after 1-2 years, leading to recurrence rates of more than 50% in the longer run (e.g. after six years, see for instance Curry et al. (2011)).

To measure abortions we make use of pregnancy-related diagnosis codes in the ICD-10 catalog. The codes O00-O08 refer to pregnancies with abortive outcomes, with spontaneous and medical abortions being sub-codes. All abortive outcomes are classified as complete or incomplete, and with or without complications. We do not distinguish between these different categories. To classify medical abortions we use the code O04. It covers surgical extractions and pharmaceutical abortions as well as voluntary abortions and unwanted miscarriages that did not result in a spontaneous abortion. The code Z640 is used to define an unwanted pregnancy. It includes women who later on have an abortion, women who carried the pregnancy

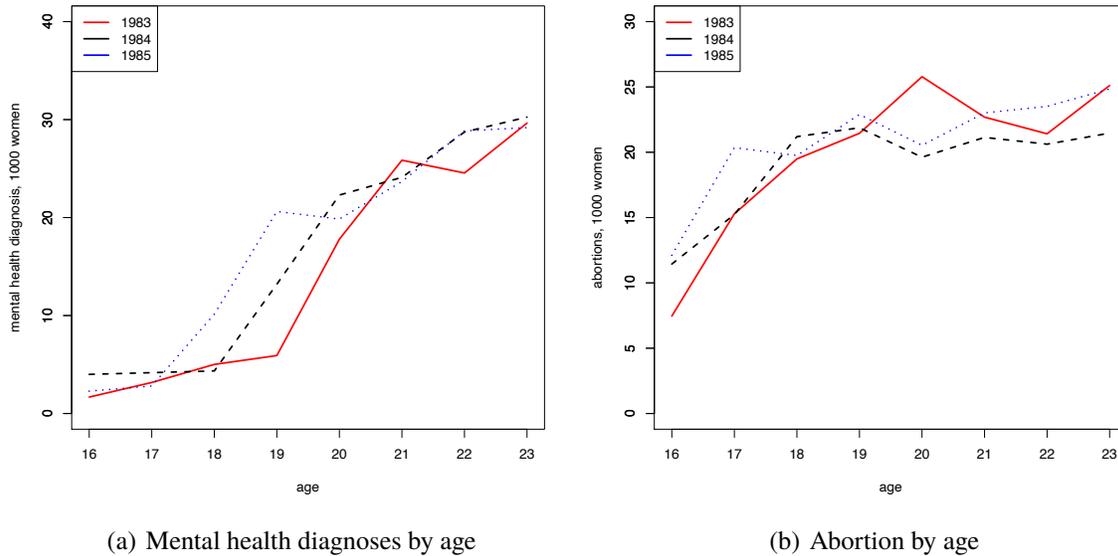


Figure 2. Mental health diagnoses and abortion by age and cohort.

to term, or women who had a spontaneous abortion. We combine these two codes to define our measure of abortion as medical abortions resulting from an unwanted pregnancy.<sup>11</sup>

Figure 2(b) shows the incidence of abortions per 1,000 women by age and birth cohort. The cohorts exhibit similar trends in abortion rates. The rates sharply increase between ages 16–18 but remain roughly constant at later ages. The numbers in Figure 2(b) closely correspond to the statistics reported by [Socialstyrelsen Sweden \(2020\)](#).<sup>12</sup>

Table 1 shows the descriptive statistics for all variables used in the empirical analysis. Our sample comprises 20,703 women at ages 16–23. The average age of women in our sample is 19.5 years. Women are on average born in 1984 which implies that our birth cohorts are of similar size. As expected for such a young sample of women, most women are single, about 20% are employed, and less than 30% hold a college degree. The annual rate of abortions is about 2% and the incidence of mental health problems per year is about 1.6%.

<sup>11</sup>We do this to distinguish the abortion choice from an involuntary termination of pregnancy. The ICD-10 also classifies spontaneous abortions/miscarriages (code O03). These include women who would have had an abortion but had a miscarriage first, as well as women who would have carried a pregnancy to term. Since we are not interested in the involuntary termination of pregnancies, we will ignore them.

<sup>12</sup>Figure B.3 in Appendix B plots the number of abortions after an unwanted pregnancy per woman in our age group. About 82% of women receive one abortion between age 16–23, about 14% receive two abortions, and about 3% receive three. Less than 1% of women in this age group undergo four or more abortions.

Table 1. Descriptive statistics for the three birth cohorts comprising our sample

	$N \times T$	mean	sd	min	max
<i>Mental health diagnoses (AMD) and abortion</i>					
Cum. mental health diagnoses (absorbing state)	146,833	.032	.175	0	1
Mental health diagnoses (non-absorbing state)	136,108	.016	.126	0	1
Abortion	136,108	.020	.140	0	1
<i>Individual characteristics of women</i>					
Single	134,464	.989	.104	0	1
Married	134,464	.010	.100	0	1
Employed	134,464	.213	.410	0	1
Log annual earnings	134,177	7.91	4.414	0	14.020
College degree	146,802	.284	.451	0	1
Age	165,624	19.5	2.291	16	23
Birth year	165,624	1984	.816	1983	1985
Year	165,624	2003	2.432	1999	2008
<i>Individual characteristics of women's mother</i>					
Employed	134,117	.837	.370	0	1
College degree	156,760	.364	.481	0	1
Log annual earnings	134,064	10.696	3.963	0	15.193
Birth year	164,992	1955	5.153	1933	1970
Married	134,117	.655	.475	0	1
Log disposable family income	133,937	12.863	.656	0	18.479
<i>Individual characteristics of women's father</i>					
Employed	131,163	.846	.361	0	1
College degree	144,968	.420	.494	0	1
Log annual earnings	131,110	11.04	4.071	0	16.396
Birth year	164,512	1952	5.833	1917	1969

The numbers refer to 10.6% of women who had an abortion in age 16-23 years. The fraction of women with diagnoses of mental health problems in this age group is 6.5%. Since our main estimation strategy requires a balanced data set, we will replace missing values with zero and construct two censoring indicators: one to flag missing observation periods and one to flag other missing values. This balancing procedure provides us with a final sample of  $N \times T = 165,624$  observations.

We also compare the incidence rates of mental health diagnoses by (non)abortive outcomes. Figure 3 shows the fraction of women who were ever diagnosed with mental health

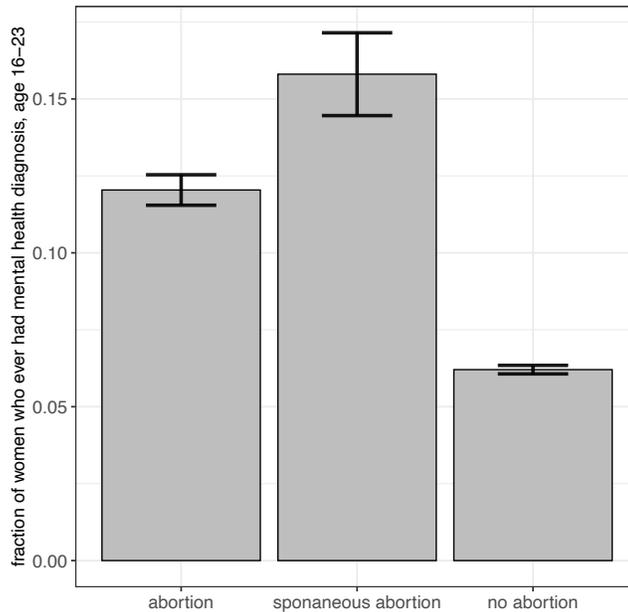


Figure 3. Mean fraction of women ever diagnosed with affective mood disorder by (non)-abortive events.

problems for women who had an abortion after an unwanted pregnancy, experienced a miscarriage, or never had any abortion. Women with abortions are approximately twice as likely to be diagnosed with mental health problems compared to women without any abortions. The highest incidence of mental health problems have women who experienced a miscarriage. Figure 3 suggests that there is a relationship between having an abortion and mental health conditions.

## 4 Empirical strategy

In the following section, we present the empirical strategy to estimate the causal effect of abortion on mental health. We discuss the shortcomings of well-established linear methods, such as OLS with time-constant, individual-specific fixed-effects, and discuss the GFE estimator that allows for a causal interpretation of the quantity of interest in this setting. In Section 4.3 we discuss the identifying assumptions of the GFE estimator in a potential outcome framework to compare it with the differences-in-differences (DiD) estimation, one of

the most frequently used estimators for causal inference in applied microeconomics.

A linear model that links an abortion  $A_{it}$  and a cumulative mental health diagnosis  $M_{it}$  is

$$M_{it} = \xi A_{it} + \tilde{\mathbf{x}}'_{it} \boldsymbol{\gamma} + \alpha_{it} + \varepsilon_{it}, \quad i = 1, \dots, N; t = 1, \dots, T, \quad (1)$$

where  $\tilde{\mathbf{x}}'_{it}$  comprises covariates for woman  $i$  and her parents, and  $\varepsilon_{it}$  is an idiosyncratic error term with  $\mathbb{E}[\varepsilon_{it}] = 0$  and  $\text{Cov}(\mathbf{X}, \boldsymbol{\varepsilon})$ .  $\alpha_{it}$  is an unobserved individual-specific fixed-effect that varies across age. The parameter of interest is  $\xi$  which captures the association of having an abortion  $A_{it}$  from an unwanted pregnancy and the incidence of a mental health diagnosis  $M_{it}$ .

Under the assumption that  $\alpha_{i0} = \alpha_{i1} = \dots = \alpha_{iT}$  for all individuals  $i = 1, \dots, N$ , i.e. the individual unobserved heterogeneity  $\alpha_{it}$  is time constant,  $\xi$  from Equation (1) can be consistently estimated using a standard estimator with individual-specific, time-constant fixed-effects. Time-constant unobserved heterogeneity would mean that decisions affecting both mental health development and abortion probabilities are independent over time. If this assumption is not satisfied, the resulting parameter estimates are biased. In our application, it seems plausible that  $\alpha_{it}$  is dynamic: abortions from an unwanted pregnancy are outcomes of decisions made by women. These depend on past decision-making and are determined by preferences. It implies that selection into unwanted pregnancies followed by an abortion is likely dynamic, and a standard individual-specific fixed-effects model fails to estimate a causal effect of abortion on mental health. Formally, for two time periods  $t = 0, 1$ , this implies that for two types of individuals,  $j$  and  $k$ , with  $\alpha_{j0} > \alpha_{k0}$ , we get  $\alpha_{j0} - \alpha_{k0} < \alpha_{j1} - \alpha_{k1}$ . In general, however, an unobserved time-varying  $\alpha_{it}$  is indistinguishable from the unobserved  $\varepsilon_{it}$  without making further assumptions.

#### 4.1 Time varying grouped fixed-effects estimator (GFE)

One potential solution to the problem described above is proposed by [Bonhomme and Manresa \(2015\)](#). They suggest clustering individuals with similar unobserved characteristics into a finite number of groups. This implies that women belonging to the same group share

the same age profile of unobserved heterogeneity,

$$M_{it} = \xi A_{it} + \tilde{\mathbf{x}}'_{it} \boldsymbol{\gamma} + \alpha_{g_{it}} + \varepsilon_{it}, \quad (2)$$

where  $\alpha_{g_{it}}$  represents time-varying, group-specific unobserved heterogeneity term for  $g \in \{1, \dots, G\}$  groups. The error term  $\varepsilon_{it}$  may contain an individual-specific, time-constant fixed-effect  $\alpha_i$ , such that  $\mathbb{E}[\varepsilon_{it} | \alpha_i] = 0$ . We write Equation (2) more compactly by defining a parameter  $\boldsymbol{\theta} = (\xi, \boldsymbol{\gamma})$  and a vector of regressors,  $\mathbf{x}_{it} = (A_{it}, \tilde{\mathbf{x}}_{it})$ ,

$$M_{it} = \mathbf{x}'_{it} \boldsymbol{\theta} + \alpha_{g_{it}} + \varepsilon_{it}, \quad i = 1, \dots, N, t = 1, \dots, T. \quad (3)$$

The GFE estimator is defined as the solution to

$$(\hat{\boldsymbol{\theta}}, \hat{\boldsymbol{\alpha}}) = \underset{(\boldsymbol{\theta}, \boldsymbol{\alpha}) \in \Theta \times \mathcal{A}^{GT}}{\operatorname{argmin}} \sum_{i=1}^N \sum_{t=1}^T (M_{it} - \mathbf{x}'_{it} \boldsymbol{\theta} - \alpha_{\hat{g}_i(\boldsymbol{\theta}, \boldsymbol{\alpha})t})^2, \quad (4)$$

where  $\hat{g}_i(\boldsymbol{\theta}, \boldsymbol{\alpha})$  is the optimal group assignment determined by

$$\hat{g}_i(\boldsymbol{\theta}, \boldsymbol{\alpha}) = \underset{g \in \{1, \dots, G\}}{\operatorname{argmin}} \sum_{t=1}^T (M_{it} - \mathbf{x}'_{it} \boldsymbol{\theta} - \alpha_{g_{it}})^2.$$

For a given number of groups  $G$ , the estimator assigns individuals to groups via clustering and estimates the coefficients  $\hat{\boldsymbol{\theta}}$  as well as the group profiles  $\hat{\boldsymbol{\alpha}}_{g_{it}}$  in an iterative procedure.<sup>13</sup> Standard errors are clustered at the individual level and obtained from analytical expressions in [Bonhomme and Manresa \(2015\)](#).

In our main specification, we will also account for individual-specific, time-constant unobserved heterogeneity  $\alpha_i$  by applying time demeaning. Thus, the solution is given as

$$(\hat{\boldsymbol{\theta}}, \hat{\boldsymbol{\alpha}}) = \underset{(\boldsymbol{\theta}, \boldsymbol{\alpha}) \in \Theta \times \mathcal{A}^{GT}}{\operatorname{argmin}} \sum_{i=1}^N \sum_{t=1}^T (\dot{M}_{it} - \dot{\mathbf{x}}'_{it} \boldsymbol{\theta} - \alpha_{\hat{g}_i(\boldsymbol{\theta}, \boldsymbol{\alpha})t})^2, \quad (5)$$

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<sup>13</sup>A well-known issue with the GFE estimator is that it is sensitive to the choice of initial values. We validate our results by randomly varying the seed and thus the starting values. Our results are robust to different seed choices.

where  $\dot{M}_{it} = M_{it} - \bar{M}_i$  and  $\dot{x}_{it} = x_{it} - \bar{x}_i$ , and  $\bar{M}_i, \bar{x}_i$  are time-demeaned quantities.

## 4.2 Choosing the number of groups

The GFE estimator requires the researcher to choose the correct number of groups. Ideally, we infer the optimal number of groups by data-driven methods, such as an information criterion as proposed by [Bonhomme and Manresa \(2015\)](#). Yet, selecting the correct number of groups is non-trivial as the choice of the information criterion heavily depends on the data generating process. This is a well-known problem whenever information criteria are used for model selection, see for example [Choi and Jeong \(2019\)](#) and [Bai and Ng \(2002\)](#). Essentially, the number of groups selected by an information criterion is a function of the penalty. The size of the penalty depends on the number of groups  $G$ , the number of individuals  $N$ , the number of covariates  $K$ , and the number of time periods  $T$ . This implies that there is no single criterion that will select the correct number of groups in any potential application.

