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The effect of maternal education on offspring's mental health

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Abstract

We estimate the causal effect of maternal education on the mental health of mother's children in late adolescence and adulthood. Theoretical considerations are ambiguous about a causal effect of maternal education on children's mental health. To identify the causal effect of maternal education, we exploit exogenous variation in maternal years of schooling, caused by a compulsory schooling law reform in West Germany. Based on data from the German Socio-Economic Panel, we find no evidence of a causal protective effect of maternal education on children's mental health. Instead, our empirical results suggest a moderate negative effect of maternal education on the daughters' mental health. We find no effects for the sons. Our investigation of potential mechanisms is consistent with the hypothesis that the negative effect of higher maternal labor supply outweighs the positive effect of an expansion in household resources.

JEL codes: I10, I21, I26, J62
Keywords: Mental Health, Education, Compulsory Schooling Reform, Intergenerational Mobility

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1 Introduction

Worldwide, mental health conditions are a leading cause of disability-adjusted life years (DALYs) and health costs. They account for 199 million DALYs or 37 percent of healthy life years lost due to noncommunicable diseases worldwide. The sum of direct and indirect costs worldwide in 2010 is estimated to equal 2.5 trillion US dollars and projected to increase to 6 trillion US dollars in 2030 (Bloom et al., 2011). Given that the financial and societal burdens of mental health impairments are high, prevention measures that alleviate mental health problems will have high financial and societal returns. In addition, contributions have shown a strong intergenerational transmission of mental health status (Johnston et al., 2013) implying long-run consequences for the children of those affected by mental health problems today.

This study is the first to estimate the causal effect of maternal education on children's mental health in adulthood. Given the high prevalence rate of mental health problems, easing the burden of mental health problems would have immediate payoffs. For instance, Layard (2016) estimates that if we relieve the societie's burden of mental illnesses, we could increase general employment by four percent and thus increase GDP. Therefore, our results are highly relevant for policy makers who are eager to alleviate the burden of mental health problems.

A large and active literature investigates the causal effect of parental education on their children's health. This empirical relation between parental education and the children's health is most likely to be subject to endogeneity from three potential sources: 1. the unobserved characteristics that are associated with education and mental health and that are transmitted across generations are likely to confound OLS estimates, 2. classical measurement error could attenuate the OLS estimates toward zero, and 3. reverse causality could also bias the OLS estimates.[1] In consequence, these studies mainly exploit three exogenous sources of variation in parental education: first, one part of the literature relies on the regional and temporal variation in the supply of educational institutions, such as colleges or schools (e.g. Currie and Moretti, 2003). A second part of the literature exploits discontinuities created by school entry laws (e.g McCrary and Royer, 2011). The third, and largest, part of the literature exploits changes in the number of years of compulsory education (e.g. Lindeboom et al., 2009; Lundborg et al., 2014; Rawlings, 2015; Chou et al., 2010). While still inconclusive, the majority of these studies point toward a positive effect of maternal education on children's health in infancy and early adolescence. However, the mental health outcomes of children

[1] Clearly, this would impose a high degree of rationality if the parents adjust their education based on the expectation that this would improve the mental health of their children.

are largely neglected in this literature.[2]

Further, an emerging literature investigates the influence of early life circumstances on adult mental health. For instance, Adhvaryu et al. (2017) investigate the effect of income shocks early in life on mental health in adulthood. In addition, Adhvaryu et al. (2015) show that temperature shocks in utero increase depressive symptoms in adulthood.[3]

This paper contributes to both strands of this literature by analyzing the causal effect of maternal education on children's mental health. In particular, we answer the following question, does maternal education affect the children's mental health in late adolescence and adulthood? Theoretical considerations are ambiguous about the effects of maternal education on children's mental health. Moreover, as previously described, the empirical relation between maternal education and children's mental health is potentially subject to endogeneity. For this reason, we exploit exogenous variation caused by a compulsory schooling law (CSL) reform that extended compulsory schooling from eight to nine years across states and time in West Germany. Since it is only parents at the lower end of the educational hierarchy for whom this CSL reform is binding, our study provides empirical evidence regarding how to alleviate the socioeconomic gradient in health across generations (Currie, 2009).

Estimating the intergenerational effect of maternal education on the mental health of children is a formidable task. On the one hand, we need information about parents and their children. On the other hand, we need information about the children's mental health. Information about both is rarely available in datasets. For this reason, our study uses data from the Socio-Economic Panel (SOEP). The SOEP entails extensive information about parent-child pairs, which makes it especially well-suited for our study. Our principal measure of mental health is the Mental Component Summary (MCS) score, based on the Short-Form 12 (SF-12) questionnaire, which comprises twelve health related questions covering both mental and physical health dimensions. The MCS score is a well-established measure for mental health in the epidemiological literature and is shown to be predictive for mental illnesses (Salyers et al., 2000). We also analyze heterogeneous effects of gender by estimating the effect of maternal education separately for sons and daughters. The literature shows that the transmission of mental health is particularly strong between mothers and daughters (Johnston et al., 2013). After establishing a causal relation between maternal education and children's mental health, we provide suggestive evidence of potential channels in accordance with our theoretical considerations.

[2] Lindeboom et al. (2009) investigate the effect of changes in the number of compulsory years of education on the mental conditions in early adolescence in the UK. They report no effect of parental education on mental conditions of children in early adolescence.

[3] Almond et al. (2018) provide a thorough review of the complete economic literature regarding the effects of early life influences and in utero experiences on later life outcomes.

Our empirical results show that there is no positive effect of maternal education on children's mental health; rather, we find a negative effect of maternal education on daughter's mental health. One year of additional maternal schooling, caused by the CSL reform, decreases the daughters' MCS score by approximately 26 percent of a standard deviation and increases the likelihood of being at risk of clinical depression by approximately 11 percentage points. In contrast, we find no evidence that the additional year of schooling for mothers affects son's mental health. This result is robust to a wide range of robustness checks.

We find that the maternal labor supply increases by approximately 17%, which also increases maternal income. Moreover, we find that these mothers have partners with more years of schooling due to the CSL reform. Thus, the evidence is in line with the hypothesis that the negative effect of the maternal labor supply outweighs the effect of the expansion in household resources due to the CSL reform.

This paper is organized as follows: Section 2 provides an overview on the theoretical link between maternal education and children's mental health. Section 3 describes our empirical strategy and provides background information about the educational and the CSL reform in West Germany. Section 4 depicts our data and sample selection. In Section 5, we discuss our results and their robustness. Section 6 is devoted to the inference and discussion of potential channels driving our results and Section 7 concludes.

