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Preface

This dissertation consists of three self-contained chapters. Each chapter applies the tools of applied microeconomics to questions related to health economics, the economics of education, and peer effects. The unifying theme of this thesis is the emphasis on the social context as a primary driver of individuals' decisions and lifetime trajectories, including their health and human capital. Largely, I focus on two topics. The first topic covers the causes and consequences of mental health problems, both during adolescence and in adulthood. The second topic investigates the role of peer effects in individuals' decision making. Chapter 1 and 2 contribute to the first topic; Chapter 3 to the latter.

The first chapter explores the importance of the social context for adolescents' mental health and human capital formation. Adolescence is a particularly important period for the development of mental health problems as many mental health disorders first manifest during this time. Which factors determine mental health and individuals' cognitive development during adolescence? What role does the social context, specifically adolescents' relative position within their peer group, play in this context? Chapter 1 of this dissertation aims to provide causal evidence in response to these questions.

Chapter 2 is based on joint work with Bettina Siflinger, Sebastian Seitz, Moritz Mendel, and Hans-Martin von Gaudecker and focuses on the topic of mental health during the CoViD-19 pandemic. In particular, we document the evolution of mental health in the Dutch working population over the course of the pandemic and contribute evidence on the potential mechanisms through which the pandemic may have affected mental health.

Chapter 3 is based on joint work with Katja Kaufmann and Yasemin Özdemir, and addresses the importance of the social context for individuals' decision-making.

In particular, we provide early evidence on how a couple's participation decision in the asset market is influenced by their siblings' financial choices and experiences.

In the following, I will briefly describe each chapter in more detail.

Chapter 1: Adolescents' Mental Health and Human Capital: The Role of Socioeconomic Rank

In Chapter 1, I investigate the causal effect of a student's relative socioeconomic status (SES) within their school cohort on mental health, social and cognitive development as well as educational attainment. The idea that relative characteristics matter for individuals' well-being and development has a long history in the sociology and social psychology literature and is related to theories on social comparison and relative deprivation. However, quantifying the causal effect of such relative attributes is challenging, primarily because reference groups and social networks are endogenously formed. Using survey data from US high schools, I address the problem of selection into peer groups and estimate a causal rank effect in a series of fixed-effects regressions. Holding fixed the level of SES, I find that a higher socioeconomic rank of students within their high school cohort leads to a reduction in depression scores, improved cognitive ability and self-esteem scores as well as higher levels of popularity in the short run. The effects of the socioeconomic rank are persistent. I show that a higher rank during high school also leads to lower depression scores and better educational attainment in adulthood. Additionally, I document a consistent pattern of steeper rank gradients for high-inequality cohorts, that is the rank effects are stronger in cohorts with a higher dispersion of SES.

Chapter 2: The CoViD-19 Pandemic and Mental Health: Disentangling Crucial Channels

Chapter 2 is based on joint work with Bettina Siflinger, Sebastian Seitz, Moritz Mendel, and Hans-Martin von Gaudecker. The CoViD-19 pandemic has disrupted the lives of people around the world, raising concerns about potentially adverse impacts on individuals' mental health. Using data from the Longitudinal Internet Studies for the Social Sciences (LISS) panel, we explore how mental health has evolved for the Dutch working population throughout the year 2020. Particularly, we explore different channels – risk of infection, labor market uncertainty, emotional loneliness, and childcare responsibilities - likely to have mediated the impact of the pandemic on individuals' mental health. Overall, we show that mental health has decreased sharply with the onset of the first lockdown and the closure of schools and childcare facilities but recovered fairly quickly. Importantly, we document distinct patterns over the course of the pandemic and across genders and channels.

Chapter 3: Peer Effects in Financial Decisions

Chapter 3 is based on joint work with Katja Kaufmann and Yasemin Özdemir. We provide causal evidence on peer effects in financial investment decisions. Do the experiences of siblings affect a couple's financial investment decision? Why are couples influenced by their siblings' financial experiences? These are the question we aim to answer in this project. In particular, we study whether siblings' experiences in the market of risky assets influence a couple's decision to enter the asset market for the first time. Using administrative data for the Netherlands, we employ an IV strategy, exploiting partially overlapping peer groups, and find evidence for sibling spillovers in finacial investment decisions. Moreover, our results are consistent with a social learning mechanism, i.e., informational spillovers seem to be key in explaining peer effects in financial investments in risky assets.

Chapter 1

Adolescents' Mental Health and Human Capital: The Role of Socioeconomic Rank

Abstract

I study the impact of a student's relative socioeconomic status during high school on their mental health, cognitive ability and educational attainment in the short- and long-run. Using data from the National Longitudinal Study of Adolescent to Adult Health, I utilize between-cohort differences in the distributions of socioeconomic status within schools in a linear fixed effects model to identify a causal rank effect. I find that increasing a student's rank by 25 percentiles improves depression scores, cognitive ability, self-esteem and popularity by around 0.08 to 0.13 standard deviations. The rank effects are persistent. I find substantial and long-lasting effects of the socioeconomic rank during high school on adult depression as well as college attendance and completion. In addition, I document a consistent pattern of steeper rank gradients in high-inequality cohorts.

1.1 Introduction

The prevalence of mental health problems and their importance for individuals' lifetime trajectories and the economy as a whole have been increasingly recognized. The estimated total cost of mental health disorders on society was around 3.5% of GDP in 2010 (OECD, 2015). In this context, the mental health of teenagers is of particular interest, as many mental health disorders arise during adolescence, leading to concerns regarding adverse impacts on teenage development. These concerns typically center around potential long-term consequences, emphasizing the importance of an unimpeded development for outcomes such as educational attainment, health, and well-being. This view is supported by a large body of evidence that documents substantial economic and social returns to interventions in adolescence and neurobiological changes in brain regions involved in cognitive and social processes during the second decade of life (Dahl et al., 2018).

A commonly held perception is that teenagers are particularly susceptible to peer influence as they experience a reorientation towards peers and away from parents (Dahl et al., 2018). The notion that social context is an important factor in the human development is widely accepted in the economics of education literature, where peer characteristics are considered important determinants in the production of human capital. Complementary to the traditional peer effects view, which typically emphasizes absolute measures of peer quality, this paper follows the idea that an individual's relative position within their peer group may shape outcomes. The idea that relative characteristics matter for individuals' well-being and development has a long history in sociology and social psychology.[1] However, quantifying the causal

[1]Social Comparison Theory, for example, posits that individuals have the innate drive to evaluate themselves and, in the absence of objective standards, do so in terms of comparisons to others (Festinger, 1954). Social comparison phenomena have been investigated in various settings with the aim to understand the processes by which individuals come to understand themselves through relative comparisons (Suls and Miller, 1977).

effect of such relative attributes is challenging, primarily because social networks are endogenously formed.

This paper provides causal evidence on the effect of the relative socioeconomic status on adolescents' mental health, cognitive ability and educational attainment in the short- and long-run.[2] Motivated by the fact that adolescents spend a significant amount of time in school, I study the role of relative status within high school cohorts, which form a natural reference group for adolescents. My baseline measure of students' socioeconomic status (SES) is the highest level of schooling completed by their head of household, which I use to assign each student the percentile rank in their cohort SES distribution.[3] Studying the role of relative status in the framework of cohort networks allows me to address selection concerns by employing a fixed-effects approach recently popularized in the rank-effect literature (Elsner and Isphording, 2017, 2018; Murphy and Weinhardt, 2020).

Intuitively, my empirical strategy relies on the observation that the ranks of students with the same socioeconomic status can vary substantially across cohorts within the same school. Such variation arises naturally due to fluctuations in the household characteristics of children of school starting age in a school's catchment area over time. As a consequence, I observe "similar" students with the same level of SES, but different relative positions within their cohorts in the same school. Roughly speaking, viewing the between-cohort fluctuations as idiosyncratic allows me to use the within-school differences in SES distributions across cohorts to estimate a causal rank effect. Formally, this view justifies an exogeneity assumption that identifies a causal parameter in a linear fixed effects model.

My empirical analysis is based on data from the National Longitudinal Study of Adolescent to Adult Health (Add Health), a nationally representative study in the

[2] According to the theory of relative deprivation, feeling socially and economically deprived relative to a reference group can shape individuals' emotions, cognitions, and behaviors (see e.g. Crosby, 1976; Smith et al., 2012; Stouffer et al., 1949).

[3] My results are robust to variations in the SES definition, as reported in Appendix A.2.

U.S. that follows students in several waves from their time during high school into adulthood. The Add Health data has four characteristics that make it particularly suitable for my research question. First, it contains detailed information on the school and cohort membership of the surveyed students, providing me with the information necessary to construct cohort networks. Since the primary sampling unit of the survey are schools, the network data is ”complete” in the sense that I observe all students within each cohort. Second, it covers multiple cohorts within the same school, a feature that is key for my empirical strategy as outlined above. Third, it contains rich information on students’ backgrounds, including parental education, allowing for the construction of different measures of socioeconomic status. Fourth, the data set contains well established outcome measures for depression and cognitive ability. Specifically, depression is measured using the Center of Epidemiological Studies Depression Scale (CES-D; Radloff, 1977) and cognitive ability is measured using the Peabody Picture Vocabulary Test (PPVT; Dunn and Dunn, 2007), an age-specific standardized ability test. Moreover, the data set contains six items similar to or modified from the original Rosenberg self-esteem scale (Rosenberg, 1965) as well as information on friendship networks that allow for the construction of a measure of popularity. These two outcomes are closely linked to mental health and social status within a peer group and, taken together with the main outcomes, provide a more comprehensive picture of adolescents’ development. Finally, students in the Add Health survey are tracked over a long period of time, allowing me to investigate whether the socioeconomic rank has effects that persist into adulthood, more than 10 years after the initial interviews took place.

My analysis produces three main findings. First, a student’s SES rank in their high school cohort has a significant and economically meaningful impact on their development in the short run. Holding the level of socioeconomic status fixed, students with a higher within-cohort rank tend to have better outcomes in terms of depression and cognitive ability. These results are supported by analogous findings which show that, ceteris paribus, higher ranked students develop higher levels of self-esteem, and are more popular, as measured by friendship nominations. Increasing a student's rank by 25 percentiles, decreases depression scores by 0.12 standard deviations and increases cognitive ability and self-esteem scores by 0.13 standard deviations. Further, such a rank shift leads to a 0.08 standard deviations increase in a student's popularity among their peers.

Second, these rank effects vary by the degree of cohort inequality, with steeper rank gradients occurring in cohorts with high levels of SES-inequality across all outcome dimensions. These documented patterns may be of independent interest and are consistent with the predictions of a relative deprivation mechanism, suggesting that the salience of inequality may be important.

Third, the effect of the socioeconomic rank during high school persists in the long-run. Students with a higher within-cohort rank during high school tend to have better mental health and educational outcomes in adulthood. Increasing a student's cohort rank by 25 percentiles increases the probability of attending and completing college by 5 and 3 percentage points, respectively, and decreases adult depression scores by 0.13 standard deviations. The latter result is consistent with the documented high persistence of depressive symptoms over the lifecycle and emphasizes the importance of adolescent mental health.

Overall, my results suggest that the relative socioeconomic status is an important determinant in shaping adolescents' outcomes, supporting the view that social context should not be treated as a second-order concern when studying human development.[4] My findings can be viewed as a justification for the design and implementation of interventions aimed at mitigating the adverse consequences of relative

[4]This view is consistent with findings from Butikofer et al. (2020), who show that the school environment causally affects adolescents' mental health and educational attainment.

deprivation.[5] In practice, such efforts could entail interventions aimed at reducing the salience of inequality in schools, such as the provision of school uniforms, subsidized school meals and leisure activities.

Related Literature My work builds on and contributes to a large body of literature that seeks to understand the determinants of human capital formation and the role of mental health.[6] A consistent finding in this literature is that circumstances and investments early in life have a disproportionate impact in shaping long-term outcomes (e.g. Campbell et al., 2014; Currie, 2009) and that large socioeconomic gaps open up at early ages and persist into adulthood (e.g. Carneiro and Heckman, 2003; Cunha et al., 2006; Currie and Goodman, 2020). These shared patterns are perhaps unsurprising, as concepts of mental health and non-cognitive skills, an important component of human capital, tend to overlap. Moreover, there is evidence that mental health affects processes relevant for the development of cognitive skills (Currie and Stabile, 2006, 2009) and that there are feedback effects of human capital on mental health.

Relating to the large and persistent socioeconomic gaps in (mental) health and human capital outcomes and their implications for lifetime inequality and intergenerational mobility, a growing literature provides estimates of the causal effect of parental background on life outcomes of children and adolescents, providing evidence on the effect of parental education and income on their children's cognitive and non-cognitive ability (Dahl and Lochner, 2012; Lundborg et al., 2014; Milligan and Stabile, 2011), educational attainment (Black et al., 2005; Holmlund et al., 2011; Oreopoulos et al., 2006) as well as health (Lundborg et al., 2014; Milligan and Stabile, 2011).

[5] Importantly, my findings should not be interpreted in support of policies furthering segregation by SES. Such a view would neglect the endogenous consequences of modified peer characteristics.

[6] Influential examples include Cunha and Heckman (2007), Cunha et al. (2006, 2010), Currie and Stabile (2006), and Currie et al. (2010).

I contribute to this literature by providing causal evidence on how parental socioeconomic status affects adolescents' mental health and human capital formation. In contrast to the previous literature, which studies the impact of absolute measures of socioeconomic status, I investigate the role of mechanisms that operate through the relative status of a student within their peer group. I draw on a rich theoretical literature[7] in sociology and social psychology that emphasizes the importance of social context, in particular social comparisons and relative deprivation, for individuals' self-evaluations, development and behavior. I investigate the empirical content of these theories by applying modern quasi-experimental techniques recently popularized in the rank effects literature discussed below.

Methodologically, my work is closely related to a growing empirical literature on ordinal rank effects. In particular, a series of recent papers have investigated how relative ability rankings during adolescence impact individuals' educational outcomes and behaviors. This line of work is motivated by the idea that individuals calibrate the perception of their abilities via peer comparisons with consequences for their educational attainment and choices (Delaney and Devereux, 2021; Elsner and Isphording, 2017; Elsner et al., 2021; Murphy and Weinhardt, 2020), risky behaviors (Elsner and Isphording, 2018), as well as the development of personality traits (Pagani et al., 2021) and depression (Kiessling and Norris, 2020).

In contrast to these papers, I treat cognitive ability as an outcome variable and seek to understand and quantify the importance of relative socioeconomic status, as measured by predetermined parental characteristics. While I employ the same fixed-effects strategy utilized in these papers, the challenges I face are different ones as I discuss in Section 1.3.

Also closely related to my work are Balsa et al. (2014) and Arduini et al. (2019), who find that differences relative to average peer characteristics in terms of socioe-

[7] Examples include Crosby (1976), Festinger (1954), Stouffer et al. (1949), Wills (1981), and Wood (1989).

conomic status and body mass index impact risk-taking behavior as measured by alcohol consumption and smoking for young males, as well as eating disorders in female teenagers. The status concerns underlying such comparison mechanisms have also been investigated in adult populations, by studying the impact of relative positions on job satisfaction (Card et al., 2012) and general well-being and satisfaction (Brown et al., 2008a). The results reported in these papers are consistent with the findings in Luttmer (2005) and Clark and Oswald (1996), who provide evidence that satisfaction and well-being depends on income relative to an environment specific reference level.

On a conceptual level, my work is also related to a vast literature on peer effects in education (e.g. Bifulco et al., 2011; Carrell et al., 2018; Sacerdote, 2011), in that I recognize the importance of peer groups. In contrast to this literature, I emphasize an individual's relative position within their peer group rather than the effects of absolute measures of peer characteristics, which I treat as nuisance parameters in my model.

Outline of the Paper The rest of this paper is organized as follows. Section 1.2 describes the Add Health data and the construction of relevant variables. Section 1.3 presents my empirical strategy and discusses threats to the identification of my model. In Section 2.4, I present the results of my empirical analysis. Section 2.5 discusses potential policy implications of my findings and concludes.

1.2 Data

The dataset used for the empirical analysis is the National Longitudinal Study of Adolescent to Adult Health (Add Health), explicitly designed to study the link between the social environment and adolescents' health and health-related behavior. During the school year 1994/95, all students in the grades 7-12 of 80 nationally representative high schools and 52 middle schools in the US completed an in-school survey. General student and parental background information, health and health-related behavior as well as information about the school and social network were collected for more than 90,000 adolescents between the age of 12 and 20. Moreover, a sample of around 20,000 students additionally completed a more comprehensive in-home questionnaire with detailed information on behavior, characteristics and health status. Respondents from this wave I home-interview were followed and re-interviewed in four subsequent waves, administered in 1996 (wave II), 2001-02 (wave III), 2008-09 (wave IV) and 2016-2018 (wave V).

The Add Health survey exhibits four features that are key for the analysis in this paper. First, it contains detailed information on the school and cohort of a student, allowing me to identify the cohort network of a student. Second, it covers multiple cohorts within the same school, allowing me to employ a fixed effects strategy with separate school and cohort fixed effects or school-by-cohort fixed effects. Third, it contains detailed information on the students' background, including parental education as a measure of students' socioeconomic status. Fourth, the data set contains well-established measures of mental health, cognitive ability, and self-esteem as well as information about the friendship networks of students. The scope and detail of the survey questions allow me to obtain an accurate and comprehensive picture of adolescents' development. Finally, students from the in-home sample are tracked over a long period of time, allowing me to study the long-term impacts of relative socioeconomic status during high school.[8]

[8] For the construction of long-term outcomes, I use information from wave IV as this is the most recent data currently available to me.

1.2.1 Outcome Measures

For the analysis of the short-run effects, the main outcomes I focus on are depression and cognitive ability as adolescence is a critical time for the development of cognitive processes and the onset of mental health problems. In addition, I also consider potential rank effects on a student's self-esteem and popularity during high school. These outcomes are closely linked to mental health and social status within the peer group and, taken together with the two main outcomes, provide a more comprehensive picture of adolescent development. In order to study potential long-run effects of the socioeconomic rank, I look at depression as well as educational attainment in adulthood, more than 10 years after the initial interview took place.

Depression Depression is a common mental disorder with potentially long-lasting effects on the individual's quality of life. In this paper, it is measured using the Center for Epidemiologic Studies-Depression Scale (CES-D), a validated international screening test designed to measure depressive symptoms in the general population (Radloff, 1977). The CES-D is one of the most commonly used self-reported measures of depressive symptoms. Psychometric properties in terms of its concurrent validity (i.e. the degree of agreement between the CES-D score and the diagnosis), reliability and internal consistency of the CES-D have been demonstrated to be good in a wide range of clinical and non-clinical populations, including adolescents (see e.g. Lewinsohn et al., 1997; Radloff, 1991; Roberts et al., 1990). The CES-D in the Add Health questionnaire consists of 19 items (e.g. *"You felt sad"*), assessing the frequency with which an individual experiences symptoms associated with depression over the course of the past week.[9] Responses are rated on a scale from 0 (*"never or rarely"*) to 3 (*"most of the time or all of the time"*), resulting in an aggregated measure of the CES-D ranging from 0 to 57, with higher values

[9] See Table A.1 in Appendix A.1 for an overview of all items.

indicating worse depressive symptoms. Respondents with a score equal to or above 16 are commonly identified to be at risk for clinical depression (Beekman et al., 1995; Radloff, 1977). In the main analysis, I use the aggregate CES-D score as a measure of depression, however, I also use the cut-off of 16 as an indicator for clinical depression in Appendix A.3.2.

Cognitive Ability As a measure of cognitive ability, I use the Adolescent Health Picture Vocabulary Test (AHPVT), an adapted 87-item version of the Peabody Picture Vocabulary Test (PPVT; Dunn and Dunn, 2007). The Peadody is an assessment of a student's receptive vocabulary and is used to measure verbal intelligence and scholastic aptitude. The test is age-specific and scores are standardized to a mean of 100 and standard deviation of 15 within each age group.

Self-esteem As a measure of a student's self-esteem, I use an adapted 6-item version of the original Rosenberg self-esteem scale (Rosenberg, 1965). The Rosenberg scale asseses an individual's perception of self-worth. In the Add Health data set, students were asked whether they agree or disagree with statements such as *"you have many good qualities"* or *"you have a lot to be proud of"*.[10] Items are scored on a 5-point Likert scale ranging from 1 (*"strongly agree"*) to 5 (*"strongly disagree"*). For the construction of the self-esteem measure, these items are reverse coded to a scale from 0 (*"strongly disagree"*) to 4 (*"strongly agree"*) and aggregated to obtain a score ranging from 0 to 24 such that higher values indicate higher levels of self-esteem.

Popularity A student's popularity among their high school peers can be regarded as a reflection of social status and peer acceptance, factors that are essential in the development of adolescents. Having good social relations can have a positive impact on their feelings of self-worth and depressive symptoms. Moreover, adolescents'

[10] See Table A.3 in Appendix A.1 for an overview of all items.

popularity during high school can be an important predictor of adult success. It has been shown that there is a wage premium associated with a student's popularity, as measured by the number of received friendship nominations (Conti et al., 2013). I use methods from social network analysis to derive a measure of a student's popularity based on their friendship network. In particular, I use detailed information on high school friendship relations collected in the Add Health dataset. During the in-school survey in 1994/95, students were asked to nominate up to five male and five female friends from a given school roaster. Friendship nominations are by nature directed, allowing me to construct a measure of popularity using the *in-degree centrality*, i.e. the number of friendship nominations each student receives. Formally, let $A_{i,j}$ be the adjacency matrix of a directed network, then the in-degree of individual i is given by $d_i = \sum_k a_{k,i}$. Received friendship nominations are then standardized within cohorts to control for differences in school and cohort size.

Long-run Outcomes In order to study long-term effects of a student's socioeconomic rank, I use information from wave IV, when individuals were between 24 and 32 years old, to construct measures of mental health and educational attainment. In particular, I use a short version of the CES-D questionnaire as an indicator for depression. The shorter CES-D score is based on 10 items, thus ranges from 0 to 30.[11] Moreover, I use a student's educational attainment in the form of college attendance and college completion dummies to obtain measures for human capital accumulation in the long-run.

[11]The 10 items asked in wave IV are indicated with an asterik in Table A.1

1.2.2 Socioeconomic Rank

To measure a student's position in comparison to their school peers, I construct their ordinal rank in terms of the socioeconomic background within their school cohort.[12] I follow Balsa et al. (2014) and define an adolescent's socioeconomic status in terms of the highest level of schooling completed by the student's head of the household.[13] In the Add Health data, parental schooling is reported by the students as a categorical variable which I translate into years of schooling by using the midpoints of these categories.[14] One key advantage of using parental schooling as a measure of adolescents' socioeconomic status is that this information is available for all students participating in the in-school questionnaire.

The ordinal rank of a student measures their households' relative position in the distribution of parental socioeconomic status within their school cohort. In a cohort with N students, the student with the lowest status in a cohort is assigned position 1 and the student with the highest status is assigned position N. To account for differences in school cohort size, the student's raw rank is translated into a percentile rank. In particular, the rank of individual i in school s and cohort c is then measured as

$$\text{Rank}_{isc} = \frac{\text{n}_{isc} - 1}{\text{N}_{sc} - 1}, \tag{1.1}$$

[12] A student's cohort refers to all students attending the same grade at the same school and time.

[13] The head of the household is assumed to be the father unless the respondent reported not living with him or the information is missing. In that case, the mother's highest level of schooling is taken.

[14] Following Balsa et al. (2014), five years of schooling are assigned to parents who completed eight or fewer years of schooling, including those cases in which the child indicated that the parent never went to school or did not know which level the parent completed. Moreover, I assign 10.5 years to parents who completed the eighth grade but did not graduate from high school, 11.5 years to parents who completed a GED, 12 years to parents who graduated from high school, 13.5 years to parents who attended a business, trade, or vocational school after high school, 14 years to parents who received some college education, 16 years to parents who graduated from a college or university, and 20 years to parents who acquired professional training beyond college.

where N_{sc} is the cohort size of school s and cohort c and n_{isc} is student i's ordinal rank position in their school cohort. Rank_{isc} is the percentile rank of student i, ranging from 0 for the lowest ranked student to 1 for the highest ranked student in a given cohort. In the case of ties, students are assigned an average rank.[15]

Given that I observe parental education as a categorical variable with only eight different values, one obvious concern in the construction of the rank is that ties occur frequently and may primarily be responsible for the variation in the rank variable. To address this concern, I use alternative definitions of parental education, including the average educational attainment of both parents. This definition of socioeconomic status helps alleviating such concerns because the variation in the level of parental SES is higher as the SES variable can take on $8^2 = 64$ different values and ties occur less frequently. Robustness checks in Section 1.4.2 show that the main results are robust to this alternative SES definition.

1.2.3 Descriptives

Sampling Criteria and Weights For the analytical sample, I only keep individuals with information from both the in-school survey and the in-home survey.[16] Students with missing information on parental socioeconomic background are dropped from the analytical sample. Moreover, I drop students with conflicting school identifiers and students in schools with less than 20 students or cohorts with less than 5 students. Finally, I only keep students with complete information on age, gender

[15] As a robustness check in Section 1.4.2, I use different ways of breaking ties. These analyses yield very similar results and can be found in Appendix A.2.

[16] This decreases the analytical sample considerably because some schools did not participate in the in-school survey or information on the student's identifier in the school interview is missing. In the main analysis, I exclude these individuals.

and race with at least one non-missing short-run outcome variable. These sampling criteria result in a sample of 13,646 students that were assigned sampling weights.[17]

Descriptive Statistics Table 1.1 reports summary statistics to describe the main outcome variables as well as sample characteristics of the respondents at the time of the initial interview (wave I) in Panel A-C. I report the mean, standard deviation, and interquartile range. 48% of the students are male, 51% are white, 21% are black, 7% are asian and 16% have a hispanic background. At the time of the initial interview, the average student age is 15.6 years. The students' depression scores range between 0 and 57 with a mean of 11. Around 20% percent of students score equal to or above a score of 16, a commonly used cutoff to indicate individuals at risk of clinical depression.[18] The average self-esteem score in the sample is 17. Popularity and cognitive ability, by construction have a mean close to 100 and 0, respectively. The analytical sample consists of 120 different schools of which 22% have fewer than 401 students, 48% have between 401 and 1,000 students and 30% have more than 1,000 students. The sample consists of 421 different school cohorts[19] with an average cohort size of 182.

Attrition Of the 13,646 individuals from the main analysis, 10,845 remain in the sample of wave IV for the long-run analysis. The summary statistics of the long-run outcome variables are presented in panel D of Table 1.1. The average CES-D score is 7.4[20], 69% of the sample has been enrolled in college, 35% have already completed a college degree. A more detailed description of the long-run sample

[17] The in-home survey of the Add Health data oversamples some groups, thus I use sampling weights in the regression analysis to account for this sampling design. See Chen and Chantala (2014) for details. Results without sampling weights are provided for the main regressions in Appendix A.3.4.

[18] See Figure A.4 in Appendix A.4.2 for the distribution of the depression score.

[19] Some schools do not have all grades 7-12.

[20] The CES-D score in wave IV only consists of 10 items instead of the 19 items in wave I.

Table 1.1: Sample Descriptives

	mean	sd	25th	median	75th	n
A. Contemporaneous Outcomes (wave I)						
	mean	**sd**	**25th**	**median**	**75th**	**n**
Depression	11.01	7.46	6.00	10.00	15.00	13,594
Cognitive ability	100.99	14.50	92.00	101.00	112.00	13,030
Self-esteem	17.13	4.49	15.00	18.00	20.00	11,858
Popularity	0.08	1.01	-0.67	-0.14	0.60	13,580
B. Individual Characteristics (wave I)						
	mean	**sd**	**25th**	**median**	**75th**	**n**
SES	13.40	3.60	12.00	12.00	16.00	13,646
Family income	47.05	54.30	22.00	40.00	60.00	10,267
Age	15.64	1.69	14.00	16.00	17.00	13,646
Male	0.48	0.50				13,646
Ever repeated a grade	0.19	0.39				13,646
White	0.51	0.50				13,646
Black	0.21	0.41				13,646
Asian	0.07	0.25				13,646
Hispanic	0.16	0.37				1,3646
C. School and Cohort Characteristics (wave I)						
	mean	**sd**	**25th**	**median**	**75th**	**n**
School characteristics:						
Small (<401 students)	0.22	0.41				120
Medium (401-1000 students)	0.48	0.50				120
Large (> 1000 students)	0.30	0.46				120
Cohort characteristics:						
Cohort size	181.62	127.73	87.00	159.00	243.00	421
Mean SES	13.36	1.37	12.52	13.25	14.12	421
SD SES	3.12	0.59	2.78	3.10	3.41	421
D. Long-run Outcomes (wave IV)						
	mean	**sd**	**25th**	**median**	**75th**	**n**
Depression	7.40	3.82	5.00	7.00	9.00	10,833
College attendance	0.69	0.46				10,845
College completion	0.35	0.48				10,845

Note: This table describes the analytical sample for the main analysis. Panels A-C describe the main outcome variables as well as individual, school and cohort level characteristics of the respondents in the sample for the short-run analysis, i.e. at the time of the initial interview (wave I). Panel D describes the outcome variables of the respondents that remain in the long-run sample (wave IV). The table displays the mean, standard deviation, and interquartile range of the variables as well as the number of observations. SES is measured in years of education (as outlined in section 1.2.2), annual family income is measured in thousands U.S. $.

can be found in Table A.3.1. The average respondent in the long-run sample is 28 years old. The sample characteristics of the 10,845 individuals that remain in the sample for the long-run analysis are fairly similar to the initial sample of 13,646 individuals, indicating that attrition is not a major concern for my analysis of the long-term effects. I further address this concern in Appendix A.2 by showing that attrition status is not related to ranks as shown in Appendix Table A.9. Moreover, I re-estimate the results for the main analysis based on students that remain in the sample in wave IV in the Appendix Table A.10 and find very similar results. I therefore conclude that attrition is unlikely to affect my long-term results.

1.3 Empirical Strategy

I seek to estimate the causal effect of a student's socioeconomic rank in their high-school cohort on a set of short and long-run outcomes related to adolescent development. To that end, I exploit variation in the socioeconomic composition of different cohorts within the same school. Such variation arises naturally due to fluctuations in the household characteristics of children of school-starting age in a school's catchment area over time. Utilizing only within-school variation allows me to address concerns regarding the non-random selection of students into schools, which confounds estimates based on global comparisons.

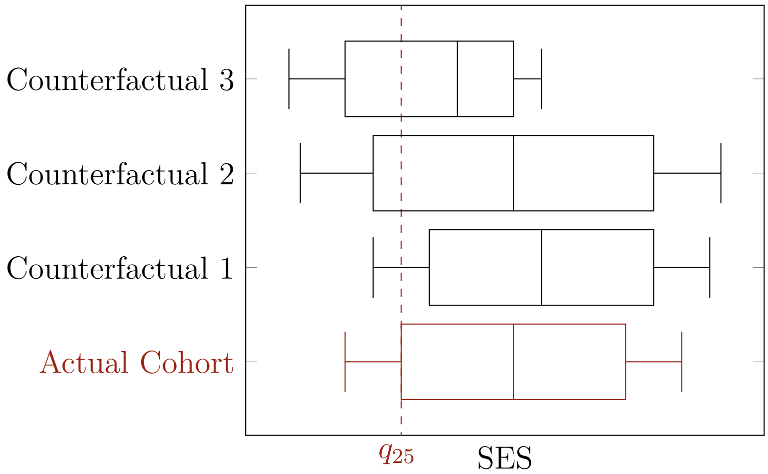
Viewing the observed within-school variation in ranks conditional on SES as quasi-random motivates a conditional mean independence assumption that identifies the causal effect of the rank variable in a linear fixed effects model. In the following, I begin by describing the mechanisms that generate the identifying variation in the composition of cohorts before discussing the functional form of my model and potential threats to its identification.

1.3.1 Intuition

Intuitively, the idea underlying my empirical strategy is the following counterfactual thought experiment: A student of a given socioeconomic background would potentially have had a different rank, had they been a member of a different cohort in their school. With this in mind, I estimate counterfactuals by comparing the outcomes of students of the same socioeconomic background in the same school that differ with respect to the socioeconomic rank assigned to them in their respective cohort. Consequently, my strategy requires variation in ranks within school-SES strata across cohorts. This identifying variation is generated by within-school differences in the shape of the SES distributions across cohorts. For example, consider a student of a given socioeconomic background in a given school and cohort such that the student is located at the 25th percentile in their actual cohort SES distribution. Figure 1.1 illustrates how this student's rank would have differed in counterfactual cohorts that differ from the factual cohort distribution with respect to the mean (Counterfactual 1), the variance (Counterfactual 2), or in general shape (Counterfactual 3). In each counterfactual cohort, the student factually positioned at the 25th percentile would have been assigned a different rank. My empirical strategy seeks to recover the causal effect of a student's relative socioeconomic cohort rank using such within-school across-cohort comparisons.