[Bonhomme and Manresa \(2015\)](#) suggest the following Bayesian information criterion (BIC):

$$BIC(G) = \frac{1}{NT} \sum_{i=1}^N \sum_{t=1}^T \underbrace{\left( M_{it} - \mathbf{x}'_{it} \hat{\theta}^{(G)} - \hat{\alpha}_{g_{it}}^{(G)} \right)^2}_{\text{objective}} + \underbrace{\hat{\sigma}^2 \frac{GT + N + K}{NT} \ln(NT)}_{\text{penalty}}, \quad (6)$$

where the penalty is the second part of Equation (6).  $\hat{\sigma}^2$  is the estimated error variance which is calculated using  $G_{max}$ , the maximum feasible number of groups chosen by the researcher.

In our simulation exercise, we show that this BIC chooses the correct number of groups if  $N$  is not much larger than  $T$ . Otherwise, this BIC does not sufficiently discriminate between the different number of groups.<sup>14</sup> As alternative criterion, we use a BIC with a modified penalty  $\hat{\sigma}^2 \frac{G(T+N-G+K)}{NT} \ln(NT)$  that assigns more weight to the number of groups. However, this alternative criterion tends to penalize too much. We will use both information criteria in

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<sup>14</sup>As discussed in [Bonhomme and Manresa \(2015\)](#), this BIC estimates  $G$  consistently only if  $N$  and  $T$  tend to infinity at the same rate. As this is not the case in our application, this BIC might overestimate the true number of groups.

conjunction with other sensitivity analyses to pick the right number of groups.

In recent work on factor models, Moon and Weidner (2015) show that under certain conditions, namely that both  $N$  and  $T$  grow to infinity, the limiting distribution of the least-squares estimator of the parameter of interest is robust to the inclusion of *additional* factors. While investigating whether the results also apply to the GFE estimator might be useful, such a theoretical exploration is beyond the scope of this paper.

### 4.3 Discussion of the assumptions on time-varying unobserved heterogeneity

Before we present our estimation results, we discuss the identifying assumptions on individual time-varying unobserved heterogeneity which are needed to identify causal effects with the GFE estimator. We compare these assumptions to those of a differences-in-differences (DiD) estimation strategy, and we discuss situations in which these assumptions are more or less likely to be maintained. For this, we make use of the potential outcome framework notation.

Let  $\tilde{\alpha}_{it}$  be the time-varying unobserved treatment assignment such that  $\tilde{\alpha}_{it} = \alpha_{it} - \alpha_{g_{it}} - \alpha_i$ . Here,  $\alpha_{g_{it}}$  refers to the group-specific profiles introduced in Section 4.1, and  $\alpha_i$  is an individual-specific, time-constant fixed-effect. The key assumption of the GFE estimator for identifying the effect of interest, i.e. abortion  $A_{it}$ , is that the expected value of mental health given that no abortion has taken place, denoted as  $M_{it}(0)$ , should be the same regardless of the “treatment assignment” (selection into abortion), and given covariates, time and *unobserved group effects*  $\alpha_{g_{it}}$ . Broadly speaking, this assumption states that the  $\alpha_{g_{it}}$  captures the relevant time-varying variation that determines dynamic selection into treatment.

$$\mathbb{E} [M_{it}(0) \mid \alpha_{g_{it}}, \alpha_i, \mathbf{x}_{it}, \tilde{\alpha}_{it}] = \mathbb{E} [M_{it}(0) \mid \alpha_{g_{it}}, \alpha_i, \mathbf{x}_{it}], \quad (7)$$

where  $\mathbf{x}_{it}$  may contain covariates as well as a time indicator. Under the assumption that the effect of interest is constant, we can write the conditional expectation of observation  $i$  under treatment as

$$\mathbb{E} [M_{it}(1) \mid \alpha_{g_{it}}, \alpha_i, \mathbf{x}_{it}, \tilde{\alpha}_{it}] = \mathbb{E} [M_{it}(0) \mid \alpha_{g_{it}}, \alpha_i, \mathbf{x}_{it}] + \xi. \quad (8)$$

Further assuming a linear functional form of the conditional mean function leads to

$$M_{it} = \alpha_i + \alpha_{g_{it}} + \xi A_{it} + \mathbf{x}'_{it} \boldsymbol{\gamma} + \varepsilon_{it}. \quad (9)$$

The DiD estimator relies on a similar set of assumptions about the potential outcomes under treatment  $M_{it}(1)$  and under non-treatment  $M_{it}(0)$ . The main difference to the GFE estimator is the restrictions imposed on time-varying unobserved heterogeneity. The reason is that the identification of a causal effect with the DiD estimator relies on group differences in a before and after comparison (conditional on treatment assignment).

Suppose we have two groups  $s \in \{0, 1\}$ , where  $s = 0$  indicates the control group and  $s = 1$  is the treatment group. We assume that

$$\mathbb{E}[M_{it}(0) \mid \alpha_{st}, \mathbf{x}_{it}, \tilde{\alpha}_{it}] = \mathbb{E}[M_{it}(0) \mid \alpha_{st}, \mathbf{x}_{it}], \quad (10)$$

where  $\alpha_{st}$  is time-varying unobserved heterogeneity that can only vary between the treatment and the control group, i.e.  $\tilde{\alpha}_{it} = \alpha_{it} - \alpha_{st}$ . The difference in these time trends is constrained to be a constant. This restriction is necessary to fulfill the parallel-trends assumption used in DiD estimation. In practice, this restricts all individuals in the treatment and the control group to have parallel unobserved heterogeneity profiles.

The crucial difference in the identifying assumptions of the GFE estimator and the DiD estimator is the restriction placed on the time-varying unobserved heterogeneity parameter: the GFE estimator puts no restrictions on  $\alpha_{g_{it}}$  but restricts the number of distinct profiles. By contrast, the DiD estimator allows all individuals to be on individual slopes, but only within treatment and control group.<sup>15</sup>

The identifying assumptions of the DiD estimator discussed above apply in a situation in which the treatment assignment is random. In the case of non-random treatment assignment

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<sup>15</sup>Even if the treatment assignment was random, the time-varying unobserved heterogeneity in the population is restricted by the parallel trends assumption. Consider a situation in which both, treatment and control group, contain two different types of individuals with non-parallel unobserved heterogeneity profiles in different proportions. In this case, the DiD estimator fails to recover the true treatment effect, even with random treatment assignment, because the parallel trends assumption would be violated. For further discussion of the identifying assumptions of the DiD estimator see [Lechner et al. \(2010\)](#)

and when using individual-level panel data, the identifying assumptions of the DiD estimator and the standard individual-specific fixed-effects estimator are essentially identical.

## 5 Results

In the following subsections, we present the main results of our estimation procedure: First, we present our findings for the effect of abortion on mental health,  $\widehat{\xi}$ , obtained from different estimators. Second, we determine the optimal number of groups and analyze the group-specific unobserved heterogeneity age profiles  $\widehat{\alpha}_{\hat{g}t}$ .

Because this method is relatively new and has not been used extensively in empirical work, we provide a detailed simulation framework.<sup>16</sup> In Appendix C we introduce a data generating process according to Equation (2) that matches certain key characteristics of our data. We will refer to our simulation exercise when interpreting certain aspects of our estimation strategy, and we also validate specification choices made in the empirical model.

### 5.1 Effect of abortion

We estimate our parameter of interest  $\xi$  from Equation (1) by using three different estimators that impose different assumptions on  $\alpha_{it}$ : the OLS estimator without individual-specific fixed-effects,  $\widehat{\xi}^{\text{OLS}}$ , i.e.  $\mathbb{E}(\alpha_{it} = 0)$ ; the OLS estimator with individual-specific fixed-effects  $\widehat{\xi}^{\text{FE}}$  i.e.  $\alpha_{it} = \alpha_i$ ; and the GFE,  $\widehat{\xi}_G^{\text{GFE}}$  with  $G = 2, 3, 4$ . Note that we control for year fixed-effects in all specifications. Thus, the  $\widehat{\xi}_G^{\text{GFE}}$  for  $G = 1$  is equivalent to the estimate  $\widehat{\xi}^{\text{FE}}$ .

Figure 4 reports the coefficient estimates for  $\widehat{\xi}$  and the associated 95% confidence intervals. The OLS estimate  $\widehat{\xi}^{\text{OLS}}$  is large and statistically significant, which is in line with positive associations found in previous studies.  $\widehat{\xi}^{\text{FE}}$  is substantially smaller by about 70%, but still positive and statistically significant.<sup>17</sup> The results for  $\widehat{\xi}_2^{\text{GFE}}$ ,  $\widehat{\xi}_3^{\text{GFE}}$  and  $\widehat{\xi}_4^{\text{GFE}}$  are extremely close to zero and precisely estimated. We attribute this precision to the large differences in the

<sup>16</sup>We also use our simulation framework with fixed  $T$  to validate the inference results in our setting, as the asymptotic results in Bonhomme and Manresa (2015) only apply for large  $N, T$ .

<sup>17</sup>We also estimated OLS models without and with individual-specific fixed-effects for anxiety disorders. The estimated coefficient for abortion is about two-third of the magnitude of the one obtained when using AMD as outcome. The results can be found in Table A.2 in Appendix A.

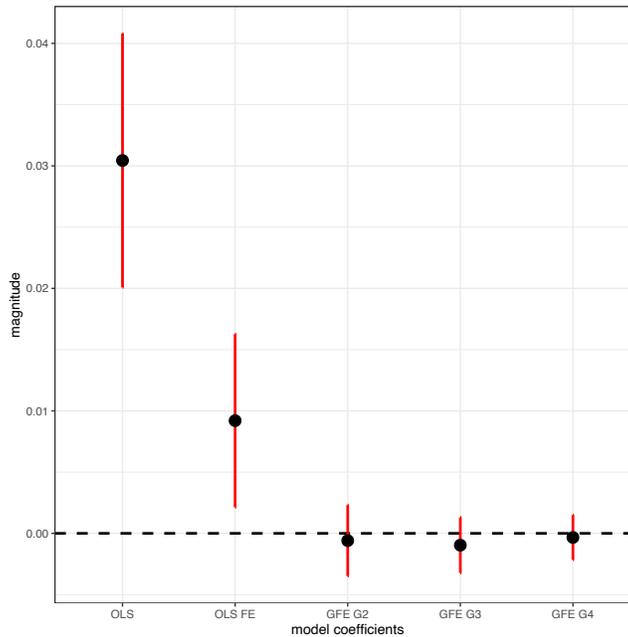


Figure 4. Plot of the estimated coefficients for the OLS estimator, the OLS estimator with individual-specific fixed-effects, and the GFE estimator with  $G = 2, 3, 4$  groups.

unobserved heterogeneity profiles. Accounting for these different patterns drastically reduces the overall variance. Adding additional groups (beyond what we consider reasonable from our criteria), further reduces the size of the estimated standard errors. We observe a similar pattern in our simulations. We attribute this to the behavior of the estimator when additional groups are added: according to the objective that the estimator tries to minimize, we group individuals with similar time-varying unobserved characteristics, thus mechanically reducing the variation in the model.

The GFE estimates are smaller than  $\hat{\xi}^{\text{FE}}$  by at least a factor of 15, and the signs are negative.<sup>18</sup> Moreover, all GFE point estimates are very similar and they all lie within each other's 95% confidence intervals. This indicates that our estimated coefficients of interest are not very sensitive to the chosen number of groups. Our simulation results confirm this behavior: as soon as we choose the correct number of groups, the estimated coefficient shrinks to zero and remains stable around zero when adding superfluous groups (see Figure C.2).

<sup>18</sup>The 95% confidence intervals for the GFE estimates and the OLS estimate with individual-specific fixed-effects,  $\hat{\xi}^{\text{FE}}$ ,  $([0.01627, 0.00213])$  only marginally overlap for  $\hat{\xi}_2^{\text{GFE}}$   $([0.00331, -0.00348])$ . We do not find any overlap in the 95% confidence intervals for the estimated coefficients with  $G = 3$  and  $G = 4$  with that of  $\hat{\xi}^{\text{FE}}$ .

Apart from the statistical significance, our results have meaningful implications for the expected incidence of mental health diagnoses resulting from an abortion. Given the incidence of mental health problems in our sample across all ages, the estimated OLS coefficient implies an increase in the probability of being diagnosed with mental health conditions from 3.2% to 6.3%. According to OLS, mental health problems almost double. While the OLS estimate with individual-specific fixed-effects is much smaller, it nonetheless predicts a significant increase in mental health problems by about 29%, to 4.1%. By contrast, the GFE estimator even predicts a marginal *decrease* in the incidence of mental health problems, regardless of the chosen number of groups. For two groups, for instance, mental health problems reduce to 3.1%, corresponding to a reduction of 1.9%. The estimated coefficients with more groups are similar in magnitude.<sup>19</sup>

The results reported in this section illustrate that allowing for group-specific time-varying unobserved heterogeneity absorbs considerable variation that may otherwise be attributed to the effect of abortion on mental health. It points to endogenous selection into abortions which is dynamic in nature. Ignoring time-varying unobserved heterogeneity leads to a considerable overestimation abortion effect.

## 5.2 Time profiles of group-specific unobserved heterogeneity

We next address the question of the optimal number of groups. First, we descriptively show how individuals are assigned to groups for an increasing number of groups. Second, we compute the BIC with two different penalty terms and discuss coefficient behaviors for different number of groups. Finally, we present the estimated profiles of unobserved heterogeneity.

Figure 5 shows how the GFE assigns women to groups for  $G = 1, 2, 3, 4, 5$ . White bars are nodes and correspond to group  $g$  for each  $G$ . The gray-shaded connections illustrate the flows of women from one group to another group when  $G$  is increased.  $G = 1$  is the

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<sup>19</sup>For  $G = 3$  the estimated GFE coefficient for abortions,  $\xi$ , is -0.0010 which leads to a reduction in mental health problems of 3.1%. The smallest coefficient and thus the smallest change is obtained for  $G = 4$ . According to the estimated coefficient, mental health problems reduce by 0.9% at the sample mean of mental health problems. The estimated coefficients for all models can be found in Table A.3 in Appendix A.