2 Theoretical Background

Maternal education can affect children's mental health either directly or indirectly. The most important indirect channel is *income*. Better-educated mothers–on average–live in households with higher household income, either obtained through higher own income or higher partner income. Higher household income could affect the children's mental health through three channels: first, it increases the availability of medical care and prevention measures. Such measures could include the availability of psychotherapists.[4] Second, higher education and income could also result in living in a better neighborhood and having a better peer group for the child, in addition to a more stimulating environment at home. Third, higher financial resources could also decrease the financial worries of the mothers, thereby easing the mental strain of the mothers and, thus also the children's mental strain. Additionally, education also lessens the maternal budget constraint indirectly via positive assortative

[4] Psychotherapy is typically covered by the universal health insurance. However, a shortage of supply in psychotherapists generates an advantage for parents who have a higher willingness to pay and can afford private psychotherapists.

mating: better-educated women tend to marry men with higher education and earnings (Lefgren and McIntyre, 2006). Consequently, positive *assortative mating* amplifies the aforementioned effects.

Higher maternal education enhances children's mental health directly through *behavioral responses*. More precisely, higher education can also influence the health and parenting behavior positively, therefore increasing the likelihood of the child having good mental health. For instance, Grossman (1972) argues better-educated individuals are more efficient producers of health. Thus, more highly educated mothers have higher health returns for given health inputs or are able to choose a better mix of health inputs. If this effect can be generalized to the production of mental health, we would expect better educated mothers to have children with higher mental health.

Moreover, according to Becker and Lewis (1973), higher earnings capacities increase the *opportunity costs* of child services, which can be thought of as a product of the number of children and the average quality of the child. Consequently, mothers with higher earnings capacities could trade-off the number of children for a higher average quality of children. However, on the other hand, a higher labor market attachment translates into higher absence from home. Higher absence from home could have a detrimental effect on children's mental health. For instance, Agostinelli and Sorrenti (2018) show that for behavioral outcomes of children, the negative effect of the maternal labor supply outweighs the small effect of the additional household income. Similarly, Dave et al. (2019) and Page et al. (2019) show that the maternal labor supply has a negative effect on behavioral and health outcomes of the respective children. This conjecture could be particularly relevant in our setting because Cygan-Rehm (2018) shows that the CSL reform increased individuals' wages and Cygan-Rehm and Maeder (2013) show that the CSL reform increased the nonmonetary returns of work for women.

In addition, higher education of the mother could reduce the likelihood of divorce (Boertien and Härkönen, 2018). Since parental divorce is shown to be associated with psychological distress among the affected children (Strohschein, 2005), as a result, we would expect higher maternal education to positively affect children's mental health status through more stable partnerships.

In conclusion, theoretical considerations predict an ambiguous effect of maternal education on children's mental health. Consequently, the direction of the causal effect of maternal education on children's mental health in adulthood remains an empirical question.

3 Method

3.1 Institutional Background

The German school system. In Germany, children enroll in elementary school (*Grundschule*) at the age of six. Typically, after four years of elementary school, children continue schooling in one of three different tracks of secondary schooling. These three different tracks differ in terms of duration and curriculum. The basic school track (*Hauptschule*) ends after four or five years and provides basic general education. In the intermediate track (*Realschule*), children finish school after ten years and experience a more extensive general education. High school (*Gymnasium*) lasts nine additional years and offers the most academic curriculum. The basic and intermediate school tracks qualify the student for an apprenticeship or vocational training. High schools provide their graduates with the university entrance qualifications.[5]

Noteworthy, allocation to the respective school tracks depends considerably on the student's academic performance in the main school subjects. The elementary school teacher usually gives a recommendation to the parents, based on the students' aptitude, as reflected in the student's grades. Whereas this recommendation is binding in some states, it is not binding in others. The parents decide which school track their child will attend if the teacher's recommendation is not binding.[6]

Usually, children with the lowest grades are assigned to the basic school track, whereas those with the best grades are assigned to high schools. Mobility between these three school tracks is possible but limited, with downward mobility being more common. Approximately two percent of all students change from the track to which they were assigned (Dustmann et al., 2016).

The Compulsory Schooling (CSL) Reform. Starting in the 1940s, the federal states in West Germany[7] implemented CSL reforms that prolonged the number of mandatory schooling years from eight to nine years. Because education policy is performed on the level of the federal states in Germany, the CSL reform was not implemented simultaneously in all German federal states. Instead, the federal states performed a staggered implementation of the CSL reform between 1946 and 1969, as depicted in Table 1. This allows us to exploit exogenous variation across time and space.

[5] In addition, high school also qualifies the student to take up an apprenticeship or vocational training.

[6] In states with binding teachers' recommendations, parents could also opt for a lower school track.

[7] This comprises the BRD without the states of the former GDR and Berlin. A complete list of states that performed the CSL reform is provided in Table 1. We do not consider Berlin because our data does not allow to distinguish between individuals who grew up in the part of Berlin that belonged to the BRD and those who grew up in the part of Berlin that belonged to the former GDR.

Table 1: Increase in compulsory schooling from eight to nine years by federal state

	First year all students were supposed to graduate after a minimum of 9 years	First birth cohort affected by change in compulsory schooling law
Hamburg	1946	1931
Schleswig-Holstein	1947	1932
Bremen	1959	1944
Lower Saxony	1962	1947
Saarland	1958	1943
North Rhine-Westphalia	1968	1953
Hesse	1968	1953
Rhineland-Palatinate	1968	1953
Baden-Wuerttemberg	1968	1953
Bavaria	1969	1955

Source: Leschinsky and Roeder (1980)

3.2 Empirical Strategy

To assess the impact of maternal education on children's mental health outcomes, we estimate the following equations:

$$\begin{aligned} MH_{it}^{C} = \beta_1 + \beta_2 ED_i + \beta_3 age_{it} + \beta_4 age_{it}^2 + \beta_5 t \\ + \sum_{s=1}^{10} \mu_s \mathbb{1}[s = s_i] + \sum_{s=1}^{10} \rho_s \mathbb{1}[s = s_i] c_i \\ + \sum_{c=1930}^{1960} \kappa_c \mathbb{1}[c = c_i] + \epsilon_{it}. \end{aligned} \tag{1}$$

$$\begin{aligned} ED_i = \gamma_1 + \gamma_2 reform_i + \gamma_3 age_{it} + \gamma_4 age_{it}^2 + \gamma_5 t \\ + \sum_{s=1}^{10} \delta_s \mathbb{1}[s = s_i] + \sum_{s=1}^{10} \zeta_s \mathbb{1}[s = s_i] c_i \\ + \sum_{c=1930}^{1960} \nu_c \mathbb{1}[c = c_i] + \eta_{it}. \end{aligned} \tag{2}$$

In equation (1), MH_{it}^{C} corresponds to the mental health outcome of child i at time t, and ED_i are the maternal years of schooling. In addition, we control for a second-order polynomial of age to account for age related changes in our mental health outcomes. Moreover, t reflects a linear survey year trend and thus accounts for secular trends in mental health. Given that we already inherently control for a linear cohort trend by including controls for age and the survey year, the specifications do not include cohort controls.