In theory there are variety of mechanisms that can generate within-school between cohort differences in SES distributions. For example, variation in the timings of birth around school year specific enrolment dates can generate differences in the share of highly educated parents between cohorts. Similarly, variations in cohort sizes, as explained by Hoxby (2000), are likely to induce differences in the shape of cohort SES distributions. While such differences are typically negligible on aggregate levels such as school districts, they can produce pronounced differences on the school level, provided there is some heterogeneity in the types of households

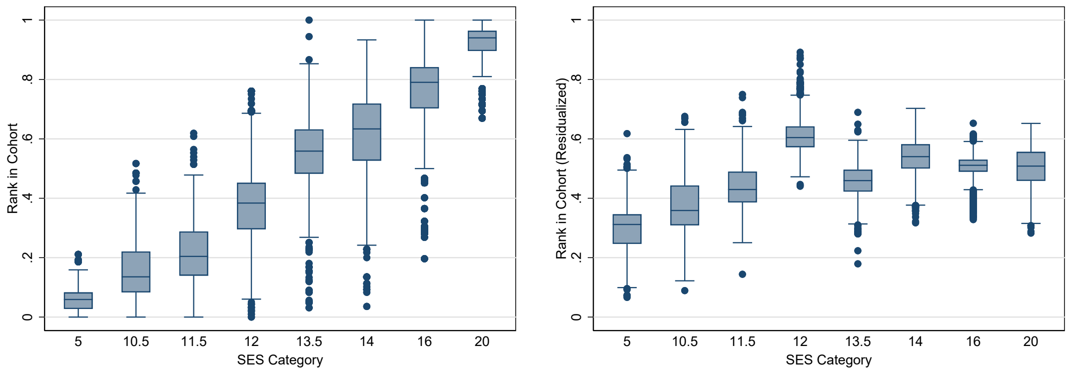
Figure 1.1: Illustration of Identifying Variation



Note: This figure illustrates how differences in the SES distribution across cohorts lead to variation in the rank variable for a fixed level of SES. The figure depicts a hypothetical cohort (red) and fixes the SES level of a student ranked at the 25th percentile in this cohort. In the three counterfactuals, I show how a student with the fixed level of SES would be ranked in cohorts with a different mean (counterfactual 1), a different variance (counterfactual 2), or a generally different shape in the SES distribution (counterfactual 3).

attracted by each school. The extent to which such variation exists in a given data set is an empirical question. Figure 1.2 shows the variation in cohort ranks within each SES category for the schools and cohorts sampled in the Add Health survey.

Figure 1.2: Unconditional and Conditional Variation in Ranks



(A) Unconditional Variation.

(B) Conditional Variation.

Note: This figure plots the variation in SES ranks for each education category (5 "8th grade or less", 10.5 "Completed 8th grade, but no high school degree", 11.5 "GED", 12 "High school degree", 13.5 "Business or vocational school after high school", 14 "Some college", 16 " College degree", 20 "Professional degree"). For each category, I display the median, the 25th and 75th percentiles, and the minimum and maximum of the rank distribution. In panel A, I plot the unconditional variation in ranks. In panel B, I present the variation in ranks conditional on separate school and cohort fixed effects as well as individual and school cohort specific controls.

While Panel A shows the unconditional variation in ranks within each SES category, Panel B displays the variation conditional on separate school and cohort fixed effects as well as individual and cohort level observables used in my preferred model specification. The figure illustrates three important points: First, globally there is substantial variation in ranks within each SES category. Second, unsurprisingly, most variation is observed around the center of the SES distribution, where almost all ranks are observed in certain cohort environments. Finally, the conditional variation in ranks is substantially smaller, which has important implications for the interpretation of my estimates, as it illustrates what type of counterfactuals my estimates are based upon. This is important to keep in mind when interpreting the rank coefficients in my model and extrapolating towards ”extreme” counterfactuals. Specifically, it is unlikely that, for a given level of SES, a student is ranked top in one cohort and bottom in a different cohort in the same school. The last observation also illustrates the main practical challenge reflected in my modelling choice: I seek to solve a trade-off between flexibility and precision. While more flexibile functional forms mitigate misspecification concerns, they come at the cost of less precise estimates. This is because in order to pin down the rank effect, I require sufficient variation in ordinal ranks within the strata defined by my model.

1.3.2 Empirical Model

I impose the following general additively separable fixed effects model that relates the outcome y_{isc} of student i in school s and cohort c to their cohort rank according to

$$y_{isc} = \beta \text{Rank}_{isc} + f\left(\text{SES}_{isc}\right) + \boldsymbol{\gamma} \mathbf{X}_{isc} + g\left(s, c\right) + u_{isc}. \tag{1.2}$$

As discussed in Section 1.2.2, the rank variable is approximately uniformly distributed on $[0, 1]$ by construction. The vector X_{isc} contains predetermined individual-level characteristics such as age in days, gender and ethnicity. The functions f and g denote flexible functional forms of a student's level of SES as well as different school and cohort fixed effects specifications.

The model parameter of interest is β, which captures the causal effect of the ordinal rank on the respective outcome. Note that, while my counterfactual thought experiment compared students within schools, the constant effects assumption underlying β justifies across-school comparisons in residualized outcomes and ranks. Following textbook arguments, β is identified under the following strict exogeneity assumption:

$$\mathbb{E}\left[u_{isc} | \text{Rank}_{isc}, \text{SES}_{isc}, \mathbf{X}_{isc}, g\left(s, c\right)\right] = 0. \tag{1.3}$$

The strict exogeneity assumption (1.3) is conditional on the functional form assumption in equation (1.2) in the sense that its interpretation and plausibility depend on the choices for the functions f and g. Consequently, the key challenge is to parameterize these functions such that assumption (1.3) is plausible, keeping in mind the flexibility-precision trade-off mentioned above. For f, I consider different dummy-specifications that capture SES-bin specific averages ($f(SES_{isc}) = \sum_{j=1}^{K} \delta_j D_j(SES_{isc})$).[21]

For g, I consider three different choices: (i) separate school (120) and cohort (6) fixed effects ($g\left(s, c\right) = \lambda_s + \lambda_c$), (ii) separate school and cohort fixed effects augmented by school cohort specific control variables ($g\left(s, c\right) = \lambda_s + \lambda_c + \alpha \mathbf{W}_{sc}$), as well as (iii) school-by-cohort (421) fixed effects ($g\left(s, c\right) = \lambda_{sc}$).

[21] In my preferred specification, I assign the SES levels to four different categories: "high school or less", "some college or vocational training", "college", and "postgraduate". The grouping of SES categories into bins is varied in Appendix A.2. Alternatively, I also consider a linear and quadratic function of SES.

My initial model contains separate school and cohort fixed effects. This model uses variation in the socioeconomic rank within schools and rules out systematic self-selection of students into schools as a confounding factor. In this model, the strict exogeneity assumption requires that all cohort level unobservables are uncorrelated with the rank variable. This model is best viewed as a rough approximation, as school cohort specific characteristics such as the average SES are mechanically correlated with the rank and likely to have an effect on outcomes via traditional peer effect mechanisms as pointed out in Elsner and Isphording (2017). Arguments along these lines motivate my second model, where I include observable school cohort characteristics to mitigate omitted variable concerns at the cohort-level. Specifically, $\mathbf{W}_{sc}$ includes the mean and standard deviation of cohort-SES, the fraction of repeaters, the gender composition, and share of white students in each school cohort.

While including a set of school cohort specific charactristics makes the strict exogeneity assumption appear more plausible, I cannot rule out the existence of relevant unobserved cohort characteristics that impact outcomes via less obvious peer-effect mechanisms. In particular, Elsner and Isphording (2017) discuss dynamic selection along unobservable cohort characteristics as a potential threat to the strict exogeneity assumption. Such concerns motivate my third model which includes school-by-cohort fixed effects, effectively ruling out that school cohort specific confounders drive my estimation results. This approach compares students across all school cohorts after removing all school cohort specific mean differences. Note, that in this model β is still estimable from differences in the shape of the SES distribution.

The last specification of my model guards my empirical results from potential confounding caused by school and school cohort specific unobservable characteristics. However, strict exogeneity also posits the absence of individual level unobservables that correlate with the residualized rank. While my research design does not allow me to rule out the existence of such individual level confounders, the institutional setting I study provides some arguments mitigating such concerns. The arguably most important behavioral assumption that I rely on is that parents cannot exactly anticipate the relative socioeconomic rank of their child in a specific cohort when making their school choice. While my design allows and accounts for choices based on (unobservable) school and cohort characteristics, school choices based on ranks would violate strict exogeneity. Abstracting from the fact that it appears unlikely that parents have the necessary information to make such a choice, rank based school choices would likely lead to strategic delays in enrolment, since there are limited school options available in each school district. Appendix A.2 contains evidence showing that the data does not support the notion of strategic enrolment delays, suggesting that rank based school choices are not a major concern for my analysis.

1.4 Results

This section presents the results of my empirical analysis. I first show the average effect of a student's socioeconomic rank on contemporaneous outcomes of adolescent development, specifically depression, cognitive ability, self-esteem, and a student's popularity. In Section 1.4.2, I show that these result hold for a series of robustness checks. I then study potential heterogeneities in the rank effect (section 1.4.3), emphasizing the role of inequality in exacerbating the impact of the socioeconomic rank. In section 1.4.4, I proceed to look into the long-term effects of socioeconomic rank on depression and educational attainment and how much of the long-run effects are mediated through the observed contemporaneous effects (section 1.4.5).

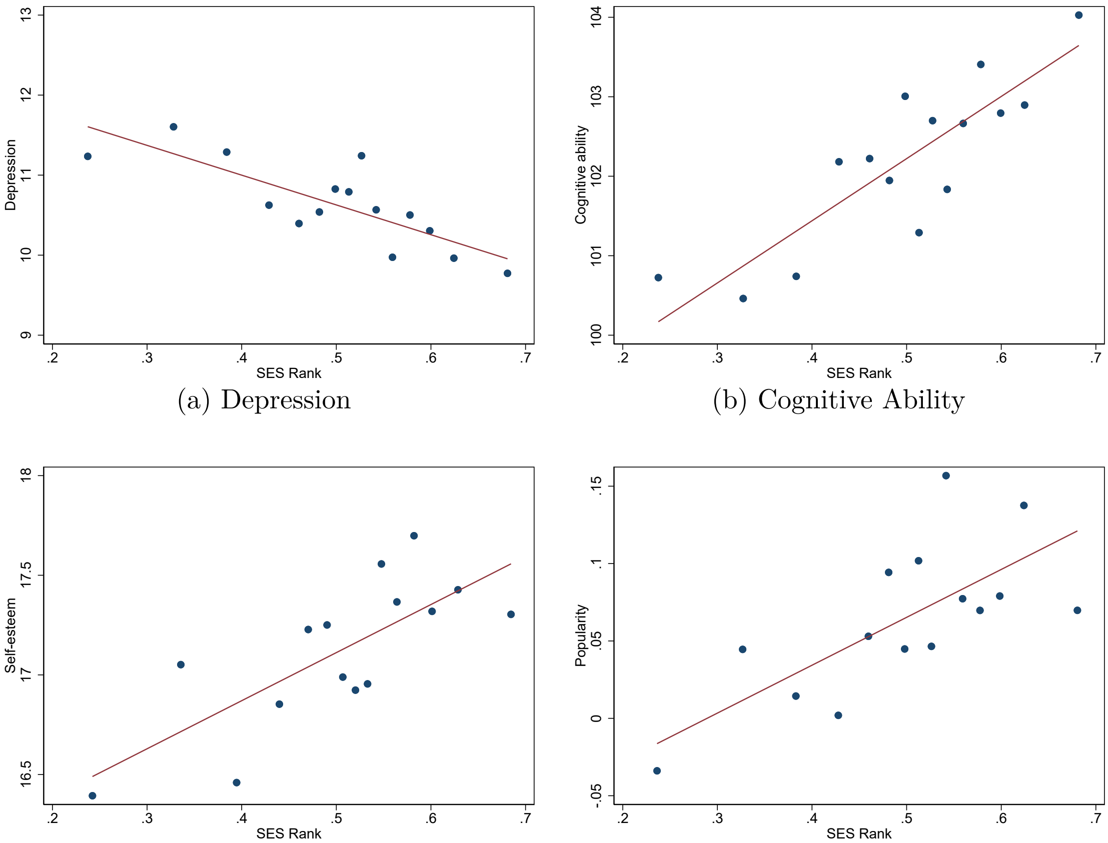
1.4.1 Average Effect of the Socioeconomic Rank

In this section, I analyze the effect of a student's socioeconomic rank within a school cohort on the contemporary outcomes depression, cognitive ability, self-esteem and popularity. Figure 1.3 visualizes OLS regressions of equation (1.2) for each of these outcomes with separate school and cohort fixed effects as well as all individual and school cohort-level controls. I find a negative relationship between the socioeconomic rank and depression: for a given level of socioeconomic status, a higher rank reduces the student's depression score, that is a higher rank is asscociated with lower depressive symptoms. Conversely, cognitive skills, self-esteem and popularity are positively related to the socioeconomic rank. Students with a higher rank have better cognitive skills, higher levels of self-esteem and receive more friendship nominations in comparison to their cohort peers.

These findings are substantiated in Table 1.2 which reports the β-coefficients for different specifications of equation (1.2) for each of the four outcome variables: depression[22], cognitive ability, self-esteem, and popularity. When interpreting the rank coefficients, it is important to keep in mind that, while the rank variable as defined in Section 1.2.2 has the support [0,1], extreme counterfactuals are unlikely to occur within a given school. In fact, Figure 1.2 demonstrates that students of the same socioeconomic background are not ranked top in one cohort and ranked bottom in a different cohort of the same school. Within a given school, the variation in the rank variable for a given level of SES is much smaller. In order to facilitate the interpretation of my results, I re-scale the coefficient estimates to represent a more realistic comparison. In particular, the reported coefficient estimates represent the

[22]In Appendix Table A.12, I additionally estimate the rank effect on the probability to be classified as being at risk for clinical depression, measured as an indicator variable for CES-D ≥ 16.

Figure 1.3: Average Effect of the Socioeconomic Rank



(a) Depression (b) Cognitive Ability

(c) Self-esteem (d) Popularity

Note: Each panel visualizes the effect of the socioeconomic rank based on the linear fixed effects specification in equation (1.2), accounting for the level of SES, individual (age in days, gender, race) and school cohort specific (mean and standard deviation of SES, fraction of repeaters, male share, and share of white students in the cohort) controls as well as for separate school and cohort fixed effects. Both the x- and y-variables are residualized and the sample mean of each variable is added back to the residuals. The panels display the average values of (a) depression, (b) cognitive ability, (c) self-esteem, and (d) popularity for 15 equally large rank bins.

effect of a 25 percentage point increase in the ordinal rank.[23] That is, the reported coefficients always compare a student that is ranked at, for example, the median to a student that is ranked at the 75th percentile of their cohort SES-distribution.

[23]This is a more realistic counterfactual, but by no means a small change given the conditional variation observed in Figure 1.2. A 25 percentatge point increase approximately corresponds to a one-standard deviation increase in the rank variable.

In column (1) of Table 1.2, I estimate the rank coefficient, controlling for the level of socioeconomic status as well as separate school and cohort fixed effects. Holding constant the level of socioeconomic status, moving from the median to the 75th percentile rank within a cohort is associated with an improvement of -1.08 points in the depression score, 2.48 points in the cognitive ability test score, 0.53 points on the self-esteem scale, and an increase in popularity by 0.10 standard deviations within a student's cohort. In column (2), when accounting for student characteristics, most of the rank coefficients are moderately smaller in absolute size, but qualitatively robust.

As discussed in section 1.3.2, this specification is unlikely to fulfill the strict exogeneity assumption because school cohort specific characteristics such as the average SES are mechanically correlated with the rank and likely to have an effect on my outcomes via traditional peer effects. In column (3), I, therefore, additionally control for school cohort specific characteristics to disentangle the socioeconomic rank effect from potential confounders at the school cohort level. The rank coefficients change only slightly. For a given level of socioeconomic status, increasing a student's rank by 25 percentiles, decreases the depression score by -0.93 points or 0.12 standard deviations[24], and increases the cognitive ability test score by 1.96 points (0.13 standard deviations) and the self-esteem score by 0.60 points (0.13 standard deviations). Further, such a rank shift leads to a 0.08 standard deviations increase in a student's popularity among their peers. These findings hold when estimating equation (1.2) using school-by-cohort fixed effects in column (4) to absorb all school cohort-specific characteristics as discussed in section 1.3.2. Overall, the estimated rank coefficients are relatively stable across specifications, lending credibility to the observed rank effects.

[24]This result is confirmed by the finding that the SES rank has a negative effect on the probability of being classified as at risk for clinical depression (CES-D $\geq$ 16). Table A.12 in Appendix A.3 shows that a 25 percentile increase in rank leads to a 4 percentage point lower likelihood of being classified as at risk for clinical depression.

Table 1.2: Average Effect of the Socioeconomic Rank

	(1)	(2)	(3)	(4)
Panel A: Depression				
CES-D	-1.08***	-0.92***	-0.93***	-0.96***
	(0.21)	(0.20)	(0.21)	(0.20)
Effect size	[-0.15]	[-0.12]	[-0.12]	[-0.13]
Number of observations	13,594	13,594	13,594	13,594
Panel B: Cognitive Ability				
Peabody	2.48***	1.79***	1.96***	1.84***
	(0.35)	(0.35)	(0.35)	(0.36)
Effect size	[0.17]	[0.12]	[0.13]	[0.13]
Number of observations	13,030	13,030	13,030	13,030
Panel C: Self-esteem				
6-item Rosenberg	0.53***	0.57***	0.60***	0.62***
	(0.13)	(0.13)	(0.13)	(0.14)
Effect size	[0.12]	[0.13]	[0.13]	[0.14]
Number of observations	11,858	11,858	11,858	11,858
Panel D: Popularity				
In-degree	0.10***	0.08***	0.08***	0.08***
	(0.02)	(0.02)	(0.02)	(0.03)
Number of observations	13,580	13,580	13,580	13,580
Level of SES	yes	yes	yes	yes
Individual controls	no	yes	yes	yes
Cohort controls	no	no	yes	no
School and cohort FE	yes	yes	yes	no
School x cohort FE	no	no	no	yes

Note: Standard errors clustered at school level in parantheses; * p<0.10, ** p<0.05, *** p<0.01. The table reports the estimated coefficients on the socioeconomic rank from different specifications of equation (1.2) with the outcome variables: depression (panel A) cognitive ability (panel B), self-esteem (panel C), and popularity (panel D). The rank variable is re-scaled such that the reported coefficients represent the effect of a 25 percentile increase in rank. Results with the original rank scale are presented in Appendix A.3.3. The effect size is calculated in terms of the standard deviation of the outcome variable. Column (1) includes separate school and cohort fixed effects and controls for the absolute level of SES. In column (2), individual controls (age in days, gender, and race) are added. In column (3), school cohort specific controls (mean and standard deviation of SES, fraction of repeaters, male share, and share of white students in the cohort) are additionally included. Column (4) controls for individual characteristics and school-by-cohort fixed effects. Wave I cross-sectional weights are used.

In order to get a better idea of the estimated effect sizes, I proceed by comparing these rank effects to the effects associated with a change in school quality. Schools differ along multiple dimensions, such as teacher quality, school facilities, or peer quality, and attending a better school is generally associated with significant gains in students' outcomes. Comparable to Murphy and Weinhardt (2020), I use the size of the school fixed effects from equation (1.2) as a benchmark for overall school quality. This allows me to compare the estimated rank effects to the effects associated with a change in school quality. For depression and cognitive ability, a one-standard deviation increase in school quality is associated with a 1.5 point decrease in the depression score and a 3.5 point increase in the cognitive ability test scores. This implies that increasing the socioeconomic rank by 25 percentiles, holding constant school quality, is equivalent to increasing school quality by approximately 0.6 standard deviations, net of the rank of a student. In Appendix A.3.5, I characterize the aspects of school quality captured by the fixed effects by providing evidence on the correlation between the estimates and standard indicators of school quality.

1.4.2 Robustness

Strategic Delay of School Entry The central identifying assumption for a causal interpretation of the rank coefficient is the strict exogeneity condition. One potential concern regarding this condition is that parents may strategically delay their child's school entry, thereby imposing a potential violation of this assumption. In order to address this concern, I restrict my sample to students whose age is sufficiently close - within one standard deviation - to the average age in their school cohort. The argument here is that, for these students, strategic delays can be plausibly ruled out as a confounding factor. Based on the results presented in Appendix Table A.4, I conclude that strategic delay is not a threat to identification. Compared to the baseline, the estimates for depression and self-esteem are moderately larger while the results for popularity are a bit smaller and less precisely estimated. The results on cognitive ability are comparable to the baseline estimate.

Functional Form and SES-bins One concern for the identification of a causal rank effect could be misspecification in the regression model. Importantly, the plausibility of the strict exogeneity assumption always depends on the functional form assumption of my regression model in equation (1.2). This includes $f(SES)$, which defines the way in which I control for the level of SES in the model. Note again, that the choice of f is subject to a flexibility-precision trade-off in the sense that a more flexible choice restricts the rank variation that remains in order to estimate the rank coefficient. In the baseline estimation, I use four different SES-bins to capture SES-bin specific averages. Alternatively, one could think of different combinations to bin the SES categories or use a linear or quadratic function of SES. The results of these estimations can be found in Appendix Table A.5. For the main outcomes, depression and cognitive ability, the results remain robust to all alternative specifications of f. For self-esteem and popularity, the results remain robust when using alternative SES-bins, however the rank effect vanishes when using a linear or quadratic specification. I do not necessarily take this as evidence against a rank effect on self-esteem and popularity because misspecification could be a bigger issue in these alternative specifications. In fact, the baseline model with SES-bin specific averages allows for more flexibility and is thus less likely to suffer from misspecification than a linear or quadratic function of SES.

Breaking Ties When computing a student's rank within a cohort, one decision one has to make is how to break ties. In the main analsis, students are assigned the average rank in case of ties. Alternative ways to break ties include assigning students the lower rank, i.e. only counting students with a strictly lower socioeconomic status when ordering students, or to assign students the higher rank, i.e. only counting students with a strictly higher socioeconomic status. In order to verify that the results are not driven by the way to break ties, I re-estimate the rank coefficient, constructing the rank variable according to each of the two alternatives. The results are presented in Appendix Table A.6. While the way to break ties has an impact on the size of the estimated regression coefficients, the results remain qualitatively robust to the alternative definitions.

Definition of Socioeconomic Status So far in this paper, the socioeconomic status of a student is measured as the educational attainment of the student's father.[25] Alternatively, I could define the socioeconomic status of a student based on mothers' educational attainment, the highest level of educational attainment or the average educational attainment of both parents. In Appendix Table A.7, I compare these different definitions of socioeconomic status and find that the estimates are robust to the precise definition. Moreover, as discussed in Section 1.2.2, using the average educational attainment of both parents allows me to address and alleviate concerns regarding the high incidence of ties in the construction of the rank variable.

Four-factor Model of Depression When originally developed, a factor analysis by Radloff (1977) showed that the CES-D can be divided into four subscales that represent different factors, but are all symptoms related to depression. The four factors identified by Radloff (1977) have been confirmed in various studies, however, alternative factor structures have been proposed as well. Using principal components analysis with varimax rotation in the Add Health data, I find 4 factors with eigenvalues greater than one that account for 51% of the variance.[26] Compared to Radloff (1977), some items loaded differently on the four factors. Table A.2 shows

[25]Exceptions are made if the student reports not living with father or the father's information is missing. In this case, the mother's educational attainment is used.

[26]The corresponding scree plot can be found in Appendix Figure A.1.

the rotated factor loadings of all items. Including items with factor loadings above 0.40 identifies 4 factors with similar interpretation to Radloff (1977):

I. Depressed affect: *bothered, appetite, blues, depressed, failure, fearful, lonely, sad, worth living*

II. Positive affect: *good, hopeful, happy, enjoyed life*

III. Somatic symptoms: *mind, tired, get started*

IV. Interpersonal problems: *unfriendly, disliked*

Each of the 4 subscales' score is computed as the sum of the items and divided by the number of items to facilitate the comparison between the subscales. Regression results of equation (1.2) with the four subscales of depression as outcome variables are presented in Table A.8. Strikingly, the socioeconomic rank of a student in their high school cohort has an impact on all four factors of the CES-D. Moreover, the effect size seems to be comparable across factors, though slightly larger for depressed affect. This confirms the main results and demonstrates that the rank effect on depression is not driven by a single factor or item in the depression score.

1.4.3 Heterogeneous Effects

In this section, I study potential heterogeneities in the effect of the socioeconomic rank along multiple dimensions. First, I explore whether the degree of inequality within a cohort impacts the magnitude to which the socioeconomic rank affects depression, cognitive ability, self-esteem, and popularity during high school. I then proceed to study heterogeneities along the individual level, including gender and race.

Exploring the Role of Inequality By construction, the measure of socioeconomic rank estimates a student's relative position, but ignores any notion of distance between peers. However, the distance between two rank positions may matter for the degree to which the socioeconomic rank affects adolescent development. In line with relative deprivation theory, higher degrees of inequality likely lead to larger differences between the desired situation and one's own, thus elicit higher degrees of envy, shame and humiliation and could intensify competition among peers. In this section, I therefore study the extent to which inequality within a student's comparison group, i.e. the school cohort, affects the socioeconomic rank gradient.

Inequality is measured using the standard deviation of the SES-distribution within a school cohort. All school cohorts are then ordered according to the magnitude of this standard deviation and divided into quintiles. Cohorts with the lowest degree of inequality are assigned to the first quintile and cohorts with the highest degree of inequality are assigned to the 5th quintile.

To test for the role of inequality in the relationship between the socioeconomic rank and students' contemporaneous outcomes, I estimate equation (1.2), interacting the rank with indicators for each inequality quintile. Table 1.3 depicts a clear pattern in the estimated coefficients on the socioeconomic rank for the different quintiles: Irrespective of the level of inequality within a cohort, the relationship between a student's socioeconomic rank and all four contemporaneous outcomes holds. However, the estimated rank coefficients increase in absolute size with the degree of inequality within a cohort. Holding fixed the level of socioeconomic status, higher ranked students gain more compared to lower ranked students when inequality is high in their cohort. This pattern is quite striking in its consistency across outcomes.

The observed pattern is reassuring as it confirms my main results and is consistent with theories of relative deprivation. From an equity perspective, these results can be viewed as a motivation for policy interventions aimed at reducing the salience of inequality as this could mitigate the adverse effects of relative deprivation.

Table 1.3: Heterogeneous Effect of the SES Rank by the Degree of Inequality

Inequality quintile	1st	2nd	3rd	4th	5th
Depression	-0.82***	-0.83***	-1.05***	-1.02***	-1.12***
	(0.22)	(0.22)	(0.25)	(0.27)	(0.30)
Cognitive ability	1.39***	1.82***	2.03***	2.20***	3.30***
	(0.40)	(0.35)	(0.36)	(0.37)	(0.51)
Self-esteem	0.37**	0.61***	0.75***	0.76***	0.76***
	(0.15)	(0.15)	(0.14)	(0.15)	(0.16)
Popularity	0.07**	0.07**	0.09***	0.09***	0.10***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)

Note: Standard errors clustered at school level in parantheses; * p<0.10, ** p<0.05, *** p<0.01. Inequality quintiles group school cohorts into quintiles based on the standard deviation of the school cohort-level SES distribution. The table reports the estimated rank coefficients when interacting the socioeconomic rank with indicators of these inequality quintiles in equation (1.2) for each outcome: depression, cognitive ability, self-esteem, and popularity. The model specification includes separate school and cohort fixed effects and controls for school cohort specific controls (mean and standard deviation of SES, fraction of repeaters, male share, and share of white students in the cohort), the level of SES, and individual controls (age in days, gender, and race). The rank variable is re-scaled such that the reported coefficients represent the effect of a 25 percentile increase in rank. Wave I cross-sectional weights are used.

Heterogeneity Along the Rank Distribution One natural question that arises is whether the observed rank effects exist along the complete rank distribution or whether they only materialize for lower-ranked students. To address this question, I construct an indicator variable, $\mathbb{1}(Rank_{isc} > 0.5)$ that takes on the value 1 if the student is ranked above the median in their school cohort and 0 otherwise. I estimate equation (1.2), interacting the rank variable with this indicator variable and report the resulting coefficient estimates and 95% confidence intervals for students below and above the median rank in their cohorts in Figure A.2. The pattern clearly points to a heterogeneous effect along the rank distribution: While the relationship between the socioeconomic rank and all contemporaneous outcomes holds for both students below and above the median rank in their cohort, the effects are stronger for students with ranks below the median.

Other Heterogeneities In a next step, I study potential effect heterogeneities along individual characteristics. Specifically, I look at differences in the rank effect by gender and race. Figure A.3 in Appendix A.4 depicts the estimated rank effects that result from interacting the rank in equation (1.2) with gender or race dummies. For gender, the results show that, along all outcomes, both boys and girls are affected by their socioeconomic rank position. If anything, girls tend to react slightly stronger to their rank position, however, the depicted differences are not statistically significant. For race, I first distinguish white students from students with any other racial background. The results show that differences between the two groups are not necessarily statistically significant, but the estimated rank coefficients are systematically stronger for white students. A more detailed split by race shows, however, that the coarse classification into white and non-white students masks substantial heterogeneities across the races and outcomes.

1.4.4 Persistence of Effects

A natural question that arises in the context of the observed socioeconomic rank effects is whether these effects are persistent. The importance of mental health as well as cognitive, non-cognitive and social skills for human capital development would suggest that the relative socioeconomic status has long-term consequences for economic success and well-being. I therefore investigate the long-term effects of socioeconomic rank on depression and educational attainment during adulthood, that is when respondents are between 24 and 32 years old. To this end, I estimate equation (1.2), using wave IV outcome measures for the 10-items CES-D score and dummies for college attendance and college completion as dependent variables.

The results of the different model specifications are presented in Table 1.4. Similarly to before, the reported coefficients represent the effect of an increase in rank by 25 percentiles. Overall, the estimated coefficients are very stable across the different model specifications, signaling that a higher within-cohort rank during high school is associated with significantly lower depression scores and better odds at attending and completing college. The coefficient estimates in column (3) and (4) imply that a 25 percentile increase in the socioeconomic rank during high school reduces depression scores by 0.48 points. This is equivalent to a reduction by 0.13 standard deviations, an effect size similar to the one reported on short-run depression. This finding is consistent with evidence documenting the persistence of mental health problems. Further, a 25 percentile increase in the socioeconomic rank is associated with a higher likelihood of attending and completing college by 5 and 3-4 percentage points.

Similar as before, I use the estimated school fixed effects from equation (1.2) to compare the rank effect to the effect of school quality on the outcomes. A one-standard deviation increase in school quality is associated with a decrease of 0.8 points in long-term depression and a 10.5 and 15.7 percentage points higher likelihood of attending and completing college, respectively. With respect to college attendance and completion, this implies that increasing the socioeconomic rank by 25 percentiles, holding constant school quality, is equivalent to increasing the school quality by 0.5 and 0.2 standard deviations, holding constant the rank of a student. In regards to mental health, such a rank increase is equivalent to an increase in school quality by 0.6 standard deviations.

Table 1.4: Persistence of the Rank Effect

	(1)	(2)	(3)	(4)
Panel A: Long-run Depression				
CES-D (10 items)	-0.47***	-0.45***	-0.48***	-0.48***
	(0.15)	(0.16)	(0.16)	(0.16)
Effect size	[-0.12]	[-0.12]	[-0.13]	[-0.13]
Number of observations	10,833	10,833	10,833	10,833
Panel B: College				
Attending college	0.06***	0.04***	0.05***	0.05***
	(0.01)	(0.01)	(0.01)	(0.01)
Completing college	0.05***	0.04***	0.03***	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)
Number of observations	10,845	10,845	10,845	10,845
Level of SES	yes	yes	yes	yes
Individual controls	no	yes	yes	yes
Cohort controls	no	no	yes	no
School and cohort FE	yes	yes	yes	no
School x cohort FE	no	no	no	yes

Note: Standard errors clustered at school level in parantheses; * p<0.10, ** p<0.05, *** p<0.01. The table reports the estimated coefficients on the socioeconomic rank from different specifications of equation (1.2) for the long-run outcomes: the 10-item CES-D (panel A) and dummies for college completion and college attendance (panel B). The rank variable is re-scaled such that the reported coefficients represent the effect of a 25 percentile increase in rank. Results with the original rank scale are presented in Appendix A.3.3. The effect size is calculated in terms of the standard deviation of the outcome variable. Column (1) includes separate school and cohort fixed effects and controls for the absolute level of SES. In column (2), individual controls (age in days, gender, and race) are added. In column (3), school cohort specific controls (mean and standard deviation of SES in the cohort, fraction of repeaters, male share, share of white students in the cohort) are additionally included. Column (4) controls for individual characteristics and school-by-cohort fixed effects. Wave IV cross-sectional weights are used.

1.4.5 Mediation Analysis

In light of the persistent effects of a student's socioeconomic rank within their high school cohort on mental health and educational attainment, an interesting question to ask is to what extent these long-run effects are mediated by the observed short-run effects. Specifically, I am interested in the importance of adolescent mental health as a mediator. Since the available data does not allow for an approriate causal mediation analysis, which at the minimum would require some type of sequential ignorability assumption (see e.g. Imai et al., 2011), which is almost certainly violated in the present context, it is beyond the scope of this paper to provide a full answer to this question.

However, my results allow me to conduct back-of-the-envelope calculations that are suggestive of the relative importance of depression for long-run educational attainment. The goal is to split the "total" effect of the rank on long-run outcomes into a "direct" effect and an "indirect" effect. The "indirect" effect refers to the effect of rank on long-run depression and educational attainment that operates through mediators. The mediators of interest, m_{isc}, are adolescent depression, dep_{isc}, cognitive ability, cog_{isc}, self-esteem, $self_{isc}$, and popularity, pop_{isc}. To this end, I estimate a set of equations, regressing each of the long-run outcomes, y_{isc}, and each of the mediators on the socioeconomic rank using (1.2). Moreover, I estimate an auxilliary regression in which the potential mediators are added as regressors when estimating equation (1.2) for the long-run outcomes. For example, in the case of depression as mediator, I estimate the following set of equations:

$$y_{isc} = \alpha_1 + \beta_1 \text{Rank}_{isc} + f\left(\text{SES}_{isc}\right) + \mathbf{X}_{isc}\boldsymbol{\gamma}_1 + g\left(s, c\right) + u_{1,isc}$$
$$m_{isc} = \alpha_2 + \beta_2 \text{Rank}_{isc} + f\left(\text{SES}_{isc}\right) + \mathbf{X}_{isc}\boldsymbol{\gamma}_2 + g\left(s, c\right) + u_{2,isc}$$

with $m_{isc} = dep_{isc}$.