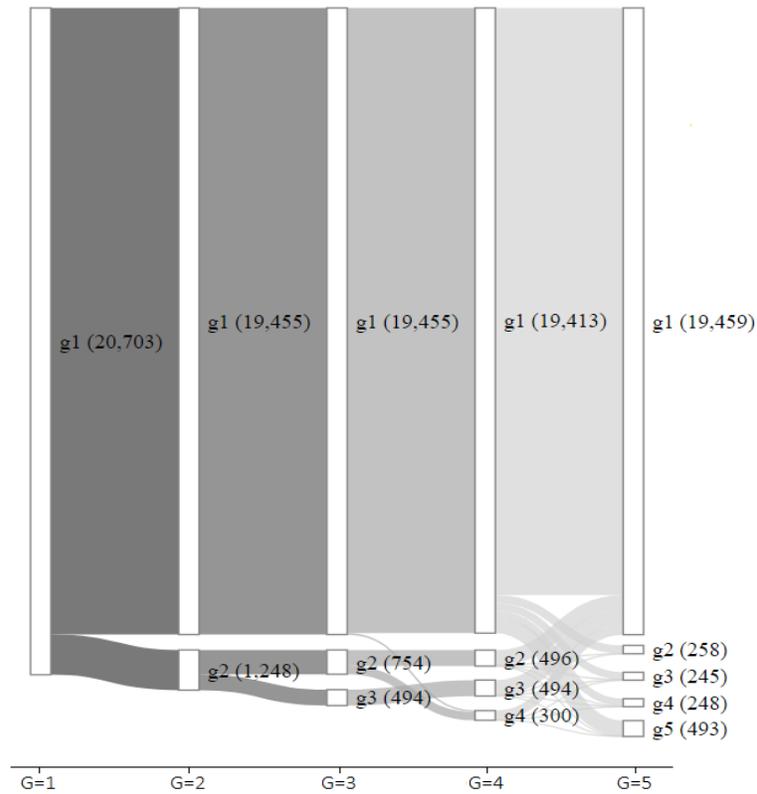


Figure 5. Group membership assignment of women for  $G = 1, 2, 3, 4, 5$ .

Note: white bars denote the groups  $g$  for each  $G$ . The number of groups  $g$  increases with  $G$ . Numbers in parentheses correspond to the number of women per group  $g$  for 20,703 women.

situation without group-specific unobserved heterogeneity, i.e. all women are on individual time-constant trajectories. For  $G = 2$ , the majority of women (93.9%) are assigned to group  $g_1$ , while a bit more than 6% of women are assigned to group  $g_2$ . Increasing  $G$  to three, only results in a split of group  $g_2$  into two subgroups denoted by  $g_2$  and  $g_3$ . The GFE does not reassign any women from group  $g_1$ . For  $G = 4$ , a new group  $g_4$  is formed which consists mainly of women who have formerly been in group  $g_2$ . A small fraction of women (42 women) is reassigned from  $g_1$  to the new group  $g_4$ . For  $G = 5$ , the group assignment becomes rather chaotic. Women from former groups  $g_2$ ,  $g_3$ , and  $g_4$  are now assigned back to group  $g_1$ . Moreover, women that formerly were in different groups for  $G = 4$  now rejoin the same group, and women from the group  $g_1$  are now assigned to groups  $g_2$ – $g_5$ . This movement pattern indicates that for  $G = 5$  groups cannot be well-separated anymore which

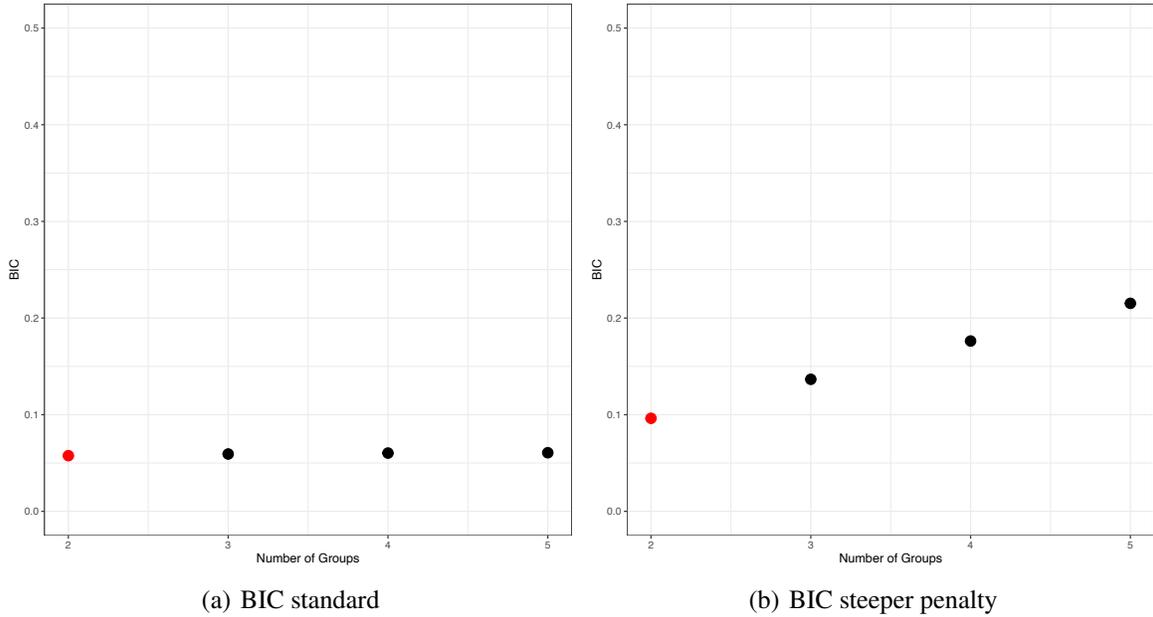


Figure 6. Results for the two information criteria for  $G = 2 - 5$  and  $G_{max} = 10$ .

is one of the assumptions on the GFE estimator (see [Bonhomme and Manresa \(2015\)](#), and [Figure C.3](#) in [Appendix C](#)). We, therefore, conclude that the GFE estimator cannot deal with more than four groups in our application.

We next assess the optimal number of groups  $G$  using the two BIC discussed in [Section 4.2](#).<sup>20</sup> The results are shown in [Figure 6](#). In our application, both criteria are minimized at  $G = 2$  (highlighted in red). The value of the standard BIC hardly varies with increasing  $G$ , which makes a clear selection difficult ([Figure 6\(a\)](#)). The BIC with the steeper penalty increases sharply in  $G$  and is unambiguously minimized at  $G = 2$  ([Figure 6\(b\)](#)). As seen in our simulations, however, this only demonstrates that the performance of these information criteria heavily depends on true DGP. Therefore we interpret the results with caution.

In our application, we observe that the coefficient estimates are stable after we reach  $G = 2$  (see [Figure 4](#)). Importantly, we observe a similar behavior in our simulations after reaching the true number of groups. Combining the insights from group movements, the BIC, and the coefficient behavior, we conclude that the true number of groups is likely to be equal to two.

<sup>20</sup>We set  $G_{max} = 10$ , which is the highest number of groups where the algorithm converges reliably.

Figure 7 presents the estimated unobserved mental health profile over age,  $\hat{\alpha}_g$ , for  $G = 2$ .<sup>21</sup> The results for  $G = 3$  and  $G = 4$  can be found in Figure B.4 in Appendix B. In the subsequent interpretation, we focus on the results for  $G = 2$ . The profiles in Figure 7 exhibit a large degree of heterogeneity across groups. The solid line represents the unobserved mental health trajectory that is practically flat. This suggests that women who share this profile have a low unobserved mental health risk at age 16, but they also are on a low-risk path. Hence, we call this group of women the “low-risk” group. The dashed line shows the trajectory of unobserved mental health risk which rises steeply with age. We refer to this group of women as the “high-risk” group. While profiles between the two groups do not strongly differ at age 16, they diverge starkly throughout adolescence and early adulthood.<sup>22</sup> Our findings suggest that there is a considerable amount of unobserved time-varying heterogeneity. Importantly, these time profiles differ greatly in both intercept and slope.

The assignment to these groups of unobserved heterogeneity is not only conditional on abortions, but also on all observables that are included in the GFE estimation. While this implies that our estimated profiles are net of these, it may nevertheless be informative to descriptively compare our two groups along with these observed individual characteristics. Table A.4 in Appendix A shows that women in the high-risk group have on average a lower socioeconomic family background, such as lower parental earnings and higher parental unemployment rates. Moreover, these women were slightly younger when they decided to terminate an unwanted pregnancy.

### 5.3 Discussion: Alternative Dynamic Processes

The findings in the previous sections suggest that the relationship between abortion and mental health disappears, once we allow for group-specific time-varying unobserved heterogeneity. As shown in Figure 7 the unobserved heterogeneity profiles diverge with age. If these

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<sup>21</sup>Here we present the mental health profiles without accounting for individual unobserved heterogeneity, for reasons of interpretability. The figures with the respective profiles net individual-specific fixed-effects can be found in Figure B.5 in Appendix B.

<sup>22</sup>Note that the high-risk group profile remains rather flat after age 22. We believe that this is due to a lack of variation in our sample.

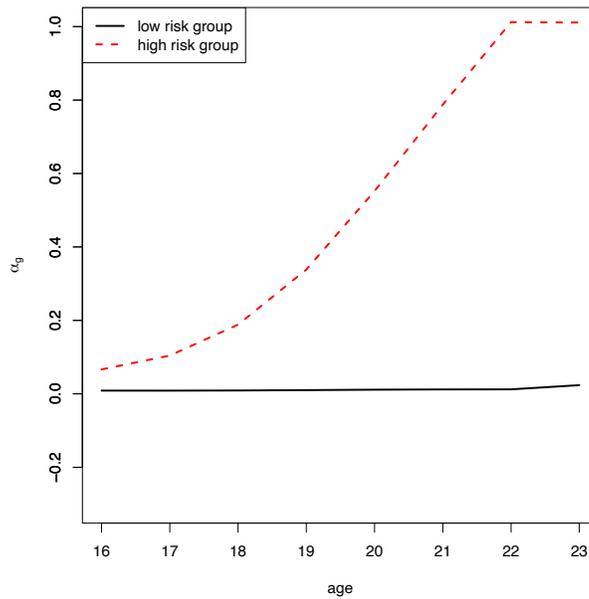


Figure 7. Latent, age dependent mental health curves for  $G = 2$  groups.

profiles represent the true underlying heterogeneity process, any DiD estimator even under randomized treatment assignment would fail to recover the true treatment effect, because the parallel trends assumption would be violated.

Our empirical results could potentially be explained by dynamic processes other than age-varying unobserved heterogeneity, such as age-dependent coefficients and dynamic treatment effects. If the effect of abortion on mental health differs by age, e.g. earlier abortions have larger effects than later abortions, then we might attribute the effect of these early abortions to age-varying unobserved heterogeneity. As a result, we would underestimate the effect of abortion on mental health. To explore this hypothesis, we estimate the individual-specific fixed-effects model allowing for age-dependent effects. As shown in Table A.5 in Appendix A, we do not find significant differences between having an abortion at an earlier age versus a later age.

Our estimated coefficients could also be biased if the effect of abortion on mental health occurs with some lag. As shown by Figure B.2 in Appendix B, mental health develops rather smoothly around the abortion event regardless of the birth cohort. This suggests that

dynamic abortion effects are not a significant concern. To explore this further, we incorporate a number of lags in the independent variable in our simulation set-up and re-estimate our main model with individual-specific fixed-effects as well as with GFE (see Figure C.6 in Appendix C). We find that the bias in the estimated GFE coefficients *increases* with the number of groups. Moreover, the fixed-effects estimator is *at least* as biased as the GFE estimator. These coefficient behaviors seem to be inconsistent with the results in our application (see Figure 4).

Formally, we cannot distinguish between these different dynamic processes discussed above. Nevertheless, the previous exercises are suggestive that time-varying unobserved heterogeneity is non-negligible in our application. A more formal discussion on how to conceptually differentiate between these processes is beyond the scope of this paper.

#### **5.4 Sources of unobserved heterogeneity: Abortions, unwanted pregnancies and other risky behavior**

A natural question that arises from our estimation results is what factors are captured by the profiles of unobserved mental health risk. While there may be several answers to this question, one potential explanation is that the estimated profiles proxy risky behaviors that are common among young women. Here, this behavior is most likely unprotected sexual activity. Besides, the estimated profiles may also capture other risky health behaviors that are correlated with risky sexual behavior, such as drug- and alcohol abuse (Cawley and Ruhm, 2011). If this were the case, adding such behaviors to our specifications would affect the estimated association between abortion and mental health. Alternatively, these trajectories could represent choice processes among different types of behaviors. In that case, observable risky behaviors would be outcomes of a similar choice process. This would imply that (1) the association of abortion and mental health should be robust to the inclusion of other risky behaviors; (2) the GFE coefficient estimate for abortions should be unaffected, but the estimates for other behaviors should behave similarly as for abortions; (3) other risky behaviors should be contemporaneously associated with the estimated profiles of unobserved

heterogeneity. In the following section, we investigate how several other observed risky health behaviors are related to abortions and mental health, and to what extent they contribute to the estimated group-specific time-varying unobserved heterogeneity.

#### **5.4.1 Mental health, abortions and other risky health behaviors**

The most important determinant for having an abortion is a woman's decision to engage in unprotected sexual activities, resulting in an unwanted pregnancy. Ex-ante, it is not clear whether an unintended pregnancy reflects such a choice, including careless use of birth control, or whether it is the result of a random failure in contraception or sexual assault. In the latter case, unwanted pregnancies would not result from engaging in unprotected sex. Consequently, such pregnancies are not in the choice set that is captured by time-varying unobserved heterogeneity linking abortion decisions and mental health trajectories.<sup>23</sup>

If the abortions we consider in our analysis are the outcome of a woman's decision to engage in unprotected sex, then this may not only result in unwanted pregnancies but also in other byproducts of unprotected sex. In our data, we observe a few other measures that have been used in the literature to assess risky sexual and health behaviors among youths (e.g. Cawley and Ruhm, 2011; Markowitz et al., 2005; Mulligan, 2016): chlamydia infections and STD screenings as measures of risky sexual behavior, and excessive alcohol consumption as one non-sexual health behavior.<sup>24</sup>

Table 2 indicates that other measures of risky health behaviors are strongly correlated with abortions as well as with unwanted pregnancies. As shown by Column (1), women who had a chlamydia infection between age 16–23 have an 11.5 percentage points higher likelihood for an abortion, implying an increase in abortions of more than 130% at the sample

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<sup>23</sup>Even if all women face the same failure probability of contraception, one may still find a positive correlation between abortion probabilities and mental health diagnoses. For instance, if women with mental health problems start to have sex at earlier ages than women without mental health problems, or if they have more sex. Then this correlation would not be indicative of risky sexual behavior.

<sup>24</sup>Chlamydia is the most frequently observed STD among youths, in particular among young women aged 15-25 years (see Danielsson et al. (2012), European Centre for Disease Prevention and Control (2020) for Sweden, and Centers for Disease Control and Prevention (2019) for the US). To measure chlamydia infections, we use the ICD-10 codes A55 and A56. To measure whether a woman received an STD screening other than HIV, we make use of the ICD-10 code Z113. Excessive alcohol consumption is measured using the ICD-10 code F110, recording "acute drunkenness (in alcoholism)".