Both equations, (1) and (2), contain a full set of indicators for the maternal state of schooling and maternal year of birth in addition to linear maternal state of schooling trends: the indicator function $\mathbb{1}[s = s_i]$ is equal to one if the maternal state of schooling s_i is equal to s and zero otherwise. Similarly, the indicator function $\mathbb{1}[c = c_i]$ is equal to one if the maternal year of birth c_i is equal to c. The cohort indicator controls for cohort-specific trends in education, and the indicator for the maternal state of schooling controls for differences in socioeconomic levels or attitudes toward education on the state level. In addition, the linear maternal state of schooling trends account for differential trends across the maternal states of schooling. The parameter of interest is β_2, which ought to reflect the effect of maternal years of schooling on children's mental health.

Noteworthy, the OLS estimates of the coefficient β_2 are very likely to be inconsistent. For instance, the unobserved characteristics, which are transmitted across generations, that determine jointly maternal years of schooling and children's mental health could confound our estimates of β_2. For instance, genetic endowments could be hypothesized to be such unobserved characteristics. Further, classical measurement error in maternal years of schooling could attenuate our OLS estimates toward zero.[8]

Since we expect the OLS estimate of β_2 to be inconsistent, we instrument maternal years of schooling with the exposure of the respective mother to the CSL reform. The indicator $reform_i$ is equal to one if the mother has been exposed to the CSL reform, and zero otherwise. The first stage then corresponds to the linear projection (2). Notably, the inclusion of maternal cohort and state of schooling indicators in addition to linear maternal state of schooling trends is key to our approach: we rely on the changes of the instrument within states and cohorts to identify our effects. This avoids using permanent differences across maternal cohorts or states as the source of identifying variation. The estimate of the coefficient β_2 then can be interpreted as the estimate of the gross effect of the additional year of schooling, caused by the CSL reform, on children's mental health.

We estimate equation (1) for the entire sample of children and for sons and daughters seperately, such that $C \in \{all, son, daughter\}$. Robust standard errors are clustered on the level of the policy assignment: the maternal state of schooling.[9]

[8]In general, reverse causality could also render our OLS estimates inconsistent. However, it is very unlikely that the contemporaneous mental health outcomes of the children affect maternal schooling.

[9]Our results are also robust to alternative methods of calculating the variance-covariance matrix. We also employ two-way clustering at the maternal state of schooling and year of birth level. Since the number of maternal states of schooling is ten, variance-covariance matrices clustered on the maternal state of schooling level are likely to be inconsistent. This is the result of the fact that asymptotic results of clustered variance-covariance matrices rely on the number of clusters approaching infinity. This is true even if we employ two-way clustering because the effective number of clusters corresponds to the cluster dimension with the fewest clusters (Cameron and Miller, 2015). Cameron et al. (2008) suggest relying on

After having established a causal effect of the additional year of maternal schooling on children's mental health, we infer the channels through which this additional year of schooling affects their children's mental health. Potential candidates in line with our theoretical considerations are the following: permanent net income of the mother, maternal labor supply, fertility, marital behavior, and the years of schooling of the mother's partner. We provide suggestive evidence for these potential mechanisms by employing our two stages least squares (2SLS) framework to proxies for these potential mechanism candidates.

The key assumption of our identification strategy is that the exposure to the CSL reform affects the children's mental health only through maternal years of schooling. For instance, the CSL reform may reflect increases in demand for education. If demand for education would have been correlated with omitted factors that also increase maternal investments in the children's mental health, then changes in this omitted factors rather than the CSL reform exposure would drive the correlation between maternal years of schooling and improvements in mental health. We argue that this is unlikely to be the case. As Petzold (1981) contends, the proponents of the CSL reform argued along two different lines of reasoning: on the one hand, pedagogues argued that the entrance into the labor market should be postponed to protect children from working conditions that may be psychologically harmful. In addition, they argued that children are not ready for the labor market after only eight years of schooling. On the other hand, politicians wanted to relieve the strain on the youth labor market by holding back labor market entrants for an additional year. Thus, the CSL reform was not a result of a general increase in demand for education that could have rendered CSL reform exposure as an invalid IV.

Furthermore, the strain on the youth labor market and the associated unemployment could also be argued to have an effect on children's mental health via maternal mental health. However, this argument was relevant only for the CSL reforms in the first states. Later, pedagogical reasons were more vocal in the discussion (Petzold, 1981). Nevertheless, in a robustness check, we exclude the earliest years to account for this potentially confounding factor.

In addition, our identification would be flawed if the implementation of the CSL reform were correlated with better medical health care in the respective state. The German health care system, is organized at the federal level. In contrast, educational policy is one of the most important policy

bootstrap-based improvements for inference with clustered errors that rely on asymptotic refinement. Consequently, as the number of clusters approaches infinity, these methods converge even more quickly to the true size of the significance test. Among those methods, the wild cluster bootstrap-t method proves to be extremely close to the true test size, even if the number of clusters is as small as six. Therefore, we also calculate p-values employing the wild cluster bootstrap-t procedure using 999 repetitions and Rademacher weights, as suggested by Cameron et al. (2008). The results are presented in Table A.1 in the appendix.

fields that is organized by the federal states alone. Thus, it is highly unlikely that the implementation of the CSL reform is correlated with the health policy in the respective state.

Moreover, the presumption that the CSL reform altered mental health only through changes in schooling would be violated if the CSL reform altered the quality of schooling or altered peer composition in classes. School quality could have been affected if the states implementing the reform could not meet the increased demand for qualified teachers in the short run and thus hired unqualified teachers or increased the student-to-teacher ratio (Lundborg et al., 2014). In a robustness check, we show that this is indeed not the case.