Finally, I estimate the auxilliary regression in which all potential mediators are added as regressors:

$$y_{isc} = \alpha_3 + \beta_3 \text{Rank}_{isc} + \beta_d \text{dep}_{isc} + \beta_c \text{cogn}_{isc} + \beta_s \text{self}_{isc} + \beta_p \text{pop}_{isc} + f\left(\text{SES}_{isc}\right) + \mathbf{X}_{isc}\boldsymbol{\gamma}_3 + g\left(s, c\right) + u_{3,isc}.$$

For each outcome, the effect mediated through depression is then defined as the product of $\beta_2\beta_d$. Dividing this product by the total rank effect β_1 yields the share of the socioeconomic rank effect mediated by depression. Analogously, the shares mediated through cognitive ability, self-esteem, and popularity are estimated.

Table 1.5: Mediation Analysis

			% of total effect mediated			
	(1)	(2)	Depression	Cognitive ability	Self-esteem	Popularity
Panel A: Long-run Depression						
CES-D (10 items)	-0.42**	-0.24	29.08	0.82	10.95	1.81
	(0.17)	(0.16)				
Number of observations	8,975	8,975				
Panel B: College						
Attending college	0.04***	0.03*	9.32	26.69	6.10	6.38
	(0.01)	(0.01)				
Completing college	0.03**	0.01	13..66	33.42	7.90	13.36
	(0.01)	(0.01)				
Number of observations	8,985	8,985				
Level of SES	yes	yes				
Individual controls	yes	yes				
Cohort controls	yes	yes				
School and cohort FE	yes	yes				
Mediators	no	yes				

Note: Standard errors clustered at school level in parantheses; * p<0.10, ** p<0.05, *** p<0.01. The table reports rank coefficients from a regression of long-run outcomes on the socioeconomic rank according to equation (1.2) with separate school and cohort fixed effects as well as controls for the level of SES, individual (age in days, gender, and race) as well as school cohort specific (mean and standard deviation of SES in the cohort, fraction of repeaters, male share, share of white students) controls. The sample is reduced to individuals with complete information on all mediators. Column (1) replicates the results from column (3) of Table 1.4 with the reduced sample size. Column (2) reports the rank coefficients from auxilliary regressions that add all potential mediators (depression, cognitive ability, self-esteem and popularity during high school) as regressors. The rank variable is re-scaled such that the reported coefficients represent the effect of a 25 percentile increase in rank. Columns (3)-(6) display the share of the rank effect that is mediated by: depression, cognitive ability self-esteem, and popularity during high school. Wave IV cross-sectional weights are used.

Table 1.5 presents the results from this exercise. It reports the total socioeconomic rank effect from regressing the long-run outcomes on the socioeconomic rank according to equation (1.2) in colum (1).[27] In column (2), all mediators are added as regressors. This reduces the rank coefficient considerably for all long-term outcomes. The right-hand side of Table 1.5 displays the computed shares of the total effect that are mediated by adolescent depression, cognitive ability, self-esteem, and popularity measured in high school for each of the three long-term outcomes. Unsurprisingly, adolescent depression is the most important mediator for the relationship between socioeoeconomic rank and the depression score in adulthood, accounting for almost 30% of the total rank effect. In combination with self-esteem, a mediator closely connected to mental health, more than 40% of the total rank effect are mediated by these two factors. Cognitive ability and popularity are only weak mediators. In comparison, cognitive ability in high school is the most important factor that mediates the effect of socioeconomic rank on college attainment, both in terms of college attendance and college completion. Roughly 30% of the rank effect on educational attainment is mediated through this factor. However, depression, self-esteem, and popularity also seem to be important channels through which the rank effect impacts educational attainment. Taken together, these three factors are almost equally important for educational attainment, compared to cognitive ability.

1.5 Conclusion

Motivated by the importance of mental health for adolescents' unimpeded development, this paper provides new causal evidence on the effect of relative parental socioeconomic status on adolescents' mental health, cognitive ability and educational attainment. I show that the relative socioeconomic status has a significant

[27]The estimated regression coefficients deviate slightly from the reported coefficients in Table 1.4 because the sample was reduced to individuals with complete information on all mediators. Otherwise, the specifications in column (1) of Table 1.5 and column (3) of Table 1.4 are identical.

and economically meaningful impact on adolescents' personal development that persists into adulthood.

The short-run effects documented in my analysis demonstrate that socioeconomic ranks impact teenagers' development along several important and interrelated dimensions. In particular, I find that higher ranks lead to reductions in depression scores, improved cognitive ability and self-esteem as well as higher levels of social integration as measured by friendship nominations.

While my data does not allow me to pin down the specific mechanisms underlying the causal rank effects, the patterns I document are consistent with theories of social comparisons and relative deprivation widely accepted in the sociology and social psychology literature. I document that the estimated rank effects are more pronounced in cohorts with higher levels of SES-inequality across all considered outcome dimensions, suggesting that social comparisons have non-negligible impacts on adolescents' mental health and behavior.

Strikingly, the rank effects on depression persist into adulthood with effect sizes almost identical to those documented in the short-run. My findings are consistent with evidence documenting high levels of persistence of mental health disorders, highlighting the importance of mental health at early ages and interventions designed to reduce risks to mental health during this period. I also find substantial long-run effects on educational attainment as measured by college attendance and completion.

An important question that arises in this context is to what extent mental health problems impede the accumulation of human capital. While the data available does not allow me to conduct an appropriate mediation analysis, I provide suggestive evidence that depression does in fact impede human capital development as measured by educational attainment. As a complete assessment of the economic costs of mental health disorders of teenagers requires quantifying this link, future research on this question is needed.

The results documented in this paper can be viewed as motivation and justification for policies aimed at reducing the salience of inequality in schools. From an equity perspective, such policies could be an effective tool to mitigate the adverse mental health and human capital consequences of relative deprivation and thus enhance educational outcomes and intergenerational mobility. Importantly, the potential gains of such policies for lower-ranked students outweigh the potential losses of higher-ranked students because the rank effects are stronger for students ranked below the median. Concrete efforts of this type could entail the provision of paid-for school meals, school uniforms and subsidized leisure activities.

Appendix A

A.1 Appendix: Outcome Measures

A.1.1 The CES-D

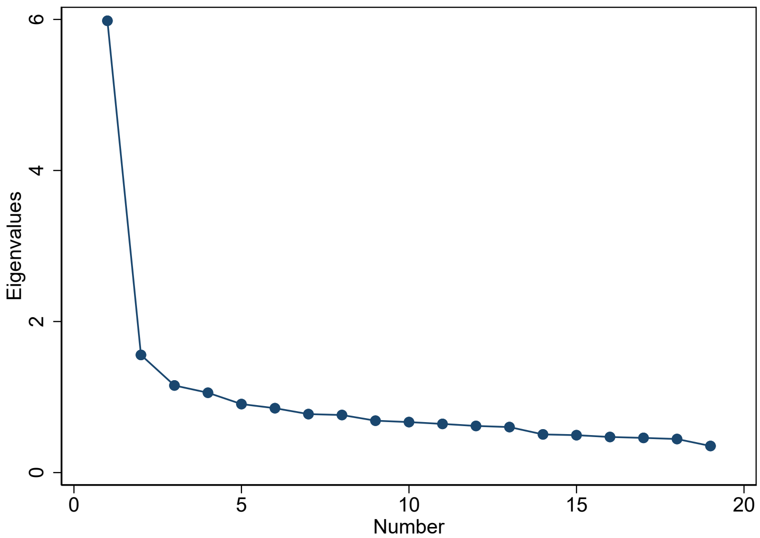
The Center of Epidemiologic Studies-Depression (CES-D) asks about the frequency with which an individual experienced symptoms associated with depression in the last week. The response options range from 0 to 3 for each item (0 = Never or rarely, 1 = Sometimes, 2 = A lot of the time, 3 = Most of the time or all of the time). Positively worded items were reverse coded. The CES-D is constructed as the sum of all items and ranges from 0 - 57 with higher scores indicating a higher degree of depressive symptoms. A score equal to or above 16 is commonly referred to as a cutoff for being at risk for clinical depression.

Table A.1: The CES-D

Measure	Item	Scale
CES-D	You were bothered by things that don't usually bother you.*	Never 0 – 3 most/all of the time
	You didn't feel like eating, your appetite was poor.	
	You felt that you could not shake off the blues, even with help from your family and your friends.*	
	You felt you were just as good as other people. (reverse coded)*	
	You had trouble keeping your mind on what you were doing.*	
	You felt depressed.*	
	You felt that you were too tired to do things.*	
	You felt hopeful about the future. (reverse coded)	
	You thought your life had been a failure.	
	You felt fearful.	
	You were happy. (reverse coded)*	
	You talked less than usual.	
	You felt lonely.	
	People were unfriendly to you.	
	You enjoyed life. (reverse coded)*	
	You felt sad.*	
	You felt that people disliked you.*	
	It was hard to get started doing things.	
	You felt life was not worth living.	

Note: This table displays the items in wave I of the Add Health data set that were used to construct the outcome variable depression (CES-D). Positively worded questions were reverse coded. The final CES-D score was computed as the sum of all items. Items marked with an asterik (*) indicate questions that were also asked during the wave IV interview and were used to construct the CES-D 10-item measure of depression in the long-run.

Figure A.1: Screeplot of a Principal Component Analysis of the CES-D



Note: This figure presents a screeplot of principal components, using all items of the CES-D in wave I. It identifies four factors with eigenvalues larger than 1.

Table A.2: Factor Loadings of CES-D Items

	Depressed Affect	Positive Affect	Somatic Symptoms	Interpersonal Problems
You were bothered by things that usually don't bother you.	**0.54**	0.09	0.33	0.07
You didn't feel like eating, your appetite was poor.	**0.46**	0.09	0.35	-0.09
You could not shake the blues, even with help from your friends and family.	**0.73**	0.14	0.21	0.05
You felt that you were just as good as other people.	0.09	**0.68**	0.04	0.12
You had trouble keeping your mind on what you were doing.	0.30	0.10	**0.60**	0.11
You felt depressed.	**0.76**	0.18	0.20	0.13
You felt that you were too tired to do things.	0.20	0.11	**0.69**	0.14
You felt hopeful about the future.	0.00	**0.76**	0.09	0.01
You thought your life had been a failure.	**0.57**	0.22	-0.01	0.33
You felt fearful.	**0.46**	0.02	0.16	0.30
You were happy.	0.32	**0.68**	0.09	0.05
You talked less than usual.	0.29	0.12	0.32	0.10
You felt lonely.	**0.65**	0.12	0.16	0.22
People were unfriendly to you.	0.09	0.02	0.15	**0.81**
You enjoyed life.	0.31	**0.68**	0.07	0.11
You felt sad.	**0.70**	0.14	0.16	0.21
You felt that people disliked you.	0.24	0.13	0.11	**0.78**
It was hard to get started doing things.	0.12	0.08	**0.70**	0.22
You felt life was not worth living.	**0.54**	0.20	-0.06	0.32

Note: This table reports factor loadings of each item in the CES-D for the four principal component factors. Bold items with loadings larger than 0.4 are assigned to the fours factors: depressed affect, positive affect, somatic symptoms, and interpersonal problems.

A.1.2 Self-Esteem Scale

To measure self-esteem, six items similar to or adapted from the Rosenberg self-esteem scale (Rosenberg, 1965) were used. Students were asked how much they agreed or disagreed on a 5-point Likert scale with the statements presented in the table below. The final score is computed as the sum of all items and ranges from 0 - 24 with higher values indicating higher self-esteem.

Table A.3: The Adapted Rosenberg Self-Esteem Scale

Measure	Do you agree or disagree that you...	Scale
Rosenberg Self-Esteem	have many good qualities	Strongly disagree 0 - 4 Strongly agree
	have a lot to be proud of	
	like yourself just the way you are	
	feel you are doing things just about right	
	feel socially accepted	
	feel loved and wanted	

Note: This table displays the items in wave I of the Add Health data set that were used to construct the self-esteem score. Items are originally rated on a scale from 1 (Strongly agree) to 5 (Strongly disagree) and were reverse coded and scaled to range from 0-4. The overall score of self-esteem is computed as the sum of all items.

A.2 Appendix: Robustness Checks

Table A.4: Test for Strategic Delay

	Depression	Cognitive ability	Self-esteem	Popularity
Rank Coefficient	-1.11***	1.90***	0.68***	0.06*
	(0.26)	(0.37)	(0.16)	(0.03)
Number of observations	9,647	9,277	8,470	9,630

Note: Standard errors clustered at school level in parantheses; * p<0.10, ** p<0.05, *** p<0.01. The table reports the estimated rank coefficients from estimating equation (1.2) with the level of SES, all individual (age in days, gender, and race) and school cohort (mean and standard deviation of SES in the cohort, fraction of repeaters, the male share, and the share of white students in a cohort) controls as well as separate school and cohort fixed effects. The rank variable is re-scaled such that the reported coefficients represent the effect of a 25 percentile increase in rank. Each column refers to a different outcome. The sample is restricted to individuals within 1 standard deviation of the average age level in the school cohort. Wave I cross-sectional weights are used.

Table A.5: Alternative SES-Bins and Functional Form

	4 SES bins (Baseline)	3 SES bins	linear SES	quadratic SES
Panel A: Depression				
CES-D	-0.93***	-0.73***	-0.70***	-0.83***
	(0.21)	(0.18)	(0.26)	(0.27)
Number of observations	13,594	13,594	13,594	13,594
Panel B: Cognitive Ability				
Peabody	1.96***	2.00***	0.95**	1.18***
	(0.35)	(0.31)	(0.41)	(0.41)
Number of observations	13,030	13,030	13,030	13,030
Panel C: Self-esteem				
6-item Rosenberg	0.60***	0.56***	0.10	0.16
	(0.13)	(0.12)	(0.16)	(0.16)
Number of observations	11,858	11,858	11,858	11,858
Panel D: Popularity				
In-degree	0.09***	0.07***	0.01	0.02
	(0.02)	(0.02)	(0.03)	(0.03)
Number of observations	13,580	13,580	13,580	13,580

Note: Standard errors clustered at school level in parantheses; * p<0.10, ** p<0.05, *** p<0.01. The table reports the estimated rank coefficients from estimating equation (1.2) with the level of SES, all individual (age in days, gender, and race) and school cohort (mean and standard deviation of SES in the cohort, fraction of repeaters, the male share, and the share of white students in a cohort) controls as well as separate school and cohort fixed effects. The rank variable is re-scaled such that the reported coefficients represent the effect of a 25 percentile increase in rank. Each column refers to a different specification of $f()$ in equation (1.2). In column (1), SES is controlled for through 4 SES-bins (”high school or less”, ”some college” ”college”, and ”postgraduate”). In column (2), SES is controlled for through 3 SES-bins (”high school or less”, ”some college” ”at least college”). In column (3) and (4), linear and quadratic functions of the SES variable are used, respectively. Wave I cross-sectional weights are used.

Table A.6: Alternative Ways to Break Ties

	Average (Baseline)	Lower	Higher
Panel A: Depression			
CES-D	-0.93***	-1.13***	-0.61***
	(0.21)	(0.25)	(0.15)
Number of observations	13,594	13,594	13,594
Panel B: Cognitive Ability			
Peabody	1.96***	2.07***	1.39***
	(0.35)	(0.46)	(0.25)
Number of observations	13,030	13,030	13,030
Panel C: Self-esteem			
6-item Rosenberg	0.60***	0.52***	0.46***
	(0.13)	(0.16)	(0.10)
Number of observations	11,858	11,858	11,858
Panel D: Popularity			
In-degree	0.09***	0.08***	0.05***
	(0.02)	(0.03)	(0.02)
Number of observations	13,580	13,580	13,580

Note: Standard errors clustered at school level in parantheses; * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The table reports the estimated rank coefficients from estimating equation (1.2) with the level of SES, all individual (age in days, gender, and race) and school cohort (mean and standard deviation of SES in the cohort, fraction of repeaters, the male share, and the share of white students in a cohort) controls as well as separate school and cohort fixed effects. The rank variable is re-scaled such that the reported coefficients represent the effect of a 25 percentile increase in rank. Different methods to calculate the rank, in particular different rules to break ties for students with the same socioeconomic status, are used. The 'Average' rank coincides with the baseline estimate; ties are assigned the average rank of the tied positions. The 'Lower' rank is computed counting the number of individuals with a strictly lower socioeconomic status. In contrast, the 'Higher' rank assigns the rank based on the number of individuals with a strictly higher socioeconomic status. Wave I cross-sectional weights are used.

Table A.7: Alternative Definitions of SES

	Father (Baseline)	Mother	Highest Education	Average Eduation
Panel A: Depression				
CES-D	-0.93***	-0.95***	-0.90***	-0.83***
	(0.21)	(0.22)	(0.26)	(0.21)
Number of observations	13,594	13,594	13,594	13,594
Panel B: Cognitive Ability				
Peabody	1.96***	2.13***	2.26***	2.05***
	(0.35)	(0.33)	(0.43)	(0.33)
Number of observations	13,030	13,030	13,030	13,030
Panel C: Self-esteem				
6-item Rosenberg	0.60***	0.35**	0.31*	0.43***
	(0.13)	(0.15)	(0.16)	(0.12)
Number of observations	11,858	11,858	11,858	11,858
Panel D: Popularity				
In-degree	0.09***	0.09***	0.08**	0.07***
	(0.02)	(0.03)	(0.03)	(0.02)
Number of observations	13,580	13,580	13,580	13,580

Note: Standard errors clustered at school level in parantheses; * p<0.10, ** p<0.05, *** p<0.01. The table reports the rank coefficients from estimating equation (1.2) with the level of SES, all individual (age in days, gender, and race) and school cohort (mean and standard deviation of SES in the cohort, fraction of repeaters, the male share, and the share of white students in a cohort) controls as well as separate school and cohort fixed effects. The rank variable is re-scaled such that the reported coefficients represent the effect of a 25 percentile increase in rank. Each column refers to a different definition on how to define a students' socioeconomic status. SES is defined as the father's educational attainment in column (1), the mother's educational attainment in column (2), the highest educational attainment of both parents in column (3), and the average parental education in column (4). Wave I cross-sectional weights are used.

Table A.8: Rank Effect on 4 Factors of Depression

	Depressed affect	Positive affect	Somatic symptoms	Interpersonal probblems
Rank coefficient	-0.07***	-0.04***	-0.05***	-0.05***
	(0.02)	(0.01)	(0.02)	(0.01)
Number of observations	13,594	13,594	13,594	13,594

Note: Standard errors clustered at school level in parantheses; * p<0.10, ** p<0.05, *** p<0.01. The table reports the coefficient on the socioeconomic rank for the 4 facors of depression that have been identified via principal component analysis in Appendix A.1.1: (i) depressed affect, (ii) positive affect, (iii) somatic symptoms, and (iv) interpersonal problems. Controls include the absolute level of SES, individual controls (age in days, gender, race) and school cohort controls (mean and standard deviation of SES in the cohort, fraction of repeaters, male share, and share of white students in the cohort) as well as separate school and cohort fixed effects. The rank variable is re-scaled such that the reported coefficients represent the effect of a 25 percentile increase in rank. Wave I cross-sectional weights are used.

Table A.9: Test for Attrition Bias

	(1)	(2)
Rank	0.01	0.00
	(0.01)	(0.01)
Number of observations	13,646	13,646
Level of SES	yes	yes
Individual conrols	yes	yes
Cohort controls	yes	no
School and cohort FE	yes	no
School x cohort FE	no	yes

Note: Standard errors clustered at school level in parantheses; * p<0.10, ** p<0.05, *** p<0.01. The table reports the estimated coefficients on the socioeconomic rank from different specifications of equation (1.2) with an indicator for attrition as outcome. The rank variable is re-scaled such that the reported coefficients represent the effect of a 25 percentile increase in rank. Column (1) uses the specification with separate school and cohort fixed effects and controls for the level of SES, and individual (age in days, gender, and race) and school cohort (mean and standard deviation of SES in the cohort, fraction of repeaters, the male share, and the share of white students in a cohort) controls. Column (2) uses school-by-cohort fixed effects and controls for the level of SES and individual characteristics. Wave I cross-sectional weights are used.

Table A.10: Short-Run Effects Based on Long-Run Sample (Wave IV)

	Depression	Cognitive ability	Self-esteem	Popularity
Rank	-0.84**	1.73***	0.53***	0.07**
	(0.24)	(0.37)	(0.14)	(0.03)
Number of observations	10,808	10,368	9,480	10,798

Note: Standard errors clustered at school level in parantheses; * p<0.10, ** p<0.05, *** p<0.01. The table reports the estimated rank coefficients from estimating equation (1.2) with the level of SES, individual (age in days, gender, and race) and school cohort (mean and standard deviation of SES in the cohort, fraction of repeaters, the male share, and the share of white students in a cohort) controls as well as separate school and cohort fixed effects. The rank variable is re-scaled such that the reported coefficients represent the effect of a 25 percentile increase in rank. The sample is restricted to individuals that remained in the long-run sample in wave IV. Wave I cross-sectional weights are used.

A.3 Appendix: Additional Tables

A.3.1 Descriptives - Long Run

Table A.11: Descriptives of the Long-Run Sample

Long-run Outcomes						
	mean	**sd**	**25th**	**median**	**75th**	**n**
Depression	7.40	3.82	5.00	7.00	9.00	10,833
College attendance	0.69	0.46	0.00	1.00	1.00	10,845
College completion	0.35	0.48	0.00	0.00	1.00	10,845

B. Individual Characteristics						
	mean	**sd**	**25th**	**median**	**75th**	**n**
SES	13.41	3.58	12.00	12.00	16.00	10,845
Family income	47.28	52.40	23.00	40.00	60.00	8,340
Age	28.48	1.73	27.00	29.00	30.00	10,845
Male	0.46	0.50				10,845
White	0.53	0.50				10,845
Black	0.21	0.41				10,845
Asian	0.06	0.23				10,845
Hispanic	0.15	0.36				10,845

Note: This table describes the sample characteristics of the individuals that remain in the sample in the long-run analysis (wave IV). Panel A describes the main long-run outcome variables. Panel B describes individual sample characteristics, measured in wave I, of this sample. The table displays the mean, standard deviation, and interquartile range of the variables as well as the number of observations. SES is measured in years of education (as outlined in section 1.2.2), annual family income is measured in thousands U.S. $.

A.3.2 Risk of Clinical Depression

Table A.12: Average Effect of the Socioeconomic Rank on Risk of Clinical Depression

	(1)	(2)	(3)	(4)
Panel A: Depression				
CES-D $\geq$ 16	-0.05***	-0.04***	-0.04***	-0.04***
	(0.01)	(0.01)	(0.01)	(0.01)
Number of observations	13,594	13,594	13,594	13,594
Level of SES	yes	yes	yes	yes
Individual controls	no	yes	yes	yes
Cohort controls	no	no	yes	no
School and cohort FE	yes	yes	yes	no
School x cohort FE	no	no	no	yes

Note: Standard errors clustered at school level in parantheses; * p<0.10, ** p<0.05, *** p<0.01. The table reports the estimated coefficients on the socioeconomic rank from different specifications of equation (1.2) with an indicator for CES-D $\geq$ 16 as outcome. The rank variable is re-scaled such that the reported coefficients represent the effect of a 25 percentile increase in rank. Column (1) only controls for separate school and cohort fixed effects and the absolute level of SES. In column (2), individual controls (age in days, gender, and race) are added. In column (3), school cohort specific controls (mean and standard deviation of SES in the cohort, fraction of repeaters, the male share, and the share of white students in a cohort) are additionally included. Column (4) controls for individual characteristics and school-by-cohort fixed effects. Wave I cross-sectional weights are used.

A.3.3 Main Results With Oringinal Rank Scale

Table A.13: Short-Run Rank Effects - Original Rank Scale

	(1)	(2)	(3)	(4)
Panel A: Depression				
CES-D	-4.32***	-3.69***	-3.72***	-3.85***
	(0.82)	(0.81)	(0.84)	(0.82)
Number of observations	13,594	13,594	13,594	13,594
Panel B: Cognitive Ability				
Peabody	9.93***	7.15***	7.82***	7.37***
	(1.41)	(1.38)	(1.39)	(1.45)
Number of observations	13,030	13,030	13,030	13,030
Panel C: Self-esteem				
6-item Rosenberg	2.13***	2.26***	2.41***	2.48***
	(0.52)	(0.53)	(0.54)	(0.55)
Number of observations	11,858	11,858	11,858	11,858
Panel D: Popularity				
In-degree	0.38***	0.32***	0.31***	0.31***
	(0.10)	(0.10)	(0.10)	(0.10)
Number of observations	13,580	13,580	13,580	13,580
Level of SES	yes	yes	yes	yes
Individual controls	no	yes	yes	yes
Cohort controls	no	no	yes	no
School and cohort FE	yes	yes	yes	no
School x cohort FE	no	no	no	yes

Note: Standard errors clustered at school level in parantheses; * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The table reports the estimated coefficients on the socioeconomic rank from different specifications of equation (1.2) with the outcome variables: depression (panel A) cognitive ability (panel B), self-esteem (panel C), and popularity (panel D). The rank variable is **not** re-scaled and the reported coefficients represent the effect of a change from the bottom rank to the top rank, i.e. from rank 0 to rank 1. Column (1) includes separate school and cohort fixed effects and controls for the absolute level of SES. In column (2), individual controls (age in days, gender, and race) are added. In column (3), school cohort specific controls (mean and standard deviation of SES in the cohort, fraction of repeaters, male share, and share of white students in the cohort) are additionally included. Column (4) controls for individual characteristics and school-by-cohort fixed effects. Wave I cross-sectional weights are used.

Table A.14: Long-Run Rank Effects - Original Rank Scale

	(1)	(2)	(3)	(4)
Panel A: Long-run Depression				
CES-D (10 items)	-1.89***	-1.80***	-1.93***	-1.91***
	(0.60)	(0.62)	(0.63)	(0.63)
Number of observations	10,833	10,833	10,833	10,833
Panel B: College				
Attending college	0.23***	0.16***	0.19***	0.18***
	(0.06)	(0.05)	(0.06)	(0.05)
Completing college	0.19***	0.14***	0.13***	0.16***
	(0.05)	(0.04)	(0.04)	(0.05)
Number of observations	10,845	10,845	10,845	10,845
Level of SES	yes	yes	yes	yes
Individual controls	no	yes	yes	yes
Cohort controls	no	no	yes	no
School and cohort FE	yes	yes	yes	no
School x cohort FE	no	no	no	yes

Note: Standard errors clustered at school level in parantheses; * p<0.10, ** p<0.05, *** p<0.01. The table reports the estimated coefficients on the socioeconomic rank from different specifications of equation (1.2) for the long-run outcomes: the 10-item CES-D (panel A) and dummies for college completion and college attendance (panel B). The rank variable is **not** re-scaled and the reported coefficients represent the effect of a change from the bottom rank to the top rank, i.e. from rank 0 to rank 1. Column (1) includes separate school and cohort fixed effects and controls for the absolute level of SES. In column (2), individual controls (age in days, gender, and race) are added. In column (3), school cohort specific controls (mean and standard deviation of SES in the cohort, fraction of repeaters, male share, share of white students in the cohort) are additionally included. Column (4) controls for individual characteristics and school-by-cohort fixed effects. Wave IV cross-sectional weights are used.

A.3.4 Main Results Without Sampling Weights

Table A.15: Short-Run Rank Effects - No Sampling Weights

	(1)	(2)	(3)	(4)
Panel A: Depression				
CES-D	-0.85***	-0.67***	-0.71***	-0.70***
	(0.15)	(0.15)	(0.15)	(0.15)
Number of observations	13,594	13,594	13,594	13,594
Panel B: Cognitive Ability				
Peabody	1.81***	1.21***	1.42***	1.42***
	(0.29)	(0.28)	(0.26)	(0.26)
Number of observations	13,030	13,030	13,030	13,030
Panel C: Self-esteem				
6-item Rosenberg	0.51***	0.52***	0.58***	0.58***
	(0.10)	(0.10)	(0.10)	(0.10)
Number of observations	11,858	11,858	11,858	11,858
Panel D: Popularity				
In-degree	0.09***	0.09***	0.09***	0.09***
	(0.02)	(0.02)	(0.02)	(0.02)
Number of observations	13,580	13,580	13,580	13,580
Level of SES	yes	yes	yes	yes
Individual controls	no	yes	yes	yes
Cohort controls	no	no	yes	no
School and cohort FE	yes	yes	yes	no
School x cohort FE	no	no	no	yes

Note: Standard errors clustered at school level in parantheses; * p<0.10, ** p<0.05, *** p<0.01. The table reports the estimated coefficients on the socioeconomic rank from different specifications of equation (1.2) with the outcome variables: depression (panel A) cognitive ability (panel B), self-esteem (panel C), and popularity (panel D). The rank variable is re-scaled such that the reported coefficients represent the effect of a 25 percentile increase in rank. Column (1) includes separate school and cohort fixed effects and controls for the absolute level of SES. In column (2), individual controls (age in days, gender, and race) are added. In column (3), school cohort specific controls (mean and standard deviation of SES in the cohort, fraction of repeaters, male share, and share of white students in the cohort) are additionally included. Column (4) controls for individual characteristics and school-by-cohort fixed effects. No weights are used.

Table A.16: Long-Run Rank Effects - No Sampling Weights

	(1)	(2)	(3)	(4)
Panel A: Long-run Depression				
CES-D (10 items)	-0.37***	-0.37***	-0.40***	-0.37***
	(0.08)	(0.08)	(0.09)	(0.09)
Number of observations	10,833	10,833	10,833	10,833
Panel B: College				
Attending college	0.05***	0.03***	0.04***	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)
Completing college	0.04***	0.03***	0.03***	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)
Number of observations	10,845	10,845	10,845	10,845
Level of SES	yes	yes	yes	yes
Individual controls	no	yes	yes	yes
Cohort controls	no	no	yes	no
School and cohort FE	yes	yes	yes	no
School x cohort FE	no	no	no	yes

Note: Standard errors clustered at school level in parantheses; * p<0.10, ** p<0.05, *** p<0.01. The table reports the estimated coefficients on the socioeconomic rank from different specifications of equation (1.2) for the long-run outcomes: the 10-item CES-D (panel A) and dummies for college completion and college attendance (panel B). The rank variable is re-scaled such that the reported coefficients represent the effect of a 25 percentile increase in rank. Column (1) includes separate school and cohort fixed effects and controls for the absolute level of SES. In column (2), individual controls (age in days, gender, and race) are added. In column (3), school cohort specific controls (mean and standard deviation of SES in the cohort, fraction of repeaters, male share, share of white students in the cohort) are additionally included. Column (4) controls for individual characteristics and school-by-cohort fixed effects. No weights are used.

A.3.5 School Quality

Table A.17: Correlation of School Fixed Effects and Indicators of School Quality

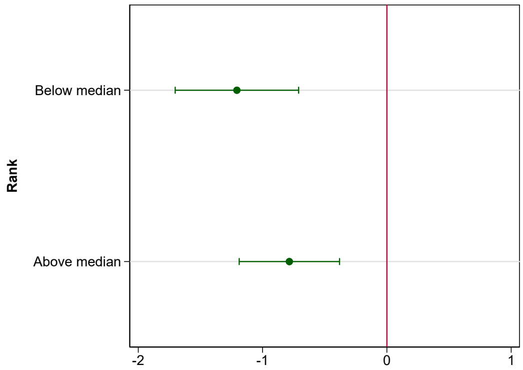
	Short-run outcomes		Long-run outcomes		
	Depression	Cognitive ability	Depression	College attendance	College completion
Average SES	-0.59***	0.94***	0.10	0.05***	-0.05***
	(0.15)	(0.35)	(0.09)	(0.01)	(0.01)
Fraction: college students	0.05	3.76	-2.04***	0.40***	0.87***
	(1.20)	(2.84)	(0.70)	(0.10)	(0.05)
Average class size	0.00	-0.17**	-0.01	0.00**	-0.00
	(0.03)	(0.07)	(0.02)	(0.00)	(0.00)
% School drop outs	-0.00	-0.02	0.03	-0.00	-0.00*
	(0.04)	(0.09)	(0.02)	(0.00)	(0.00)
Teacher-student ratio	-4.38*	-0.05	-1.69	0.46**	0.03
	(2.35)	(5.55)	(1.37)	(0.19)	(0.11)
% Teachers with MA or higher	-0.00	-0.00	0.00	0.00	-0.00
	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)
Additional controls:					
School size	yes	yes	yes	yes	yes
Region	yes	yes	yes	yes	yes
Urbanicity	yes	yes	yes	yes	yes
School type	yes	yes	yes	yes	yes

Note: Standard errors reported in parantheses; * p<0.10, ** p<0.05, *** p<0.01. The table reports the estimated coefficients of a regression of the school fixed effects on different indicators of school quality. The school fixed effects are estimated from equation (1.2) with the level of SES, individual (age in days, gender, and race) and school cohort (mean and standard deviation of SES in the cohort, fraction of repeaters, the male share, and the share of white students in a cohort) controls as well as separate school and cohort fixed effects for each of the main short- and long-run outcomes: depression and cognitive ability in the short-run and depression, college attendance, and college completion in the long-run. Measures of school quality include the school-average SES, the fraction of students attending college in the long-run, the average class size, the average percent of dropouts across all grades, the ratio of full-time teachers to students, and the percent of teachers with at least an MA degree.