Table 2. Correlations between abortions and other risky health behavior among women aged 16–23 years

	Ever had	
	an abortion from an unwanted pregnancy	an unwanted pregnancy
Ever had a chlamydia diagnosis	0.145*** (0.014)	0.153*** (0.015)
Ever had a STD screening	0.014* (0.007)	0.025*** (0.008)
Ever had a diagnosis of acute drunkenness	0.098*** (0.024)	0.125*** (0.025)
Sample mean in %	10.6	12.6
Number women	20,703	
Number observations	165,624	

Standard errors clustered on the individual level; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ ; OLS regressions with individual-specific FE of ever had an unwanted pregnancy on ever had a diagnosis on chlamydia/had an STD screening/diagnosis on excessive drinking. Control variables: woman: relationship status (single, in a relationship), log earnings, college degree, employed; mother: log earnings, employed, college degree, relationship status; father: log earnings, employed, college degree; log household disposable income; year fixed-effects, municipality FE, year of birth FE for woman/mother/father; indicator missing observations.

mean. Women are 1.4 percentage points more likely to have an abortion if they had an STD screening, which corresponds to an increase of 13% at the mean. Abortions are also strongly correlated with excessive alcohol consumption increasing the probability of abortion by 9.8 percentage points or 92% at the sample mean. Column (2) shows the correlations between unwanted pregnancies and other health behaviors. The correlations are somewhat stronger than for abortions but otherwise very similar and highly statistically significant.

Given the strong correlation between abortions and other risky health behaviors, we next examine whether these health behaviors are omitted control variables, or whether they are outcomes of a similar choice as abortions.<sup>25</sup> We re-estimate our main specifications of mental health and abortions (Figure 4) and gradually add excessive drinking, chlamydia infections, and STD screenings. If the correlation between mental health and abortions

<sup>25</sup>Abortions might follow a different selection process than unwanted pregnancies. In our sample, 82% of women with an unwanted pregnancy undergo an abortion. When controlling for unwanted pregnancies the abortion effect becomes small and insignificant. Selection into abortion does not seem to be different from selection into an unwanted pregnancy.

Table 3. Estimated correlations between mental health development and risky behavior

	Woman has mental health problems at 16–23				
	OLS FE				GFE
	(1)	(2)	(3)	(4)	(5)
Abortion	0.0092** (0.0036)	0.0091** (0.0036)	0.0092** (0.0036)	0.0091** (0.0036)	–0.0006 (0.0015)
Acute drunkenness	0.0084 (0.0159)			0.0083 (0.0159)	–0.0009 (0.0038)
Chlamydia infection		0.0051 (0.0051)		0.0039 (0.0050)	0.0007 (0.0023)
STD screening			0.0084** (0.0035)	0.0081** (0.0035)	0.0006 (0.0017)
Number women			20,703		
Observations			165,624		

Standard errors clustered on the individual level; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ ; Columns (1)–(4): OLS regression of cumulative mental health diagnoses on abortion and current risky health behavior, controlling for individual-specific FE. Column (5): GFE estimation with  $G = 2$  groups and individual-specific FE. Control variables: woman: relationship status (single, in a relationship), log earnings, college degree, employed; mother: log earnings, employed, college degree, relationship status; father: log earnings, employed, college degree; log household disposable income; year fixed-effects, municipality FE, year of birth FE for woman/mother/father; indicator missing observations.

can be explained by other risky behaviors, we should expect a change in the estimated coefficient on abortion. Columns (1)–(4) in Table 3 display the estimated coefficients for specifications with individual-specific fixed-effects (OLF FE). All estimated correlations are positive, suggesting that engaging in one of these behaviors increases the probability of being diagnosed with mental health problems. The estimated coefficients on excessive drinking and STD screenings are similar in magnitude as the coefficient on abortion but only the one on STD screenings is significantly different from zero. Importantly, adding these health behaviors as control variables barely changes the estimated association between abortion and mental health. Column (5) presents the results when using the GFE estimator with two groups. Controlling for other behaviors does not change the estimated effect of abortion on mental health. The estimated coefficients for other health behaviors are between 5 to 10 times smaller compared to Column (4).

The relationship between mental health and abortions could be influenced by past- rather

than current health behaviors (e.g. [Elkington et al., 2010](#); [Hallfors et al., 2005](#)). Table A.6 in Appendix A shows the results when using past instead of contemporaneous diagnoses on acute drunkenness, chlamydia infections, and STD screenings. While we find strong and significant associations between mental health and past health behaviors, the estimated coefficient on abortion is again robust to adding them as controls across all specifications. Overall, our results suggest that the health behaviors we observe are unlikely to cause the omitted variable bias that can be observed in OLS regressions with individual-specific fixed-effects. Rather, they appear to be outcomes of similar unobserved decisions as abortions from unwanted pregnancies.

#### 5.4.2 Unobserved heterogeneity profiles and risky health behaviors

To strengthen our interpretation, we investigate whether the estimated profiles of unobserved mental health risk,  $\hat{\alpha}_{gt}$ , are predictive for the other observed risky health behaviors. To this end, we regress STD screenings, chlamydia infections, and excessive drinking on  $\hat{\alpha}_{gt}$  and covariates, and plot the group-specific predictions against  $\hat{\alpha}_{gt}$ . Figure 8 shows the predicted probabilities for the two groups of unobserved mental health risk.<sup>26</sup> In the high-risk group, the probability of STD screenings and chlamydia infections steeply increases with  $\hat{\alpha}_{gt}$ . By contrast, the predictions are almost flat in the low-risk group. For diagnoses on alcohol intoxication, the differences in predicted probabilities across groups are small, and they are even slightly negative for high-risk women. An explanation for this finding is that diagnoses on excessive drinking are made at earlier ages than diagnoses on risky sexual behavior. For instance, [Marcus and Siedler \(2015\)](#) find a declining pattern of hospitalization after alcohol intoxication among women from age 15. While we do not observe diagnoses on excessive drinking before the age of 16, we observe a similar decline in the incidence of excessive drinking between ages 16–20 (see Figure B.6 in Appendix B). Besides, Figure 8 shows that women with a high unobserved mental health risk have higher probabilities of engaging in risky sexual behavior, which confirms the suggested correlation between risky behaviors and

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<sup>26</sup>The respective coefficient estimates can be found in Table A.7 in Appendix A.

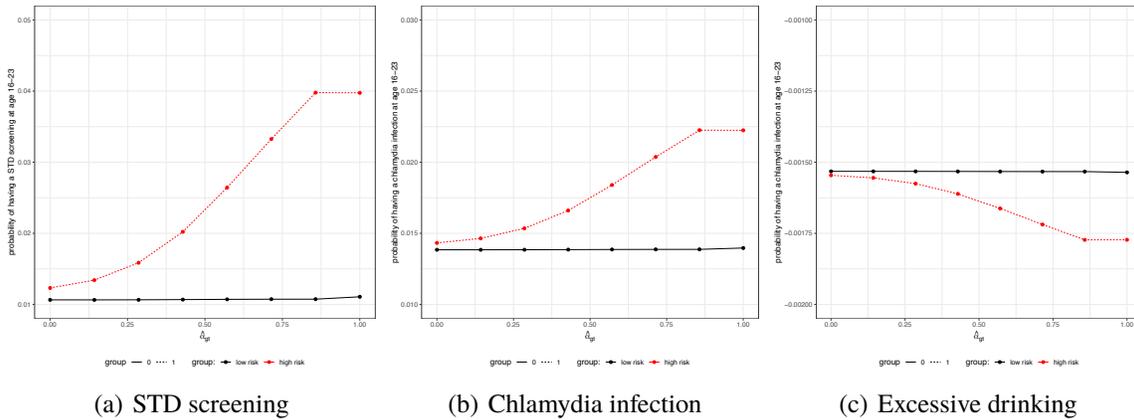


Figure 8. Predicted risk of being diagnosed with a certain health behavior by group-specific unobserved mental health risks.

unobserved heterogeneity.

The findings strengthen our interpretation that time-varying unobserved heterogeneity captures the decision process of women to engage in risky behavior. Researchers commonly fail to observe these decisions. Instead, one observes actual behaviors which are outcomes of these decisions. However, we have shown controlling for an array of observable behaviors is not sufficient to obtain an unbiased estimate, at least not in our application. Here, the GFE estimator seems necessary as it can account for the unobserved process of decision-making in estimation.

## 6 A stylized framework of mental health and risky behavior

The empirical results obtained in Section 5.4 point towards the following explanation: women differ considerably in their decisions to engage in risky health behaviors which are reflected in differences in estimated profiles across groups. One reason for this large amount of group-specific heterogeneity could be that women are endowed with different preferences, leading to differences in dynamic decision making and thus to different mental health trajectories. O'Donoghue and Rabin (2001) discuss the role of time-inconsistent preferences for risky behaviors, such as unprotected sexual activities, observed among youths. Present-biased preferences make unprotected sex today more likely since teenagers weigh the benefits today

much higher than potential future costs (Levine, 2001). This behavioral bias affects all types of risky and non-forward-looking behaviors, not only risky health behavior, e.g., educational choices like dropping out of school. A recent study by Cobb-Clark et al. (2020) suggests that self-control problems explain differences in the correlation between depression and risk-taking behaviors, such as an unhealthy diet or a lack of exercising.

To complement our empirical analysis, we formulate a theoretical model of risky choices and mental health in which we allow for both to be endogenous. Engaging in risky behavior leads to a short-term benefit but harms the trajectory of mental health. At the same time, mental health conditions change the preferences for risky behavior and thereby shape its time paths. To allow for heterogeneity in decisions across the two groups of women, we introduce non-standard time preferences. Women with estimated profiles of high unobserved mental health risks are endowed with a high degree of present bias, thus over-weighting the current pleasure compared to future mental health risks. Women with a flat estimated risk profile have preferences that are close to time consistency. As such, our model closely follows the literature in behavioral economics.

Our model offers an interpretation for differences in inter-temporal decision-making across the two groups of women and the consequences on mental health development. Of course, it is not the only model that could be used to explain the observed patterns. For instance, heterogeneity in decision-making and mental health could be driven by heterogeneity in impatience reflected in different time discounting without present bias. By introducing a present bias, we stress the importance of *now* regardless of what happens in the future (Laibson, 1997; O'Donoghue and Rabin, 2015). It seems plausible to assume that a woman who is at a party and meets a handsome guy, decides in the “heat-of-the-moment” to have unprotected sex even though she may be aware of future costs, e.g. in mental health. However, if you ask her whether she should behave this way at the next party, she certainly would say no. The behavioral literature discusses several other models that incorporate anomalies in discounted utility, such as “visceral influences”, habit formation or projection bias (for a discussion, see Frederick et al., 2002). Our exploratory theoretical analysis does not aim at

differentiating between these different models.

## 6.1 A DGP for mental health, risky behavior and abortion

We formulate a data generating process (DGP) of risky decision making, abortion, and mental health. For simplicity, we assume that latent mental health  $M$  is generated as follows,

$$M_{it+1} = \psi M_{it} + \zeta \rho_{it} + \epsilon_{it}. \quad (11)$$

Woman  $i$ 's mental health at age  $t + 1$ ,  $M_{it+1}$ , is determined by her mental health state at age  $t$ , her risky choice,  $\rho_{it}$ , and a iid mental health production shock  $\epsilon_{it} \sim N(0, \sigma_\epsilon)$ . To keep the model tractable we ignore covariates that may influence mental health.<sup>27</sup>

Abortion probabilities do not enter Equation (11) directly but are correlated with risky choices. We model the probability of having an abortion  $A$  at age  $t$ ,  $A_{it}$ , as a function of unobserved risky choices  $\rho_{it}$  (systematically varying with  $A_{it}$ ), and an idiosyncratic error (e.g.  $\eta_{it} \sim N(0, \sigma_\eta)$ ),

$$Pr(A_{it} = 1) = \mathbb{E} [\mathbb{I}(\rho_{it} + \eta_{it} > 0)], \quad \text{increasing in } \rho_{it}. \quad (12)$$

Together with Equation (11), Equation (12) implies that a regression of observed  $M_{it}$  on  $A_{it}$  would produce a spurious correlation in a regression even with individual-specific fixed-effects. Since we only observe the abortion but not women's decision to engage in unprotected sex,  $\rho_{it}$ , we could interpret the observed abortion as a signal for risky decision making.

## 6.2 Preferences

We assume that women are sophisticated decision-makers, which implies that they know about their self-control problems when making choices in the future (O'Donoghue and Rabin, 1999). At each age  $t$ , a woman enjoys flow utility,  $u(\rho_t, M_t)$ , which is a function of

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<sup>27</sup>In our empirical analysis we proxy this latent mental health status by observed diagnoses.

mental health and choices regarding risky behavior.<sup>28</sup> We assume  $u$  to be a constant relative risk aversion (CRRA) utility function with mental health dependent risk aversion minus quadratic mental health costs,<sup>29</sup>

$$u(\rho_t, M_t) = \text{sgn}(1 - a - cM_t) \cdot \rho^{(1-a-cM_t)} - bM_t^2. \quad (13)$$

The parameter  $a$  is the baseline level of risk aversion, which is modified by the mental health dependent term  $c \cdot M_t$ . This implies that women with positive values in  $c$  become more risk-averse as their mental health problems increase, thus allowing for additional heterogeneity in preferences.<sup>30</sup> The second term captures direct costs of mental health problems, which are determined by  $b$ .

For simplicity, we formulate an infinite horizon decision problem and focus on the first eight periods, corresponding to ages 16–23 in our data. Realized future utility at age  $t$  is given by

$$u(\rho_t, M_t) + \beta \sum_{\tau=t+1}^{\infty} \delta^{\tau-t} u(\rho_{\tau}, M_{\tau}),$$

where the first term,  $u(\rho_t, M_t)$  is the current flow utility. The second term aggregates future flow utility using  $\beta - \delta$  discounting.  $\delta$  is the usual exponential discount factor. The parameter  $\beta$  induces the self-control problem. For  $\beta = 1$ , the model is one with standard exponential discounting. For  $0 < \beta < 1$ , a woman exhibits some degree of present bias.

For a current level of mental health  $M_t$ , the problem a woman solves at age  $t$  is given by

$$\max_{\rho_t} u(\rho_t, M_t) + \mathbb{E}_t \left[ \beta \sum_{\tau=t+1}^{\infty} \delta^{\tau-t} u(\rho_{\tau}^*(M_{\tau}^*), M_{\tau}^*) \right]. \quad (14)$$

<sup>28</sup>We subsequently suppress the individual subscript  $i$  for ease of notation.

<sup>29</sup>In the CRRA term, we multiply by the sign of the exponent rather than dividing by it which would be more common. This is useful when calibrating/estimating the model because in this formulation small changes in risk aversion do not have a strong impact on the levels of utility.

<sup>30</sup>In earlier versions of their paper, Cronin et al. (2020) allowed preferences to vary with current mental health. They found relatively little heterogeneity and thus removed this dependence from preferences, effectively setting  $c = 0$ . We also have experimented with the simpler and more common CRRA specification. The results are similar but not quite as good.

The  $\rho_t^*$  are the optimal decisions of the future selves as functions of current mental health.  $M_t^*$  is the mental health trajectory that arises starting at  $M_t$  when choosing  $\rho_t$  at age  $t$  and choosing  $\rho_\tau^*(M_\tau^*)$  at later ages. Solving this problem for every possible value of  $M_t$  pins down the decision function  $\rho_t^*$ .  $\mathbb{E}_t$  denotes the conditional expectation given information at age  $t$ .