In addition, peer quality in class could have been affected negatively if the CSL reforms postponed tracking. Tracking in school usually occurs after the fourth grade. In contrast, the CSL reform increases the number of mandatory years of schooling from eight to nine years. Thus, tracking had already occured for those affected by the CSL reform. Therefore, school tracking is not changed due to the CSL reform.[10]

One additional threat to our identification strategy could be that the CSL reform resulted in changed fertility patterns. For instance, schooling could either delay fertility or change the fertility pattern at the extensive or intensive margin. Indeed, Cygan-Rehm and Maeder (2013) confirm fertility effects of education exploiting the very same CSL reform that we are exploiting. They show that education negatively affects completed fertility. This effect operates through a postponement of first births and no catch-up later in life. This would thread the validity of our estimates if this effect would be attributable to any channel other than schooling. For instance, McCrary and Royer (2011) argue that educational reforms that mechanically delay fertility would cause the order condition of the respective IV strategy to be violated. The reason why such an "incarceration" effect threatens our IV strategy is that the CSL reform affects children's mental health through different channels rather than schooling, e.g. age at first birth, and thus would introduce additional endogenous regressors. Consequently, we would need an additional instrument. However, Cygan-Rehm and Maeder (2013) attribute the changed fertility pattern to increased opportunity costs of childbearing for women. In particular, affected women are more likely to choose self-employment and end up in more prestigious jobs. Clearly, these are outcomes that are mediated by schooling. Thus, we argue that changed fertility patterns are not a threat to our identification strategy.

Finally, we have to assure that individuals do not self-select into the reform based on expected outcomes. This is likely to be a valid assumption since it would impose an unreasonable high degree

[10]Table A.2 indicates no evidence for an effect of the CSL reform on the track choice of individuals born between 1930 and 1960. Consequently, we can rule out that individuals did adjust their track choice in response to the CSL reform.

of rationality if we contend that individuals self-select into longer schooling based on the expectation that longer schooling increases the mental health outcomes of the respective children.[11]

Our 2SLS estimate then reflects the effect of maternal education on the children's mental health for those mother-child pairs whose mothers complied with the CSL reform (Imbens and Angrist, 1994). The CSL reform plausibly changed education only for mothers who would have finished schooling after eight years if they would have not been forced to stay in school one additional year. These are mainly mothers who attended the basic school track. Thus, our instrument affects only those individuals who are at the lower end of the educational distribution. In addition, a socioeconomic gradient in health outcomes is well documented (World Health Organization and Calouste Gulbenkian Foundation, 2014). Consequently, our results may provide guidance regarding how to alleviate the socioeconomic gradient in health outcomes.

4 Data

The SOEP is a representative longitudinal study of private households. It started in 1984 and surveys–in the most recent wave–approximately 15,000 households and more than 25,000 persons living in Germany annually.[12] Among other topics, the panel covers household composition, occupational biographies, employment, earnings, health, and parenting behaviors (Goebel et al., 2018).

Moreover, children in the respective household are surveyed after growing up and interviewed annually thereafter, even after having left the parental household. Hence, the SOEP entails detailed information about mother-child pairs. Consequently, we are able to link the educational information of SOEP respondents to their children's mental health outcomes.

Starting in 2002, the SOEP introduced a special health module that is now administered biannually. This survey module includes the SF-12 questionnaire, which comprises twelve health-related questions that cover both mental and physical health dimensions.[13] The questions refer to the health status within the four weeks preceding the interview. Hence, it refers to the current health status (Andersen et al., 2007). The MCS score, as a continuous measure of the mental health status, is extracted by means of an explorative factor analysis and is normed to have mean 50 and standard deviation 10 in the 2004 population of the SOEP (Andersen et al., 2007). Higher MCS scores indi-

[11] Similarly, we have to be able to rule out defiers. Defiers are individuals who do not take up the additional year of schooling if they are induced to do so by their reform status and take up the additional year of schooling if they were not subject to the reform. Since this school reform changed the number of mandatory years of schooling, noncompliance was very low.

[12] We use SOEPv32. DOI: 10.5684/soep.v32

[13] See Figure A.1 in the appendix.

cate better mental health. The MCS score is widely used in the epidemiological literature and has high predictive power for mental illnesses (Salyers et al., 2000).

In addition, we also derive an indicator for being at risk of developing symptoms of a mental disorder based on the MCS score. An MCS score below 45.6 has high predictive power for showing symptoms of a clinically relevant mental disorder within thirty days (Vilagut et al., 2013).[14] Based on this threshold, we derive an indicator for being at risk of mental disorder.

A distinct advantage of the MCS score is that it infers the mental health status indirectly. Direct measures, such as a self-reported diagnosis of mental illnesses, are prone to underreporting. For example, Bharadwaj et al. (2017) show that individuals tend to underreport mental illnesses 36 percent of the time in surveys compared to administrative data on diagnoses. This behavior is consistent with the existence of mental health stigmata.

However, the SF-12 questionnaire could also be subject to underreporting, since the health context is obvious to the respondents. Therefore, as an additional indicator of mental well-being, we draw on the question on life satisfaction that is also included in the SOEP annually. Satisfaction with life is inferred by answers to the following single-item question: "How satisfied are you at present with your life as a whole?" Answers are based on an eleven-point Likert scale, ranging from zero "completely dissatisfied" to ten "completely satisfied". This measure is used as an indicator for mental well-being in the literature but is asked in a completely different context in the survey interview. Thus, misreporting or underreporting should not be a major issue with this outcome.

To construct the CSL reform indicator, we need information on the place of schooling and the year of birth.[15] Fortunately, the SOEP entails information about the last state of schooling. This information is available for approximately 20 percent of the SOEP sample. For the remaining mothers, we take the state in which the mothers were living when they were first surveyed.[16] This procedure is consistent with the procedure applied in previous studies exploiting the CSL reform in Germany (e.g. Pischke and von Wachter, 2008; Cygan-Rehm and Maeder, 2013).

We restrict our sample to the mother-child pairs for which we have observations about the children's mental health outcomes from 2002 through 2015. In addition, we keep information for

[14]A specificity of 86 percent is associated with the threshold of 45.6. That is, 86 percent of the individuals with an MCS score below 45.6 actually exhibit symptoms of a clinical relevant a mental disorder within 30 days preceding the screening (Vilagut et al., 2013).

[15]We assume that all children enter school the year they turn six. Since we do not observe the level of compliance with the school enrollment guidelines among our sample, as a robustness test, we drop the pivotal cohort. Our estimates are robust to this test. The results are available from the authors.

[16]For 83.46% of the respondents in the SOEP, the stated state of schooling corresponds to the first state in which the respondents, who stated their state of schooling, were surveyed first. If we condition on individuals with a school-leaving degree from the basic track, our complier group, this number increases to 90.76%.

mother-child pairs for which we have information about years and place of schooling in addition to year of birth. We exclude observations of mothers migrated to Germany after World War II. Further, we restrict our sample such that our mothers are born between 1930 and 1960. Finally, we keep observations of mother-children pairs if we have at least one observation for the MCS score and life satisfaction of the respective child. To increase the precision of our estimates, we exploit all available information per mother-child pair. That is, we make use of the longitudinal structure of the data.