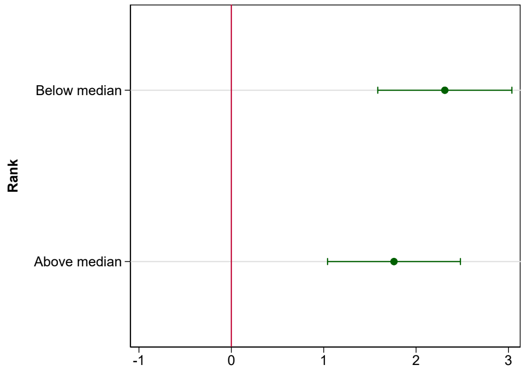
A.4 Appendix: Additional Figures

A.4.1 Heterogeneous Effects

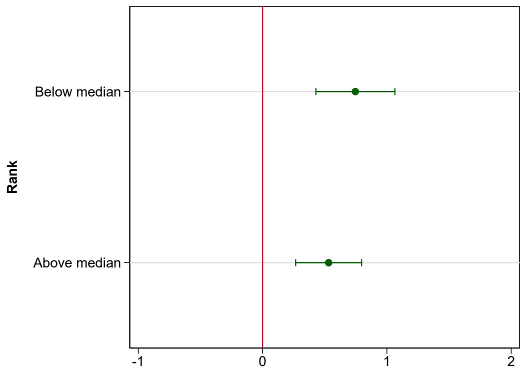
Figure A.2: Heterogeneity by Rank



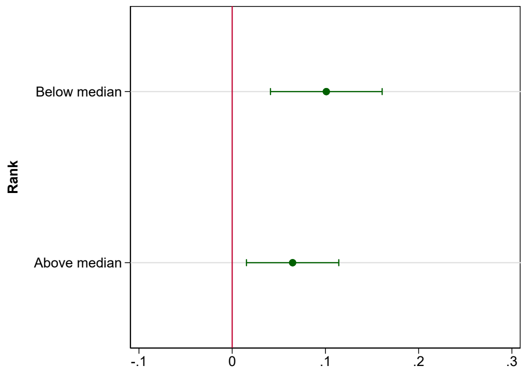
(a) Depression



(b) Cognitive ability



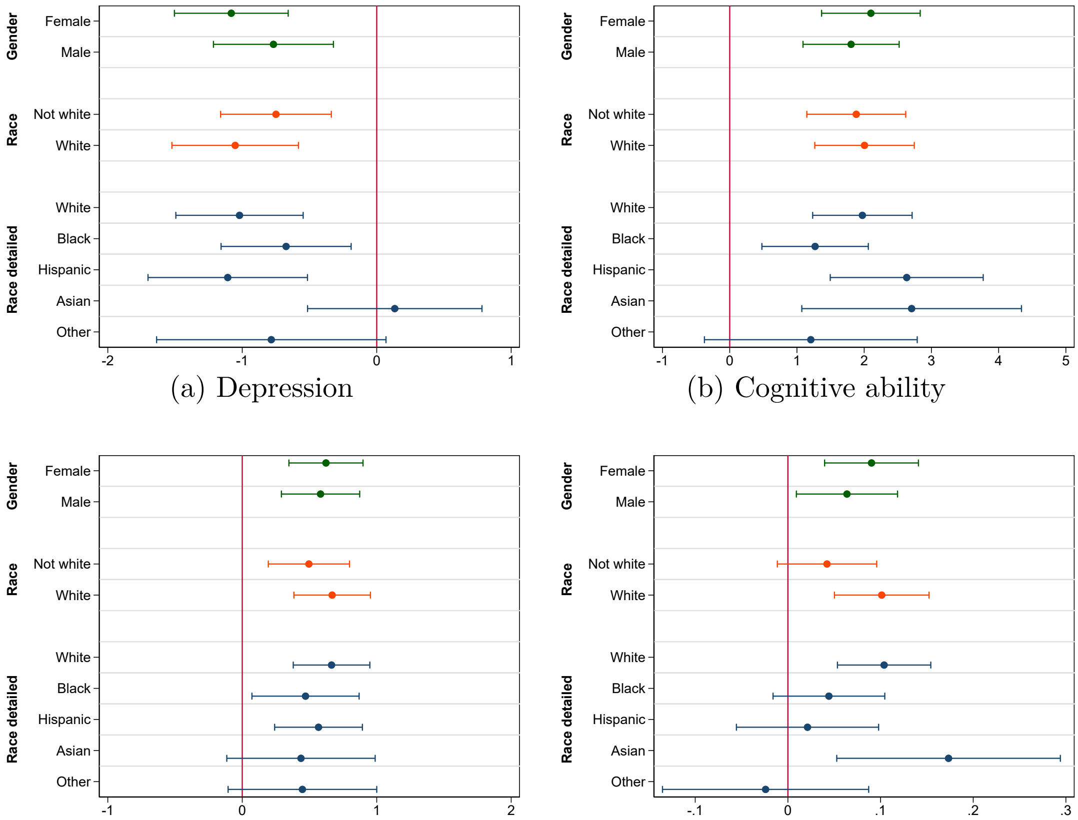
(c) Self-esteem



(d) Popularity

Note: The figure shows the rank effect for each of the short-run outcomes for the two subgroups: (i) students with a rank at or below the median, and (ii) students with a rank above the median. It displays point estimates with 95% confidence intervals. To get the depicted coefficients, the rank is interacted with an indicator variable $\mathbb{1}(Rank_{isc} > 0.5)$ in equation (1.2) with separate school and cohort fixed effects and controls for the level of SES, individual (age in days, gender, and race) and school cohort (mean and standard deviation of SES in the cohort, fraction of repeaters, the male share, and the share of white students in a cohort) characterictics. The depicted rank coefficients are re-scaled to represent the effect of a 25 percentile increase in the socioeconomic rank. Wave I cross-sectional weights are used.

Figure A.3: Heterogeneity by Individual Characteristics



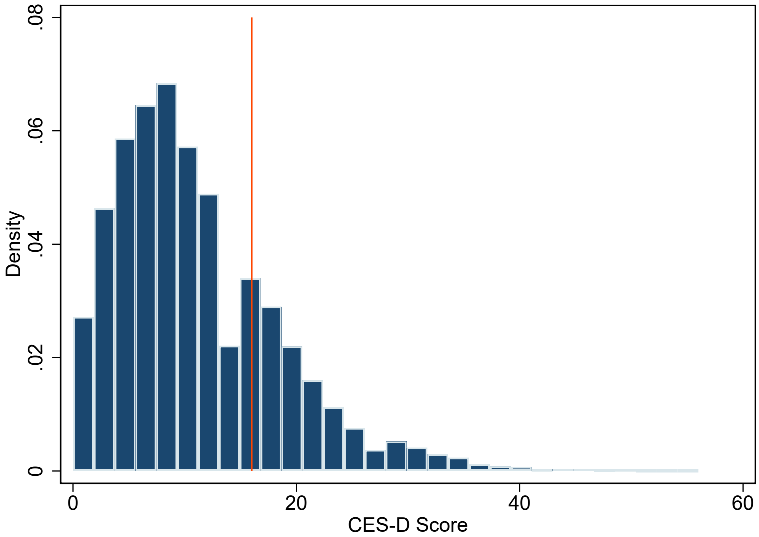
(a) Depression (b) Cognitive ability

(c) Self-esteem (d) Popularity

Note: The figure shows heterogeneities in the rank effect for each of the four outcomes by gender and race. It displays point estimates with 95% confidence intervals. To get the depicted coefficients, the rank is interacted with dummies for either gender or race in equation (1.2) with separate school and cohort fixed effects and controls for the level of SES, individual (age in days, gender, and race) and school cohort (mean and standard deviation of SES in the cohort, fraction of repeaters, the male share, and the share of white students in a cohort) characterictics. The rank variable is re-scaled such that the depicted coefficients represent the effect of a 25 percentile increase in rank. Wave I cross-sectional weights are used.

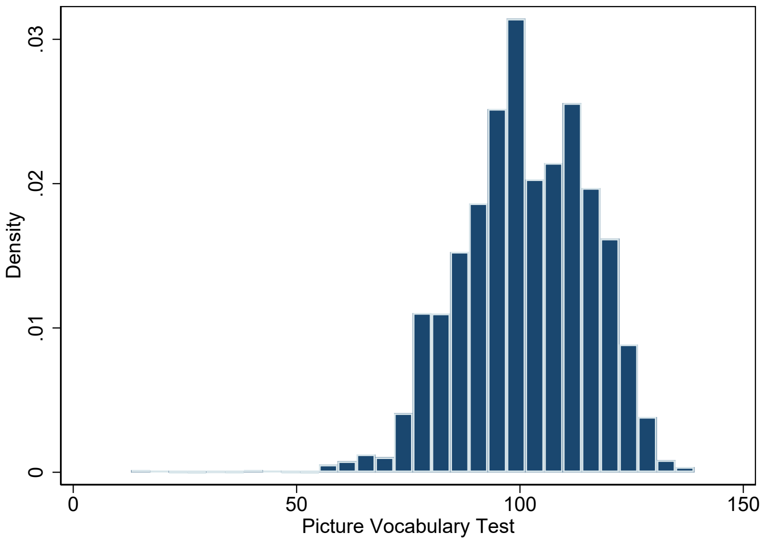
A.4.2 Outcome Measures

Figure A.4: Distribution of the Depression Score



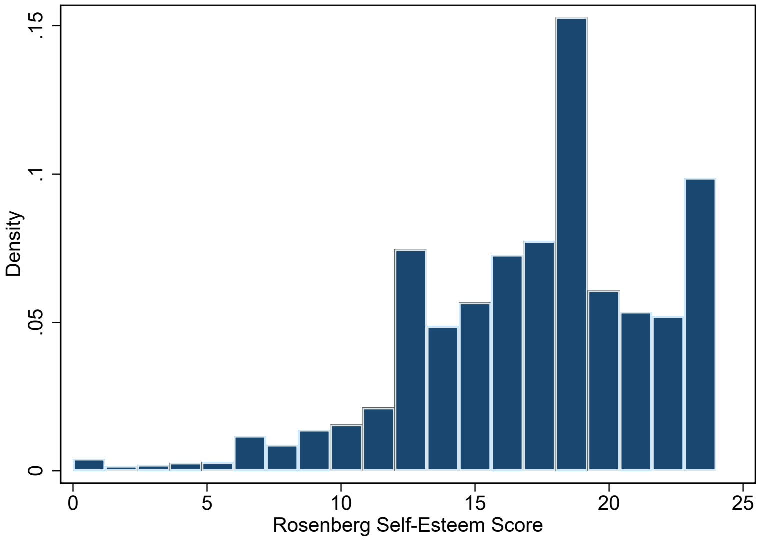
Note: The figure displays the distribution of depression scores (CES-D) in the short-run sample with 13,594 observations. The red line represents a score of 16, which is a common cut-off for being at risk for clinical depression.

Figure A.5: Distribution of the Cognitive Ability Score



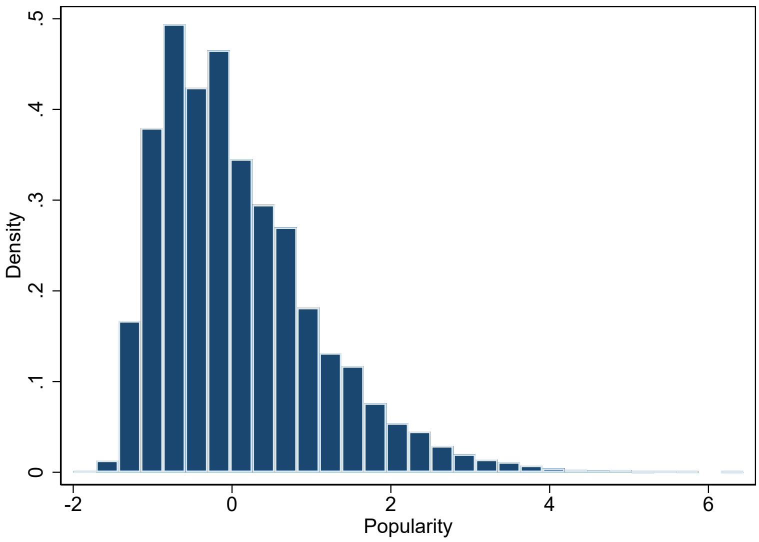
Note: The figure displays the distribution of the cognitive (Peabody) test scores in the short-run sample with 13,030 observations.

Figure A.6: Distribution of the Self-esteem Score



Note: The figure displays the distribution of the self-esteem scores in the short-run sample with 11,858 observations.

Figure A.7: Distribution of the Popularity Score



Note: The figure displays the distribution of the popularity scores in the short-run sample with 13,580 observations.

Chapter 2

The CoViD-19 Pandemic and Mental Health: Disentangling Crucial Channels

Abstract

Since the start of the CoViD-19 pandemic, a major source of concern has been its effect on mental health. Using pre-pandemic information and five customized questionnaires in the Dutch LISS panel, we investigate how mental health in the working population has evolved along with the most prominent risk factors associated with the pandemic. Overall, mental health decreased sharply with the onset of the first lockdown but recovered fairly quickly. In December 2020, levels of mental health are comparable to those in November 2019. We show that perceived risk of infection, labor market uncertainty, and emotional loneliness are all associated with worsening mental health. Both the initial drop and subsequent recovery are larger for parents of children below the age of 12. Among parents, the patterns are particularly pronounced for fathers if they shoulder the bulk of additional care. Mothers' mental health takes a particularly steep hit if they work from home and their partner is designated to take care during the additional hours.

2.1 Introduction

Starting in early 2020, the CoViD-19 pandemic and policy measures to slow its spread have upended the lives of billions of people. From early on, researchers and practitioners have been pointing towards possible adverse effects on population mental health through a variety of channels. Some of the most prominent pathways identified by prior literature include worries about and occurrence of the health effects of contracting the virus (e.g., Hollingue et al., 2020; Kämpfen et al., 2020); anxiety about job and income losses in the wake of the global recession caused by the pandemic (e.g. Davillas and Jones, 2021; Kämpfen et al., 2020; Witteveen and Velthorst, 2020); increased stress in families with children affected by closures of schools and daycare, especially when parents need to meet the requirements of their jobs and the needs of their children at the same time (Etheridge and Spantig, 2020; Zamarro and Prados, 2021); and increased loneliness through the loss of social contacts (Etheridge and Spantig, 2020).

We add to the literature on the early evidence on the pandemic in a variety of ways. First, except for some studies in the U.K. (e.g., Davillas and Jones, 2021; Etheridge and Spantig, 2020) and the U.S. (e.g. Kämpfen et al., 2020; Zamarro and Prados, 2021), few papers have been able to exploit probability samples with pre-pandemic information on mental health at the individual level. However, the response to the first wave of the pandemic in both of these countries was late and arguably not very efficient in containing the spread of the virus. In contrast, countries like Germany, the Netherlands, or most Scandinavian countries, all took rather efficient measures against the first wave of the SARS-CoV-2 pandemic. Schools and daycare centers were closed and so were many customer-facing businesses. However, there was no general curfew, in contrast to many Southern European countries. At the same time, the well-developed welfare systems cushioned the socio-economic consequences. In this study, we consider the case of the Netherlands. In particular, we use data from the LISS panel, an Online panel based on a probability sample of the Dutch population.

Besides widening the geographic scope of studies by adding a prototypical country from North-Western Europe, we also expand in the time dimension by covering the entire year 2020. Our pre-pandemic information stems from November 2019 when the five-item Mental Health Inventory (MHI-5) was assessed as part of the annual LISS questionnaire on health. We then have comparable measures from the first two weeks of the spring lockdown in March 2020, from May, June, September, and December. The summer was characterized by low infection rates and a fairly normal life; gatherings of large groups being the exception. However, cases increased rapidly again during September. By December, the Netherlands had been in another lockdown for several months. The temporal structure allows us to assess the mental health impact of the pandemic beyond the initial lockdown period.

Finally, we can exploit customized data that allows us to consider the above-mentioned channels jointly. They show very distinct temporal patterns, which allows us to disentangle them in a series of fixed effects regressions. We focus on the working population because we expect very different ways of how the pandemic would impact the mental health of older people.

Our results show that on average, mental health takes a very substantial drop during the period of high uncertainty early in the first lockdown. These mean values recover quickly before dropping again towards the end of the year. MHI-5 scores for December 2020 are very similar to values in November 2019. For the March-December period, there thus is a clear hump-shaped pattern. Mean levels of psychological distress are higher in women, a result that is well documented in the literature (e.g., Kessler et al., 1993; Van de Velde et al., 2010) and during the CoViD-19 pandemic for other countries (e.g., Etheridge and Spantig, 2020 or Davillas and Jones, 2021 for the U.K., Pedraza et al., 2020 for several countries). MHI-5 scores by gender evolve almost in parallel over the period under study, which cautions against the interpretation of regressions in levels as measuring the impact of the pandemic.

Beneath the averages, there is substantial heterogeneity across genders and the four channels that we consider. Mental health falls in perceived infection risk, maybe more so for women. On the other hand, the effects of labor market risk are substantially more pronounced for men, which is consistent with them contributing the larger share of income in most families. Increases in emotional loneliness, measured using the de Jong-Gierveld scale, are associated with drops in mental health for both genders, but more so for women (this is consistent with results in Etheridge and Spantig, 2020).

The hump-shape of the MHI-5 evolution over the March-December period is more pronounced for parents. This is consistent with the onset of the spring lockdown being a particularly stressful period for them, as they had to cope with closed schools and daycare facilities from one day to the next while managing their usual work simultaneously. At the same time, one may expect that over the summer, they were affected less by the restrictions that still were present on many leisure activities and long-distance travel.

Among parents, there are important differences by gender and by how the extra care duties created by the closures of schools and daycare facilities were met. If parents shared the latter, the initial drop was small if present at all; MHI-5 scores are substantially higher over the summer of 2020 than in November 2019. In contrast, if only one parent shouldered the additional childcare, that parent has consistently lower scores over the year. The drop in March was particularly pronounced for fathers who take on the additional duties themselves. Investigating these patterns further, we show that the effects of caregiver duties are strongest for fathers who work many hours from home.

Finally, we exploit time use data from November 2019 and April 2020, which contain a direct measure of the number of hours worked from home while being responsible for children at the same time. There are strong gender differences: Mental health is hump-shaped in such hours for men and U-shaped for women. The respective peak/trough is found at around 15-20 such hours. These different patterns are consistent with the fraction of total working hours spent simultaneously on childcare and work. Since men work longer hours, men still have plenty of time to get some work done if they spend 15-20 hours taking care of children, too. For women, this is much less the case. Altogether, our results are consistent with a "mom is never off duty when home"-effect.

In the next section, we describe the Dutch setting, our data, and we describe the evolution of mental health and the important covariates over the period from late 2019 and throughout 2020. We then present the results of our various fixed effects regressions, relegating a discussion to the last part of the paper, where we also draw conclusions.

2.2 Context, Data, and Stylized Facts

In this section, we outline the setting for our analysis. We first describe the institutional context in the Netherlands, putting particular emphasis on the temporal evolution of social distancing policies enacted to reduce the spread of SARS-CoV-2. Following that, we provide an overview of the dataset we collected. We then describe the evolution of mental health and the key explanatory variables from late 2019 through the year 2020.

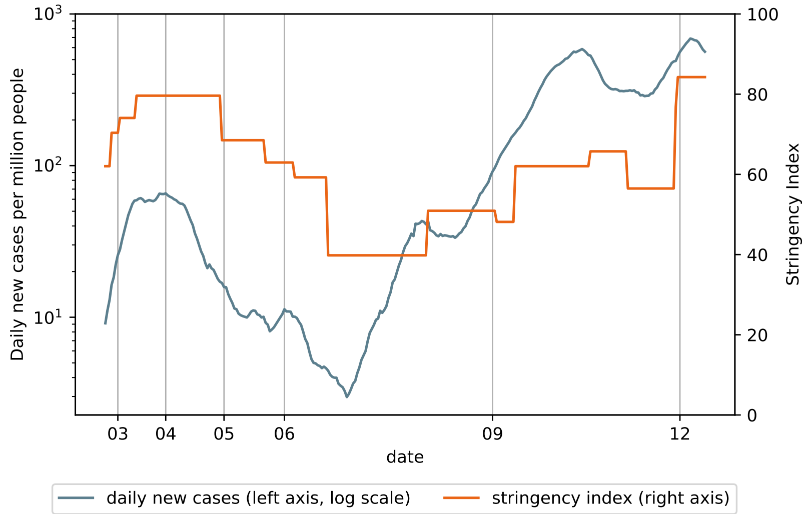
2.2.1 The CoViD-19 Pandemic and Social Distancing Policies in the Netherlands

Figure 2.1 displays the evolution of confirmed SARS-CoV-2 infections in the Netherlands along with a stringency index of non-pharmaceutical interventions. The first infection in the Netherlands was detected in late February 2020. By mid-March, more than 10 new cases per million inhabitants were confirmed each day. Despite limited testing, this number had reached 60 by the end of March and stayed roughly at that level for the first three weeks of April. It declined thereafter and reached 10 again in mid-May, staying there or somewhat below until late July 2020. Infection rates started to rise again in August. By mid-September, they had surpassed the peak of the first wave and in late October, they hit almost 600 new daily cases per million inhabitants. During November, this number decreased to a value below 300 but steeply rose again and peaked just below 700 before Christmas.

Similar to other countries, the initial rise in infections prompted the Dutch government to impose restrictions on economic and social life to slow down the virus' spread. In mid-March, all schools and childcare facilities were closed along with restaurants, cafes, bars, and other businesses involving personal contacts. People were advised to stay at home, to keep a distance of at least 1.5 meters to each other, and to avoid social contacts; the number of visitors at home was restricted to a maximum of three individuals.

While most of these policy measures resembled those in other European countries, they did not involve a general curfew and some measures were much more lenient. Businesses, such as stores for clothes, utilities, or coffee shops remained open as long as they could guarantee to maintain the social distancing rules. The government advised everybody to stay at home, but people were allowed to go outside without any official permission, and they were allowed to meet a maximum of

Figure 2.1: CoViD-19 Cases and Oxford Response Stringency Index in the Netherlands in 2020



Note: The figure displays the evolution of daily new cases as rolling 7-day average, based on Ritchie et al. (2020) on the left axis (blue line) along with the Oxford Response Stringency Index (right axis, orange line). The latter measures the stringency of policies restricting the economic and social life to stop the spread of SARS-CoV-2 (Hale et al., 2020). The vertical lines indicate different waves of data collection for this project.

three other non-household members as long as social distancing was maintained. Public locations were still accessible and traveling or the use of public transportation was possible throughout. Beginning in May, the restrictions were gradually lifted. Daycare facilities and primary schools were among the first areas to open up again, secondary schools followed in early June. With the exception of bans of larger (inside) gatherings, social and economic life was largely back to what it was before the pandemic.

In mid-October, the Dutch government imposed another lockdown in response to the steep rise in infection numbers. Many of the restrictions were stricter than those imposed in March 2020: Besides the closure of restaurants, bars, museums, and other public places, opening hours for shops were limited and the sale of alcohol was prohibited after 8 p.m. An important difference was that schools and daycare centers remained open. Along with a temporary sharpening of the measures in early November, this brought infection rates down for some time. However, their rise during the first half of December prompted a great tightening effective from December 15. All shops except supermarkets and essential services were closed along with childcare facilities and schools.

2.2.2 Data and Sample Construction

Our empirical analysis uses the Longitudinal Internet Studies for the Social Sciences (LISS), which is a high-quality panel data set based on a probability sample of the Dutch population. The LISS panel has been running since 2007 and comprises roughly 7,000 individuals from about 4,000 households. Each month, respondents are invited to complete questionnaires lasting 15-30 minutes on average. The information solicited from respondents includes a set of ten core questionnaires repeated every year as well as questionnaires designed by external researchers.

Our baseline measure of mental health stems from the core questionnaire on health administered in November 2019. We included the same measure in a set of modules that we designed to track the consequences of the pandemic (Gaudecker et al., 2020b). In these questionnaires, we asked about mental health, labor market outcomes, and expectations during the CoViD-19 crisis. The initial module was fielded in late March 2020, a few days into the first lockdown. Five more modules followed in April, May, June, September, and December. All CoViD-19 survey modules were addressed to all panel members aged 16 years and older; response rates exceeded 80% in all waves.

The basic structure of our data is an individual-level panel with up to six time series observations.[1] We make the following restrictions on our sample. We keep

[1]Because of the short time span between the initial wave in late March and the second wave in April, we did not ask about mental health in April.

household heads and their partners for whom we have at least two observations. We restrict the sample to individuals up to age 70 who reported to be employed or self-employed just before the pandemic started while working positive hours. Key explanatory variables are family structures and caregiver arrangements, which we elicit in March and April 2020 and require to be present. Our resulting sample consists of 10,525 observations of 2,353 individuals; 1,138 men and 1,215 women.

2.2.3 Mental Health and Family Structure

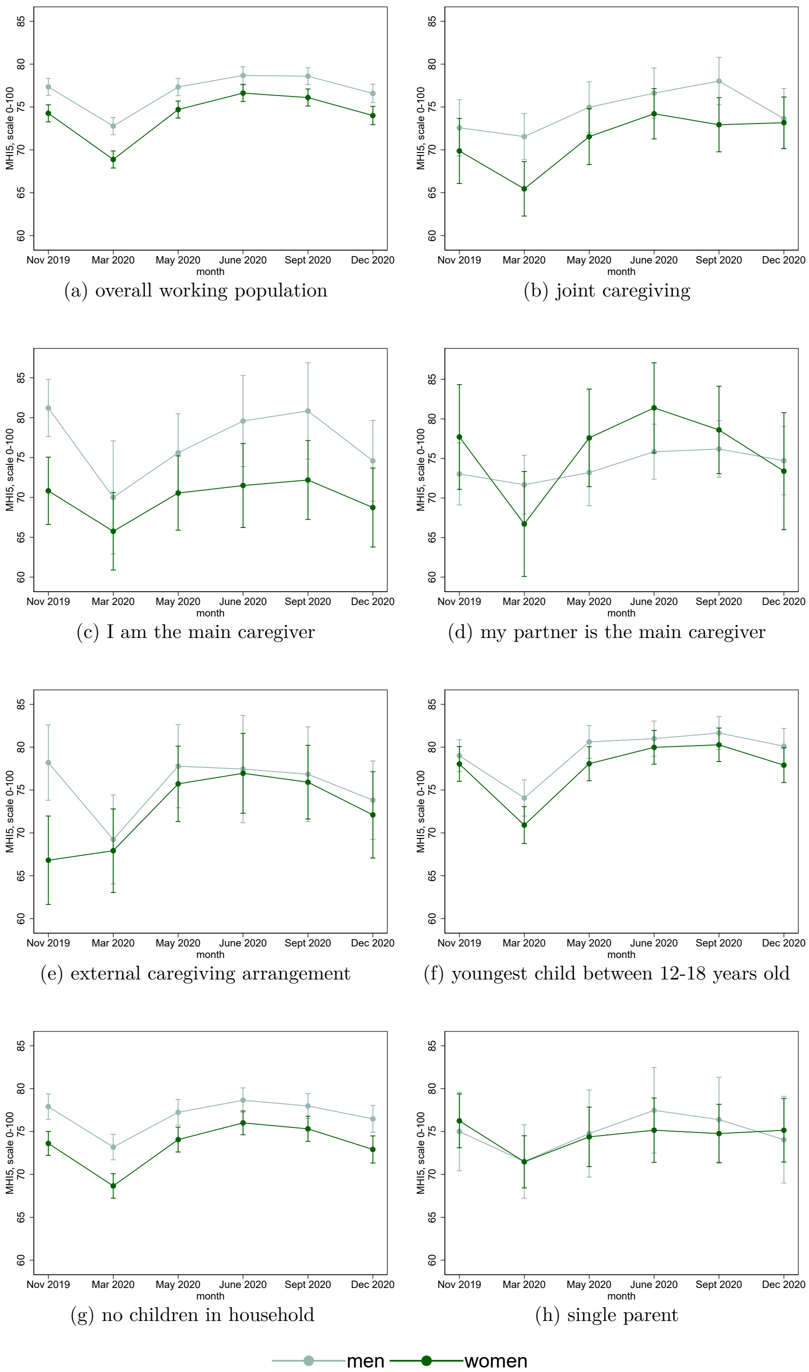
The core LISS questionnaire on health contains the MHI-5 (Mental Health Inventory 5) measure, which is a brief, validated international instrument for assessing mental health in adults (see, e.g., Berwick et al., 1991; Thorsen et al., 2013). We included this measure in our CoViD-19 surveys, too. MHI-5 is a five-item subscale of the Short Format 36 (SF-36), a comprehensive tool to measure different dimensions of health. Hoeymans et al. (2004) compare the MHI-5 measure to the General Health Questionnaire (GHQ-12) for the Dutch population. They find both measures to be similarly predictive for mental health problems.

The MHI-5 instrument consists of five separate questions to assess how people felt in the past four weeks (see Appendix B.1 for details). Each answer comes on a six-point scale. To obtain the MHI-5 score, all answers are coded on scales from zero to five such that higher values indicate better health. Individual values are summed up and multiplied by four. The resulting MHI-5 score ranges from zero to 100, with zero representing very poor mental health and 100 representing its best possible level. The medical literature generally uses cutoffs between 52 and 76 to dichotomize the measure (e.g., Cuijpers et al., 2009; Hoeymans et al., 2004; Thorsen et al., 2013). Values below the cutoff are interpreted as indicative of mental health problems. Probably the most common cutoff is 60, which is also used by official statistics (Statistics Netherlands, 2015). In our analyses, we use the raw score in order to work with a near-continuous measure; averages are typically in the 65-80 range. Our measure is meaningful in the sense that it is indicative of variation in this critical range (as opposed to measuring changes between nearly optimal and optimal; or close to zero and zero).

Panel (a) of Figure 2.2 displays the evolution of the MHI-5 score across time and gender for the working population. At any point in time, men exhibit higher mental health scores than women. By far the lowest value of mental health is recorded during the first two weeks of the lockdown in March 2020. Relative to November 2019, there is a drop of about 5 points. Already in May, average mental health was close to its initial value and surpassed that over the summer, dropping again in December. Except for the March-May period, the overall pattern is consistent with a hump-shape over the calendar year, which is found in studies on the seasonality of mental health (see, e.g., Magnusson, 2000, for an overview). The salient feature thus is a transitory shock upon the introduction of the first lockdown, which brought with it lots of uncertainty in many dimensions and dramatic changes in daily living arrangements.

The closure of schools and daycare centers may put parents at particular risk for developing mental health problems. We expect this to differ by how partners share the additional burden of taking care of their children during the time where they would usually be in school or daycare. We asked for these arrangements in March and April 2020 if a child below the age of 12 was present in the household, which is the case for a little less than a quarter of our sample (see Panel A in Table 2.1). For these households, we distinguish between sharing the additional duties (the most common arrangement), taking it on oneself, the partner taking it on, and making use of other arrangements (emergency care for essential workers, grandparents, etc.). For the precise construction of these variables, see Appendix B.2. The last

Figure 2.2: Evolution of Mental Health by Family Structure and Arrangements Made for Additional Childcare Duties During School/Daycare Closures



(a) overall working population

(b) joint caregiving

(c) I am the main caregiver

(d) my partner is the main caregiver

(e) external caregiving arrangement

(f) youngest child between 12-18 years old

(g) no children in household

(h) single parent

Note: Each panel shows this evolution separately for men (light) and women (dark). Means are estimated on the sample of the working population and conditional on the primary form of care arrangement and family structure as stated in March or April 2020. Vertical bars depict 95-% confidence intervals.

three household categories are single parents, parents of adolescents, and households without underage children.

The remaining panels of Figure 2.2 break down the evolution of the MHI-5 score by these household structures and arrangements regarding the extra childcare. There are important differences across these categories and gender; we focus on the most salient features. Generally, the hump-shape is more pronounced for two-parent families with small children than for the remaining population. There are some exceptions and particularly salient patterns. If partners share the additional responsibilities (Figure 2.2b), there hardly is a drop in the MHI-5 score for men at the beginning of the first lockdown. The same decline is largest for both genders if fathers are responsible for the additional caregiving duties (Figures 2.2c for men and 2.2d for women); in both cases, the recovery is equally steep. Our analysis in Section 2.4 will separate these patterns from other channels.

2.2.4 Evolution of Key Explanatory Variables

As described in the introduction, we expect that the pandemic-driven health risk, labor market risk, and emotional loneliness will be predictors of mental health in addition to household structure and childcare arrangements. Panel B of Table 2.1 presents the evolution of the remaining explanatory variables across time and gender. For the precise construction of these variables, see Appendix B.2.

We set perceived infection risk to zero for November 2019, when CoViD-19 was not known yet. The initial uncertainty surrounding the disease in March 2020 is reflected in a very high perceived chance of contracting the disease. Thereafter, perceived risk tracks infection rates in the Netherlands. At 25-40%, the probabilities seem fairly large throughout, likely reflecting a well-known bias towards 50% (e.g., Wakker, 2010). From May onwards, women always perceive a higher chance of being infected with SARS-CoV-2.