We solve the model outlined above by using backward recursion. This is a variation of classical dynamic programming that takes into account the time-inconsistency introduced through  $\beta$ .<sup>31</sup> As in the empirical analysis, we allow for two groups of women with different unobserved mental health risks. We assume two different degrees of present bias, defined by two parameters  $\beta_1, \beta_2 \in (0, 1)$  (Laibson, 1997), while all other parameters are assumed to be the same across groups.

To estimate the behavioral parameters of interest, we match the model moments to the observed moments for group-specific unobserved mental health trajectories. We perform a simulated annealing procedure (e.g. Goffe et al., 1994) to avoid being stuck in local minima of the mean-squared objective function and refine the solution using the Nelder-Mead algorithm.

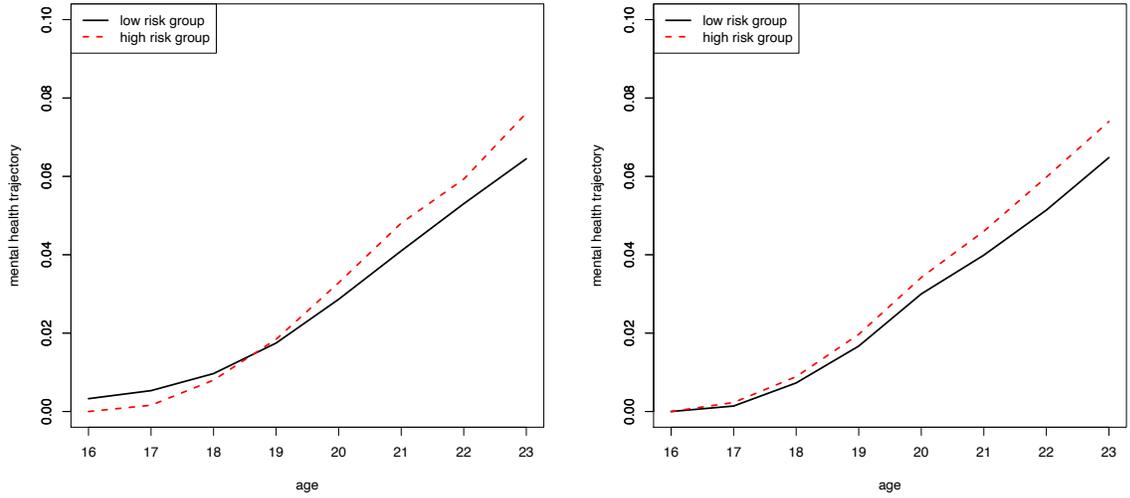
### 6.3 Results

Figure 9(a) plots the average mental health trajectories for the two groups of women in our sample. Women belonging to the group with a high unobserved mental health risk exhibit a steeper observed mental health trajectory than women with low unobserved risk. In the high-risk group, about 7.6 of 100 women have been diagnosed with mental health problems by age 23. Women with low unobserved mental health risk are on a somewhat lower mental health trajectory. On average, about 6.4 of 100 women have received a mental health diagnosis by age 23. This amounts to a difference in mental health problems of about 19% at age 23.

Table 4 displays the estimated parameters obtained from moment matching. We find a clear difference in the estimated present bias between the two groups. For the low-risk group, the estimate for  $\beta_1$  is close to one, indicating that this group of women has almost no present bias. The high-risk group exhibits a considerable degree of present bias of about  $\hat{\beta}_2 = 0.598$ .

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<sup>31</sup>Details about the model solution can be found in Appendix D.



(a) Data moments of mental health trajectory

(b) Simulated moments of mental health trajectory

Figure 9. Mental health trajectories from the data and the model.

The estimated period (yearly) discount factor  $\hat{\delta}$  is 0.925 and well in the range commonly found in the literature. For the low risk group, the estimated one-year discount factor is  $\hat{\beta}_1 \cdot \hat{\delta} = 0.944$ . The corresponding estimated one year discount factor for the high risk group is  $\hat{\beta}_2 \cdot \hat{\delta} = 0.553$ . Both values are well in the range found in the literature (see [Laibson \(1997\)](#) or [Frederick et al. \(2002\)](#)). These estimates imply that women in the high-risk group discount the future much more strongly than women in the low-risk group. Thus, high-risk women may be much more prone to trading off short-term utility obtained from risky behavior, e.g. immediate sexual pleasure, against long-run (future) mental health deficits. As a result, these women face a more pronounced deterioration in mental health, see [Figure 9](#).

[Figure 9\(b\)](#) shows the group-specific mental health trajectories obtained from the estimated parameters.<sup>32</sup> While we cannot perfectly replicate the trajectories in the data i.e. the intersection at age 18–19, we do obtain a close match between the simulated trajectories and data moments. This suggests that heterogeneity in the present bias can generate most of the group-specific heterogeneity in observed mental health trajectories.

<sup>32</sup>Since we do not observe risky choices in our data, we cannot match corresponding moments and estimate the trajectories for risky choices.

Table 4. Estimated time preference parameters obtained from simulated method of moments

	$\hat{\beta}_1$	$\hat{\beta}_2$	$\hat{\delta}$
Estimates time preferences	1.021	0.598	0.925

Estimated parameters for time preferences obtained from final Nelder-Mead optimization after having applied simulated annealing (SA) for global optimization. For SA, we set the initial temperature to 1000 and the reduction of the temperature to 0.8. We set the number of inner loop iterations to 200. For more details about the SA procedure see [Husmann et al. \(2017\)](#). The full set of parameter estimates for time preferences, flow utility and mental health dynamics can be found in [Table A.8](#) in [Appendix A](#).

Figure 9(b) does not only illustrate the mental health trajectories for the high-risk group and the low-risk group. It also shows the counterfactual mental health trajectory for women in the high-risk group if they did not exhibit self-control problems. According to our numbers, mental health problems in the high-risk group could be reduced by about 19% by age 23 if women’s self-control problems could be reduced to the level of the low-risk group.<sup>33</sup> A recent study by [Alan and Ertac \(2018\)](#) investigates how an intervention in the classroom that aims at improving children’s patience and self-control affects inter-temporal decision making. One result of this study is that 9–10 year-old children who were identified as present biased in the baseline benefit the most from the intervention by delaying immediate gratification. The study finds that girls are particularly responsive to the intervention in the medium run, i.e. when they are 12–13 years old. [Alan and Ertac \(2018\)](#) do not consider risky health behavior as an outcome. Yet, such a classroom intervention could be a promising to also reduce risky health behaviors among adolescents by reducing self-control problems early on.

## 7 Conclusion

In this study, we use individual-level administrative records from Sweden and a novel grouped fixed-effects (GFE) estimator to estimate the effect of abortion on mental health in young women. The idea of the GFE estimator is that individuals who share similar unobserved

<sup>33</sup>We do a back-of-the-envelope calculation using the cost on individual inpatient and outpatient care contacts for affective mood disorders. From age 16–23, average mental health care costs per woman are about 389 USD in the low-risk group and about 1,517 USD in the high-risk group. Given the share of women in the low-risk (93.9%) and high-risk group (6.1%), the average costs are about 458 USD. These costs would be reduced by 69 USD or 15.1% per woman if high-risk women had the same mental health trajectory as low-risk women.

characteristics are clustered in groups. Within these groups, unobserved heterogeneity is allowed to vary with age. By applying the GFE, we estimate a precise null-effect of abortion on mental health. This result stands in sharp contrast to a positive and significant relationship between abortion and mental health obtained from OLS estimates with individual-specific fixed-effects stressing the importance to account for time-varying unobserved heterogeneity.

In our main specification with two groups, a small but significant share of women exhibits a high unobserved mental health risk. The majority of women have a low unobserved risk to develop mental health problems. We show that the estimated profiles likely capture decisions that result in risky health behaviors, such as unprotected sex or excessive drinking. As these decisions are generally unobserved by researchers, the GFE estimator is necessary to obtain an unbiased estimate of the parameter of interest. Based on these insights, we propose a model of risky choices and mental health. Heterogeneity in decision-making and mental health is generated by differences in self-control problems across groups. The estimated parameters from moment matching suggest a large degree of self-control problems in the group of women with high unobserved mental health risk. Our model can explain observed disparities in mental health trajectories across groups.

Our work has several implications. First, we show that an abortion from an unintended pregnancy does not lead to more mental health problems. Abortion opponents thus cannot use mental health problems as an argument for more restrictive abortion policies. Second, the estimated null-effects imply that there are no additional mental health care costs associated with abortion. Together with existing evidence on adverse economic outcomes, restrictive abortion policies thus are unlikely to be welfare-enhancing. Third, self-control problems and associated risky behaviors rather than abortions may trigger mental health problems. Thus, policymakers should find tools to identify and reduce self-control problems at early ages rather than providing cost-intensive general mental health screenings.

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## Appendix

### A Additional Tables

Table A.1. Age, children born, abortions, AMD diagnoses per 1,000 women in Skåne and calendar year, age 16-23 for birth cohorts 1983-1985

Calendar year	Age	Children born	Abortions	Mental health diagnoses
1999	16.00	0.93	7.46	1.68
2000	16.49	2.21	13.34	3.59
2001	16.98	3.43	15.54	3.80
2002	17.98	7.10	20.99	4.33
2003	18.99	11.04	22.43	13.66
2004	20.00	14.93	21.75	22.81
2005	20.99	17.67	21.02	22.92
2006	21.96	26.46	22.89	27.31
2007	22.50		22.50	29.18
2008	23.00		24.85	29.55

The number of children born in the region of Skåne is not available in our data for 2007 and 2008.

Table A.2. Association between abortion and anxiety disorders

	Anxiety disorder	
	OLS	FE
Abortion	0.027*** (0.005)	0.006* (0.003)
Constant	0.092*** (0.022)	0.093*** (0.010)
Number women	20,703	20,703
Observations	165,624	165,624

Standard errors clustered on the individual level; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ ; OLS regression of cumulative anxiety disorder diagnoses on abortion (Column (1)), and controlling for individual-specific fixed-effects (Column (2)); Control variables: woman: relationship status, log earnings, college degree, employed; mother: log earnings, employed, college degree, relationship status; father: log earnings, employed, college degree; log household disposable income; year fixed-effects, municipality FE, year of birth FE for woman/mother/father; indicator missing observations.

Table A.3. All estimated coefficients from the GFE estimator with  $G = 2, 3, 4$ , OLS without and with individual-specific, fixed-effects (OLS FE)

	GFE			OLS	OLS FE
	$G = 2$	$G = 3$	$G = 4$		
Abort	-0.0006 (0.0015)	-0.0010 (0.0012)	-0.0003 (0.0009)	0.0306*** (0.0053)	0.0092*** (0.0036)
Single	-0.0013 (0.0053)	0.0008 (0.0032)	-0.0014 (0.0025)	0.0383*** (0.0113)	0.0123 (0.0117)
Married	0.0001 (0.0063)	0.0034 (0.0043)	0.0015 (0.0031)	0.0507*** (0.0151)	0.0304** (0.0138)
College	0.0018 (0.0020)	0.0019 (0.0014)	0.0006 (0.0012)	0.0010 (0.0022)	0.0051 (0.0045)
Employed	-0.0009 (0.0013)	-0.0008 (0.0011)	-0.0005 (0.0009)	0.0035 (0.0035)	0.0015 (0.0025)
Log earnings	-0.0001 (0.0001)	-0.0001 (0.0001)	-0.0001 (0.0001)	-0.0017*** (0.0004)	-0.0008*** (0.0003)
Mother: employed	0.0025** (0.0012)	0.0010 (0.0010)	0.0005 (0.0007)	-0.0021 (0.0040)	-0.0006 (0.0045)
Mother: married	-0.0013 (0.0011)	-0.0015* (0.0008)	-0.0010 (0.0007)	-0.0104*** (0.0023)	0.0037 (0.0029)
Mother: log earnings	0.0000 (0.0001)	0.0000 (0.0001)	-0.0001 (0.0001)	-0.0011*** (0.0004)	-0.0005 (0.0004)
Mother: college degree				0.0001 (0.0018)	
Father: employed	0.0023* (0.0013)	0.0014 (0.0011)	0.0012 (0.0008)	-0.0023 (0.0041)	0.0012 (0.0029)
Father: log earnings	-0.0001 (0.0001)	0.0000 (0.0001)	0.0000 (0.0001)	-0.0011*** (0.0004)	-0.0009*** (0.0003)
Father: college degree				-0.0016 (0.0018)	
Household: log disp. income	0.0000 (0.0004)	0.0002 (0.0002)	0.0000 (0.0002)	-0.0001 (0.0010)	-0.0023*** (0.0009)
Censor: missing years	-0.0010 (0.0055)	0.0013 (0.0034)	-0.0007 (0.0028)	-0.0224* (0.0127)	-0.0225 (0.0125)
Censor: missing values	0.0019 (0.0039)	0.0031 (0.0022)	-0.0014 (0.0017)	-0.0042 (0.0081)	-0.0201 (0.0234)
Constant				0.0894*** (0.0337)	

Standard errors clustered on individual level; \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ ; Number of observations: 165,624; number women: 20,703; Additional controls: year fixed-effects, municipality FE, year of birth FE for woman/mother/father.

Table A.4. Comparison of socioeconomic characteristics of women, mothers and fathers by low risk group and high risk group

	Mean			<i>t</i> -statistic
	low risk group	high risk group	group differences	( <i>p</i> -value)
<i>A. Sample of women all ages</i>				
Mother: college degree	0.336	0.330	0.006	1.064 (0.287)
Father: college degree	0.360	0.371	-0.011*	-1.834 (0.067)
Mother: employed	0.843	0.833	0.010**	2.041 (0.041)
Father: employed	0.848	0.834	0.015***	3.173 (0.002)
Mother: log earnings	10.764	10.533	0.231***	4.420 (0.000)
Father: log earnings	11.060	10.884	0.176***	3.281 (0.001)
Mother: married	0.671	0.681	-0.010	-1.622 (0.105)
Father: married	0.687	0.693	-0.006	-0.971 (0.332)
Woman: single	0.990	0.987	0.003**	2.185 (0.029)
Woman: employed	0.207	0.211	-0.004	-0.808 (0.419)
Woman: college degree	0.307	0.348	-0.041***	-6.928 (0.000)
Woman: age at abortion	19.617	19.499	0.118	1.610 (0.108)
<i>B. Sample of women at age 16</i>				
Mother: college degree	0.333	0.319	0.014	0.811 (0.418)
Father: college degree	0.352	0.359	-0.007	-0.380 (0.704)
Mother: employed	0.844	0.809	0.035**	2.452 (0.014)
Father: employed	0.863	0.838	0.025*	1.863 (0.063)
Mother: log earnings	10.800	10.461	0.339**	2.314 (0.021)
Father: log earnings	11.225	11.049	0.176	1.212 (0.226)
Mother: married	0.698	0.718	-0.021	-1.271 (0.204)
Father: married	0.708	0.723	-0.016	-0.981 (0.327)

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ ; results obtained from *t*-test comparing sample means of the high risk group and the low risk group; group variances are assumed to be unequal; *p*-values refer to the alternative hypothesis that group differences are not equal.