The summary statistics for our sample are reported in Table 2. Our sample consists of 3326 unique mother-children pairs. Among these, we observe 2131 mothers, with an average of 1.56 children. On average, we have 7.38 observations for life satisfaction and 3.7 observations for the MCS score. Given that the MCS score is standardized to have mean 50 and standard deviation of 10 in the 2004 SOEP population, we do not see any large deviations with respect to the MCS score in our sample. Twenty-nine percent of the children are at risk of developing symptoms of a clinically relevant mental disorder according to our indicator.[17] Overall, 50 percent of our observations belong to the treatment group and 49 percent of the mothers went to the basic school track. Our average child is 29.72 years old.

Table 2: Summary Statistics

Variable	Mean	Std. Dev.	Min.	Max.	Observations
Children's characteristics					
LS	7.25	1.64	0	10	24550
MCS	49.49	9.63	0.56	71.81	12312
Mental disorder (MCS)	0.29	0.46	0	1	12312
Female	0.47	0.5	0	1	25055
Age	29.72	9.14	16	65	25055
Year of birth	1978.09	8.91	1950	1996	25055
Maternal characteristics					
Maternal years of schooling	9.75	1.71	8	13	25055
Mother in basic track	0.49	0.5	0	1	25055
Maternal reform exposure	0.5	0.5	0	1	25055
Maternal year of birth	1949.66	8.09	1930	1960	25055

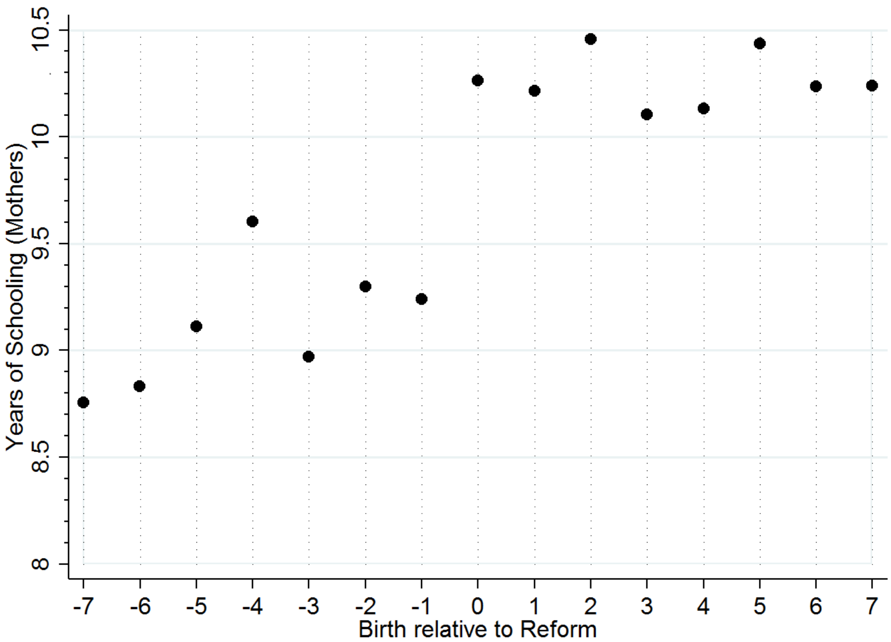
Notes: SOEPv32 waves 2002 to 2015. Number of unique mother-child pairs: 3326.

Figure 1 displays the reform effect on mothers' schooling and is a visual representation of our first stage. On the x-axis, we display the cohort relative to the first cohort that was affected in the

[17] For Germany, the lifetime incidence of *diagnosed* depression is reported to be 19 percent (Wittchen et al., 2010).

respective state of schooling. As we can observe, the CSL reform caused a considerable increase in the average years of education, independent of the time trend in education. Thus, we argue that there is sufficient exogenous variation in maternal years of schooling, conditional on the maternal cohort and state of schooling.

Figure 1: Average years of schooling per cohort



Notes: SOEPv32. Own calculations. The sample comprises all females born between 1930 and 1960. The graph shows the average number of years of schooling against the year of birth relative to the first cohort which was affected by the CSL reform in each state.

5 Results

5.1 Maternal schooling and children's mental health

We begin the discussion of our results with the pooled sample including all mother-child pairs, irrespective of the children's gender in Table 3. In Table 3 - 5, the three panels (A-C) represent the three aforementioned outcome variables: life satisfaction, the MCS score, and our binary indicator of being at risk of developing mental disorder symptoms. Both nonbinary outcomes are standardized to have mean zero and standard deviation one. The first two columns present results from simple OLS regressions of equation (1). Column II includes age controls and a survey year trend in addition to the state and wave fixed effects in the specification depicted in the first column. Columns III and

IV report the results from the 2SLS estimations alongside the results of the respective first stages.[18] Again, column IV adds age controls and a survey year trend to the specification in the third column. Based on the pooled sample with all mother-child pairs included, we infer that maternal years of schooling are positively associated with children's life satisfaction, depicted in panel A in Table 3. In addition, the estimates in panel B show that maternal years of schooling is negatively associated with the children's MCS score, at least in the specification in column II, and there is no significant association with the risk of developing mental health disorders (panel C in Table 3). Thus, based on OLS estimates, the descriptive evidence does not reveal a consistent pattern in the association between maternal schooling and her children's mental health. However, classical measurement error or confounders could cause our estimates to be attenuated towards zero.

In contrast, the 2SLS estimates in columns III and IV of Table 3 indicate no effect of the additional year of maternal schooling, caused by the CSL reform, on children's mental health. The point estimates from the 2SLS estimation are small and negative for life satisfaction and the MCS score. In addition, the point estimates are small and positive for being at risk of having a mental disorder. However, none of these estimates are near significance at conventional levels. The F-statistic of our first-stage coefficient is well above ten, indicating the absence of a weak first stage. The first stage coefficient is 0.87 in our main specification in column IV and in line with existing studies relying on the SOEP (e.g., Cygan-Rehm and Maeder, 2013).