Table 2.1: Summary Statistics for the Key Explanatory Variables

A. Household structure / arrangements for extra childcare (measured in Mar/Apr 2020)

	child below age 12				**child aged 12-18**	**no child in household**	**single parent**
	jointly	myself	partner	other arrangement			
men	0.11	0.02	0.07	0.04	0.23	0.48	0.05
women	0.09	0.05	0.02	0.05	0.22	0.48	0.09

B. Time-varying measurements

	Nov '19	**Mar '20**	**May '20**	**June '20**	**Sept '20**	**Dec '20**	**overall**
Perceived CoViD-19 infection risk							
men	0	0.48	0.30	0.25	0.30	0.33	0.28
women	0	0.48	0.33	0.28	0.36	0.40	0.32
Labor market outcomes and expectations							
reduction working hours > 25%							
men	0	0.15	0.22	0.19	0.20	0.16	0.15
women	0	0.24	0.30	0.24	0.22	0.19	0.20
subjective probability of job loss							
men	0.016	0.037	0.032	0.028	0.024	0.026	0.028
women	0.015	0.043	0.024	0.023	0.025	0.016	0.025
Number of observations							
men	846	1020	849	808	840	812	5,175
women	874	1088	884	828	878	798	5,350

Note: The information on household structure / caregiver arrangements in Panel A is available for 1,138 men and 1,215 women. The exact wording of all questions used to collect the variables in the table can be found in Appendix B.2. Further explanatory variables, such as age and education levels, are presented in the Appendix, Table B.2.

The share of people whose working hours are reduced by at least 25% relative to their respective baseline is 15% for men and 24% for women in March 2020. This share reaches its maximum towards the end of the first lockdown in May (22% and 30% respectively). For men, it remains higher than the initial response throughout the year. By contrast, for women, it falls below the March value in the second half of 2020, but the level remains higher than for men. Subjective job loss probabilities follow a different pattern. For both genders, there is a large peak in March 2020, before gradually falling off over the rest of the year. This temporal variation is substantially more pronounced for women, who have a significantly higher value than men only in March. In December, their perceived probabilities are one percentage point lower than men's. On average, job loss probabilities seem well aligned with actual changes in employment. For example, the rates of unemployment and of non-participation in the labor force rose by one percentage point each over the course of 2020 (e.g. Gaudecker et al., 2020a; Meekes et al., 2020).

At baseline and in April and June, we have measures for the de Jong-Gierveld Loneliness Scale (Gierveld and Tilburg, 2006), which ranges from 0 to 12 with higher values indicating stronger emotional loneliness. We extrapolate to the remaining periods.[2] Men and women have similar scores just below 1.9 in November 2019 and just above that value in June 2020 (see Table B.3). For both genders, there is a substantial increase during the first lockdown, which is more pronounced for women (2.3) than for men (2.1).

2.3 Empirical Strategy

Our main specification is a linear model that exploits the panel dimension of our data. To allow the effects of household structure and caregiver arrangements—which we measure in March or April 2020—to vary over time, we add a full set of interactions with survey month fixed effects. We estimated all regressions separately for men and women. Our model for mental health M_{it} of individual i in survey month t thus becomes:

$$
\begin{aligned}
M_{it} = &\sum_{k=1}^{6} \beta_{kt} \mathbb{I}(Caregiver_i = k) \cdot \mathbb{I}_t + \delta Inf_{it} + Labor'_{it}\gamma + \theta Lonely_{it} + \eta_t \\
&+ \alpha_i + \varepsilon_{it},
\end{aligned}
\tag{2.1}
$$

[2]This will be less of an issue once the 2020 LISS core health questionnaire, fielded in November, will be available.

where η_t is a survey month fixed effect, α_i is an individual-specific fixed effect, and ε_{it} are unobservables that we assume to be iid across individuals. $\mathbb{I}(Caregiver_i = k)$ is an indicator taking the value one if individual i falls into one of our seven categories of caregiver arrangements and household types. $\mathbb{I}_t$ is an indicator for a particular survey month. The parameters β_{kt} capture the impact of caregiver arrangement k in survey month t on mental health, relative to the respective reference category. Inf_{it} is the perceived infection risk, $Labor_{it}$ is a vector comprising the indicator for a large reduction in working hours and the subjective risk of job loss. $Lonely_{it}$ represents the loneliness score; its impact on mental health is measured by the parameter θ.

We cluster standard errors at the level of the individual. The reference category for the caregiver / household type variable is joint organization of childcare. The reference period is November 2019.

A particularly stressful situation for parents, which would not be sufficiently captured by Equation (2.1), is working from home while being responsible for young children at the same time. We may expect this double burden to increase the risk of mental health problems. In a second set of regressions, we thus interact the number of hours worked from home, $Hrshome_{it}$, with the k categories of childcare arrangements and household types:

$$\begin{aligned} M_{it} =& \psi Hrshome_{it} + \sum_{k=1}^{6} \beta_k \mathbb{I}(Caregiver_i = k) \cdot Hrshome_{it} + \delta Inf_{it} + Labor'_{it}\gamma \\ &+ \theta Lonely_{it} + \eta_t + \alpha_i + \varepsilon_{it}. \end{aligned} \tag{2.2}$$

In this specification, β_k thus measure the impact of an additional hour spent working from home for an individual with caregiver arrangement k on mental health. ψ measures the impact of an increase in home office hours for parents who organize childcare jointly during the lockdown. We estimate Equation (2.2) using the full sample over all survey months as well as for a sub-sample, which contains only observations until May 2020. This allows us to gauge whether an effect is exclusively due to the period where schools and daycare centers had been closed unexpectedly or whether it persists to a time where they were mostly open but home office hours still high.

2.4 Results

Section 2.2 has shown that the potential channels mediating the direct and indirect impact of the pandemic on mental health follow distinct patterns over time and across genders. We now explore individual trajectories in order to judge these channels' relative importance, before zooming in on additional childcare duties in Section 2.4.2. Our main specifications are the fixed effects regressions from Equations (2.1) and (2.2), which we estimate separately for men and women.

2.4.1 Predictors of Mental Health During the CoViD-19 Pandemic

Table 2.2 presents the estimation results of our main specification for men and women. Because the interaction of time and caregiver / household structure variables leads to a large number of coefficients, we only present those for parents of young children in the main text.

Labor market uncertainty has a much stronger effect for men than for women. A reduction in working hours of at least 25% relative to the working hours in the pre-crisis period leads to a significant reduction in men's mental health score by 1.2 points. An effect of similar magnitude obtains for a ten percentage point increase in the probability to lose one's job. For women, the point estimates are considerably smaller; the difference between genders is statistically significant for perceived unemployment risk. It is well-known from earlier work that recessions negatively impact mental health (e.g. Frasquilho et al., 2016; McInerney et al., 2013). Because for most Dutch households, male earnings play a substantially larger role in total household income than female earnings, the gender differences do not come as a surprise.

Table 2.2: Predictors of Mental Health in the November 2019–December 2020 period

	men	women
prob: becoming infected	-1.48	-2.12**
	(0.91)	(1.00)
reduced working hours: yes	-1.19***	-0.66
	(0.43)	(0.43)
prob: becoming unemployed	-9.62***	-3.16
	(2.01)	(2.14)
loneliness	-0.41***	-1.01***
	(0.15)	(0.17)
March 2020 (reference: sharing extra childcare duties)	-0.41	-1.37
	(1.52)	(1.99)
May 2020 (reference: sharing extra childcare duties)	3.06**	4.42***
	(1.35)	(1.52)
June 2020 (reference: sharing extra childcare duties)	4.21***	5.93***
	(1.26)	(1.85)
September 2020 (reference: sharing extra childcare duties)	5.25***	5.78***
	(1.35)	(1.69)
December 2020 (reference: sharing extra childcare duties)	0.44	3.02
	(1.43)	(1.87)
extra childcare: myself x March 2020	-8.88***	-2.17
	(3.16)	(3.03)
extra childcare: myself x May 2020	-7.55***	-4.37*
	(2.58)	(2.24)
extra childcare: myself x June 2020	-5.38*	-4.69
	(2.85)	(2.93)
extra childcare: myself x September 2020	-4.09	-4.70
	(2.69)	(3.00)
extra childcare: myself x December 2020	-6.10**	-5.43*
	(2.72)	(3.16)
extra childcare: partner x March 2020	0.48	-3.44
	(2.37)	(4.02)
extra childcare: partner x May 2020	-2.01	-2.02
	(2.03)	(3.27)
extra childcare: partner x June 2020	-1.12	-0.15
	(2.22)	(3.54)
extra childcare: partner x September 2020	-1.34	-1.97
	(1.94)	(3.80)

Continued on next page

Table 2.2 – *Continued from previous page*

	men	women
extra childcare: partner x December 2020	1.97	-3.54
	(2.23)	(3.56)
extra childcare: other arrangement x March 2020	-8.03***	1.90
	(2.84)	(3.78)
extra childcare: other arrangement x May 2020	-1.57	4.16
	(2.53)	(3.34)
extra childcare: other arrangement x June 2020	-3.49	3.44
	(2.99)	(3.21)
extra childcare: other arrangement x September 2020	-5.70**	3.98
	(2.65)	(3.27)
extra childcare: other arrangement x December 2020	-2.34	2.07
	(2.72)	(3.85)
observations	5,175	5,350
number of individuals	1,138	1,215
individual specific FE	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1; standard errors clustered on the individual level. The table presents the estimated coefficients of the main channels on mental health obtained from Equation (2.1). We control for a full set of interactions between survey month and categories of caregiver arrangements and household structure. The reference period is November 2019, the reference category are parents who share the extra childcare that becomes necessary during the closure of school and daycare centers. To economize on space, we do not report all regression coefficients; the full set of results can be found in the Appendix, Table B.4.

Men and women experience a reduction in mental health as their emotional loneliness increases. For men, an increase in the loneliness score by 1 point leads to a decrease in the MHI-5 score of 0.4 points. The same reaction is 1 point for women; the difference between genders is significant. Given that the scale varies from 0 to 12, these are large effects; due to the measurement error induced by extrapolation, we expect them to be a lower bound. The gender differences mirror findings for the U.K. reported in Etheridge and Spantig (2020).

The coefficients on the survey month fixed effects show the development of mental health for parents who jointly organize the additional childcare duties caused by the closure of schools and childcare facilities during the first lockdown. After controlling for covariates, the average drop in March is small and insignificant for both genders. For the May-September period, MHI-5 scores are substantially higher compared to

November 2019, before falling again. These estimates by and large confirm the patterns outlined in Figure 2.2.

Relative to this trajectory, parents who were solely responsible for taking on the additional childcare duties experience substantially larger reductions in mental health throughout 2020. The pattern is more pronounced among men, where the average drop in the MHI-5 score between November 2019 and March 2020 is around 9 points. The recovery from this shock is slow and significantly worse than for fathers who share caregiver duties with their partners. For women, the initial shock is much smaller; patterns look similar to men from June onward. These coefficients suggest a substantial burden on the mental health of parents in couples where additional childcare is not shared.[3] In December 2020, MHI-5 scores are significantly below their pre-pandemic values.

As may be expected from Figure 2.2d, there hardly is a change in men's mental health if their partner has compensated for school and daycare closures. Coefficients are small and insignificant, always working against the hump-shaped pattern. For women whose partner is mainly responsible for additional childcare duties, controlling for covariates cuts the drop upon the onset of the pandemic in Figure 2.2d by more than half and renders it insignificant. All estimated coefficients are negative and insignificant.

Summing up, our results show that the patterns from Figure 2.2 for differences by caregiver arrangement are broadly confirmed in the fixed effects analysis. Moreover, exposure to infection risk and emotional loneliness predicts deterioration in mental health among both genders of similar magnitude. For loneliness, the reaction is somewhat stronger among women. By contrast, for men, the pandemic significantly operates through labor market channels. This seems plausible since

[3]Unfortunately, the small sample size prevents us to investigate further whether this is a pure choice or due to more exogenous factors like work schedules of essential workers early in the pandemic.

men are frequently the main breadwinner, implying that the prospect of losing their job may generate more anxiety.

2.4.2 A Double Burden of Home Office and Childcare Duties?

The results from our main specification have revealed that men who were mainly responsible to handle additional childcare duties experienced the largest initial reduction in mental health, more than women in the same category and also more than fathers with other types of childcare arrangements. One reason behind this might be that the primary reason for men to exclusively take on the extra childcare is that they can work from home whereas their partners cannot. Indeed, among families with fathers being the main caregiver, they work more than 20 hours from home whereas mothers' home office hours are below 4. As there are no reductions in working hours for parents of underage children relative to the remaining population (Holler et al., 2021), these men would be faced with the task of working and taking care of their children at the same time.

To shed light on this channel, Table 2.3 reports the results of fixed effects regressions that include an interaction of home office hours and the extra caregiver/household structure variable. Panel A shows the most important results for the full set of time periods; Panel B focuses on the comparison between November 2019 and the first lockdown period, which included closed schools and daycare.

Among the reference group—couples sharing the extra childcare burden—an additional hour of working from home does not significantly reduce mental health. For both genders, the coefficients are close to zero and precisely estimated. When fathers take over the main caregiver responsibility, an extra hour worked from home leads to a reduction of 0.17-0.18 points in the MHI-5 score. On average, these fathers

Table 2.3: Effect of Hours Worked from Home by Arrangement for Extra Childcare Duties on Mental Health

	men	women
A. all periods		
hours worked from home (reference: sharing extra childcare duties)	-0.02	-0.01
	(0.04)	(0.05)
extra childcare: myself x hours worked from home	-0.18***	-0.10
	(0.06)	(0.11)
extra childcare: partner x hours worked from home	-0.01	-0.33**
	(0.06)	(0.13)
extra childcare: other arrangement x hours worked from home	0.09	0.16
	(0.09)	(0.12)
observations	5,175	5,350
number of individuals	1,138	1,215
individual specific FE	yes	yes
survey month fixed effects	yes	yes
B. during lockdown of schools/childcare		
hours worked from home (reference: sharing extra childcare duties)	-0.01	-0.10
	(0.05)	(0.08)
extra childcare: myself x hours worked from home	-0.17*	-0.09
	(0.09)	(0.14)
extra childcare: partner x hours worked from home	-0.08	-0.70***
	(0.10)	(0.21)
extra childcare: other arrangement x hours worked from home	-0.03	0.44**
	(0.13)	(0.18)
observations	2,715	2,846
number of individuals	1,133	1,212
individual specific FE	yes	yes
survey month fixed effects	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1; standard errors clustered on the individual level; The table presents the estimation results of home office hours by extra childcare duties on mental health for the working population from equation 2.2 separately for men and women. Panel A shows the regression results for the working population in all survey waves from November 2019 to December 2020. Panel B shows the regression results when we restrict our results to November 2019 and the period of the first lockdown, which included closed schools and daycare centers, from March to May 2020. The baseline period is November 2019. The table only presents the coefficients on home office working hours and interactions between home office working hours and the set of childcare arrangements for children under 12 years of age. The full set of interactions, infection risk, labor market channel and social interaction channel is shown in the Appendix, Table B.5.

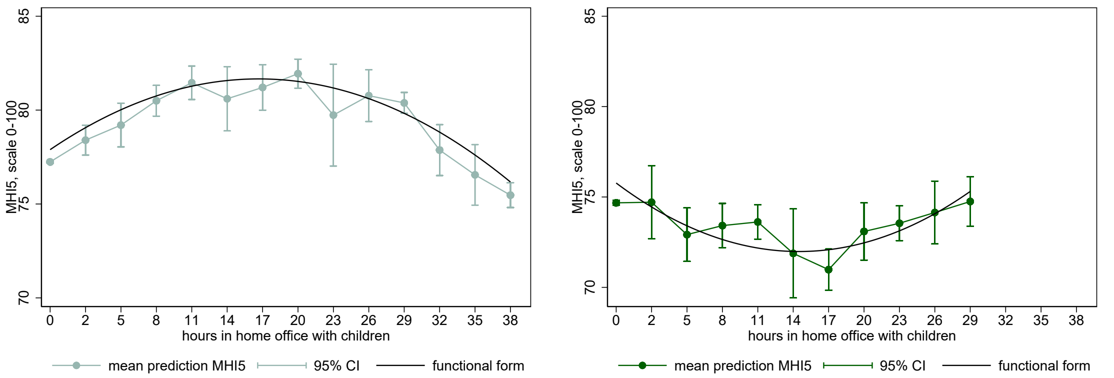
work about 20 hours from home, so their mental health is reduced by about 3.5 points more than for similar men in couples who share the additional responsibilities. The point estimate for women is smaller but imprecisely estimated.

No differential effect of home office hours is visible for fathers whose partner takes the main responsibility for additional childcare. By contrast, women whose partner takes the main responsibility for additional childcare experience a large and significant decrease in mental health as home office hours increase; the effect is substantially larger when concentrating on the initial lockdown period in Panel B. Together with the findings in Figure 2.2d and Table 2.2, this coefficient suggests that women are particularly at risk for developing mental health problems in situations where partners are supposed to take care of the children while they work from home. Complementary to this, women's mental health develops significantly better during the first lockdown period if they are working long hours from home and other childcare arrangements are available. This effect washes out when considering our entire study period.

Finally, we take another look from a different angle using a direct measure for hours worked from home while being responsible for childcare at the same time. The measure is included in a time use survey. The survey is comparable to a similar one from November 2019 but adapted to the lockdown situation (see Gaudecker et al., 2020b, for the precise wording of the questions). Time use refers to the past week; this week falls into the four-week assessment period for mental health in the November and May questionnaires, respectively.

Figure 2.3 shows predicted mental health scores from a fixed effects regression of mental health on a quadratic function in hours worked from home while being responsible for childcare at the same time. The regression specification also includes the remaining three channels and survey month fixed effects. We plot up to the 99^{th} percentile of the distribution of hours worked from home with kids present,

Figure 2.3: Predicted Mental Health Score by Hours Worked From Home While Simultaneously Taking Care of Children



(a) Men (b) Women

Note: The figure plots predicted mental health against hours in home office while taking care of children. Predicted values are obtained from an OLS regression with individual-specific fixed effects of mental health on a quadratic function in hours worked from home while being responsible for children at the same time. The estimation is based on a sample of 1,035 men and 1,091 women who participated in the survey in November 2019 and May 2020. We control for measures of labor market risk, infection risk, and social interaction channels, and survey month fixed effects. The estimated coefficients from the quadratic specification can be found in the Appendix, Table B.6. We use the average of fixed effects to adjust the level of mental health based on the quadratic function to those in the data. The predicted values use bins of three hours.

which is 40 hours for men and 30 hours for women. For men, we find a hump-shaped relationship between mental health and home office hours, which reaches its maximum around 18 and its minimum in the right tail of the distribution. For women the pattern looks opposite, suggesting that women who work around 15 hours from home and take care of their children at the same time have the lowest mental health score.

Importantly, there are no systematic differences in November 2019—neither in mental health nor working hours—along the distribution of hours worked from home with kids. In particular, in November 2019, mental health is about the same whether or not somebody reports positive home office hours with children in April 2020. The patterns thus do not seem to be driven by selection or regression to the mean. Furthermore, total working hours in April do not have a clear relationship with the amount of home office hours with children. To be precise, holding the ability to work from home constant by conditioning on positive hours worked from home, there is no difference between parents who mind their children at the same time and workers who never do so.[4]

2.5 Discussion and Conclusion

We have analyzed how changes in the mental health of a representative sample of the Dutch working population evolved from before the CoViD-19 pandemic through its first year. Upon the onset of the first lockdown, amidst a period of high uncertainty in many dimensions, mental health dropped very sharply. It recovered over the summer before dropping slightly again, so that December 2020 values are comparable to those from November 2019. Investigating the joint evolution with several potential mediators identified in the literature—household structure and arrangements for taking care of children during the period of school closures, SARS-CoV-2 infection risk, employment prospects, and lack of social interactions—we document substantial heterogeneity.

Mental health falls in perceived infection risk, maybe more so for women. On the other hand, the effects of labor market risk are substantially more pronounced for men, which is consistent with them contributing the larger share of income in most families. Increases in emotional loneliness are associated with drops in mental health for both genders, but more so for women (this is consistent with results in Etheridge and Spantig, 2020).

The hump-shape of the MHI-5 evolution over the March-December period is more pronounced for parents. The onset of the spring lockdown was a particularly

[4]Among all mothers working from home, those who also take care of their children work on average 31.3 hours in total. Women who do not mind their children at the same time work 31.2 hours. Fathers in home office work on average 40.1 hours when not taking care of their children at the same time compared to 36.8 hours when also taking care of children. These differences in working hours are about the same for mothers (30.1 vs 31.2) and fathers (41.8 vs 36.8) in November 2019.

stressful period for them. They had to cope with closed schools and daycare facilities from one day to the next while managing their usual work at the same time. We do not find clear gender effects and in fact, some of the largest drops are found for fathers when they are solely responsible for the additional childcare. In contrast to this, much of the international literature has found larger effects for women. However, these studies often lack pre-pandemic measures of mental health. Thus, the estimated effect may capture potential level differences in mental health between men and women rather than the additional effect of the pandemic on mental health (e.g. Adams-Prassl et al., 2020; Zamarro and Prados, 2021). A notable exception is Etheridge and Spantig (2020), which finds that mothers with substantial child care duties are particularly affected in terms of psychological distress during the first wave of the pandemic in the UK. While not directly comparable, our results for the Netherlands generally go in a different direction. This likely has to do with a very high share of part-time work among women (more than 60% worked less than 30 hours per week in 2017, see OECD, 2018) and very flexible work arrangements, which are mandated by a 2016 law. In general, our results paint a nuanced picture of the effects of the pandemic in two-parent families, which depend on the degree the extra burden during school and daycare closures is shared between partners and on the fraction of working time that is performed from home while simultaneously being responsible for children.

Taken together, our results are consistent with literature showing large but transitory impacts of negative aggregate shocks on mental well-being. For example, Deaton (2012) finds a large impact of the Great Recession in late 2008 and early 2009. These values subsequently recovered despite the fact that unemployment remained high. During the CoViD-19 pandemic in the Netherlands, mental health indicators substantially improved for parents after the period when schools were closed. Despite a second lockdown in December, mental health in the working population was similar to that before the pandemic. Our results are best explained by short-run anxiety associated with a novel and negative situation characterized by uncertainty and quick subsequent adoption.

Appendix B

B.1 Appendix: Measure for Mental Health – the MHI-5 Score

The variable of interest in our analysis is the so-called MHI-5 (Mental health inventory 5) measure. The MHI-5 is a brief, valid, and reliable international instrument for assessing mental health in adults. It is a five-item subscale of the Short Format 36 (SF-36), a comprehensive international standard to measure health. Several studies have validated the MHI-5 as a measure for depression (e.g., Thorsen et al., 2013). Participants are asked about their feelings over the past month:

- I felt very anxious
- I felt so down that nothing could cheer me up
- I felt calm and peaceful
- I felt depressed and gloomy
- I felt happy

For each item individuals can choose the following six answer categories: never, seldom, sometimes, often, mostly, continuously. For positive items (calm and peaceful, happy) the answers are coded from zero to five. Negative items are coded the other way around (five to zero). The responses are reported on a 6-point scale. They are then summed up and multiplied by four, such that the resulting score ranges from 0 to 100, whereby a higher number is associated with a better mental health. A score below 60 is associated with mental health problems (e.g. Statistics Netherlands, 2015).

Because we wanted to focus on the period during lockdown, we used the time frame of "the past seven days" in March 2020. To investigate possible impacts of the change in time frame, we included a survey experiment in the May 2020 wave, where some individuals were asked about their feelings over both the last month and the last week.

Table B.1: Reported Levels of Mental Health When Randomly Varying the Order of the Time Frame of the Mental Health Question, May 2020

	Order in which question was asked	
Time frame	past 7 days, past month	past month, past 7 days
past 7 days	80.6	81.1
	(14.9)	(15.5)
past month	80.5	79.4
	(14.2)	(15.5)
observations	851	859

Note: This table displays the mean and standard deviation in the MHI-5 scores based on a survey experiment in May 2020, where some individuals were asked about their mental health over both ”the past seven days” (row 1) and ”the past month” (row 2). The order of the time frame was randomized. In column 1, individuals were first asked about their mental health over the past seven days and later about their mental health over the past month. In column 2, the order of the time frame was reversed.

The most relevant comparison is between the two cells on the diagonal, which are the first items that were asked. Individuals who were first asked for their mental health for the past seven days, on average have a score of 80.6. Individuals who were first asked to report their mental health in the last month have an average score of 79.4. The difference between these two levels of mental health is not statistically significant (t-statistic$=$ 1.56, p-value $=$ 0.12). This answers the question whether the time frame matters in a relatively stable period of time.

B.2 Appendix: Explanatory Variables

We investigate the impact of the pandemic on mental health within individuals across time through four channels: additional childcare duties, perceived infection risk, labor market prospects and the lack of social interactions. We use the LISS survey to construct a number of predictors to capture those channels.

To measure childcare duties, we utilize information on the household structure and care giving arrangements within families. In particular, we combine information on the number of children below the age of 12 living in a household with information on the domestic situation (family composition) both elicited in the November 2019 wave and complemented by the March 2020 wave whenever data in November was not available. The latter variable assigns individuals to one out of five groups ”single”, ”relationship with children”, ”relationship without children”, ”single with children” and ”other”. Finally, we complement this information with information on the care arrangements for the additional childcare duties during the closure of schools and daycare facilities in March 2020.

Overall, this yields 7 different categories:

- caregiver “jointly”: both respondent and partner share the additional childcare duties (child < 12)
- caregiver “myself”: only respondent takes on additional duties (child < 12)
- caregiver “partner”: only partner takes on additional duties (child < 12)
- caregiver “other arrangement”: all arrangements not involving parents (child < 12)
- child aged 12-18 years
- no (dependent) child

- single parent

In addition to increased childcare responsibilities, parents often had to work more hours from home because employers were mandated to send their employees into home office if feasible. Hence, parents often faced a double-burden of working from home while taking care of their children. We investigate this double-burden channel utilizing the time-use survey which was in the field in April 2020. This survey explicitly asks for the amount of hours spent in home office while being in charge of looking after the children. We censor the replies at the usual level of full-time hours, which is 40 in the Netherlands.

Next, the second channel included is the infection channel, which has been elicited in all 2020 waves. In the March 2020 wave, participants were asked on a 7-point scale about the likelihood of contracting the virus ("no chance" to "certain", plus a separate option "already infected"). In all subsequent waves, respondents were asked about the perceived probability of getting infected on a scale from 0 to 100. To make the measure comparable across waves, we transform the 7-scale likelihood from March by assigning each answer a value from 0 to 7.[5] Despite the difference in how this subjective probability was elicited between the March and the subsequent waves, we do not expect that this has a major impact on our results. The reported probabilities are very persistent between March and May and there is no particular reason to believe that this should change dramatically in the month after.

We then re-scale the measure to range from 0 to 1 for all periods. The resulting outcome then measures the subjectively perceived infection risk. For November 2019 (pre-Covid) we set the perceived infection risk to zero because at that point in time SARS-CoVid-2 was not yet discovered.

[5]"already happened" also gets assigned a 7.

The third channel we explore links labor market expectations and outcomes of the pandemic to mental health outcomes. To this end, we construct two measures. First, we make use of information on changes in total working hours relative to before the crisis to capture the direct effect of the CoViD-19 crisis on individuals' labor market outcomes. Gaudecker et al. (2020a) argue that this is the most useful measure in the presence of firing restrictions and large-scale economic support programs.[6] Important for our purposes, Holler et al. (2021) show that parents did not reduce their hours more than other workers during the time where childcare facilities and schools were closed. Hence, there was no additional labor supply restriction due to additional childcare duties. In order to focus on substantial disruptions, we construct a dummy variable indicating whether working hours have reduced by more than 25 percent relative to their pre-CoViD-19 level. The amount of hours worked are elicited in all waves studied.

As a second measure, we use an individual's subjective probability to become unemployed over the next three months to measure the medium-term repercussions of the pandemic-induced recession on their labor market prospects. Such measures have proved to be good predictors of individuals' behaviors with respect to future consequences, such as consumption and savings (see for instance Curtin, 2003; Hendren, 2017; Pettinicchi and Vellekoop, 2019; Stephens Jr., 2004). Since we did not ask the question about unemployment expectations in June, we extrapolate the numbers from May. For November 2019 we use job loss expectations from the work and schooling questionnaire fielded in April 2019. Since the April 2019 wave asks for the "job loss probability over the next twelve months" but the 2020 survey waves

[6]Note that the rates of non-employment and unemployment increased only by about one percentage point each in the Netherlands between March and September of 2020, compared to 1.3 and 3.5 percentage points in the U.S. according to Bureau of Labor Statistics https://www.bls.gov/charts/employment-situation/civilian-labor-force-participation-rate.htm and https://fred.stlouisfed.org/series/UNRATE

elicit the probability of being unemployed in 3 months,[7] we re-scale the job-loss expectation for November 2019 to mirror unemployment expectations with a mean of 1.5%. In robustness exercises, we show that our results are not sensitive to the targeted level for re-scaling by (a) using the original job-loss expectation measure with a mean of 7% (Appendix Tables B.9 and B.13) and (b) by varying the targeted mean between 0.5% to 5.5% (Appendix Tables B.10 – B.12 and B.14 – B.16).

Finally, for the social interaction channel we use the 6-item De Jong-Gierveld loneliness questionnaire (Gierveld and Tilburg, 2006). Participants are asked to what extent the following statements apply to them:

- I experience a void around me
- There are plenty of people I can fall back on in case of trouble
- I know many people I can fully trust
- There are enough people with whom I feel closely connected
- I miss people around me
- I often feel let down

For each item, individuals can choose the following answer categories: yes, more or less, no. For positive items the answers are coded from 0 (yes) to 2 (no). Negative items are coded the other way around. The answers are summed up into a loneliness score ranging from 0 to 12, with higher values indicating a stronger feeling of loneliness. Since we did not ask these questions about loneliness in every wave (only November 2019; April, June, November 2020), we extrapolate the values from

[7] More precisely, the April 2019 wave asks for the probability of losing the current job within the next 12 months. However, this does not imply that the individual expects to be unemployed at any given point during this year. In the 2020 CoViD-surveys, we explicitly ask for 'being unemployed' in 3 months, thus not only losing the current position, but also not finding a new one.

April to March and May 2020 and the values from June to September and December 2020.[8] In our main specification, we kept also those individuals who answered the loneliness question only once during the pandemic, and extrapolated these answers across all waves except November 2019.

[8]We will use the November 2020 values for the December 2020 wave once they are available to us.

B.3 Appendix: Additional Tables

Table B.2: Sample Characteristics by Gender Over All Survey Periods

	men	women
age	47.54	45.76
low education	0.14	0.13
medium education	0.37	0.40
high education	0.49	0.47
observations	5,175	5,350

Note: Gender-specific averages over the survey period from November 2019 to December 2020.

Table B.3: Average Number of Hours Working From Home and Average Score on Jong-Gierveld Loneliness Scale, by Survey Period and Overall

	Nov '19	March '20	May '20	June '20	Sept '20	Dec '20	overall
Hours worked from home							
men	4.6	17.6	15.3	13.6	11.0	15.7	13.1
women	3.1	12.0	10.1	8.6	6.8	8.8	8.4
Loneliness score							
men	1.86	2.15	2.14	1.96	1.90	1.98	2.00
women	1.85	2.33	2.32	1.93	1.98	1.92	2.07
Number of observations							
men	846	1020	849	808	840	812	5,175
women	874	1088	884	828	878	798	5,350

Note: The exact design and timing of questions to construct the loneliness score can be found in Appendix B.2.

B.3.1 Full Set of Estimation Results

Table B.4: Full Set of Estimation Results for Equation (2.1)

	men	women
prob: becoming infected	-1.48	-2.12**
	(0.91)	(1.00)
reduced working hours: yes	-1.19***	-0.66
	(0.43)	(0.43)
prob: becoming unemployed	-9.62***	-3.16
	(2.01)	(2.14)
loneliness	-0.41***	-1.01***
	(0.15)	(0.17)
March 2020 (reference: sharing extra childcare duties)	-0.41	-1.37
	(1.52)	(1.99)
May 2020 (reference: sharing extra childcare duties)	3.06**	4.42***
	(1.35)	(1.52)
June 2020 (reference: sharing extra childcare duties)	4.21***	5.93***
	(1.26)	(1.85)
September 2020 (reference: sharing extra childcare duties)	5.25***	5.78***
	(1.35)	(1.69)
December 2020 (reference: sharing extra childcare duties)	0.44	3.02
	(1.43)	(1.87)
caregiver: myself x March 2020	-8.88***	-2.17
	(3.16)	(3.03)
caregiver: myself x May 2020	-7.55***	-4.37*
	(2.58)	(2.24)
caregiver: myself x June 2020	-5.38*	-4.69
	(2.85)	(2.93)
caregiver: myself x September 2020	-4.09	-4.70
	(2.69)	(3.00)
caregiver: myself x December 2020	-6.10**	-5.43*
	(2.72)	(3.16)
caregiver: partner x March 2020	0.48	-3.44
	(2.37)	(4.02)
caregiver: partner x May 2020	-2.01	-2.02
	(2.03)	(3.27)
caregiver: partner x June 2020	-1.12	-0.15
	(2.22)	(3.54)
caregiver: partner x September 2020	-1.34	-1.97
	(1.94)	(3.80)
caregiver: partner x December 2020	1.97	-3.54
	(2.23)	(3.56)
caregiver: other arrangement x March 2020	-8.03***	1.90
	(2.84)	(3.78)
caregiver: other arrangement x May 2020	-1.57	4.16
	(2.53)	(3.34)

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Table B.4 – *Continued from previous page*

	men	women
caregiver: other arrangement x June 2020	-3.49	3.44
	(2.99)	(3.21)
caregiver: other arrangement x September 2020	-5.70**	3.98
	(2.65)	(3.27)
caregiver: other arrangement x December 2020	-2.34	2.07
	(2.72)	(3.85)
child aged 12-18 x March 2020	-3.35*	-4.40**
	(1.75)	(2.21)
child aged 12-18 x May 2020	-0.95	-3.38*
	(1.52)	(1.78)
child aged 12-18 x June 2020	-1.66	-3.35
	(1.45)	(2.08)
child aged 12-18 x September 2020	-2.22	-3.33*
	(1.54)	(1.93)
child aged 12-18 x December 2020	1.21	-2.57
	(1.64)	(2.08)
no child x March 2020	-1.67	-0.48
	(1.60)	(2.08)
no child x May 2020	-1.53	-1.13
	(1.44)	(1.65)
no child x June 2020	-1.73	-1.66
	(1.40)	(1.95)
no child x September 2020	-3.15**	-1.46
	(1.47)	(1.82)
no child x December 2020	0.25	-1.20
	(1.52)	(1.98)
single parent x March 2020	1.89	0.32
	(2.66)	(2.61)
single parent x May 2020	0.12	-2.07
	(2.41)	(2.03)
single parent x June 2020	0.98	-3.53
	(2.77)	(2.46)
single parent x September 2020	-0.03	-4.41*
	(2.53)	(2.35)
single parent x December 2020	2.14	-1.30
	(2.77)	(2.36)
observations	5,175	5,350
number of individuals	1,138	1,215
individual specific FE	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1; standard errors clustered on the individual level; The table presents the estimated coefficients of the main channels on mental health obtained from Equation 2.1. We control for a full set of interactions between survey month and categories of caregiver arrangements and household structure. The reference period is November 2019, the reference category are parents who share the extra childcare that becomes necessary during the closure of school and daycare centers.