Table A.5. The effect of abortion on mental health by age, OLS with individual-specific fixed-effects

	OLS FE
Abortion	0.005 (0.013)
Abortion × Age 17	-0.010 (0.018)
Abortion × Age 18	-0.001 (0.016)
Abortion × Age 19	-0.005 (0.015)
Abortion × Age 20	-0.003 (0.015)
Abortion × Age 21	-0.002 (0.016)
Abortion × Age 22	-0.007 (0.016)
Abortion × Age 23	0.021 (0.019)
Age 17	0.008*** (0.002)
Age 18	0.018*** (0.003)
Age 19	0.034*** (0.004)
Age 20	0.057*** (0.006)
Age 21	0.079*** (0.007)
Age 22	0.098*** (0.007)
Age 23	0.115*** (0.008)
Observations	17,584

Standard errors clustered on the individual level; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ ; OLS regression of cumulative mental health diagnoses on age-dependent abortion, controlling for individual-specific fixed-effects; Control variables: woman: relationship status, log earnings, college degree, employed; mother: log earnings, employed, college degree, relationship status; father: log earnings, employed, college degree; log household disposable income; municipality FE, year of birth FE for woman/mother/father; indicator missing observations.

Table A.6. Estimated impact of risky health behaviors in the past on mental health development

Before age $t$ , woman had	Woman has mental health problems at 16–23				
	OLS FE				GFE
	(1)	(2)	(3)	(4)	(5)
Abortion	0.009*** (0.004)	0.009** (0.004)	0.009** (0.004)	0.009** (0.004)	–0.0006 (0.0015)
Acute drunkenness	0.120*** (0.022)			0.118*** (0.022)	–0.0034 (0.0034)
Chlamydia infection		0.018*** (0.007)		0.013** (0.007)	–0.0003 (0.0021)
STD screening			0.017*** (0.004)	0.016*** (0.004)	0.0001 (0.0014)
Number women			20,703		
Observations			165,624		

Standard errors clustered on the individual level; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ ; Columns (1)–(4): OLS regression of cumulative mental health diagnoses on abortion and past risky health behavior, controlling for individual-specific fixed-effects. Column (5): GFE estimation with  $G = 2$  and individual-specific fixed-effects. Control variables: woman: relationship status, log earnings, college degree, employed; mother: log earnings, employed, college degree, relationship status; father: log earnings, employed, college degree; log household disposable income; year fixed-effects, municipality FE, year of birth FE for woman/mother/father; indicator missing observations.

Table A.7. Correlations between profiles of age-dependent unobserved heterogeneity and risky health behaviors

	unwanted pregnancy	STD screening	chlamydia infection	acute drunkenness
<i>A. OLS without individual-specific fixed-effects</i>				
$\hat{\alpha}_g$	0.029*** (0.004)	0.010*** (0.004)	0.008*** (0.003)	-0.0002 (0.0012)
Constant	0.010 (0.012)	0.068*** (0.016)	0.014* (0.007)	-0.0015 (0.0025)
<i>B. OLS with individual-specific fixed-effects</i>				
$\hat{\alpha}_g$	0.026*** (0.004)	0.009** (0.004)	0.007*** (0.002)	-0.0001 (0.001)
Number women		20,703		
Observations		165,624		

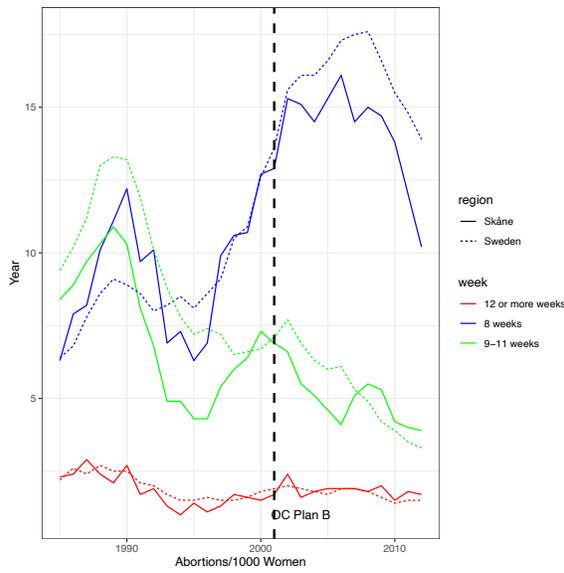
Standard errors clustered on the individual level; \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . OLS regression of current risky health behaviors on estimated profiles of unobserved mental health risk; Estimated profiles of unobserved heterogeneity  $\hat{\alpha}_g$  for  $G = 2$ ; Control variables: woman: relationship status, log earnings, college degree, employed; mother: log earnings, employed, college degree, relationship status; father: log earnings, employed, college degree; log household disposable income; municipality FE, year of birth FE for woman/mother/father; indicator missing observations.

Table A.8. Estimated parameters obtained from simulated method of moments (SMM)

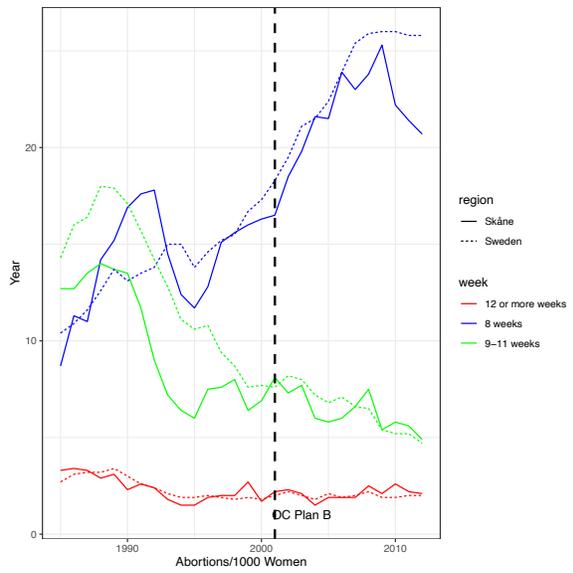
Time preferences			Flow utility			Mental health dynamics			
$\hat{\beta}_1$	$\hat{\beta}_2$	$\hat{\delta}$	$\hat{a}$	$\hat{b}$	$\hat{c}$	$\hat{\psi}$	$\hat{\sigma}_\epsilon$	$\hat{\xi}$	$\hat{\kappa}$
1.021	0.598	0.925	0.864	0.186	0.058	0.962	0.613	0.447	2.543

Estimated parameters for preferences, flow utility and mental health dynamics obtained from final Nelder-Mead optimization after having applied simulated annealing (SA) for global optimization; SA parameters: initial temperature = 1000, reduction of temperature = 0.8, number inner loop iterations = 200; More details about SA procedure can be found in [Husmann et al. \(2017\)](#).

## B Additional figures



(a) Abortions by gestation week, age -19



(b) Abortions by gestation week, age 20-24

Figure B.1. Aggregate trends in abortions by gestation week around the introduction of over the counter emergency contraception (OC Plan B).

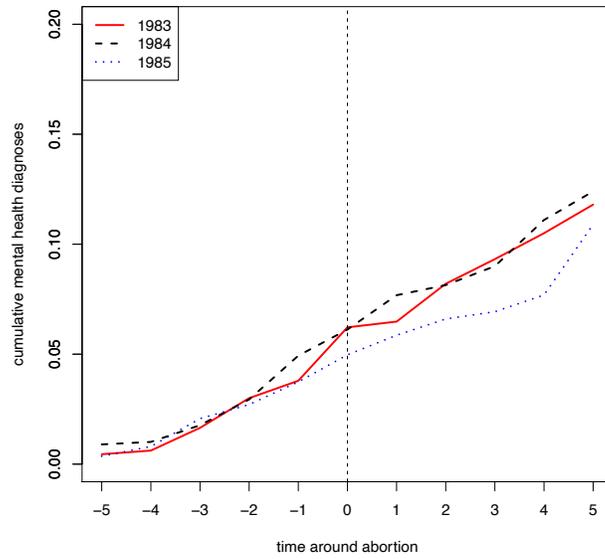


Figure B.2. Mental health development around time of abortion by birth cohort.

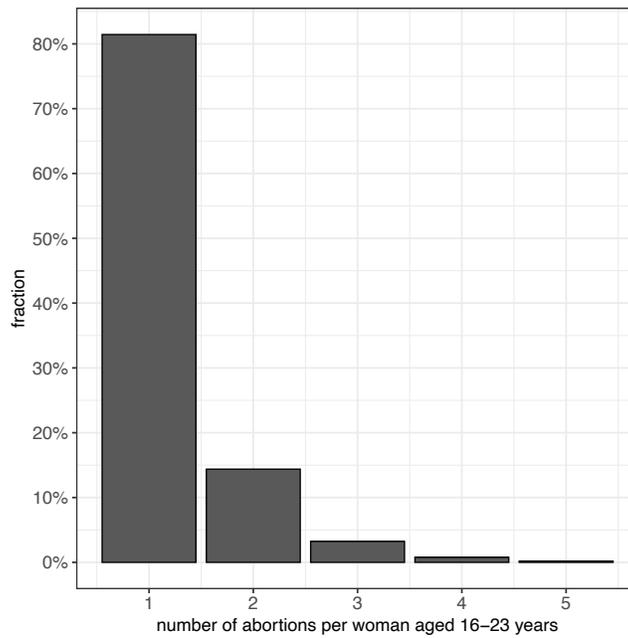
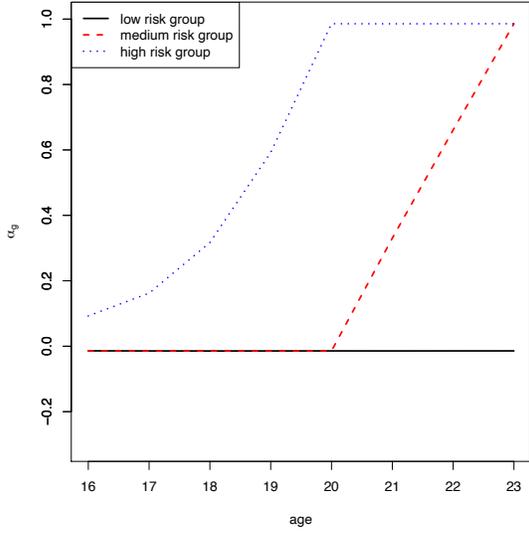
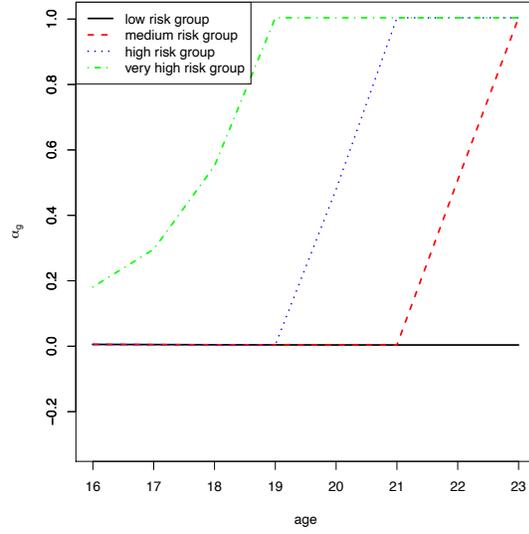


Figure B.3. Number of abortions after an unwanted pregnancy per woman, aged 16-23 years.

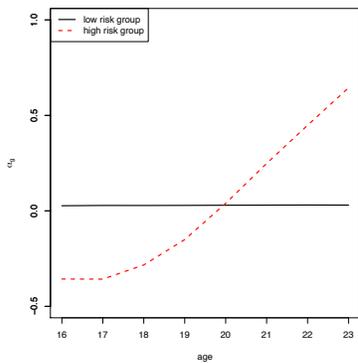


(a)  $\hat{\alpha}_{g_t}, G = 3$

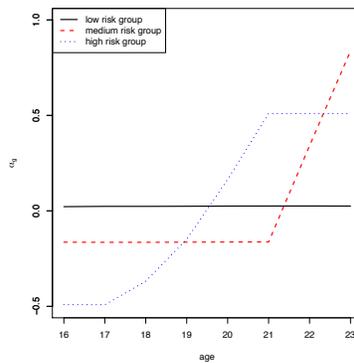


(b)  $\hat{\alpha}_{g_t}, G = 4$

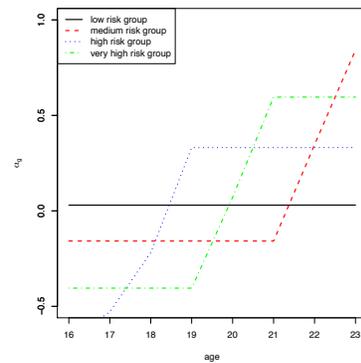
Figure B.4. Estimated time profiles of group-specific unobserved heterogeneity for  $G = 3$  and  $G = 4$ .



(a)  $G = 2$



(b)  $G = 3$



(c)  $G = 4$

Figure B.5. Estimated time profiles of group-specific unobserved heterogeneity with individual-specific fixed-effects for  $G = 2, 3, 4$

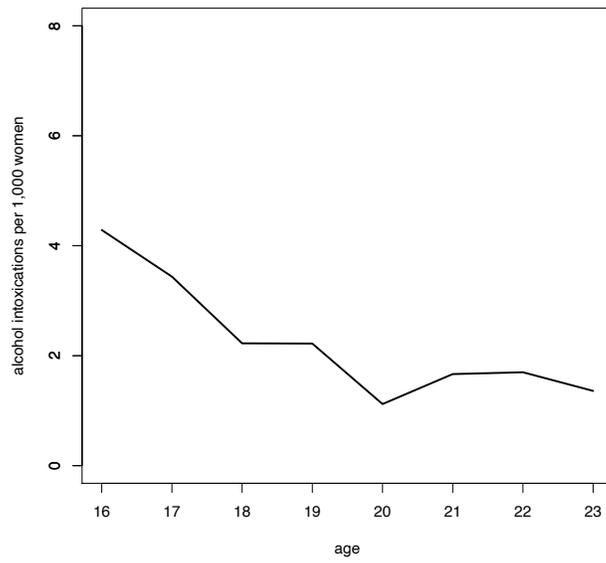


Figure B.6. Number of alcohol intoxication per 1,000 women, age 16-23.

## C Simulation Example

We build a general simulation set-up to check for known problems, such as dependence on starting values. This simulation set-up also aids us with the interpretation of our results, as well as with validating some specification choices, specifically determining the optimal number of groups. All replication files for this simulation are available on [https://github.com/LJanys/Mental\\_Health\\_Abortions\\_Risky\\_Behaviors](https://github.com/LJanys/Mental_Health_Abortions_Risky_Behaviors).