5.2 Heterogeneous effects by gender

In the next step, we analyze whether there is effect heterogeneity between sons and daughters. The results corresponding to those presented in Table 3 for a sample only including daughters are reported in Table 4 and only including sons in Table 5. We start the discussion with the results from the mother-daughter pairs. Again, the OLS estimates in columns I and II do not reveal a clear pattern. However, the 2SLS effects in columns III and IV of Table 4 are more pronounced and more precisely estimated in the mother-daughter sample compared to the pooled sample. According to our 2SLS estimates in column IV of Table 4, we can observe that the additional year of schooling, caused by the CSL reform, decreases the daughter's MCS by about 0.258 standard deviations. Given a standard deviation of about 10, this amounts to 2.58 points in the MCS score. To put that into perspective, Marcus (2013) shows that unemployment of the husband decreases the wife's MCS score by about 3.18 points and Cygan-Rehm et al. (2017) show that the lower bound of the causal

[18] Since our being at risk of a mental illness indicator is calculated from the MCS score, the first stages in panels B and C are identical. Thus, we only report them once.

Table 3: Maternal years of schooling and children's mental health

	(I) OLS	(II) OLS	(III) 2SLS	(IV) 2SLS
Panel A: Life satisfaction				
Maternal years of schooling	0.045***	0.029***	-0.025	-0.034
	(0.005)	(0.004)	(0.082)	(0.073)
First stage				
Maternal reform exposure			0.913	0.877
			(0.139)	(0.126)
F-stat first stage			43.09	48.71
Individuals	3326	3326	3326	3326
Individual-time observations	24550	24550	24550	24550
Panel B: MCS score				
Maternal years of schooling	-0.004	-0.014**	-0.069	-0.078
	(0.007)	(0.007)	(0.081)	(0.079)
Panel C: Being at risk of having a mental disorder				
Maternal years of schooling	0.000	0.004	0.018	0.021
	(0.004)	(0.003)	(0.032)	(0.030)
First stage				
Maternal reform exposure			0.908	0.863
			(0.136)	(0.126)
F-stat first stage			44.55	46.88
Individuals	3326	3326	3326	3326
Individual-time observations	12312	12312	12312	12312
State and wave FE	Y	Y	Y	Y
Age controls and survey year trend	N	Y	N	Y

Notes: SOEPv32 waves 2002 to 2015. All regressions include maternal cohort and state of schooling indicators in addition to linear maternal state of schooling trends. Robust standard errors in parentheses are clustered on the maternal state of schooling level. Significance: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

effect of unemployment on the female's MCS score is 0.456 standard deviations.[19] However, Ware et al. (1996) show that, at the individual level, a change of approximately 7.9 units in the MCS score is required to consider the change clinically relevant. Taking this result as a benchmark, we conclude that the effect of maternal schooling on children's MCS score is rather moderate.

Additionally, maternal schooling increases the risk of developing mental disorder symptoms by 11 to 13 percentage points (panel C, columns III and IV of Table 4). Compared to an overall mean of 29 percent, this amounts to a relative effect size of about 37 percent. Interpreting this score, we must keep in mind a specificity of about 86 percent. That is, 86 percent of those who have a score below 45.6 indeed exhibit symptoms of a clinically relevant mental health disorder within the 30

[19] Eibich (2015) estimates that retirement increases men's mental health by about 0.535 standard deviations or 5.1 points.

Table 4: Maternal years of schooling and daughters' mental health

	(I) OLS	(II) OLS	(III) 2SLS	(IV) 2SLS
Panel A: Life satisfaction				
Maternal years of schooling	0.045***	0.032***	-0.138	-0.093
	(0.008)	(0.006)	(0.197)	(0.152)
First stage				
Maternal reform exposure			0.712	0.810
			(0.146)	(0.114)
F-stat first stage			23.74	50.62
Individuals	1540	1540	1540	1540
Individual-time observations	11594	11594	11594	11594
Panel B: MCS score				
Maternal years of schooling	-0.019	-0.031***	-0.304**	-0.258**
	(0.012)	(0.012)	(0.148)	(0.118)
Panel C: Being at risk of having a mental disorder				
Maternal years of schooling	0.011**	0.016***	0.133*	0.113**
	(0.005)	(0.005)	(0.074)	(0.055)
First stage				
Maternal reform exposure			0.761	0.833
			(0.145)	(0.117)
F-stat first stage			27.46	50.50
Individuals	1540	1540	1540	1540
Individual-time observations	5789	5789	5789	5789
State and wave FE	Y	Y	Y	Y
Age controls and survey year trend	N	Y	N	Y

Notes: SOEPv32 waves 2002 to 2015. All regressions include maternal cohort and state of schooling indicators in addition to linear maternal state of schooling trends. Robust standard errors in parentheses are clustered on the maternal state of schooling level. Significance: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

days preceding a clinical interview (Vilagut et al., 2013). To date, very few studies employ such a threshold. Adhvaryu et al. (2017) evaluate the effect of an income shock during early childhood on adult mental health in Ghana. They construct a measure for severe stress based on the Kessler Psychological Distress Scale. They show that an exogenous increase in income during childhood decreases the likelihood of having severe stress in adulthood by approximately 6.2 percentage points, or 50 percent, relative to the mean. In comparison to this relative effect size, we conclude that the effect presented in Table 4 is also rather moderate.

We do not find a significant effect of the additional year of maternal schooling, caused by the CSL reform, on the mental health outcomes of her sons (Table 5). In summary, the gender-specific analysis supports the finding that there is no empirical evidence pointing towards a positive effect

Table 5: Maternal years of schooling and sons' mental health

	(I) OLS	(II) OLS	(III) 2SLS	(IV) 2SLS
Panel A: Life satisfaction				
Maternal years of schooling	0.046***	0.028***	0.016	-0.028
	(0.008)	(0.006)	(0.059)	(0.058)
First stage				
Maternal reform exposure			1.055	0.906
			(0.168)	(0.153)
F-stat first stage			39.31	35.05
Individuals	1786	1786	1786	1786
Individual-time observations	12956	12956	12956	12956
Panel B: MCS score				
Maternal years of schooling	0.008	0.000	0.081	0.074
	(0.010)	(0.010)	(0.099)	(0.117)
Panel C: Being at risk of having a mental disorder				
Maternal years of schooling	-0.009**	-0.006	-0.050	-0.049
	(0.004)	(0.004)	(0.037)	(0.043)
First stage				
Maternal reform exposure			1.005	0.859
			(0.165)	(0.151)
F-stat first stage			37.12	32.39
Individuals	1786	1786	1786	1786
Individual-time observations	6523	6523	6523	6523
State and wave FE	Y	Y	Y	Y
Age controls and survey year trend	N	Y	N	Y

Notes: SOEPv32 waves 2002 to 2015. All regressions include maternal cohort and state of schooling indicators in addition to linear maternal state of schooling trends. Robust standard errors in parentheses are clustered on the maternal state of schooling level. Significance: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

of maternal schooling on the next generation's mental health. This is also in line with the results for life satisfaction presented in panel A of Tables 4 and 5 which show no significant effect of maternal schooling on daughters and sons life satisfaction. In contrast, our results even suggest a moderate negative impact on the MCS-based mental health measures of the daughters. After discussing the robustness of our results in the next subsection, in Section 6, we analyze the potential mechanisms that may explain these results.