Table B.5: Full Set of Estimation Results for Equation (2.2)

	men	women
A. all periods		
March 2020	-1.69**	-2.82***
	(0.72)	(0.71)
May 2020	2.38***	2.72***
	(0.55)	(0.60)
June 2020	3.27***	3.87***
	(0.54)	(0.57)
September 2020	3.19***	3.71***
	(0.52)	(0.61)
December 2020	1.42**	1.36**
	(0.56)	(0.66)
hours worked from home (reference: sharing extra childcare duties)	-0.02	-0.01
	(0.04)	(0.05)
caregiver: myself x hours worked from home	-0.18***	-0.10
	(0.06)	(0.11)
caregiver: partner x hours worked from home	-0.01	-0.33**
	(0.06)	(0.13)
caregiver: other arrangement x hours worked from home	0.09	0.16
	(0.09)	(0.12)
caregiver: child aged 12-18 x hours worked from home	-0.05	0.02
	(0.04)	(0.06)
caregiver: no child x hours worked from home	-0.01	0.02
	(0.04)	(0.05)
caregiver: single parent x hours worked from home	-0.09	0.01
	(0.06)	(0.09)
prob: becoming infected	-1.53*	-1.98*
	(0.90)	(1.01)
reduced working hours: yes	-1.66***	-0.56
	(0.47)	(0.45)
prob: becoming unemployed	-9.64***	-3.16
	(2.01)	(2.14)
loneliness	-0.40***	-0.96***
	(0.15)	(0.16)
observations	5,175	5,350
number of individuals	1,138	1,215
individual specific FE	yes	yes
B. during spring lockdown (schools/childcare closed)		
March 2020	-1.30	-2.83***
	(1.00)	(0.97)
May 2020	2.91***	2.99***
	(0.75)	(0.77)

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Table B.5 – *Continued from previous page*

	men	women
hours worked from home (reference: sharing extra childcare duties)	-0.01	-0.10
	(0.05)	(0.08)
caregiver: myself x hours worked from home	-0.17*	-0.09
	(0.09)	(0.14)
caregiver: partner x hours worked from home	-0.08	-0.70***
	(0.10)	(0.21)
caregiver: other arrangement x hours worked from home	-0.03	0.44**
	(0.13)	(0.18)
child aged 12-18 x hours worked from home	-0.06	0.10
	(0.06)	(0.10)
no child x hours worked from home	-0.04	0.15*
	(0.05)	(0.09)
single parent x hours worked from home	-0.10	0.14
	(0.08)	(0.12)
prob: becoming infected	-1.47	-1.89
	(1.50)	(1.73)
reduced working hours: yes	-2.61***	-2.14***
	(0.82)	(0.77)
prob: becoming unemployed	-10.58***	-2.07
	(2.82)	(2.98)
loneliness	-0.67***	-1.04***
	(0.25)	(0.27)
observations	2,715	2,846
number of individuals	1,133	1,212
individual specific FE	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1; standard errors clustered on the individual level; Panel A shows the regression results of equation (2.2) for the working population in all survey waves from November 2019 to December 2020. Panel B shows the regression results when we restrict our sample to November 2019 and the first wave from March to May 2020. We control for a full set of interactions between survey month and categories of caregiver arrangements and household structure. The reference period is November 2019, the reference category are parents who share the extra childcare that becomes necessary during the closure of school and daycare centers.

Table B.6: Estimated Effects of Home Office Hours With Children on Mental Health

	men	women
hours in home office with children	0.426**	-0.489*
	(0.195)	(0.282)
(hours in home office with children)^2	-0.012**	0.016*
	(0.005)	(0.009)
prob: becoming infected	0.486	2.412
	(2.271)	(2.834)
reduced working hours: yes	-2.367**	-0.881
	(1.174)	(1.088)
prob: becoming unemployed	-7.165*	-4.821
	(4.170)	(5.225)
loneliness	-0.411	-0.966***
	(0.252)	(0.296)
observations	1,656	1,707
number of individuals	1,035	1,091
individual specific FE	yes	yes
survey month fixed effects	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1; standard error clustered on the individual level in parentheses. The table presents the estimated coefficients of a quadratic specification of the hours in home office with children on mental health. The estimation is based on a sample of 1,656 men and 1,707 women who participated in the survey in November 2019 and May 2020. In all specifications, we control for individual specific fixed effects and survey month fixed effects.

B.3.2 Balanced Sample

Table B.7: Estimation Results for Equation (2.1), Balanced Sample

	men	women
prob: becoming infected	-1.53	-0.08
	(1.13)	(1.37)
reduced working hours: yes	-1.23**	-0.68
	(0.53)	(0.61)
prob: becoming unemployed	-10.28***	-3.79
	(2.88)	(2.97)
loneliness	-0.73***	-1.00***
	(0.18)	(0.21)
March 2020 (reference: sharing extra childcare duties)	-0.26	-4.67
	(1.80)	(3.27)
May 2020 (reference: sharing extra childcare duties)	4.79***	2.25
	(1.77)	(2.10)
June 2020 (reference: sharing extra childcare duties)	3.98**	6.60***
	(1.54)	(2.51)
September 2020 (reference: sharing extra childcare duties)	4.02**	3.74*
	(1.62)	(2.01)
December 2020 (reference: sharing extra childcare duties)	-0.83	2.81
	(1.97)	(2.67)
caregiver: myself x March 2020	-9.39**	-5.98
	(4.10)	(4.63)
caregiver: myself x May 2020	-8.36***	-4.37
	(3.22)	(2.87)
caregiver: myself x June 2020	-3.26	-8.78**
	(3.04)	(3.86)
caregiver: myself x September 2020	-2.49	-4.22
	(3.47)	(4.27)
caregiver: myself x December 2020	-4.51	-10.99**
	(3.37)	(5.22)
caregiver: partner x March 2020	1.22	-2.54
	(3.39)	(5.42)
caregiver: partner x May 2020	-2.39	0.88
	(2.27)	(3.77)
caregiver: partner x June 2020	-1.06	0.00
	(2.69)	(4.39)
caregiver: partner x September 2020	0.72	3.42
	(2.29)	(3.47)
caregiver: partner x December 2020	4.72	-3.14
	(2.93)	(4.60)
caregiver: other arrangement x March 2020	-3.17	7.46
	(3.06)	(4.89)
caregiver: other arrangement x May 2020	-2.29	8.27**
	(3.01)	(4.01)

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Table B.7 – *Continued from previous page*

	men	women
caregiver: other arrangement x June 2020	-3.62	4.31
	(3.69)	(3.86)
caregiver: other arrangement x September 2020	-3.72	5.97
	(3.23)	(3.79)
caregiver: other arrangement x December 2020	-1.33	4.04
	(3.25)	(4.58)
child aged 12-18 x March 2020	-3.12	-3.70
	(2.05)	(3.49)
child aged 12-18 x May 2020	-1.95	-1.66
	(1.95)	(2.41)
child aged 12-18 x June 2020	-0.61	-4.61*
	(1.74)	(2.78)
child aged 12-18 x September 2020	-0.29	-1.90
	(1.84)	(2.34)
child aged 12-18 x December 2020	3.17	-2.16
	(2.16)	(2.89)
no child x March 2020	-2.82	1.18
	(1.90)	(3.34)
no child x May 2020	-3.94**	-0.17
	(1.87)	(2.20)
no child x June 2020	-1.71	-3.47
	(1.69)	(2.59)
no child x September 2020	-2.86	-0.73
	(1.76)	(2.15)
no child x December 2020	1.22	-1.97
	(2.04)	(2.75)
single parent x March 2020	0.84	0.77
	(3.39)	(3.94)
single parent x May 2020	-0.23	-0.93
	(2.97)	(2.59)
single parent x June 2020	0.95	-6.15**
	(3.26)	(3.11)
single parent x September 2020	0.16	-3.72
	(3.18)	(2.89)
single parent x December 2020	6.25*	-2.75
	(3.25)	(3.20)
observations	3,036	2,922
number of individuals	506	487
individual specific FE	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1; standard errors clustered on the individual level; The table presents the estimated coefficients of the main channels on mental health obtained from estimating Equation 2.1 on a balanced sample. We control for a full set of interactions between survey month and categories of caregiver arrangements and household structure. The reference period is November 2019, the reference category are parents who share the extra childcare that becomes necessary during the closure of school and daycare centers.

Table B.8: Estimation Results for Equation (2.2), Balanced Sample

	men	women
A. all periods		
March 2020	-1.77**	-5.00***
	(0.88)	(0.95)
May 2020	2.75***	1.87**
	(0.66)	(0.75)
June 2020	3.45***	3.05***
	(0.63)	(0.73)
September 2020	2.91***	2.73***
	(0.63)	(0.78)
December 2020	1.71**	0.68
	(0.71)	(0.87)
hours worked from home (reference: sharing extra childcare duties)	0.01	-0.02
	(0.04)	(0.06)
caregiver: myself x hours worked from home	-0.24***	-0.16
	(0.07)	(0.14)
caregiver: partner x hours worked from home	-0.07	-0.43***
	(0.10)	(0.11)
caregiver: other arrangement x hours worked from home	0.24***	0.18
	(0.09)	(0.19)
child aged 12-18 x hours worked from home	-0.09*	0.05
	(0.05)	(0.08)
no child x hours worked from home	-0.07	0.02
	(0.05)	(0.06)
single parent x hours worked from home	-0.08	0.04
	(0.07)	(0.12)
prob: becoming infected	-1.66	0.32
	(1.11)	(1.38)
reduced working hours: yes	-1.76***	-0.78
	(0.56)	(0.61)
prob: becoming unemployed	-10.41***	-3.87
	(2.89)	(3.00)
loneliness	-0.68***	-0.98***
	(0.18)	(0.22)
observations	3,036	2,922
number of individuals	506	487
individual specific FE	yes	yes
B. during lockdown of schools/childcare		
March 2020	-1.74	-4.16***
	(1.23)	(1.26)
May 2020	3.01***	2.60***
	(0.91)	(0.98)

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Table B.8 – *Continued from previous page*

	men	women
hours worked from home (reference: sharing extra childcare duties)	0.04	-0.11
	(0.06)	(0.11)
caregiver: myself x hours worked from home	-0.24**	-0.15
	(0.10)	(0.18)
caregiver: partner x hours worked from home	-0.11	-0.61***
	(0.14)	(0.15)
caregiver: other arrangement x hours worked from home	0.18	0.52**
	(0.13)	(0.22)
child aged 12-18 x hours worked from home	-0.08	0.17
	(0.07)	(0.14)
no child x hours worked from home	-0.12*	0.13
	(0.07)	(0.11)
single parent x hours worked from home	-0.09	0.18
	(0.09)	(0.15)
prob: becoming infected	-1.14	-1.00
	(1.96)	(2.27)
reduced working hours: yes	-3.12***	-1.84*
	(1.02)	(1.05)
prob: becoming unemployed	-12.89***	-4.30
	(3.78)	(3.94)
loneliness	-0.92***	-1.11***
	(0.31)	(0.33)
observations	1,518	1,461
number of individuals	506	487
individual specific FE	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1; standard errors clustered on the individual level; Panel A shows the regression results of equation (2.2) for the working population in a balanced sample in all survey waves from November 2019 to December 2020. Panel B shows the regression results when we restrict our balanced sample to November 2019 and the first wave from March to May 2020. We control for a full set of interactions between survey month and categories of caregiver arrangements and household structure. The reference period is November 2019, the reference category are parents who share the extra childcare that becomes necessary during the closure of school and daycare centers.

B.3.3 Re-scaling of Unemployment Expectations (UE)

Table B.9: Estimation Results for Equation (2.1), Original UE

	men	women
prob: becoming infected	-1.55*	-2.12**
	(0.91)	(1.00)
reduced working hours: yes	-1.21***	-0.67
	(0.43)	(0.43)
prob: becoming unemployed	-6.60***	-1.61
	(1.58)	(1.71)
loneliness	-0.41***	-1.01***
	(0.15)	(0.17)
March 2020 (reference: sharing extra childcare duties)	-0.98	-1.56
	(1.55)	(1.99)
May 2020 (reference: sharing extra childcare duties)	2.48*	4.24***
	(1.38)	(1.54)
June 2020(reference: sharing extra childcare duties)	3.63***	5.74***
	(1.29)	(1.86)
September 2020 (reference: sharing extra childcare duties)	4.67***	5.60***
	(1.37)	(1.71)
December 2020 (reference: sharing extra childcare duties)	-0.13	2.85
	(1.45)	(1.88)
caregiver: myself x March 2020	-8.87***	-2.20
	(3.26)	(3.04)
caregiver: myself x May 2020	-7.42***	-4.37*
	(2.59)	(2.24)
caregiver: myself x June 2020	-5.25*	-4.68
	(2.89)	(2.93)
caregiver: myself x September 2020	-3.96	-4.71
	(2.74)	(3.01)
caregiver: myself x December 2020	-5.83**	-5.42*
	(2.79)	(3.16)
caregiver: partner x March 2020	0.74	-3.34
	(2.40)	(4.01)
caregiver: partner x May 2020	-1.72	-1.89
	(2.05)	(3.28)
caregiver: partner x June 2020	-0.85	-0.02
	(2.23)	(3.55)
caregiver: partner x September 2020	-1.06	-1.87
	(1.96)	(3.81)
caregiver: partner x December 2020	2.21	-3.44
	(2.24)	(3.58)
caregiver: other arrangement x March 2020	-7.84***	2.00
	(2.85)	(3.78)
caregiver: other arrangement x May 2020	-1.31	4.25

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Table B.9 – *Continued from previous page*

	men	women
	(2.56)	(3.35)
caregiver: other arrangement x June 2020	-3.23	3.54
	(3.04)	(3.21)
caregiver: other arrangement x September 2020	-5.44**	4.09
	(2.72)	(3.28)
caregiver: other arrangement x December 2020	-2.14	2.18
	(2.74)	(3.86)
child aged 12-18 x March 2020	-3.11*	-4.32*
	(1.76)	(2.21)
child aged 12-18 x May 2020	-0.73	-3.29*
	(1.54)	(1.79)
child aged 12-18 x June 2020	-1.43	-3.25
	(1.47)	(2.08)
child aged 12-18 x September 2020	-1.94	-3.22*
	(1.55)	(1.94)
child aged 12-18 x December 2020	1.48	-2.47
	(1.65)	(2.09)
no child x March 2020	-1.50	-0.42
	(1.62)	(2.09)
no child x May 2020	-1.37	-1.04
	(1.47)	(1.66)
no child x June 2020	-1.56	-1.56
	(1.42)	(1.95)
no child x September 2020	-2.98**	-1.37
	(1.49)	(1.83)
no child x December 2020	0.39	-1.13
	(1.53)	(1.98)
single parent x March 2020	1.78	0.39
	(2.67)	(2.62)
single parent x May 2020	0.03	-1.98
	(2.48)	(2.05)
single parent x June 2020	0.87	-3.44
	(2.82)	(2.47)
single parent x September 2020	-0.23	-4.34*
	(2.58)	(2.37)
single parent x December 2020	2.09	-1.21
	(2.81)	(2.37)
observations	5,175	5,350
number of individuals	1,138	1,215
individual specific FE	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1; standard errors clustered on the individual level; The table presents the estimated coefficients of the main channels on mental health obtained from Equation (2.1). We control for a full set of interactions between survey month and categories of caregiver arrangements and household structure. The reference period is November 2019, the reference category are parents who share the extra childcare that becomes necessary during the closure of school and daycare centers. No re-scaling of the original unemployment expectations for November 2019.

Table B.10: Estimation Results for Equation (2.1), Nov. 2019 UE With Mean 0.5%

	men	women
prob: becoming infected	-1.46	-2.12**
	(0.91)	(1.00)
reduced working hours: yes	-1.19***	-0.66
	(0.43)	(0.43)
prob: becoming unemployed	-9.67***	-3.34
	(2.01)	(2.14)
loneliness	-0.40***	-1.01***
	(0.15)	(0.17)
March 2020 (reference: sharing extra childcare duties)	-0.27	-1.31
	(1.52)	(1.99)
May 2020 (reference: sharing extra childcare duties)	3.20**	4.49***
	(1.34)	(1.52)
June 2020 (reference: sharing extra childcare duties)	4.36***	6.00***
	(1.26)	(1.86)
September 2020 (reference: sharing extra childcare duties)	5.40***	5.85***
	(1.35)	(1.69)
December 2020 (reference: sharing extra childcare duties)	0.59	3.09*
	(1.43)	(1.87)
caregiver: myself x March 2020	-8.92***	-2.18
	(3.15)	(3.03)
caregiver: myself x May 2020	-7.59***	-4.39*
	(2.56)	(2.24)
caregiver: myself x June 2020	-5.43*	-4.71
	(2.84)	(2.93)
caregiver: myself x September 2020	-4.14	-4.72
	(2.69)	(2.99)
caregiver: myself x December 2020	-6.15**	-5.45*
	(2.71)	(3.16)
caregiver: partner x March 2020	0.41	-3.49
	(2.37)	(4.02)
caregiver: partner x May 2020	-2.08	-2.06
	(2.03)	(3.27)
caregiver: partner x June 2020	-1.19	-0.20
	(2.22)	(3.54)
caregiver: partner x September 2020	-1.41	-2.01
	(1.94)	(3.80)
caregiver: partner x December 2020	1.90	-3.58
	(2.23)	(3.55)
caregiver: other arrangement x March 2020	-8.10***	1.87
	(2.84)	(3.78)
caregiver: other arrangement x May 2020	-1.64	4.12
	(2.52)	(3.34)
caregiver: other arrangement x June 2020	-3.55	3.40
	(2.98)	(3.21)

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Table B.10 – *Continued from previous page*

	men	women
caregiver: other arrangement x September 2020	-5.78**	3.94
	(2.64)	(3.28)
caregiver: other arrangement x December 2020	-2.41	2.03
	(2.71)	(3.86)
child aged 12-18 x March 2020	-3.42*	-4.43**
	(1.74)	(2.21)
child aged 12-18 x May 2020	-1.02	-3.41*
	(1.52)	(1.78)
child aged 12-18 x June 2020	-1.72	-3.39
	(1.45)	(2.08)
child aged 12-18 x September 2020	-2.29	-3.37*
	(1.53)	(1.93)
child aged 12-18 x December 2020	1.14	-2.61
	(1.64)	(2.08)
no child x March 2020	-1.71	-0.51
	(1.60)	(2.08)
no child x May 2020	-1.58	-1.16
	(1.44)	(1.65)
no child x June 2020	-1.77	-1.69
	(1.39)	(1.95)
no child x September 2020	-3.20**	-1.49
	(1.47)	(1.82)
no child x December 2020	0.20	-1.24
	(1.52)	(1.98)
single parent x March 2020	1.91	0.28
	(2.66)	(2.61)
single parent x May 2020	0.14	-2.11
	(2.40)	(2.03)
single parent x June 2020	1.01	-3.57
	(2.76)	(2.46)
single parent x September 2020	-0.02	-4.45*
	(2.52)	(2.35)
single parent x December 2020	2.15	-1.34
	(2.76)	(2.36)
observations	5,175	5,350
number of individuals	1,138	1,215
individual specific FE	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1; standard errors clustered on the individual level; The table presents the estimated coefficients of the main channels on mental health obtained from Equation (2.1). We control for a full set of interactions between survey month and categories of caregiver arrangements and household structure. The reference period is November 2019, the reference category are parents who share the extra childcare that becomes necessary during the closure of school and daycare centers. Unemployment expectations in November 2019 are re-scaled to have mean 0.5%.

Table B.11: Estimation Results for Equation (2.1), Nov. 2019 UE With Mean 3%

	men	women
prob: becoming infected	-1.51*	-2.12**
	(0.91)	(1.00)
reduced working hours: yes	-1.19***	-0.66
	(0.43)	(0.43)
prob: becoming unemployed	-9.15***	-2.77
	(1.94)	(2.07)
loneliness	-0.41***	-1.01***
	(0.15)	(0.17)
March 2020 (reference: sharing extra childcare duties)	-0.61	-1.46
	(1.53)	(1.99)
May 2020 (reference: sharing extra childcare duties)	2.85**	4.34***
	(1.35)	(1.52)
June 2020 (reference: sharing extra childcare duties)	4.00***	5.84***
	(1.27)	(1.85)
September 2020 (reference: sharing extra childcare duties)	5.04***	5.70***
	(1.35)	(1.69)
December 2020 (reference: sharing extra childcare duties)	0.22	2.94
	(1.43)	(1.87)
caregiver: myself x March 2020	-8.84***	-2.17
	(3.18)	(3.04)
caregiver: myself x May 2020	-7.49***	-4.36*
	(2.59)	(2.24)
caregiver: myself x June 2020	-5.33*	-4.68
	(2.87)	(2.93)
caregiver: myself x September 2020	-4.03	-4.69
	(2.71)	(3.00)
caregiver: myself x December 2020	-6.02**	-5.41*
	(2.74)	(3.16)
caregiver: partner x March 2020	0.57	-3.39
	(2.37)	(4.02)
caregiver: partner x May 2020	-1.90	-1.96
	(2.04)	(3.27)
caregiver: partner x June 2020	-1.02	-0.09
	(2.22)	(3.54)
caregiver: partner x September 2020	-1.24	-1.92
	(1.95)	(3.81)
caregiver: partner x December 2020	2.06	-3.49
	(2.23)	(3.57)
caregiver: other arrangement x March 2020	-7.94***	1.95
	(2.84)	(3.78)
caregiver: other arrangement x May 2020	-1.47	4.21
	(2.54)	(3.34)
caregiver: other arrangement x June 2020	-3.39	3.49
	(3.01)	(3.21)

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Table B.11 – *Continued from previous page*

	men	women
caregiver: other arrangement x September 2020	-5.60**	4.03
	(2.68)	(3.27)
caregiver: other arrangement x December 2020	-2.24	2.13
	(2.72)	(3.85)
child aged 12-18 x March 2020	-3.26*	-4.36**
	(1.75)	(2.21)
child aged 12-18 x May 2020	-0.87	-3.33*
	(1.53)	(1.78)
child aged 12-18 x June 2020	-1.57	-3.30
	(1.46)	(2.08)
child aged 12-18 x September 2020	-2.12	-3.28*
	(1.54)	(1.93)
child aged 12-18 x December 2020	1.31	-2.52
	(1.64)	(2.08)
no child x March 2020	-1.60	-0.44
	(1.60)	(2.08)
no child x May 2020	-1.47	-1.09
	(1.45)	(1.65)
no child x June 2020	-1.66	-1.61
	(1.41)	(1.94)
no child x September 2020	-3.08**	-1.42
	(1.48)	(1.82)
no child x December 2020	0.31	-1.16
	(1.52)	(1.98)
single parent x March 2020	1.86	0.36
	(2.66)	(2.61)
single parent x May 2020	0.09	-2.02
	(2.43)	(2.03)
single parent x June 2020	0.95	-3.48
	(2.79)	(2.46)
single parent x September 2020	-0.08	-4.37*
	(2.55)	(2.36)
single parent x December 2020	2.12	-1.25
	(2.78)	(2.36)
observations	5,175	5,350
number of individuals	1,138	1,215
individual specific FE	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1; standard errors clustered on the individual level; The table presents the estimated coefficients of the main channels on mental health obtained from Equation (2.1). We control for a full set of interactions between survey month and categories of caregiver arrangements and household structure. The reference period is November 2019, the reference category are parents who share the extra childcare that becomes necessary during the closure of school and daycare centers. Unemployment expectations in November 2019 are re-scaled to have mean 3%.

Table B.12: Estimation Results for Equation (2.1), Nov. 2019 UE With Mean 5.5%

	men	women
prob: becoming infected	-1.54*	-2.12**
	(0.91)	(1.00)
reduced working hours: yes	-1.20***	-0.67
	(0.43)	(0.43)
prob: becoming unemployed	-7.64***	-2.01
	(1.72)	(1.86)
loneliness	-0.41***	-1.01***
	(0.15)	(0.17)
March 2020 (reference: sharing extra childcare duties)	-0.88	-1.54
	(1.54)	(1.99)
May 2020 (reference: sharing extra childcare duties)	2.58*	4.26***
	(1.37)	(1.53)
June 2020 (reference: sharing extra childcare duties)	3.73***	5.76***
	(1.28)	(1.85)
September 2020 (reference: sharing extra childcare duties)	4.77***	5.61***
	(1.37)	(1.71)
December 2020 (reference: sharing extra childcare duties)	-0.04	2.86
	(1.44)	(1.88)
caregiver: myself x March 2020	-8.84***	-2.19
	(3.23)	(3.04)
caregiver: myself x May 2020	-7.43***	-4.36*
	(2.60)	(2.24)
caregiver: myself x June 2020	-5.27*	-4.67
	(2.88)	(2.93)
caregiver: myself x September 2020	-3.97	-4.70
	(2.73)	(3.01)
caregiver: myself x December 2020	-5.89**	-5.41*
	(2.77)	(3.16)
caregiver: partner x March 2020	0.70	-3.35
	(2.39)	(4.02)
caregiver: partner x May 2020	-1.77	-1.91
	(2.04)	(3.28)
caregiver: partner x June 2020	-0.89	-0.03
	(2.23)	(3.54)
caregiver: partner x September 2020	-1.11	-1.87
	(1.96)	(3.81)
caregiver: partner x December 2020	2.17	-3.45
	(2.24)	(3.58)
caregiver: other arrangement x March 2020	-7.85***	1.99
	(2.84)	(3.78)
caregiver: other arrangement x May 2020	-1.35	4.24
	(2.56)	(3.34)
caregiver: other arrangement x June 2020	-3.27	3.54
	(3.03)	(3.21)

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Table B.12 – *Continued from previous page*

	men	women
caregiver: other arrangement x September 2020	-5.48**	4.08
	(2.71)	(3.27)
caregiver: other arrangement x December 2020	-2.15	2.17
	(2.73)	(3.85)
child aged 12-18 x March 2020	-3.15*	-4.32*
	(1.76)	(2.21)
child aged 12-18 x May 2020	-0.77	-3.30*
	(1.54)	(1.78)
child aged 12-18 x June 2020	-1.47	-3.26
	(1.47)	(2.08)
child aged 12-18 x September 2020	-1.99	-3.23*
	(1.55)	(1.94)
child aged 12-18 x December 2020	1.43	-2.48
	(1.64)	(2.09)
no child x March 2020	-1.53	-0.42
	(1.61)	(2.08)
no child x May 2020	-1.40	-1.05
	(1.46)	(1.65)
no child x June 2020	-1.59	-1.57
	(1.42)	(1.95)
no child x September 2020	-3.01**	-1.38
	(1.49)	(1.82)
no child x December 2020	0.38	-1.13
	(1.53)	(1.98)
single parent x March 2020	1.80	0.39
	(2.67)	(2.62)
single parent x May 2020	0.05	-1.98
	(2.47)	(2.04)
single parent x June 2020	0.90	-3.44
	(2.81)	(2.46)
single parent x September 2020	-0.17	-4.34*
	(2.57)	(2.37)
single parent x December 2020	2.10	-1.21
	(2.80)	(2.36)
observations	5,175	5,350
number of individuals	1,138	1,215
individual specific FE	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1; standard errors clustered on the individual level; The table presents the estimated coefficients of the main channels on mental health obtained from Equation (2.1). We control for a full set of interactions between survey month and categories of caregiver arrangements and household structure. The reference period is November 2019, the reference category are parents who share the extra childcare that becomes necessary during the closure of school and daycare centers. Unemployment expectations in November 2019 are re-scaled to have mean 5.5%.

Table B.13: Estimation Results for Equation (2.2), Original UE

	men	women
A. all periods		
March 2020	-2.07***	-2.94***
	(0.72)	(0.71)
May 2020	1.98***	2.62***
	(0.55)	(0.60)
June 2020	2.86***	3.77***
	(0.54)	(0.58)
September 2020	2.79***	3.60***
	(0.53)	(0.62)
December 2020	1.02*	1.26*
	(0.57)	(0.66)
hours worked from home (reference: sharing extra childcare duties)	-0.02	-0.01
	(0.04)	(0.05)
caregiver: myself x hours worked from home	-0.18***	-0.10
	(0.06)	(0.11)
caregiver: partner x hours worked from home	-0.02	-0.32**
	(0.06)	(0.13)
caregiver: other arrangement x hours worked from home	0.09	0.16
	(0.09)	(0.12)
caregiver: child aged 12-18 x hours worked from home	-0.05	0.02
	(0.04)	(0.06)
caregiver: no child x hours worked from home	-0.01	0.02
	(0.04)	(0.05)
caregiver: single parent x hours worked from home	-0.09	0.01
	(0.06)	(0.09)
prob: becoming infected	-1.60*	-1.98*
	(0.90)	(1.01)
reduced working hours: yes	-1.71***	-0.57
	(0.48)	(0.45)
prob: becoming unemployed	-6.95***	-1.69
	(1.59)	(1.72)
loneliness	-0.41***	-0.96***
	(0.15)	(0.16)
observations	5,175	5,350
number of individuals	1,138	1,215
individual specific FE	yes	yes
B. during spring lockdown (schools/childcare closed)		
March 2020	-1.62	-2.88***
	(1.00)	(0.97)
May 2020	2.55***	2.97***
	(0.75)	(0.78)

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Table B.13 – *Continued from previous page*

	men	women
hours worked from home (reference: sharing extra childcare duties)	-0.02	-0.10
	(0.05)	(0.08)
caregiver: myself x hours worked from home	-0.17*	-0.09
	(0.09)	(0.14)
caregiver: partner x hours worked from home	-0.09	-0.70***
	(0.10)	(0.21)
caregiver: other arrangement x hours worked from home	-0.02	0.44**
	(0.13)	(0.18)
child aged 12-18 x hours worked from home	-0.06	0.10
	(0.06)	(0.10)
no child x hours worked from home	-0.04	0.14*
	(0.05)	(0.09)
single parent x hours worked from home	-0.09	0.13
	(0.08)	(0.12)
prob: becoming infected	-1.57	-1.91
	(1.50)	(1.73)
reduced working hours: yes	-2.70***	-2.17***
	(0.82)	(0.77)
prob: becoming unemployed	-6.40***	-0.19
	(2.09)	(2.09)
loneliness	-0.70***	-1.04***
	(0.25)	(0.27)
observations	2,715	2,846
number of individuals	1,133	1,212
individual specific FE	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1; standard errors clustered on the individual level; The table presents the estimation results of home office hours by caregiver duties on mental health for the working population from equation (2.2) separately for men and women. Panel A shows the regression results for the working population in all survey waves from November 2019 to December 2020. Panel B shows the regression results when we restrict our results to November 2019 and the first lockdown, which included closed schools and daycare centers, from March to May 2020. The baseline period is November 2019. No re-scaling of the original unemployment expectations for November 2019.

Table B.14: Estimation Results for Equation (2.2), Nov. 2019 UE With Mean 0.5%

	men	women
A. all periods		
March 2020	-1.60**	-2.79***
	(0.72)	(0.71)
May 2020	2.46***	2.75***
	(0.55)	(0.60)
June 2020	3.36***	3.91***
	(0.54)	(0.57)
September 2020	3.28***	3.74***
	(0.53)	(0.61)
December 2020	1.51***	1.39**
	(0.57)	(0.66)
hours worked from home (reference: sharing extra childcare duties)	-0.02	-0.01
	(0.04)	(0.05)
caregiver: myself x hours worked from home	-0.18***	-0.10
	(0.06)	(0.11)
caregiver: partner x hours worked from home	-0.01	-0.33**
	(0.06)	(0.13)
caregiver: other arrangement x hours worked from home	0.09	0.16
	(0.09)	(0.12)
caregiver: child aged 12-18 x hours worked from home	-0.05	0.01
	(0.04)	(0.06)
caregiver: no child x hours worked from home	-0.02	0.02
	(0.04)	(0.05)
caregiver: single parent x hours worked from home	-0.09	0.01
	(0.06)	(0.09)
prob: becoming infected	-1.50*	-1.98*
	(0.90)	(1.01)
reduced working hours: yes	-1.65***	-0.55
	(0.47)	(0.45)
prob: becoming unemployed	-9.61***	-3.32
	(2.00)	(2.15)
loneliness	-0.40***	-0.96***
	(0.15)	(0.16)
observations	5,175	5,350
number of individuals	1,138	1,215
individual specific FE	yes	yes
B. during spring lockdown (schools/childcare closed)		
March 2020	-1.22	-2.80***
	(1.00)	(0.97)
May 2020	2.99***	3.01***
	(0.75)	(0.78)

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Table B.14 – *Continued from previous page*

	men	women
hours worked from home (reference: sharing extra childcare duties)	-0.01	-0.10
	(0.05)	(0.08)
caregiver: myself x hours worked from home	-0.17*	-0.09
	(0.09)	(0.14)
caregiver: partner x hours worked from home	-0.08	-0.70***
	(0.10)	(0.22)
caregiver: other arrangement x hours worked from home	-0.03	0.44**
	(0.13)	(0.18)
child aged 12-18 x hours worked from home	-0.06	0.10
	(0.06)	(0.10)
no child x hours worked from home	-0.05	0.14*
	(0.05)	(0.09)
single parent x hours worked from home	-0.09	0.13
	(0.08)	(0.12)
prob: becoming infected	-1.42	-1.89
	(1.50)	(1.73)
reduced working hours: yes	-2.61***	-2.13***
	(0.82)	(0.77)
prob: becoming unemployed	-10.55***	-2.47
	(2.79)	(3.01)
loneliness	-0.67***	-1.04***
	(0.25)	(0.27)
observations	2,715	2,846
number of individuals	1,133	1,212
individual specific FE	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1; standard errors clustered on the individual level; The table presents the estimation results of home office hours by caregiver duties on mental health for the working population from equation (2.2) separately for men and women. Panel A shows the regression results for the working population in all survey waves from November 2019 to December 2020. Panel B shows the regression results when we restrict our results to November 2019 and the first lockdown, which included closed schools and daycare centers, from March to May 2020. The baseline period is November 2019. Unemployment expectations in November 2019 are re-scaled to have mean 0.5%.