Our data is generated by the general data generating process as outlined in Equation (4), except that we disregard the cumulative properties with the following specific values and varying over four different sample sizes:

- true number of groups:  $G = 3$
- true parameter value of interest:  $\xi = 0$
- number of cross-sectional observations:  $N = 1000, 1500, 2000, 10000$
- number of time periods:  $T = 10$

$\alpha_{g_i}$ , the unobserved grouped fixed-effects trajectories, are correlated with the contemporaneous probability of having an abortion, as well as the mental health diagnosis, inducing an omitted variable bias in the OLS and FE estimates. The true unobserved heterogeneity profile curves are depicted in the left-hand panel of Figure C.1. Their analytical expressions are given by

$$\alpha_{g_1} = t(0.002) \tag{C.1}$$

$$\alpha_{g_2} = -1 + e^{(t/10)^{1/2}} \tag{C.2}$$

$$\alpha_{g_3} = -1 + e^{(t/10)^{1.2}} \tag{C.3}$$

The group membership is determined by the value of the unobserved, individual-specific fixed-effects  $\alpha_i$ : For each individual, we draw from a binomial distribution whether or not the abortion takes place in each period, with a vector of probabilities for the three groups

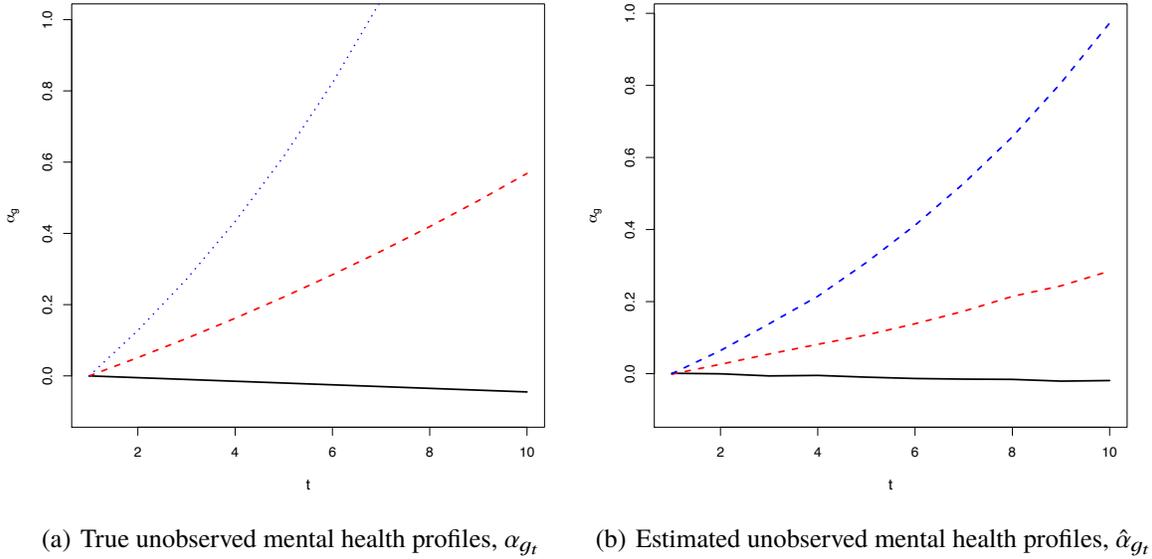


Figure C.1. True and estimated mental health profiles over time

of  $p_{g_1} = 0$ ,  $p_{g_2} = 0.1$  and  $p_{g_3} = 0.3$ . This results in a contemporaneous correlation of the unobserved  $\alpha_{g_t}$  and the abortion probability of 0.11, which is the source of the omitted variable bias. To match the characteristics in our real data, we define the groups to not be of equal size: The largest group is group one (“low-risk group”), which comprises 70% of individuals; group two (“medium-risk group”) comprises 20% of individuals; and group three (“high-risk group”) is the smallest group, with 10% of individuals.

With this DGP, we compare the results of the simulations along three margins:

- (1) we ascertain that the estimated curves of the unobserved heterogeneity are comparable to the true ones and to investigate adding “superfluous” groups.
- (2) the estimated parameters for the OLS estimator ( $\hat{\xi}_{sim}^{OLS}$ ), the individual-specific fixed-effects estimator ( $\hat{\xi}_{sim}^{FE}$ ) and the grouped fixed-effects estimator ( $\hat{\xi}_{sim,G}^{GFE}$ ) behave similar to the pattern we observe in our empirical analysis.
- (3) the chosen information criterion is reliably minimized at the correct number of groups.

The right-hand side of Figure C.1 shows the estimated unobserved heterogeneity profiles

obtained from the GFE estimator for  $N = 10,000$  observations. The profiles look very similar, indicating that the GFE can reliably estimate the group-specific profiles of unobserved heterogeneity.

The resulting estimates for the parameter of interest in the different specifications for the effect of mental health are displayed in Figure C.2. The OLS estimator overestimates the effect by a significant amount due to the omitted variable bias, but even in OLS with individual-specific fixed-effects, the effect estimate remains sizable and significant for sample sizes similar to ours, although we reduced  $N$  by half to reduce computation time. When we control for dynamic grouped fixed-effects, the estimate  $\hat{\xi}$  shrinks toward zero and the confidence interval includes zero.

The GFE estimator is not “identified” in the sense that it requires the number of groups to be known, i.e. chosen by the researcher. Note that the optimal number of groups in our simulation example is three. As shown in Figure C.2, the GFE correctly estimates a zero effect when the correct number of groups is chosen. However, for  $G = 2$  the estimated coefficient is heavily upward biased to a similar amount as the OLS estimator with individual-specific fixed-effects. By contrast, selecting too many groups does not bias the estimated coefficients. This indicates that the GFE estimator consistently estimates the true effect, once the number of groups corresponds at least to the optimal one, at least for our data generating process.

Figure C.3 displays the estimate profiles of unobserved heterogeneity for  $N = 10,000$  observations and a varying number of groups. Figures C.3(a) and C.3(b) show that the time profiles do not exhibit a sufficient amount of unobserved heterogeneity which results in biased coefficient estimates (see Figure C.2). By contrast, adding more groups than necessary does not imply that the estimator does not assign any individual observations to these superfluous groups. Rather, the GFE splits existing groups which leads to an “overfitting” of the time profiles (see Figures C.3(d) and C.3(e)). This behavior is similar to what we observe in our empirical, real data application. Adding more groups splits up the existing groups and the generated trajectories of unobserved mental health profiles for the additional groups are similar to the group that was split up.

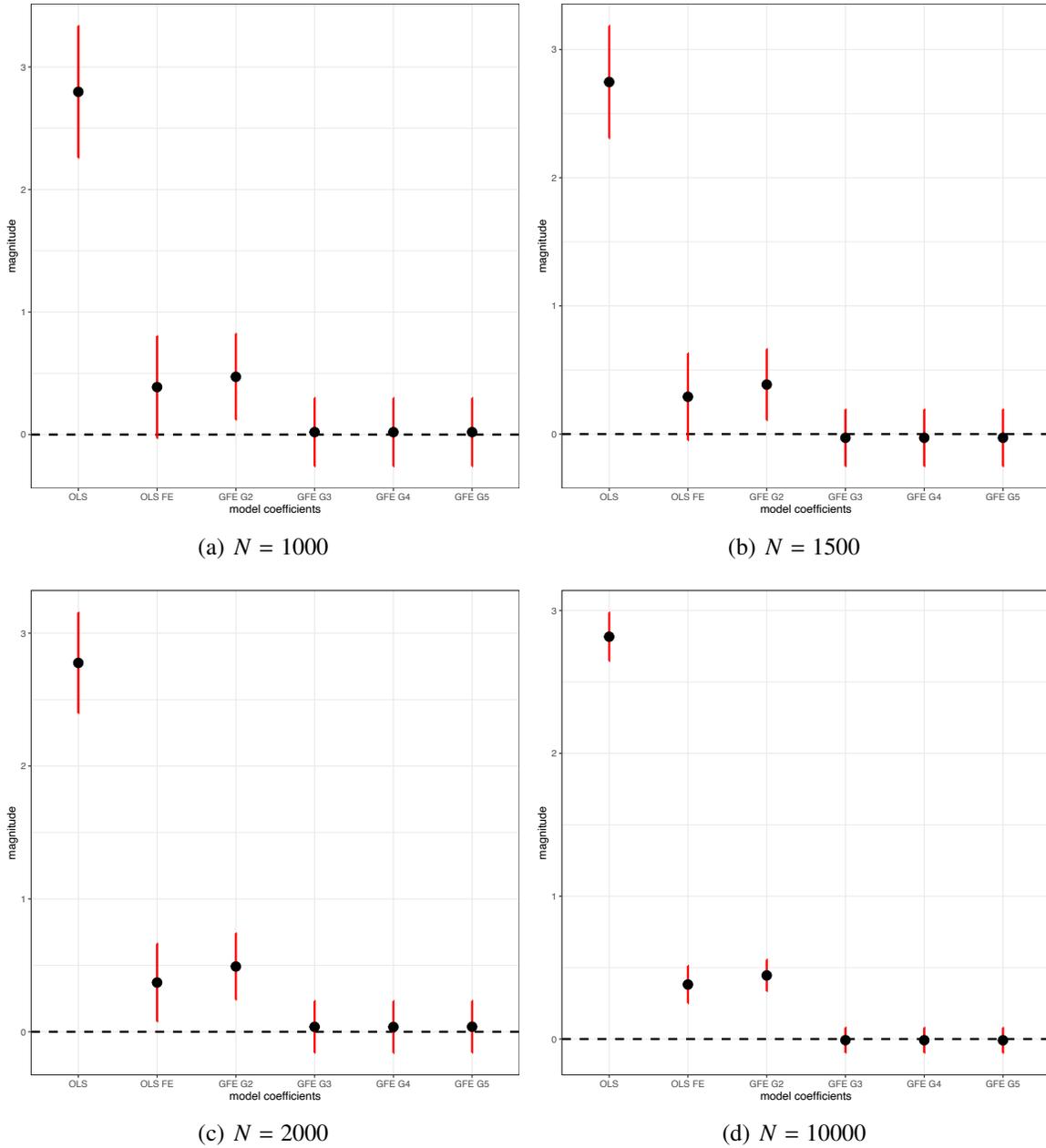
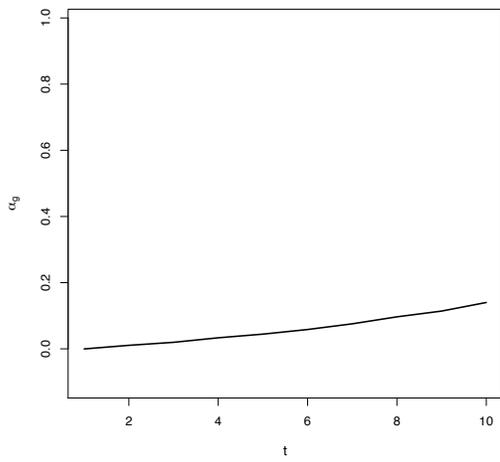
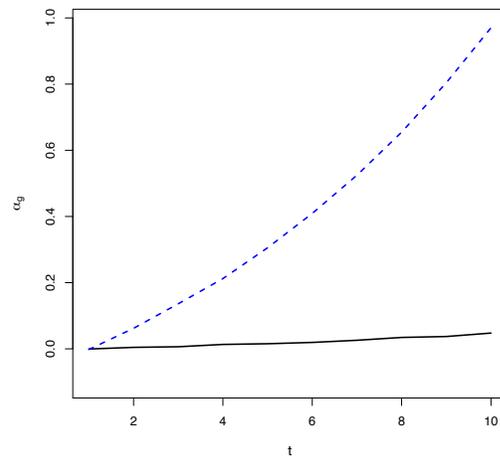


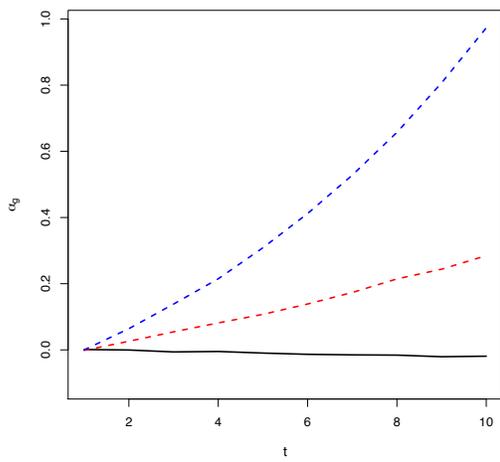
Figure C.2. Estimated coefficient  $\xi$  for different model specifications from left to right: (1) OLS, (2) individual-specific fixed-effects (OLS FE), (3) GFE estimator with two groups, (4) GFE estimator with three groups, (5) GFE estimator with four groups, (6) GFE estimator with five groups. Confidence intervals are depicted in red and are calculated using analytical standard errors.



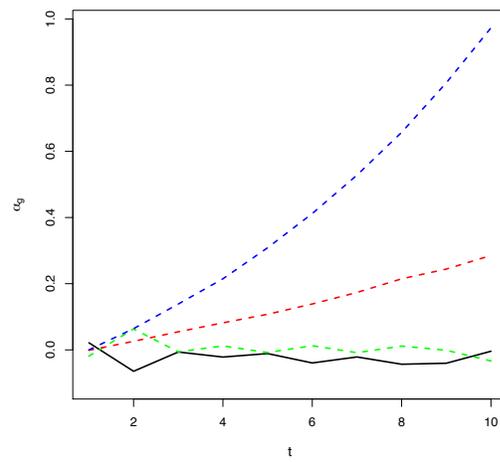
(a)  $G = 1$



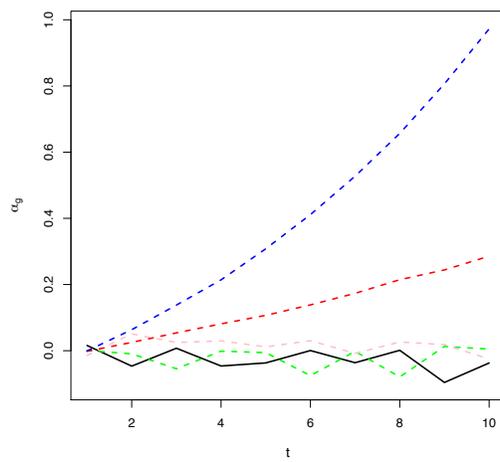
(b)  $G = 2$



(c)  $G = 3$



(d)  $G = 4$

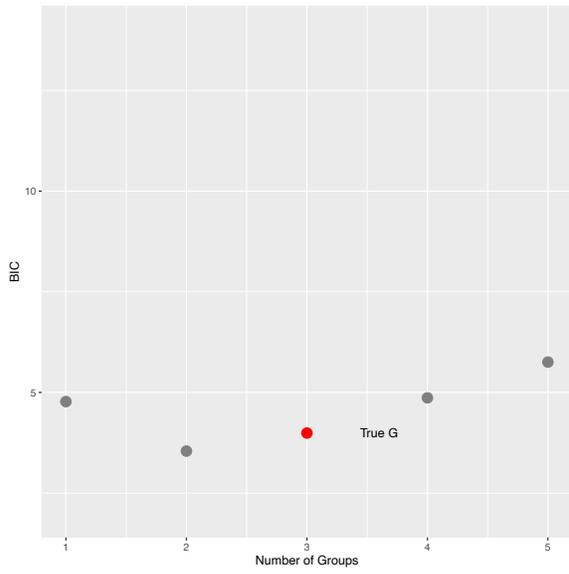


(e)  $G = 5$

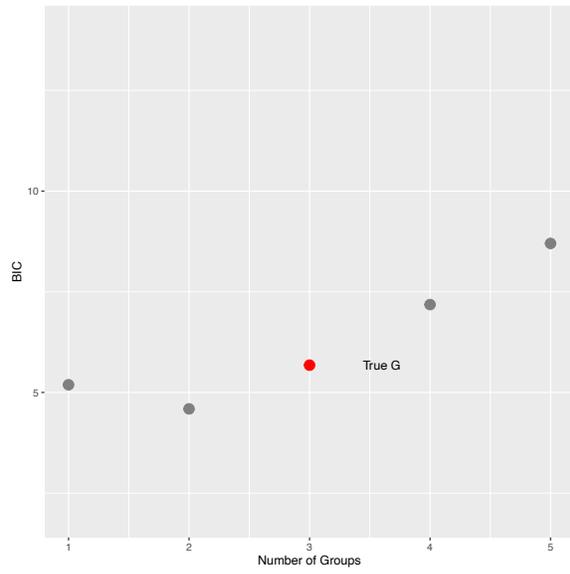
Figure C.3. Estimated grouped fixed-effects profiles  $\alpha_g$ , for  $G = 1, \dots, 5$ , for one simulation run.