5.3 Robustness Tests

One concern is that our results are driven by the aggregation of the subscales of the SF-12 questionnaire. The MCS score corresponds to a weighted average of the eight subscales that enter the factor analysis, with the weights corresponding to the factor score coefficients. The factor score coefficients that are attached to the subscales describing the physical health dimension[20] are close to zero but negative. Thus, if the additional year of schooling had a causal effect on a subscale that reflects a physical health dimension, we would observe a rather mechanical decrease in the MCS score. To rule out that our results are driven by changes in the physical health dimension, we employ our estimation strategy separately to the eight subscales of the SF-12 questionnaire. The results are presented in Table 6. The first four columns refer to the subordinates of the MCS score. The last four columns refer to the subordinates of the PCS score.

From results in the Table 6, it is clear that our findings are not driven by the described mechanical effect. If we do not distinguish between daughters and sons (panel A, Table 6), we confirm a negative effect on the subscale "Mental health" and a positive effect on the subscale "Physical functioning". The additional year of maternal schooling increases the children's "Physical functioning" by about 0.21 standard deviations and decreases children's subscale "Mental health" by 0.14 standard deviations. Further, if we distinguish between daughters and sons, we observe that the additional year of maternal schooling has a negative effect on the daughters' "Mental health" subscale. The effect size amounts to 0.30 of a standard deviation. We do not observe any negative effect on the respective subscales of the sons but see a positive effect on the subscale "Social functioning." However, we observe a positive effect of the additional year of maternal schooling on the sons' "Physical functioning" subscale. Thus, we are confident that our results are not driven by the aggregation of the SF-12 subscales but rather reflect a true mental health effect.

[20]We characterize the subscales "Physical functioning", "Role phyiscal", "Bodily pain" and "General health" as physical health dimensions.

Table 6: Maternal years of schooling and the children's SF-12 subscales

	Mental health				Physical health			
	Ment. health	Role emot.	Soc. funct.	Vitality	Gen. health	Bodily pain	Role phys.	Phys. funct.
Panel A: Daughters and sons ($N = 3326$, Individual-time observations : 12312)								
Maternal years of schooling	-0.136**	0.056	0.068	-0.035	0.073	0.089	0.064	0.212***
	(0.069)	(0.092)	(0.086)	(0.053)	(0.047)	(0.090)	(0.092)	(0.048)
Panel B: Daughters ($N = 1540$, Individual-time observations : 5789)								
Maternal years of schooling	-0.298**	-0.042	-0.127	-0.128	0.055	0.056	-0.001	0.109
	(0.129)	(0.106)	(0.096)	(0.104)	(0.065)	(0.070)	(0.099)	(0.079)
Panel C: Sons ($N = 1786$, Individual-time observations : 6523)								
Maternal years of schooling	-0.011	0.143	0.224*	0.052	0.055	0.108	0.139	0.284***
	(0.085)	(0.098)	(0.121)	(0.139)	(0.077)	(0.145)	(0.112)	(0.064)

Notes: SOEPv32 waves 2002 to 2015. All regressions include maternal cohort and state of schooling indicators in addition to linear maternal state of schooling trends. In addition, all specifications also include a second-order polynomial in the children's age in addition to linear survey year trends. Robust standard errors in parentheses are clustered on the maternal state of schooling level. * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Another possible threat to our identification strategy is the fact that the states that were the first to implement the CSL reform might have faced organizational problems. Examples of such organizational problems include a shortage of teachers during that period or a decreased teacher to student ratio. These organizational problems could have altered the school quality, which would invalidate the exogeneity assumption of our 2SLS strategy. One potential remedy for this threat is that we drop those states implementing the CSL reform first. These are Hamburg and Schleswig-Holstein. Other states followed with twelve-years difference. Based on the conjecture that this time span is long enough to prepare for the CSL reform, we drop all observations from Hamburg and Schleswig-Holstein and perform the main regressions in our favorite specification. The results are presented in Table A.3 in the appendix. Similar to our main results, we observe that maternal years of schooling has a negative effect on the daughter's MCS score and a positive effect on the likelihood of being at risk of a mental disorder. The estimates are of similar magnitude to our main results.

A third threat to the validity of our identification strategy is that the earliest maternal cohorts were born during World War II. Individuals born during the war may have been traumatized by it. In conclusion, this would lead to lower mental health, which may be transmitted across generations. In a similar vein, high unemployment among labor market entrants was put forward as a reason for the CSL reform (Petzold, 1981). This was most relevant for the early years. Later, more pedagogical reasons, such as labor market readiness, replaced the early arguments of youth unemployment. If

Table 7: Maternal years of schooling and changes in maternal household outcomes

	Divorced	Married	Number of children	Educ. of partner
	(1)	(2)	(3)	(4)
Maternal years of schooling	0.055	0.027	0.137	0.606***
	(0.057)	(0.019)	(0.093)	(0.156)
First stage				
Maternal reform exposure	0.620	0.589	0.582	0.801
	(0.137)	(0.132)	(0.131)	(0.143)
F-stat first stage	20.46	20.05	19.62	31.22
Observations	2080	2110	2100	1735

Notes: SOEPv32 waves 2002 to 2015. 2SLS regressions. All regressions include maternal cohort and state of schooling indicators in addition to linear maternal state of schooling trends. In addition, all regression include a second-order polynomial in the first born offspring's age and a linear survey year trend as observed at the first available observation. Robust standard errors in parentheses are clustered on the maternal state of schooling level. Significance: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

unemployment affects mental health and if mental health transmits across generations, this would again violate our exogeneity assumption. As a robustness test, we keep all observations born after 1945 and perform the analysis in this more restricted subsample. The results are presented in Table A.4. Based on these calculations, we now even observe a negative effect of the additional year of schooling on the children's mental health if we do not distinguish between daughters and sons. Notably, this result is driven mainly by the mental health outcomes of daughters. In contrast, maternal years of schooling has no effect on the son's MCS-based mental health outcomes in this specification. However, maternal years of schooling now has a negative effect even on the son's life satisfaction.

6 Potential Mechanisms

In section 5, we uncovered a moderate negative causal effect of maternal education on daughters' mental health. In this section, we try to uncover potential underlying mechanisms. In particular, in this discussion, we focus on the MSC-based outcomes. According to our theoretical considerations, we would expect that higher education causes changes in the home environment or in maternal labor market outcomes. To analyze this, we apply our estimation strategy to proxies of maternal household and labor market outcomes.