Table B.15: Estimation Results for Equation (2.2), Nov. 2019 UE With Mean 3%

	men	women
A. all periods		
March 2020	-1.81**	-2.87***
	(0.72)	(0.71)
May 2020	2.24***	2.68***
	(0.55)	(0.60)
June 2020	3.13***	3.83***
	(0.54)	(0.57)
September 2020	3.05***	3.66***
	(0.52)	(0.61)
December 2020	1.28**	1.32**
	(0.56)	(0.66)
hours worked from home (reference: sharing extra childcare duties)	-0.02	-0.01
	(0.04)	(0.05)
caregiver: myself x hours worked from home	-0.18***	-0.10
	(0.06)	(0.11)
caregiver: partner x hours worked from home	-0.01	-0.33**
	(0.06)	(0.13)
caregiver: other arrangement x hours worked from home	0.09	0.16
	(0.09)	(0.12)
caregiver: child aged 12-18 x hours worked from home	-0.05	0.02
	(0.04)	(0.06)
caregiver: no child x hours worked from home	-0.01	0.02
	(0.04)	(0.05)
caregiver: single parent x hours worked from home	-0.09	0.01
	(0.06)	(0.09)
prob: becoming infected	-1.56*	-1.98*
	(0.90)	(1.01)
reduced working hours: yes	-1.67***	-0.56
	(0.47)	(0.45)
prob: becoming unemployed	-9.29***	-2.80
	(1.94)	(2.08)
loneliness	-0.41***	-0.96***
	(0.15)	(0.16)
observations	5,175	5,350
number of individuals	1,138	1,215
individual specific FE	yes	yes
B. during spring lockdown (schools/childcare closed)		
March 2020	-1.41	-2.86***
	(1.00)	(0.97)
May 2020	2.78***	2.97***
	(0.75)	(0.77)

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Table B.15 – *Continued from previous page*

	men	women
hours worked from home (reference: sharing extra childcare duties)	-0.01	-0.10
	(0.05)	(0.08)
caregiver: myself x hours worked from home	-0.17*	-0.09
	(0.09)	(0.14)
caregiver: partner x hours worked from home	-0.08	-0.70***
	(0.10)	(0.21)
caregiver: other arrangement x hours worked from home	-0.02	0.44**
	(0.13)	(0.18)
child aged 12-18 x hours worked from home	-0.06	0.11
	(0.06)	(0.10)
no child x hours worked from home	-0.04	0.15*
	(0.05)	(0.09)
single parent x hours worked from home	-0.10	0.14
	(0.08)	(0.12)
prob: becoming infected	-1.53	-1.90
	(1.50)	(1.73)
reduced working hours: yes	-2.63***	-2.15***
	(0.82)	(0.77)
prob: becoming unemployed	-9.92***	-1.42
	(2.72)	(2.80)
loneliness	-0.68***	-1.04***
	(0.25)	(0.27)
observations	2,715	2,846
number of individuals	1,133	1,212
individual specific FE	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1; standard errors clustered on the individual level; The table presents the estimation results of home office hours by caregiver duties on mental health for the working population from equation (2.2) separately for men and women. Panel A shows the regression results for the working population in all survey waves from November 2019 to December 2020. Panel B shows the regression results when we restrict our results to November 2019 and the first lockdown, which included closed schools and daycare centers, from March to May 2020. The baseline period is November 2019. Unemployment expectations in November 2019 are re-scaled to have mean 3%.

Table B.16: Estimation Results for Equation (2.2), Nov. 2019 UE With Mean 5.5%

	men	women
A. all periods		
March 2020	-1.99***	-2.92***
	(0.72)	(0.71)
May 2020	2.05***	2.63***
	(0.55)	(0.60)
June 2020	2.94***	3.78***
	(0.54)	(0.57)
September 2020	2.86***	3.62***
	(0.53)	(0.61)
December 2020	1.10*	1.27*
	(0.56)	(0.66)
hours worked from home (reference: sharing extra childcare duties)	-0.02	-0.01
	(0.04)	(0.05)
caregiver: myself x hours worked from home	-0.18***	-0.10
	(0.06)	(0.11)
caregiver: partner x hours worked from home	-0.02	-0.32**
	(0.06)	(0.13)
caregiver: other arrangement x hours worked from home	0.09	0.16
	(0.09)	(0.12)
caregiver: child aged 12-18 x hours worked from home	-0.05	0.02
	(0.04)	(0.06)
caregiver: no child x hours worked from home	-0.01	0.02
	(0.04)	(0.05)
caregiver: single parent x hours worked from home	-0.09	0.01
	(0.06)	(0.09)
prob: becoming infected	-1.59*	-1.98*
	(0.90)	(1.01)
reduced working hours: yes	-1.70***	-0.57
	(0.47)	(0.45)
prob: becoming unemployed	-7.94***	-2.08
	(1.73)	(1.87)
loneliness	-0.41***	-0.96***
	(0.15)	(0.16)
observations	5,175	5,350
number of individuals	1,138	1,215
individual specific FE	yes	yes
B. during spring lockdown (schools/childcare closed)		
March 2020	-1.57	-2.88***
	(1.00)	(0.97)
May 2020	2.61***	2.96***
	(0.75)	(0.78)

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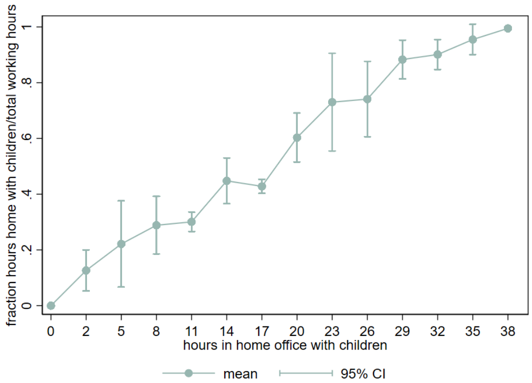
Table B.16 – *Continued from previous page*

	men	women
hours worked from home (reference: sharing extra childcare duties)	-0.02	-0.10
	(0.05)	(0.08)
caregiver: myself x hours worked from home	-0.17*	-0.09
	(0.09)	(0.14)
caregiver: partner x hours worked from home	-0.09	-0.70***
	(0.10)	(0.21)
caregiver: other arrangement x hours worked from home	-0.02	0.44**
	(0.13)	(0.18)
child aged 12-18 x hours worked from home	-0.06	0.10
	(0.06)	(0.10)
no child x hours worked from home	-0.04	0.14*
	(0.05)	(0.09)
single parent x hours worked from home	-0.10	0.14
	(0.08)	(0.12)
prob: becoming infected	-1.57	-1.91
	(1.50)	(1.73)
reduced working hours: yes	-2.68***	-2.16***
	(0.82)	(0.77)
prob: becoming unemployed	-7.75***	-0.53
	(2.34)	(2.36)
loneliness	-0.69***	-1.04***
	(0.25)	(0.27)
observations	2,715	2,846
number of individuals	1,133	1,212
individual specific FE	yes	yes

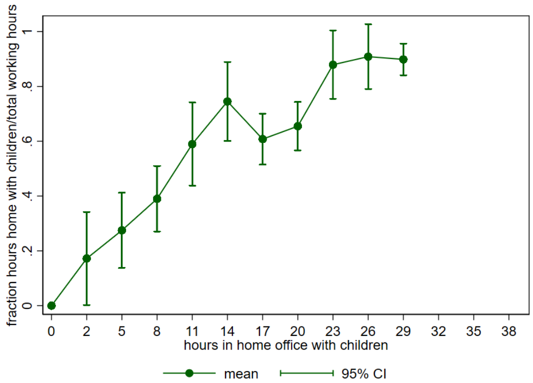
Note: *** p<0.01, ** p<0.05, * p<0.1; standard errors clustered on the individual level; The table presents the estimation results of home office hours by caregiver duties on mental health for the working population from equation (2.2) separately for men and women. Panel A shows the regression results for the working population in all survey waves from November 2019 to December 2020. Panel B shows the regression results when we restrict our results to November 2019 and the first lockdown, which included closed schools and daycare centers, from March to May 2020. The baseline period is November 2019. Unemployment expectations in November 2019 are re-scaled to have mean 5.5%.

B.4 Appendix: Additional Figures

Figure B.1: Mean Fraction of Home Office Hours With Children and Total Working Hours by Categories of Home Office Hours With Children



(a) Men



(b) Women

Chapter 3

Peer Effects in Financial Decisions

Abstract

We study whether, to what extent and how families' financial decisions are affected by their peers, in particular by their (adult) siblings. We provide causal evidence of peer effects in financial decisions, making use of Dutch administrative data and an IV strategy with partially overlapping peer groups. We find that positive asset market experiences of siblings generate positive spillover effects in terms of first-time investments in risky assets. These effects are primarily driven by the siblings of the female partner. Results suggest that informational spillovers constitute the main underlying mechanism.

3.1 Introduction

Financial investment decisions, including the participation decision to invest in risky assets, are among the most important decisions of a household with long-lasting consequences for the household's financial well-being. Despite evidence on the benefits of diversification across different investment types and the historically good performance of the stock market, participation in the risky asset market remains low in many countries (see, e.g., Guiso et al., 2003).[1] For example, in the Netherlands, only around 15% of households held risky financial assets in the form of shares and bonds in the year 2020.[2]

Low participation in the stock and bond market has severe and long-lasting consequences for both individuals' financial well-being as well as for society as a whole. From an individual's perspective, non-participation leads to slower wealth accumulation, fewer opportunities for consumption smoothing, and poorer retirement readiness. Moreover, non-participation also has important implications for society as a whole. It involves lower aggregate investments and exacerbates inequality because non-participation and financial mistakes are particularly pronounced among low-income households (e.g. Campbell, 2006; Guiso and Sodini, 2013).

The literature has long stressed the importance of cognitive ability and competencies such as financial literacy and financial awareness in relation to financial investment decisions and mistakes (see, e.g., Agarwal and Mazumder, 2013; Guiso and Jappelli, 2005; Lusardi and Mitchell, 2017; Van Rooij et al., 2011). Given that financial investment decisions, particularly the decisions to invest in risky assets, are challenging due to the complexity of financial instruments and the level

[1] The phenomenon that stock market participation is low despite a substantial risk premium and gains related to diversification is often referred to as the stock market participation or non-participation puzzle (see Guiso and Sodini, 2013, for an overview).

[2] Authors' own calculation based on open data from CBS/ Statline using the "Wealth of Household" component.

of uncertainty involved in such investments, individuals may rely on financial advice to reduce informational entry barriers. Gennaioli et al. (2014) emphasize trust as an important factor in financial investment decisions, and survey evidence suggests that a significant fraction of individuals rely on their peers for financial advice (Gaudecker, 2015; Lieber and Skimmyhorn, 2018).

This paper provides causal evidence on family peer effects in financial decisions. We study the extent to which a couple's decision to invest in risky assets for the first time (i.e., entry decision) is affected by financial investments of their siblings. In particular, we explore the average effect of siblings' positive experiences in the risky asset market, defined as a positive change in the value of their risky assets, on a couple's first-time entry decision. Next, we zoom in on potential heterogeneities to determine which couples are influenced by their siblings, which allows us to identify possible mechanisms through which peer effects in financial investment decisions may operate.

Identifying peer effects is challenging because correlations in peer outcomes can stem from various sources, including the endogenous effect via peer outcomes, the exogenous effect via peer characteristics, and the correlated effect via unobserved shocks affecting peers simultaneously (see, e.g., Manski, 1993). The literature has proposed different strategies to overcome this problem by, e.g., using randomized peer groups (see, e.g. Sacerdote, 2001) or partially overlapping peer groups (see, e.g. Bramoullé et al., 2009; De Giorgi et al., 2020). In this paper, we follow the latter approach using high-quality administrative data from the Netherlands. The data maintained by Statistics Netherlands (CBS) contains detailed demographic and geographic information on the entire Dutch population, thus allowing for the construction of sibling and neighborhood peer networks. Specifically, assuming that couples communicate with their own neighbors regarding financial investments, but not with the neighbors of their siblings, we exploit the information from these two distinct peer groups in an IV strategy where we instrument siblings' financial outcomes with their neighbors' average outcomes. Moreover, the administrative data contains detailed information on households' financial wealth based on tax records, including a split between safe assets (bank and savings accounts) and risky assets (shares, bonds, etc.), which allows us to construct a measure of couples' financial investment decisions: first-time participation in the risky asset market.

Our results are twofold. First, we find overall evidence for positive sibling spillovers. Couples with a higher fraction of siblings with a positive experience in the risky asset market are more likely to enter the stock and bond market themselves. In particular, an 11 percentage point (i.e., one standard deviation) increase in the fraction of siblings with positive experiences increases a couples' likelihood of asset market entry by 0.25 percentage points which translates to a 4% increase. Moreover, these couples also enter the risky asset market with a higher initial investment value (an increase of 10%). Interestingly, these effects seem to be entirely driven by the siblings of the female partner in a couple. Allowing siblings of the female and male partner to have a distinct influence, we find that a standard deviation increase of the females' siblings with a positive experience (i.e., 16 percentage points) leads to an 8% increase of the couples' entry likelihood (i.e., 0.5 percentage points). This positive spillover remains robust to the inclusion of siblings' negative experiences.

Second, to pin down potential mechanisms through which peer effects in financial investments may arise, we present evidence from heterogeneity analyses. In particular, we are interested in determining which couples are influenced by their siblings. Our findings suggest that couples from a high-SES background and couples where at least one partner is employed in the financial sector are not influenced by their siblings' experiences in the risky asset market. These findings are consistent with a mechanism related to the transmission of information, i.e., peer effects arise because information is passed on from the informed/ experienced sibling participating in the risky asset market to the (financially) uninformed sibling.

Our paper adds to the following three strands of literature. First, we contribute to the existing literature on peer effects in overall financial decisions. Evidence from field and natural experiments suggests that peers matter for retirement savings decisions (see, e.g., Beshears et al., 2015; Duflo and Saez, 2003), consumption decisions (see, e.g., Agarwal et al., 2021), asset purchases (see, e.g., Bursztyn et al., 2014; Haliassos et al., 2019), insurance take-up (Cai et al., 2015), and charitable giving or public goods provision (see, e.g., Lieber and Skimmyhorn, 2018; Shang and Croson, 2009). While these papers provide valuable insights into the existence and underlying mechanisms of peer effects in various financial decisions, the evidence stemming from experimental settings might not fully reflect the complexity of naturally occurring peer interactions. Very few papers investigate financial peer effects in a non-experimental set up. De Giorgi et al. (2020) provide causal evidence that couples are significantly influenced by peers in their consumption decisions by exploiting partially overlapping coworker networks of couples. Building on the same identification strategy, we use naturally occurring peer groups of siblings and neighbors and use administrative data on the entire Dutch population to provide causal evidence on peer effects in financial investment decisions.

Second, the paper relates directly to the growing literature on peer effects in stock market participation. The majority of existing work finds a positive correlation between individuals' and their peers' financial investment outcomes.[3] Among professionals, correlations among same-stock purchases prevail (Hong et al., 2005). Using Norwegian data, Hvide and Östberg (2015) provide evidence of positive peer effects among coworkers' same-stock purchases. Despite positive spillovers, the authors show that the quality of stock purchases does not improve and, in some cases,

[3]Earlier work indirectly proves the importance of social networks by showing that social households are more likely to participate compared to non-social households (Hong et al., 2004).

even seem to lead to a propagation of financial mistakes. Similarly, correlations between households' and their neighbors' investments are found in the US population (see, e.g., Ivković and Weisbenner, 2007). While all these findings underline the importance of peers in the financial investment decision, they are mainly correlational and not able to pin down causal influences. A notable exception is Brown et al. (2008b), who identify the causal effects of neighbors' stock market participation on individual participation. Conceptually similar to a partially overlapping peer groups strategy as employed in this paper, the authors exploit that some neighbors still reside in their birth community while others moved away to construct instruments for current local peers. They find that a 10 percentage point increase in the average ownership in one's community increases individual participation by 4 percentage points. These findings support our instrument choice, which exploits that siblings' financial decisions are influenced by their immediate neighbors.

Patacchini and Rainone (2017) underline the importance of the peer group definition and the level of trust that comes with it by differentiating between strong and weak ties. They consider smaller-sized peer groups of friends and find only evidence of spillovers in financial activity participation among long-lasting relationships. Most of this literature considers rather large and unspecific peer groups, such as neighbors or coworkers. However, given the unobservable nature of stock purchases, influences among smaller social groups, who interact more frequently and openly about financial investments might provide new insights. We contribute to the literature by providing causal evidence of peer effects in the risky asset market entry decision among a naturally occurring peer group of siblings among whom the trust level might be higher than among neighbors, coworkers, or friends.

Third, we contribute to findings on the stock market participation puzzle – the phenomenon that stock market participation is low despite a substantial risk premium and gains related to diversification. Literature has shown that lack of awareness and informational barriers (see, e.g., Guiso and Jappelli, 2005) as well as lack of trust (see, e.g., Guiso et al., 2008) prevent many individuals from stock market participation. Also, individuals that are less financially literate are significantly less likely to invest in stocks (Van Rooij et al., 2011), and upon participation, they are more likely to invest inefficiently (Calvet et al., 2007). Differentiating between informed (high SES or financially educated) and uninformed/ financially less sophisticated couples, we show that spillovers among siblings prevail mainly among uninformed couples facing entry barriers.

The rest of this paper is structured as follows. Section 3.2 describes the construction of our sample based on Dutch administrative data and presents the empirical strategy. In Section 3.3, we present the main results of our empirical analysis. Section 3.4 discusses potential future pathways and concludes.

3.2 Data and Empirical Strategy

3.2.1 Data

For the empirical analysis, we use Dutch administrative data maintained by Statistics Netherlands (Centraal Bureau voor de Statistiek, CBS) covering the entire Dutch population. The register includes individual and family characteristics (including household structure, education, and occupation) and geographic information, which allow us to construct family and neighborhood networks, as well as detailed information on household wealth for the 2006-2019 period. Based on information from the Dutch tax authorities, which complement data from tax records with information from financial institutions, annual data on household wealth (including information on different types of assets and debts) are available starting in the year 2006. We use this information to construct our main outcome variable: first-time participation in the risky asset market.

Sample Construction The wealth data is available at the household level from 2006 to 2019. Since the wealth data is reported as of January 1 of the respective year and refers to the Dutch population at the end of the preceding year, we assign the wealth information to the previous calendar year, i.e., the wealth data from January 1, 2006, refers to the year 2005. Thus, we have information on households' wealth between 2005 and 2018. However, to account for the potentially disrupting effect of the financial crisis, we only consider the sample period 2009–2018 for our analysis.[4] Our sample then consists of an annual rolling panel of couples who cohabit or are married for at least two years[5], where at least one of the partners has a sibling. We only consider couples where one of the partners is assigned as the head of household in the wealth records. This way, the wealth data can be unambiguously attributed to the couple's financial decision (and no other party within the same household). Furthermore, we limit the analytical sample to couples we first observe while both partners are aged 20-30. We then follow these couples over our entire observation period for as long as the relationship holds. This implies that we also observe these couples when they are older than 30. In our data, the average age of entry is approximately 29 years,[6] which is consistent with patterns documented in the literature (see, e.g., Fagereng et al., 2017, for participation rates over the life-cycle). Since we exclude all couples that already purchased assets before 2009 (so that the decision of first-entry already happened) from our analysis, the age restriction ensures that we capture a couple's first participation in the asset market instead of a re-entry decision.

[4]In robustness checks, we vary this cut-off date to be further away from the financial crisis and show that our findings remain robust.

[5]We exclude same-sex couples in order to consider differential effects for the siblings of the male and female partner.

[6]This number refers to the average age of entry of all individuals in our data that were aged 20-30 in the year 2005.

Outcome Variable Based on information from Dutch tax records, we observe annual household wealth and construct a measure of couples' investment decisions. In particular, the CBS data contains detailed information about households' assets that allow us to distinguish between safe assets (bank and savings accounts) and risky assets (shares, bonds, etc.). The main outcome of interest indicates whether, in a given year, a couple invests in risky assets for the first time, which we also refer to as first-time risky asset market participation (or the entry decision). Using the total value of a household's risky assets, we construct an indicator variable that equals one if a couple reports a positive value in risky assets and zero otherwise. First-differencing then yields the desired outcome that is one when the couple reports a positive value in risky assets for the first time and zero otherwise. As explained, the age restriction that both partners are aged 20-30 years old the first time we observe financial information about them makes it plausible to assume that we capture the first entry decision. Moreover, we consider the value of risky asset investments upon entering the asset market for the first time as an additional outcome variable.

Peer Networks Using the municipal register data (Gemeentelijke Basis Administratie, GBA), we can identify household structures and link the demographic and geographic information of both partners to identify family and neighborhood peers. In particular, we define siblings of both partners based on having the same mother. Importantly, for the construction of sibling outcomes and characteristics, we use all siblings irrespective of age and marital status. However, we only consider siblings that make independent financial decisions, i.e., the sibling herself or their cohabiting/married partner must be assigned as the head of household in the wealth records. The purpose is to exclude, e.g., siblings living with their parents or in shared flats (with e.g. friends, other students, etc.) for whom the household's wealth information cannot be unambiguously attributed to the sibling's decision. For identification purposes, we exclude siblings living in the same neighborhood as the main couple from the construction of sibling variables (see Section 3.2.5 for a discussion).

Siblings This paper aims to explore the influence of siblings' asset market experience on the decision to invest in risky assets for the first time. In particular, using the information on the history of investments, we construct a measure of siblings' positive (negative) change in the value of their asset investments. To that end, we consider all siblings that invested in risky assets in the previous year and construct an indicator variable that equals one if the sibling experienced a positive (negative) change in the total value of their holdings and zero otherwise (i.e., an indicator of positive change conditional on a sibling's asset market participation in the previous year). We refer to this as siblings' positive (negative) experiences in the risky asset market. Since the literature provides evidence for selective communication in which positive stock performance is favored and more likely to be transmitted among investors (Han et al., 2021; Lane et al., 2021), we only consider the impact of siblings' positive changes in the value of risky assets in our main specification. The results remain robust when we additionally account for the negative changes in siblings' asset holdings.

Neighborhoods Neighbors are defined at the neighborhood level (*"buurt"*), the lowest regional level available to us. The Netherlands is divided into twelve provinces, which are further sub-divided into around 350 municipalities (*"gemeenten"*). Municipalities consist of different districts (*"wijk"*), each of which is an aggregation of one or more neighborhoods (*"buurt"*). While this subdivision into neighborhoods may change over time, we use the classification from the year 2019 to define time-invariant neighborhood areas. In our sample, each couple has on average around 1,800 neighbors.

3.2.2 Summary Statistics

Given our sample restrictions, we end up with a sample of $42,793$ couples who did not enter the asset market yet and where at least one of the partners has one or multiple siblings with non-missing information. We use this sample to investigate spillovers on the household level, i.e., we impose a single peer network and pool the information of both partners' siblings to generate an average of all available siblings to the couple. To allow for differential effects of siblings of the male and female partner, we look at $16,442$ couples who did not enter the asset market yet and where at least one partner has a sibling with non-missing information.[7] In Appendix C.1, we show that despite the difference in sample size, the two samples are comparable in terms of composition and characteristics (see Tables C.1 and C.2).

In our empirical analysis, we utilize both of these samples to study the effect of (i) both partners' siblings as a joint peer network (using the full sample of $42,793$ couples) and (ii) separate peer networks of the male and female partner's siblings (using the restricted sample of $16,442$ couples). Table 3.1 reports the summary statistics of the full sample of couples where a joint sibling network is considered.[8]

Of the $42,793$ couples in the full sample, more than 80% are married and have, on average, 2 children living in the household. The average wealth of households, i.e., the balance of assets and liabilities, is $51,911$ EUR. Among the female partners, less than 1% are still studying, roughly 2% are unemployed, and 14% have no income. Defining financial education by employment in a financial sector occupation[9], around

[7] The large reduction in sample size is due to the household approach in the first sample that pools siblings' information. In particular, if one partner has a sibling with missing variables and the other has a sibling with information, we can create a sibling average. When allowing for separate sibling variables (the restricted sample), we can only consider siblings of each partner that have no missings, which reduces the sample size.

[8] Summary statistics for the restricted sample are reported in Appendix Table C.1.

[9] Financial occupations include employment in the following sectors: banks, insurance and health insurance funds, lending companies, and business services. This variable is measured in the last years before the individual enters the rolling panel.

Table 3.1: Summary Statistics - Household Characteristics and Decisions

	mean	sd		mean	sd
Household' characteristics			**Financial decisions**		
Married	0.8148	[0.3657]	Entry asset market	0.0628	[0.2426]
Number of children	2.1204	[0.9147]	Risky asset value	401	[10,236]
Wealth	51,911	[349,846]	log. Value at entry	0.4716	[1.9041]
Bank balance	19,381	[30,429]	**Neighborhood characteristics**		
Female partners' characteristics			Frac. married	0.8761	[0.0643]
Student	0.0075	[0.0459]	Avg. wealth	202,195	[159,880]
Unemployed	0.0210	[0.0608]	Frac. with risky assets	0.2239	[0.0767]
No income	0.1400	[0.2717]	Avg. risky asset value	14,221	[20,957]
Financial education	0.1167	[0.2711]	Frac. with pos. asset change	0.0653	[0.0253]
Age	33.8894	[2.7194]	Frac. with neg. asset change	0.0456	[0.0183]
Male partners' characteristics			Number of neighbors	1,778	[1,413]
Student	0.0013	[0.0168]			
Unemployed	0.0174	[0.0590]			
No income	0.0129	[0.0561]			
Financial education	0.1663	[0.3218]			
Age	35.6407	[2.2969]			
N couples	42,793		N couples	42,793	

Note: The table reports the summary statistics of the full sample, i.e. of couples that did not enter the asset market prior to 2009 and where at least one of the partners has at least one sibling. This sample is used in the empirical analysis to impose a single peer network, i.e. information from all siblings of both partners are pooled into a single peer average. Averages of asset values, wealth, and bank balance include zeros. Appendix Table C.1 displays comparable statistics for the restricted sample.

12% of the female partners are financially educated. The average age of female partners over the analysis period is 34 years.[10] Similarly, of the male partners, approximately 1.7% are unemployed, 1.3% do not have an income, and almost all male partners have completed their education. The male partner is, on average, 2 years older than their female partner and 36 years old over the analysis period. With around 16% being employed in a financial sector, the male partners are, on average, more likely to be financially educated.

In the full sample, 6.3% of couples enter the risky asset market for the first time during our observation period. The average value of risky assets held by households is 401 EUR.[11] With an average wealth of 202, 195 EUR, neighbors of the main couples

[10] Recall, that while we impose the restriction that couples are aged $20 - 30$ when we first observe them, we follow these couples over the years such that our sample also includes them when they are older.

[11] This figure includes households with zero risky asset holdings.

Table 3.2: Summary Statistics - Joint Sibling Network

	mean	sd
Sibling characteristics		
Married	0.7873	[0.3605]
Number of children	1.6384	[0.7462]
Wealth	101,468	[420,492]
Student	0.0327	[0.0710]
Unemployed	0.0218	[0.0415]
No income	0.0700	[0.1111]
Age	35.3875	[4.2476]
Siblings' financial decisions		
Frac. risky assets	0.1805	[0.2939]
Frac. with pos. asset change	0.0625	[0.1123]
Frac. with neg. asset change	0.0341	[0.0642]
Siblings' neighborhood characteristics		
Frac. with risky assets	0.2307	[0.0589]
Frac. with pos. asset change	0.0655	[0.0205]
Frac. with neg. asset change	0.0458	[0.0150]
N couples	42,793	

Note: The table reports the summary statistics of the couple's siblings in the full sample, i.e. siblings of couples that did not enter the asset market prior to 2009 and where at least one of the partners has a sibling. The averages reported in this table are for the joint sibling network, i.e., based on information from siblings of both partners. Averages of asset values and wealth include zeros. Appendix Table C.2 displays comparable statistics, separately for the siblings of the male and female partner, for the restricted sample.

are more wealthy and significantly more likely to invest in risky assets. Around 22% of the neighbors have risky assets. The higher participation and higher wealth levels among the neighbors is not surprising given that the main couples are quite young due to our imposed age restriction.

Table 3.2 displays summary statistics for the main couple's siblings and their neighbors, averaged over the siblings of both the male and female partner in the full sample. More than 78% of siblings are married, are on average 35 years old, and have, on average, 1.6 children. Around 18% of siblings hold risky assets, and around 6% of siblings experience a positive change in their risky asset value.[12]

3.2.3 Empirical Strategy

We model the conditional participation probability of a household using an additively separable linear in means fixed-effects model that relates the entry decision to a set of household characteristics, average peer characteristics, sibling experiences, as well as household and time fixed effects. Using the samples described in the previous section, we consider (i) a joint peer network of the household, i.e., we average over the siblings of both partners, and (ii) separate peer networks for the male and female partners' siblings, i.e., we allow peer effects to differ between the siblings of the male and female partner in a couple. Our modeling approach yields the following estimation equation in first-differences:

$$\Delta y_{it} = \alpha_1 \Delta X_{it} + \beta_1 \Delta \bar{y}^S_{-it} + \beta_2 \Delta \bar{X}^S_{-it} + \gamma_1 \Delta \bar{y}^N_{-it} + \gamma_2 \Delta \bar{X}^N_{-it} + \delta_t + \Delta u_{it}, \tag{3.1}$$

where the outcome variable Δy_{it} indicates whether household i entered the asset market in period t (i.e., i having a positive value of risky asset holdings for the first time), $\Delta \bar{y}^S_{-it}$ denotes the fraction of siblings that experienced a positive change in the value of their risky assets from period $t-1$ to t (i.e., the endogenous peer effect) and $\Delta \bar{y}^N_{-it}$ measures the fraction of couple i's neighborhood-peers that experienced a positive change in the value of their assets from period $t-1$ to t (excluding household i).[13] The average household characteristics, X_{it}, include a couple's marriage/cohabitation status, the number of children in the household, household

[12]Summary statistics for the siblings of couples in the restricted sample are reported in Appendix Table C.2 and include sample characteristics for the male and female partner's siblings separately.

[13]This neighborhood term is referred to as the "individual IVs".

wealth, as well as age (in categories) and the couple's labor force participation[14], separately for the male and female partner. Similarly, $\bar{X}^S_{-it}$ (i.e., the exogenous peer effect) and $\bar{X}^N_{-it}$ refer to average sibling and neighborhood characteristics, respectively.[15] Moreover, δ_t denotes year fixed-effects to control for different changes in the entry probability over time and u_{it} is an error term that can contain unobserved time-varying heterogeneity.

The first-differenced equation (3.1) corresponds to a level-equation that relates the participation probability of a household to the average number of times the siblings experienced a positive change in the value of their asset holdings (excluding their entry decision), household, sibling and neighbor characteristics, as well as household-level and time fixed-effects.

As mentioned above, we estimate two different specifications of the model using (i) the sample that considers the joint peer groups of both partners, as well as (ii) separate peer groups (and effects) for the male and female partners' siblings. In the first specification, the relevant sibling variables are defined as $\Delta\bar{y}^S_{-it} = \frac{1}{N^S_{it}} \sum_{j \in S_{it}} \Delta y^S_{jt}$ and $\Delta\bar{X}^S_{-it} = \Delta\left(\frac{1}{N^S_{it}} \sum_{j \in S_{it}} X^S_{jt}\right)$, where the set S_{it} contains the indices of all relevant siblings of couple i in period t and N^S_{it} denotes the cardinality of this set. Analogously, in the second specification, the sibling variables are defined separately for the male and the female partner.

The main parameters of interest are the β's in Equation (3.1), measuring a direct effect of siblings' positive experiences, i.e., a positive change in the value of risky assets (endogenous peer effect), on a couple's decision to invest in risky assets for

[14]Being employed is the left-out reference category and we include categories for being a student, being unemployed, and having no income.

[15]The sibling controls, $\bar{X}^S_{-it}$, contain averages of the same variables as used for the household, with the only difference that the average age of the siblings is used instead of categories. We also include dummies for the male and female partners having a sibling, as well as their interaction. The neighborhood controls, $\bar{X}^N_{-it}$, include average wealth in the neighborhood, the fraction of neighbors with positive wealth, the (lagged) fraction of neighbors with assets and a very high value of assets, the (lagged) fraction of neighbors with a mortgage, and the marriage rate in the neighborhood.

the first time. We mainly consider siblings' positive change in the value of their risky assets as the relevant measure. So we are interested in whether the fraction of siblings who had a positive change in their asset holdings influences a couple's decision to purchase risky assets for the first time. In an additional specification we extend the siblings' measures by allowing for an impact of their negative experiences.