Finally, we investigate the finite sample behavior of the BIC criterion with two different penalty terms (Figures C.4 and C.5) in a setting with large  $N$  and fixed  $T$ . As discussed in Section 5.2 the BIC preferred by Bonhomme and Manresa (2015) (BIC standard) does not discriminate sufficiently for all  $G \geq 1$  in our application. Our simulation exercise clearly shows that the number of groups selected by the BIC standard depends on the number of observations  $N$  relative to the number of time periods  $T$ . As shown in Figure C.5, the BIC selects the correct number of groups,  $G = 3$ , for 1,000 observations. However, when we increase  $N$ , the number of groups selected by this BIC increases, indicating that the penalization used in this BIC is not steep enough. As in our application, the BIC standard remains practically unchanged when increasing the number of groups once  $G > 1$ .

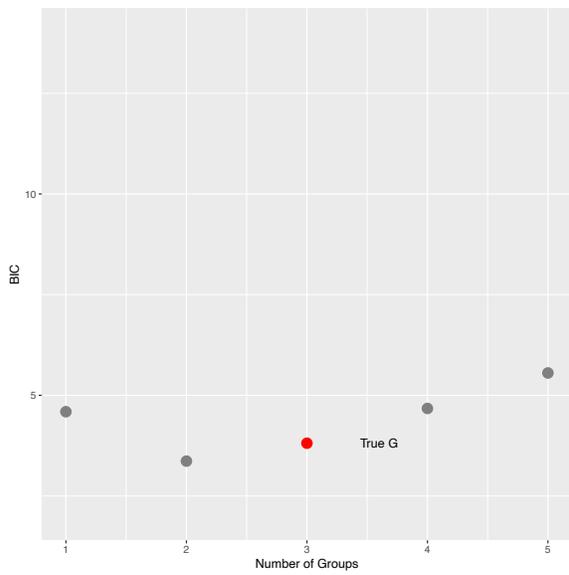
By contrast, the BIC with a steeper penalty term (in  $G$ ) always chooses two groups regardless of the number of observations. As indicated by the steep increase in the value of this BIC, the penalization with respect to the number of groups is too strong (Figure C.4). We observe a similar behavior in our application.



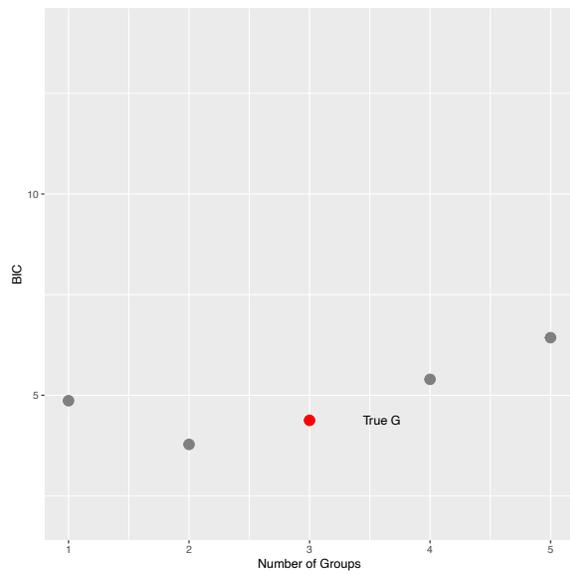
(a)  $N = 1000$



(b)  $N = 1500$



(c)  $N = 2000$



(d)  $N = 10000$

Figure C.4. Results for the BIC with the steeper penalty in terms of  $G$ .

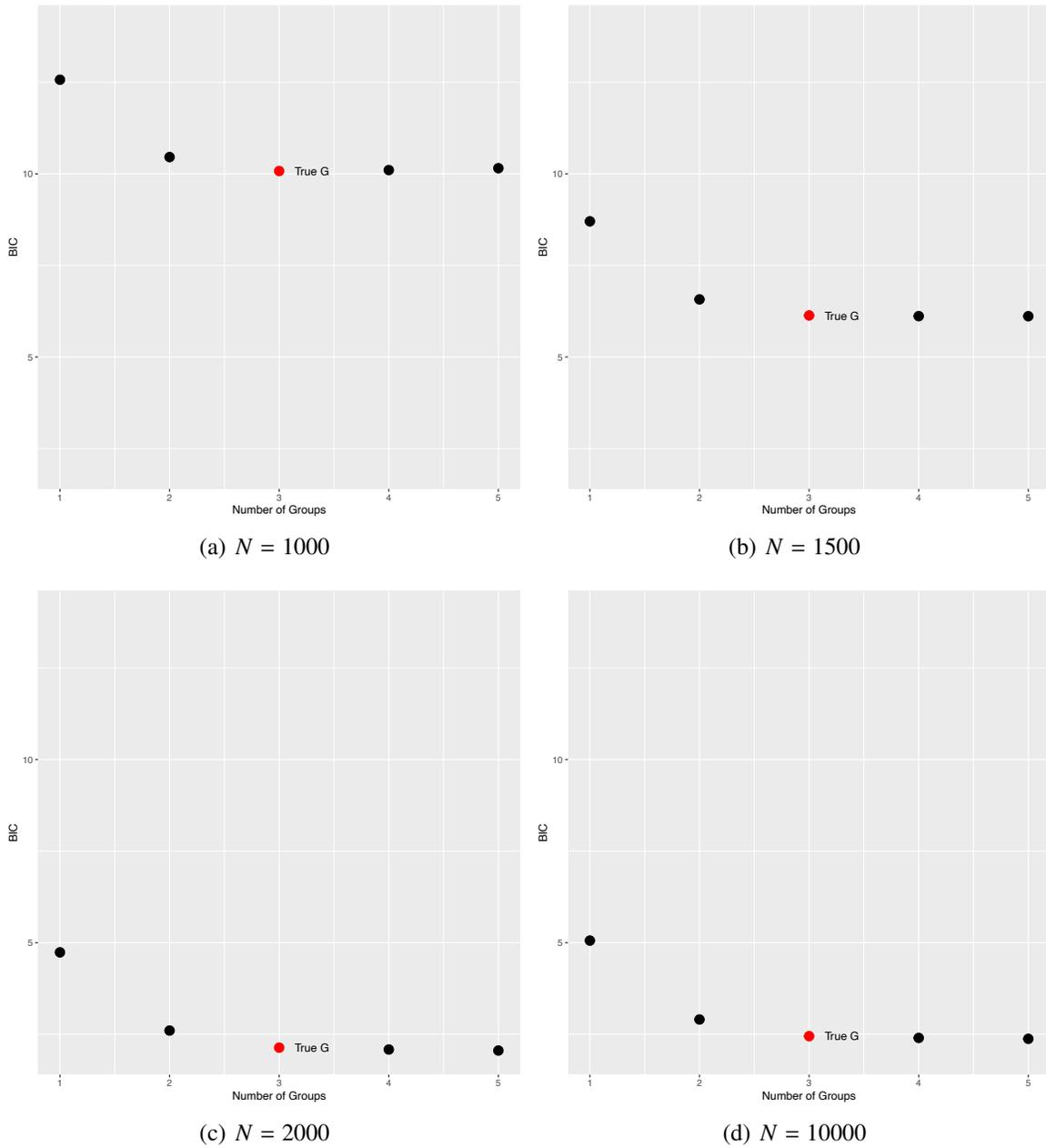


Figure C.5. Results for the BIC with the less steep penalty used in [Bonhomme and Manresa \(2015\)](#).

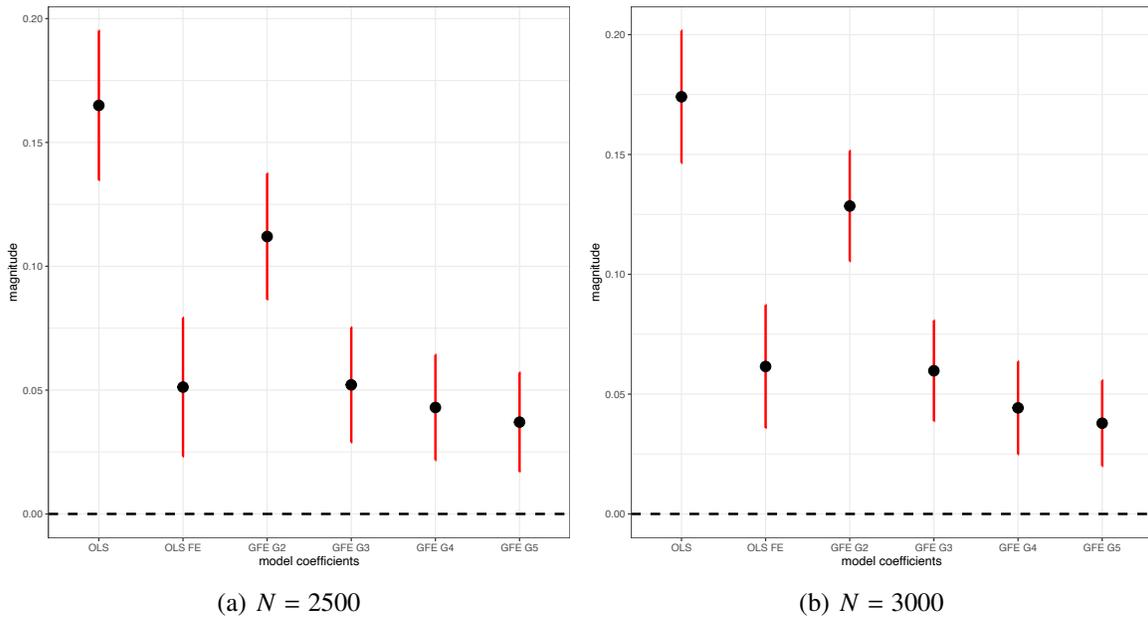


Figure C.6. Estimated coefficient for  $\xi$  with lags for different model specifications from left to right: (1) OLS, (2) OLS with individual-specific fixed-effects (OLS FE), (3) GFE estimator with two groups, (4) GFE estimator with three groups, (5) GFE estimator with four groups, (6) GFE estimator with five groups. Confidence intervals are depicted in red and are calculated using analytical standard errors. The true contemporaneous effect is  $\xi_0 = 0.2$  and the true data generating process contains lags of the form  $\xi_1, \xi_2, \dots, \xi_5 = 0.1$ , that are ignored in estimation.

## D Model solution

Numerically, the decision function  $\rho_t^*$  can be computed by backward induction over  $t$ , starting with a guess in the far future which does not affect behavior in initial periods. The backward recursion is a variation of classical dynamic programming that takes into account the time-inconsistency introduced through  $\beta$ . To facilitate formulating the dynamic program, we denote by  $F_t(M_t)$  the (classically, without  $\beta$ ) discounted sum of future flow utilities given the decisions  $\rho^*$  at age  $\tau > t$

$$F_t(M_t) = u(\rho_t^*(M_t), M_t) + \mathbb{E}_t \left[ \sum_{\tau=t+1}^{\infty} \delta^{\tau-t} u(\rho_\tau^*(M_\tau^*), M_\tau^*) \right]. \quad (\text{D.1})$$

Every woman's optimization problem specified by Equation (14) of computing  $\rho_t^*(M_t)$  can be written compactly in terms of  $F$  as

$$\rho_t^*(M_t) = \arg \max_{\rho_t} \{u(\rho_t, M_t) + \beta \delta \mathbb{E}_t[F_{t+1}(M_{t+1}^*)]\} \quad (\text{D.2})$$

Moreover, the functions  $F_t$  satisfy the recursion

$$F_t(M_t) = u(\rho_t^*(M_t), M_t) + \delta \mathbb{E}_t[F_{t+1}(M_{t+1}^*)] \quad (\text{D.3})$$

Our numerical approach via backward induction thus looks as follows. We initialize by guessing the terminal condition  $F_T(M_T) = \bar{u}(M_T) = 0$  for  $T = 100$ .<sup>34</sup> In order to sequentially compute the decision function  $\rho_t^*$  and the functions  $F_t$ , we alternate two steps backwards in time. Assume that  $F_{t+1}$  is already known. Then, we can compute  $\rho_t^*(M_t)$  by solving the problem in Equation (D.2) for every value of  $M_t$ . Once  $\rho_t^*(M_t)$  is known, we can compute  $F_t$  from Equation (D.3) and go back one more step in time. When solving this problem computationally, we first discretize the state variables  $M$  and  $\rho$  over a suitable grid.<sup>35</sup> Thus,

<sup>34</sup>This guess is incorrect but can be expected not to affect behavior in early time periods  $t = 1, \dots, 8$ . We verify this by checking that results do not change if we initialize instead at  $T = 200$ .

<sup>35</sup>We assume that the choice of  $\rho$  is discrete and the possible values are  $0, 0.05, 0.1, \dots, 0.95, 1$ . For  $M$  we simulate 1000 trajectories for the two most extreme values of  $\rho$ ,  $\rho_t \equiv 0$  and  $\rho_t \equiv 1$ . We use the maximum and minimum of resulting mental health trajectories to determine the boundaries of the grid. We choose 200 equidistant levels

the maximizations do not have to be performed for every possible value of  $M$  and  $\rho$ , but only for every value on the grid. The resulting discrete functions  $F_t$  are interpolated using monotone Hermite splines. To compute the conditional expectations at each age, we take a Monte Carlo average over 1,000 possible scenarios for  $M_{t+1}$  for the next step given each combination  $(M_t, \rho_t)$ . In this way, the functions  $F_t$  and  $\rho_t^*$  can be computed backward in time one by one. With the resulting decision functions  $\rho_t^*$ , we then simulate 10,000 optimal mental health trajectories  $M_t^*$  and the associated risky behavior  $\rho_t^*(M_t^*)$ .

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in between.