Estimating causal peer effects using Equation (3.1) is not directly possible due to potential reflection and endogeneity issues. We circumvent these, following the literature, exploiting partially overlapping peer groups (see, e.g., Bramoullé et al., 2009; De Giorgi et al., 2020, 2010; and see, e.g., Nicoletti et al., 2018 for a recent application). This is an instrumental variable strategy that exploits multiple peer networks of an individual. In particular, assuming a couple is in contact with their siblings and neighbors but not with their siblings' neighbors, we instrument the fraction of siblings with a positive change in asset value ($\Delta\bar{y}^S_{-it}$) with the average asset experience of the siblings' neighbors in the past. Employing first-differences allows us to control for time-constant unobservables on the individual, family and neighborhood level.

In the first stage, we find, consistent with the literature (see, e.g., Brown et al., 2008b), that siblings' asset market experience is influenced by their geographical peers' decisions. The fist-stage is given by

$$\Delta\bar{y}^S_{-it} = \alpha_1\Delta X_{it} + \beta_2\Delta\bar{X}^S_{-it} + \gamma_1\Delta\bar{y}^{N^S}_{-it-1} + \gamma_2\Delta\bar{X}^N_{-it} + \delta_t + \Delta u_{it} \tag{3.2}$$

where $\Delta\bar{y}^S_{-it}$ measures the fraction of siblings that experienced a positive change in the value of their assets from $t-1$ to t; the instrument $\Delta\bar{y}^{N^S}_{-it-1}$ is lagged by one year and measures the fraction of siblings' neighborhood-peers that experienced a positive change in the value of their assets from $t-2$ to $t-1$ (excluding household i's siblings); X_{it}, $\bar{X}^S_{-it}$, $\bar{X}^N_{-it}$, δ_t, and u_{it} are defined as in Equation (3.1). All standard errors are clustered at the household level.

3.2.4 Assumptions and Identification

It is well established in the literature that identifying peer effects is challenging due to selection and reflection issues. There is a need to isolate the direct influence of peers' outcomes on the individual. As Manski (1993) explains, it is hard to distinguish between the three potential ways peers can influence each other. In our context, correlations in siblings' financial investment decisions could be due to a direct influence (endogenous effect), i.e., a couple i purchases risky assets because their sibling experiences a change in the value of their risky assets. Second, there could be an influence via sibling characteristics (exogenous effect), e.g., having a sibling with a financial education could lead a couple i to purchase assets. Third, there could be unobserved shocks affecting both the couple and their siblings simultaneously (correlated effects). We are interested in the endogenous peer effect, i.e., whether the decision of a sibling influences a couple directly. To overcome the reflection problem between the main household and their siblings, we use an IV strategy exploiting partially overlapping peer groups.

This approach is a common way of solving the endogeneity problem in the context of peer effects as shown, among others, by Bramoullé et al. (2009), De Giorgi et al. (2010), and Blume et al. (2015). Identification is reached using a network structure with intransitive traits, i.e., exploiting peers-of-peers. Under the assumption that each couple interacts with its siblings and neighbors, but not with the neighbors of its siblings, we use financial decisions and characteristics of siblings' neighbors as instruments for siblings' financial experiences. The instrument exploits that an individual is more likely to purchase (risky) assets with more neighbors experiencing positive returns (see Kaustia and Knüpfer, 2012, who show that high peer returns (on neighborhood or zip-code level) are associated with an increased likelihood of stock market entry).[16] For each regression, we report underidentification and weak identification statistics (see Kleibergen and Paap, 2006, for details) testing the relevance of instruments (via the matrix rank) and – given relevance – testing for weak instruments, respectively.

3.2.5 Threats

There are four potential threats to the identification strategy used. First, if siblings have similar residential preferences leading them to sort into similar neighborhoods, observed spillovers could be due to selection and thus lead to an overestimation bias. To solve this potential selection problem, we control for characteristics of the main couple's neighborhood ($\Delta\bar{X}^N_{-it}$ and $\Delta\bar{y}^N_{-it}$ in Equation (3.1)) so that the estimated effects are net of similarities in residential areas. In particular, as Nicoletti et al. (2018), we control for "individual IVs", i.e., the analog of the instrument used for the siblings' financial decisions is included for the own neighborhood, which in our case is the fraction of neighbors experiencing a positive change in their risky asset holdings. In Table C.5, we show that without controls for the couples' neighborhood, spillovers would indeed be overestimated, in particular spillovers of the female partner's siblings.

Second, if couples know the neighbors of their siblings and are in contact with them, the exclusion restriction might fail. This could occur if siblings live in the same neighborhood and interact with the same people. We exclude this possibility in our sample by excluding siblings living in the same neighborhood as the main couple from the construction of the sibling averages.

[16]For further neighborhood effects on financial decisions, see, e.g., Brown et al. (2008b), who show that stock market participation increases with the participation in the local community.

Third, there could be potential feedback or reversed causality effects leading siblings to influence their neighbors, leading to a correlation of the error term of the main equation with the instruments. Since there is no natural timing of asset purchase, the only possibility of exploiting timing is to consider past purchases. Our main peer measure of interest is whether siblings had a positive experience in the asset market. This measure requires that siblings already participate in the stock market, which avoids the reversed causality between the main couple and her siblings. To ensure no feedback between siblings and neighbors, we use lagged measures of the siblings' neighbors as instruments. Also, in our context, we are not worried about feedback effects in the first stage because of the peer group size (on average, $1,800$ neighbors), which implies that the influence of one sibling on the average neighborhood participation is expected to be rather small (see, e.g., Bramoullé et al., 2009, who explain how in a setup with varying group sizes the role of one peer on the average peer outcome diminishes with group size).

Fourth, another reason that the exclusion restriction could be violated are correlated shocks affecting both siblings and some of their neighbors. For example, if a big firm employing a large group of individuals, introduces a change in some regulation, or experiences some investment profits/losses and siblings live in the same district or municipality in which this firm is largely present, then firm-level changes would confound results (via affecting instruments and couples simultaneously). We investigate such concerns by including different levels of regional fixed effects and show that our findings are robust to aggregate level shocks on the respective level (see Section 3.3.3 for a detailed discussion).

3.3 Results

In this section, we present evidence on peer effects in financial investment decisions, specifically how the participation decision of households is affected by their siblings' positive experiences in the asset market. Estimating the linear-in-means approximation from Equation (3.1), we first analyze average sibling peer effects (Section 3.3.1) before exploring heterogeneities to shed light on potential mechanisms through which peer effects in financial investments may operate (Section 3.3.2).

3.3.1 Main Results

In Table 3.3, we report results of sibling peer effects on couples' financial investment decisions estimated via two-stage least squares (2SLS). In particular, we explore the effect of siblings' positive experiences in the risky asset market on couples' decisions to enter the asset market for the first time. For all regressions, we report under-identification and weak instrument statistics. The F-statistics are generally above conventional thresholds, confirming the relevance of our instruments used in the first stage.

We begin by imposing a single peer network for each household in columns [1] and [2]. In this case, $\Delta\bar{y}^S_{-it}$ and $\Delta\bar{X}^S_{-it}$ in Equation (3.1) pool information of both partners' siblings in a single measure and we estimate Equations (3.1) and (3.2) on the full sample. Our findings support the existence of sibling spillovers in financial investments. In column [1], the reported 2SLS estimates are positive and significant for siblings' positive experiences in the asset market. In particular, a one-standard-deviation increase in the fraction of siblings with a positive experience (equivalent to an 11 percentage point increase) leads to an increase in the likelihood of a couple's first-time participation by 0.25 percentage points. This corresponds to an increase of 4% relative to the mean of ever entering the risky asset market. However, the

Table 3.3: Entry Decision - Sibling Spillover

	[1]	[2]	[3]	[4]
All Siblings				
positive experience	0.023*	0.011		
	[0.012]	[0.028]		
negative experience		0.018		
		[0.036]		
Sibling of female partner				
positive experience			0.030**	0.060**
			[0.015]	[0.025]
negative experience				-0.043
				[0.029]
Sibling of male partner				
positive experience			0.010	-0.013
			[0.015]	[0.027]
negative experience				0.032
				[0.031]
Underid	578.13	156.24	148.40	70.99
p-value	0.0000	0.0000	0.0000	0.0000
weakid	614.43	76.30	77.69	17.04
p-dif. pos.			0.3940	0.0770
N observations	300,983	300,977	104,210	104,194
N couples	42,792	42,792	16,322	16,322

Note: Each column of this table reports a separate 2SLS regression. Columns 1–2 are based on the full sample and Columns 3–4 are based on the restricted sample. For each regression the dependent variable is first-time risky asset market participation. Individual controls include marriage and cohabitation status, number of children in the household, household wealth, age (in categories, separately for male and female partner), and labor market status (separately for male and female partner). Contextual controls for siblings are analogues to individual controls. Contextual controls for neighbors include fraction with positive (negative) change in the value of their risky assets, wealth, marriage status, fraction with positive wealth, fraction with risky assets (lagged), fraction with very high asset values, and the fraction of mortgage holders (lagged). We instrument each sibling measure reported by the fraction of neighbors that experienced a positive (negative) change in the value of their risky assets. In each regression, we control for year fixed effects. Standard errors are clustered on household level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

peer effects of siblings' positive experiences in the asset market becomes insignificant in column [2], when we additionally include the fraction of siblings with a negative change in their asset value.

To disentangle the effects stemming from different parts of the sibling network, we allow for the effects of the male partner's siblings to differ from the effects of the female partner's siblings by entering them as separate regressors. For this analysis we run the regression on the reduced sample as described in Section 3.2.2. The results are reported in columns [3] and [4] of Table 3.3. Our findings suggest that the sibling peer effects are primarily driven by the siblings of the female partner. An increase in the fraction of the female partner's siblings with a positive change in their asset value by one standard deviation, or 16 percentage points, leads to a 0.5 percentage points higher likelihood of a couple entering the asset market in a given year. This corresponds to an increase of more than 8% relative to the mean of ever entering the risky asset market. The effect of the male partner's siblings is substantially smaller and statistically insignificant. In the case of separate peer networks, the positive effect of the female partner's siblings persists and gets stronger if we additionally account for the impact of siblings' negative changes in their asset value.

In addition to the first-time entry decision, we explore whether siblings' experiences in the asset market influence a couple's investment level upon entry. Table 3.4 displays the 2SLS estimates using the log value of risky assets upon entry into the asset market as an outcome variable. Overall, the pattern is comparable to the previous results on first-time entry. When considering the siblings of both partners in a single peer network, we find positive peer effects of siblings' positive experiences in the asset market. This effect again becomes insignificant when additionally accounting for siblings' negative changes in asset value. However, when separating a couple's peer network into the siblings of the male and female partner, we find that the observed sibling peer effects are mainly driven by the female partner's sibling and remain robust to the inclusion of siblings' negative experiences in the asset market. In our preferred specification (column [3]), an increase in the fraction of siblings with positive experiences in the asset market by one standard deviation, or 16 percentage points, leads to an increase in the log value of initial investments of 0.045 log points, or equivalently an increase of 10% at the mean.

Table 3.4: Entry Decision (Log Value of Risky Assets) - Sibling Spillover

	[1]	[2]	[3]	[4]
All Siblings				
positive experience	0.240**	0.074		
	[0.094]	[0.223]		
negative experience		0.246		
		[0.296]		
Sibling of female partner				
positive experience			0.283**	0.540***
			[0.119]	[0.204]
negative experience				-0.376
				[0.242]
Sibling of male partner				
positive experience			0.085	-0.126
			[0.116]	[0.217]
negative experience				0.298
				[0.252]
Underid	578.12	156.22	148.40	70.99
p-value	0.0000	0.0000	0.0000	0.0000
weakid	614.42	76.29	77.69	17.04
p-dif. pos.			0.2996	0.0432
N observations	300,980	300,974	104,210	104,194
N couples	42,792	42,792	16,322	16,322

Note: Each column of this table reports a separate 2SLS regression. Columns 1–2 are based on the full sample and Columns 3–4 are based on the restricted sample. For each regression the dependent variable is the log value of risky assets upon first-time investment. Individual controls include marriage and cohabitation status, number of children in the household, household wealth, age (in categories, separately for male and female partner), and labor market status (separately for male and female partner). Contextual controls for siblings are analogues to individual controls. Contextual controls for neighbors include fraction with positive (negative) change in the value of their risky assets, wealth, marriage status, fraction with positive wealth, fraction with risky assets (lagged), fraction with very high asset values, and the fraction of mortgage holders (lagged). We instrument each sibling measure reported by the fraction of neighbors that experienced a positive (negative) change in the value of their risky assets. In each regression, we control for year fixed effects. Standard errors are clustered on household level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Overall, our evidence confirms the existence of sibling peer effects on financial investment decisions. Siblings' positive experiences in the risky asset market increase both the likelihood of first-time participation and the amount invested upon first entry. The observed peer effects seem to be mainly driven by the female partner's siblings. In the remainder of this paper, we investigate the underlying mechanisms behind sibling spillovers in financial decisions. In particular, we are interested in which couples are influenced by their siblings' financial experiences to get an idea about the potential mechanisms behind these peer effects.

3.3.2 Heterogeneities

What are potential explanations for the observed sibling spillovers in financial investment decisions? The literature has primarily focused on two potential explanations: (i) social learning, i.e., peer effects arise due to informational spillovers from sophisticated to unsophisticated peers, and (ii) social utility (see, e.g., Bursztyn et al., 2014). The latter channel encompasses both preferences for possessing similar assets as one's peers as well as a "keeping up with the Joneses" motive. While we cannot directly determine the motivations of couples entering the asset market in response to their siblings' experiences, we utilize the rich socio-demographic information in the administrative data to infer likely mechanisms. In particular, we explore possible heterogeneities in sibling peer effects to determine which couples are influenced by their siblings' financial experiences.

First, we investigate how couples are influenced by their siblings' positive experiences depending on their socioeconomic background, defined based on their parents' percentile groups in the national wealth distribution in 2009, i.e., at the beginning of our observation period. In particular, we define a couple to have a "high SES" background if either the male or female partner's parents are in the top 25-th per-

Table 3.5: Heterogeneous Results - by Couple Characteristics

	[1]	[2]	[3]	[4]
	High SES		Financial education	
	no	yes	no	yes
Sibling of female partner				
positive experience	0.050**	0.014	0.044**	0.009
	[0.025]	[0.020]	[0.018]	[0.029]
Sibling of male partner				
positive experience	-0.005	0.009	-0.007	0.028
	[0.023]	[0.022]	[0.020]	[0.022]
Underid	41.73	93.56	72.43	57.04
p-value	0.0000	0.0000	0.0000	0.0000
weakid	21.73	48.18	36.98	30.82
N observations	39,435	64,775	77,674	26,536
N couples	6,565	9,757	11,974	4,348

Note: Each column of this table reports a separate 2SLS regression based on the restricted sample. Columns 1–2 compare sibling influence for couples with low and high SES, respectively. Columns 3–4 compare sibling influences by couples' financial education. High SES is defined by at least one partner having parents in the top 25% of the national wealth distribution. Financial education is defined by at least one partner being employed in a financial occupation sector. For each regression the dependent variable is first-time risky asset market participation. Individual controls include marriage and cohabitation status, number of children in the household, household wealth, age (in categories, separately for male and female partner), and labor market status (separately for male and female partner). Contextual controls for siblings are analogues to individual controls. Contextual controls for neighbors include fraction with positive (negative) change in the value of their risky assets, wealth, marriage status, fraction with positive wealth, fraction with risky assets (lagged), fraction with very high asset values, and the fraction of mortgage holders (lagged). We instrument each sibling measure reported by the fraction of neighbors that experienced a positive change in the value of their risky assets. In each regression, we control for year fixed effects. Standard errors are clustered on household level. *** p<0.01, ** p<0.05, * p<0.1.

centile of the national wealth distribution in the calendar year 2009[17]. The idea is that a couples' "high SES" classification captures their own wealth level (i.e., the ability to invest in risky assets), but also their financial literacy as the latter has been documented to depend on socioeconomic background (see, e.g., Lusardi and Mitchell, 2014). Second, we allow sibling spillovers to differ by the couples' financial education as a more direct measure of financial literacy. We define financial educa-

[17] Using the national wealth distribution in the year 2006 yields similar results.

tion as at least one partner being employed in a financial sector occupation before entering the asset market for the first time.[18]

We estimate the 2SLS specification of column [3] in Table 3.3 separately by the couples' characteristics, i.e., we consider the male and female partners' siblings in separate peer networks and focus on the impact of positive sibling experiences. Columns [1] and [2] of Table 3.5 show that sibling spillovers are driven by couples with low SES. While we find significantly positive spillover effects (of the female partner's siblings) for couples without a high SES family background, the coefficient is substantially smaller and insignificant for high SES couples. This suggests that the sibling spillovers are related to financial literacy and this is further substantiated by our results on financial education (in columns [3] and [4]). In particular, we find that sibling spillovers are only relevant for couples where none of the partners is classified as financially educated.

These results are consistent with a social learning mechanism. Couples from a high-SES background and couples where at least one partner is financially educated are likely to have sufficient information and competencies to make their own financial investment decisions. Thus we would not expect couples from a high-SES background or financially educated couples to respond to such informational spillovers and this is indeed what we find. Instead, sibling spillovers in financial decisions appear to result from the transmission of information and knowledge from informed peers, more specifically peers that participate in the asset market, to financially uninformed couples.

[18]Financial occupations include employment in the following sectors: banks, insurance and health insurance funds, lending companies, and business services. This variable is predetermined and measured in the last year before each individual enters the rolling sample.

3.3.3 Robustness

Financial Crisis When defining the analytical sample, we specifically exclude the period of the financial crisis of 2007-2008, restricting our sample period to the years 2009-2018. In a robustness exercise, we test whether the specified cut-off date for the financial crisis affects our main results. Table C.3 reports the estimated effects of siblings' positive experiences in the asset market for different sample periods, starting as early as 2008 or as late as 2011. While the sample size and coefficient estimates vary slightly, the results remain robust to the different cut-off dates for the financial crisis.

Regional Fixed Effects In Section 3.2.5 we explain that the exclusion restriction is violated if there are correlated shocks affecting both siblings and their neighbors at the same time. To mitigate such concerns we test whether correlated shocks on different regional levels confound our results by estimating our main specification including municipality, district, or neighborhood fixed effects, respectively. Table C.4 shows only minor changes in coefficient sizes and a slight increase compared to the main findings. This suggests that, if anything, our main findings slightly underestimate spillovers among siblings' financial decisions.

Neighborhood Controls While we chose a conservative specification as our preferred one, we also test whether it is necessary to control for all possible neighborhood characteristics of the main couple. In Table C.5, we report our main results in columns [1]. In columns [2], we control for neighborhood characteristics but leave out the "individual IVs", in columns [3], we do not control for anything on the neighborhood level, and in columns [4], we only include "individual IVs". Hereby "individual IVs" are the fraction of neighbors experiencing a positive change in their risky asset holdings, i.e., the analog of the instruments we use for the siblings. Comparing across specifications, we find that average neighborhood characteristics have no sizable impact on the main findings. "Individual IVs", on the other hand, seem crucial; excluding them increases the coefficient size. As previously described in Section 3.2.5, excluding these controls will lead to an overestimation of any spillover due to siblings' selection into similar neighborhoods, which we successfully control for in our main specification.

Additional Sample Restriction Among the restricted sample, only very few couples have sibling networks solely on one side, i.e., only the female or male partner has at least one sibling. To ensure that results are not driven by extreme outcomes of siblings from only one partner, we perform the main analysis on a sample of couples, where each partner has at least one sibling with non-missing information. First, we show that the sample is comparable to the full and restricted sample in terms of characteristics and average financial decisions (see Table C.6). Second, we show that our main results stay valid and even get slightly stronger in terms of the effect size (see Table C.7).

3.4 Discussion and Conclusion

In this paper, we provide causal evidence on sibling peer effects in households' financial investment decisions. We document the importance of sibling spillovers for a couple's asset market participation decision: Increasing the fraction of siblings with a positive change in the value of their risky assets raises a couple's likelihood to enter the asset market significantly. Accounting for separate peer effects of the partners' siblings, we find that this effect seems to be entirely driven by the siblings of the female partner.

The rich socio-demographic information in the Dutch administrative data does not only allow us to identify family peer effects, but also enables us to examine the relevance of potential mechanisms through which peer effects in financial investment decisions may operate. The results of our heterogeneity analyses are consistent with a "social learning" mechanism. Distinguishing between couples that are likely to be well-informed and financially literate (i.e., couples with a high-SES background or financial education) and couples that are less financially literate, we find that sibling spillovers prevail among the uninformed couples; a result consistent with informational spillovers as a mechanism behind the observed peer effects.

Our results can be extended in different directions. First, we intend to explore peer effects in different networks. While we currently focus on peer networks defined by family relationships, specifically siblings, employment links could be used to identify the network of co-workers. This would allow us to investigate the role of different networks in couples' financial decisions. Regarding the striking gender patterns in the context of sibling spillovers, it would, for example, be interesting to study whether men and women rely on different networks to make financial decisions.

Second, we would like to further investigate potential mechanisms through which peer effects in financial investments operate. To this end, we want to extend our heterogeneity analysis to not only study which couples are influenced by their siblings' experiences in the asset market, but also which siblings influence their decisions. Our findings suggest that social learning is an important mechanism in this context. Peer effects may therefore be welfare improving if uninformed individuals can benefit from the financial knowledge of their peers. However, peer effects may also propagate financial mistakes if individuals rely on unsophisticated investors.

Appendix C

C.1 Appendix: Additional Summary Statistics

Table C.1: Summary Statistics - Household Characteristics and Decisions

	mean	sd		mean	sd
Household' characteristics			**Financial decisions**		
Married	0.8504	[0.3381]	Entry asset market	0.0578	[0.2334]
Number of children	2.2942	[1.0119]	Risky asset value	355	[5,036]
Wealth	65,114	[456,396]	log. value at entry	0.4372	[1.8459]
Bank balance	19,480	[32,551]	**Neighborhood characteristics**		
Female partners' characteristics			Frac. married	0.8839	[0.0647]
Student	0.0058	[0.0389]	Avg. wealth	210,464	[170,860]
Unemployed	0.0185	[0.0578]	Frac. with risky assets	0.2221	[0.0743]
No income	0.1673	[0.2961]	Avg. risky asset value	14,057	[20,897]
Financial education	0.1060	[0.2604]	Frac. with pos. asset change	0.0652	[0.0247]
Age	34.2483	[2.6244]	Frac. with neg. asset change	0.0458	[0.0178]
Male partners' characteristics			Number of neighbors	1,758	[1,414]
Student	0.0012	[0.0172]			
Unemployed	0.0157	[0.0557]			
No income	0.0121	[0.0529]			
Financial education	0.1524	[0.3103]			
Age	35.8930	[2.2092]			
N couples	16,442		N couples	16,442	

Note: The table reports the summary statistics of the restricted sample, i.e. of couples that did not enter the asset market prior to 2009 and where we split the couple's peer group into the siblings of the male and female partner, respectively.

Table C.2: Summary Statistics - Siblings' Characteristics and Decisions

Female partner	mean	sd	**Male partner**	mean	sd
Siblings' characteristics			**Siblings' characteristics**		
Frac. married	0.7652	[0.4019]	Frac. married	0.7831	[0.3899]
Avg. number of children	1.8374	[1.0138]	Avg. number. of children	1.9372	[1.0532]
Avg. wealth	101,580	[479,757]	Avg. wealth	112,003	[511,308]
Frac. student	0.0222	[0.0632]	Frac. student	0.0164	[0.0516]
Frac. unemployed	0.0191	[0.0494]	Frac. unemployed	0.0181	[0.0489]
Frac. no income	0.0797	[0.1546]	Frac. no income	0.0791	[0.1611]
Avg. age	34.2610	[9.4623]	Avg. age	35.3984	[9.7848]
Siblings' financial decisions			**Siblings' financial decisions**		
Frac. with risky assets	0.1629	[0.2997]	Frac. with risky assets	0.1682	[0.3036]
Frac. with positive change	0.0757	[0.1600]	Frac. with positive change	0.0795	[0.1632]
Frac. with negative change	0.0417	[0.0898]	Frac. with negative change	0.0447	[0.0939]
Siblings' neighborhood characteristics			**Siblings' neighborhood characteristics**		
Frac. with risky assets	0.2151	[0.0831]	Frac. with risky assets	0.2159	[0.0843]
Frac. with pos. asset change	0.0616	[0.0266]	Frac. with pos. asset change	0.0621	[0.0270]
Frac. with neg. asset change	0.0433	[0.0191]	Frac. with neg. asset change	0.0436	[0.0194]
N couples	16,442		N couples	16,442	

Note: The table reports the summary statistics of the couple's siblings in the restricted sample, i.e. siblings of couples that did not enter the asset market prior to 2009 and where we split the couple's peer group into the siblings of the male and female partner, respectively. The averages are reported separately for the siblings of the male and female partners.

C.2 Appendix: Robustness Checks

Table C.3: Entry Decision - Robustness to Financial Crisis

Sample starting date	2008	2009 (baseline)	2010	2011
	[1]	[2]	[3]	[4]
Frac. with positive change in risky assets				
Sibling of female partner	0.028**	0.030**	0.024**	0.038**
	[0.014]	[0.015]	[0.011]	[0.015]
Sibling of male partner	0.008	0.010	0.010	0.010
	[0.014]	[0.015]	[0.011]	[0.015]
Underid	157.62	148.40	144.37	100.84
p-value	0.0000	0.0000	0.0000	0.0000
weakid	82.83	77.69	75.53	52.11
p-dif. of coefs	0.3824	0.3940	0.4274	0.2552
N observations	106,920	104,210	102,897	87,308
N couples	16,728	16,322	16,108	14,668

Note: Each column of this table reports a separate 2SLS regression based on the restricted sample. Column 2 is our baseline specification presented in Table 3.3. In the remaining columns, we vary the starting date of the sample period, as indicated in the header, to demonstrate that our results are not disrupted by the period of the financial crisis. For each regression the dependent variable is first-time risky asset market participation. Individual controls include marriage and cohabitation status, number of children in the household, household wealth, age (in categories, separately for male and female partner), and labor market status (separately for male and female partner). Contextual controls for siblings are analogues to individual controls. Contextual controls for neighbors include fraction with positive (negative) change in the value of their risky assets, wealth, marriage status, fraction with positive wealth, fraction with risky assets (lagged), fraction with very high asset values, and the fraction of mortgage holders (lagged). We instrument each sibling measure reported by the fraction of neighbors that experienced a positive change in the value of their risky assets. In each regression, we control for year fixed effects. Standard errors are clustered on household level. *** p<0.01, ** p<0.05, * p<0.1.

Table C.4: Entry Decision - Robustness to Regional Fixed Effects

	Baseline [1]	Municipality FE [2]	District FE [3]	Neighborhood FE [4]
Frac. with positive change in risky assets				
Sibling of female	0.030**	0.037*	0.045***	0.037**
	[0.015]	[0.020]	[0.017]	[0.015]
Sibling of male	0.010	0.009	0.011	0.018
	[0.015]	[0.021]	[0.017]	[0.015]
Underid	148.40	133.76	154.88	159.96
p-value	0.0000	0.0000	0.0000	0.0000
weakid	77.69	65.81	79.08	83.60
N observations	104,210	103,754	104,129	104,205
N couples	16,322	16,092	16,282	16,320

Note: Each column of this table reports a separate 2SLS regression based on the restricted sample. In column [1], we replicate the estimates from column [3] of Table 3.3. In columns [2] – [4], we include additional fixed effects at different levels of aggregation: municipality fixed effects in column [2], district fixed effects in column [3] and neighborhood fixed effects in column [4]. For each regression the dependent variable is first-time risky asset market participation. Individual controls include marriage and cohabitation status, number of children in the household, household wealth, age (in categories, separately for male and female partner), and labor market status (separately for male and female partner). Contextual controls for siblings are analogues to individual controls. Contextual controls for neighbors include fraction with positive (negative) change in the value of their risky assets, wealth, marriage status, fraction with positive wealth, fraction with risky assets (lagged), fraction with very high asset values, and the fraction of mortgage holders (lagged). We instrument each sibling measure reported by the fraction of neighbors that experienced a positive change in the value of their risky assets. In each regression, we control for year fixed effects. Standard errors are clustered on household level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table C.5: Entry Decision - Robustness to Neighborhood Controls

		Estimation without neighbors':		
	Baseline	Ind. IVs	Controls & ind. IVs	Controls
	[1]	[2]	[3]	[4]
Frac. with positive change in risky assets				
Sibling of female	0.030**	0.037**	0.037**	0.030**
	[0.015]	[0.015]	[0.014]	[0.015]
Sibling of male	0.010	0.017	0.017	0.009
	[0.015]	[0.014]	[0.014]	[0.015]
Underid	148.40	148.07	147.93	148.34
p-value	0.0000	0.0000	0.0000	0.0000
weakid	77.69	77.54	77.47	77.67
N observations	104,210	104,274	104,333	104,227
N couples	16,322	16,326	16,331	16,325

Note: Each column of this table reports a separate 2SLS regression based on the restricted sample. In column [1], we replicate the estimates from column [3] of Table 3.3. In the baseline, we control for both the "individual IVs" and all neighborhood characteristics as outlined in Section 3.2.3. In columns [2] – [4], we vary the controls we include for the couple's own neighborhood. In column [2], we drop the "individual IVs" as a control. In column [3], we drop all controls for the couple's neighbors. In column [4], we only control for the "individual IVs", but do not control for any other neighborhood characteristics. For each regression the dependent variable is first-time risky asset market participation. Individual controls include marriage and cohabitation status, number of children in the household, household wealth, age (in categories, separately for male and female partner), and labor market status (separately for male and female partner). Contextual controls for siblings are analogues to individual controls. Contextual controls in for neighbors, in case included, are the fraction with positive (negative) change in the value of their risky assets, wealth, marriage status, fraction with positive wealth, fraction with risky assets (lagged), fraction with very high asset values, and the fraction of mortgage holders (lagged). We instrument each sibling measure reported by the fraction of neighbors that experienced a positive change in the value of their risky assets. In each regression, we control for year fixed effects. Standard errors are clustered on household level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table C.6: Summary Statistics - Both Partners Have Siblings

	mean	sd		mean	sd
Household' characteristics			**Financial decisions**		
Married	0.8581	[0.3303]	Entry asset market	0.0587	[0.2351]
Number of children	2.3290	[1.0316]	Risky asset value	359	[5,027]
Wealth	66,227	[473,712]	log. value at entry	0.4455	[1.8645]
Bank balance	19,531	[33,374]	**Neighborhood characteristics**		
Female partners' characteristics			Frac. married	0.8836	[0.0659]
Student	0.0061	[0.0398]	Avg. wealth	212,241	[167,981]
Unemployed	0.0185	[0.0577]	Frac. with risky assets	0.2235	[0.0750]
No income	0.1731	[0.2994]	Avg. risky asset value	14,299	[21,789]
Financial education	0.1070	[0.2610]	Frac. with pos. asset change	0.0654	[0.0249]
Age	34.2488	[2.6218]	Frac. with neg. asset change	0.0458	[0.0179]
Male partners' characteristics			Number of neighbors	1,690	[1,359]
Student	0.0013	[0.0179]			
Unemployed	0.0156	[0.0549]			
No income	0.0122	[0.0541]			
Financial education	0.1555	[0.3129]			
Age	35.8917	[2.2045]			
N couples	14,685		N couples	14,685	

Note: The table reports the summary statistics of the sample with couples that did not enter the asset market prior to 2009 and where both partners have at least one sibling with full information.

Table C.7: Entry Decision - Robustness to Further Sample Restrictions

	Baseline [1]	Both partners have a sibling [2]
Frac. with positive change in risky assets		
Sibling of female	0.030**	0.037**
	[0.015]	[0.016]
Sibling of male	0.010	0.003
	[0.015]	[0.015]
Underid	148.40	120.08
p-value	0.0000	0.0000
weakid	77.69	63.18
N observations	104,210	92,517
N couples	16,322	14,578

Note: Each column of this table reports a separate 2SLS regression. Column 1 is based on the restricted sample and replicates the estimates from column [3] of Table 3.3. In Column 2 we restrict the sample to couples where both partners have at least one sibling with full information. For each regression the dependent variable is the first-time risky asset market participation. Individual controls include marriage and cohabitation status, number of children in the household, household wealth, age (in categories, separately for male and female partner), and labor market status (separately for male and female partner). Contextual controls for siblings are analogues to individual controls. Contextual controls for neighbors include fraction with positive (negative) change in the value of their risky assets, wealth, marriage status, fraction with positive wealth, fraction with risky assets (lagged), fraction with very high asset values, and the fraction of mortgage holders (lagged). We instrument each sibling measure reported by the fraction of neighbors that experienced a positive change in the value of their risky assets. In each regression, we control for year fixed effects. Standard errors are clustered on household level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

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Curriculum Vitae

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Eidesstattliche Erklärung

Ich versichere hiermit, dass ich die vorliegende Dissertation selbstständig und ohne Benutzung anderer als der angegebenen Quellen und Hilfsmittel angefertigt und die den benutzten Quellen wörtlich oder inhaltlich entnommenen Stellen als solche kenntlich gemacht habe. Diese Arbeit hat in gleicher oder ähnlicher Form noch keiner Prüfungsbehörde vorgelegen.

Mannheim, 15. April 2022

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Michaela Paffenholz