Our proxies for the home environment are whether the mother was divorced between age 25 and 55, whether she was married between age 25 and 55[21], the number of children of the mother[22],

[21]To be more specific, we take the marital status of the mother at the last available observation.
[22]The youngest mother in our sample is at least 42 years old. Thus, the mothers in our sample are at the end of their reproductive age or close to the end of their reproductive age. Therefore, the number of children in the family in our

and the years of schooling of the partner. The results are reported in Table 7. Varying sample sizes are explained by missing observations in the outcome variables. We observe that the mothers in our sample have partners with higher education. The additional year of maternal schooling increases the partner's years of schooling by approximately 0.61 years. Given that the partners of the mothers have an average of ten years of schooling, this amounts to a relative effect size of six percent. We do not find a significant effect on the other analyzed outcomes.

Our proxies for mothers' labor market outcomes, which potentially affect the mental health of the daughters, are average weekly working hours, unemployment status, the logarithm of the net income of the mother, self-employment status, and whether the mother ever obtained a vocational degree. Since we expect differences in the mental health outcomes of the children to stem from permanent differences rather than transitory shocks to these outcomes, we focus on permanent outcomes if applicable. That is, we take the individual average of the weekly working hours as well as the logarithm of the monthly maternal net income, deflated to year 2010 euros, from all available observations from the mother's prime working age. The prime working age is between 25 and 55, which overlaps with the relevant parenting time. We also take the last available observation on whether the mothers were unemployed or self-employed. The indicator for vocational degrees is equal to one of the mother has obtained a vocational degree. Turning to the results in Table 8, we observe that mothers with one additional year of maternal schooling work on average 4.5 hours per week more. Given a sample mean of 26.9 average weekly working hours, this amounts to a relative effect size of about 16.7 percent, which translates into an increase in the permanent net income of the mother of approximately 21.8 percent or 219 euros. We don't find an effect on the other outcomes.

Thus, in summary, we find that mothers affected by the reform in fact have a more favorable household environment (more resources and more highly educated partners) but spend more time away from home working. Thus, in our sample, the negative effect of a reduction of mothers' time available for childcare seems to dominate the positive effect of additional household resources. This result is consistent with recent evidence by Agostinelli and Sorrenti (2018), Dave et al. (2019) and Page et al. (2019), who investigate the effect of maternal labor supply on children's behavioral and health outcomes. For instance, Agostinelli and Sorrenti (2018) show that the productive effect of the additional income is negligible for behavioral outcomes, which leads to a negative overall effect of an increase of maternal labor supply.[23]

specifications can be regarded a proxy for completed fertility.

[23] In particular, Agostinelli and Sorrenti (2018) exploit changes in the Earned Income Tax Credit to estimate the causal

Table 8: Maternal years of schooling and own labor market outcomes

	Avg. weekly working hours	Unemployed	Avg. log net income	Self-employed	Voc. degree
	(1)	(2)	(3)	(4)	(5)
Maternal years of schooling	4.425***	-0.025	0.218**	-0.003	-0.046
	(1.085)	(0.044)	(0.088)	(0.017)	(0.066)
First stage					
Maternal reform exposure	0.722	0.683	0.775	0.671	0.570*
	(0.163)	(0.172)	(0.146)	(0.166)	(0.168)
F-stat first stage	19.56	15.70	28.12	16.40	11.48
Observations	1542	1876	1530	1706	1464

Notes: SOEPv32 waves 2002 to 2015. 2SLS regressions. All regressions include maternal cohort and state of schooling indicators in addition to linear maternal state of schooling trends. In addition, all regression include a second-order polynomial in the first born offspring's age and a linear survey year trend as observed at the first available observation. Robust standard errors in parentheses are clustered on the maternal state of schooling level. Significance: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

One reason for this result may be that the infrastructure for childcare outside the family was not yet well developed at the time these mothers entered the labor market. Thus, this does not necessarily carry over to more recent cohorts, which benefit from massive increases in availability of kindergardens (for those under three year old) and an increase in paternal participation in child rearing.

7 Conclusion

The incidence and prevalence of mental health disorders is increasing globally (Bloom et al., 2011). With contributions suggesting a substantial intergenerational transmission of mental health status (Johnston et al., 2013), this trend could result in negative long-run consequences, even affecting the next generation. In this paper, we analyze whether maternal education has a protective effect on the mental health status of their children in adulthood.

Using exogenous variation in education induced by a CSL reform in Germany, we find that the additional year of maternal schooling does not have a protective effect on her children's mental health in adulthood. Moreover, we find suggestive evidence pointing at a negative effect on the daughters' outcomes. This result is robust.

In addition, the investigation of potential mechanisms point towards an increase in maternal labor supply at the intensive margin and household resources. Thus, the evidence is consistent with

effect of additional income. To account for potential labor supply responses, they instrument maternal labor supply by labor demand shocks via a shift-share instrument.

the negative effect of the maternal labor supply outweighing the effect of the additional household resources. This is consistent with recent findings by Agostinelli and Sorrenti (2018), Dave et al. (2019) and Page et al. (2019). In particular, Agostinelli and Sorrenti (2018) estimate that the effect of additional family income is close to zero and emphasize the differential impact of family income on cognitive and behavioral outcomes.

Future research should also aim at investigating alternative mechanisms. For instance, psychologists provide a different fruitful avenue to explain our results: in contrast to the preceding perspectives, the resources theory literature in psychology seeks to explain the adverse effects of own education on mental health. Individuals are hypothesized to strive for resources (e.g., objects, characteristics, or conditions) and to preserve and protect existing resources. Any loss of resources is assumed to cause stress (Hobfoll, 1989). Consequently, individuals invest resources to obtain formal qualifications and expect to enhance their resources as a result. If these expectations are not fulfilled, a downward spiral of losses begins to cause psychological distress. Consequently, if individuals expect to enhance their resources because of the increased education of their parents, we would expect individuals to suffer from stress if these expectations are not fulfilled. For instance, if individuals expect to be able to strive for higher school leaving degrees because their parents went to school longer, we would expect that individuals perceive psychological distress if these expectations are not fulfilled. Indeed, based on the same compulsory schooling reform, Piopiunik (2014) shows that education is only transmitted along the mother-son line and not along the mother-daughter line. Thus, if our conjecture is true, we would observe that maternal education causes psychological distress among daughters, which is exactly what we observe.

Our results have important policy implications. Together with the evidence from Kamhöfer et al. (2017), who do not find a protective effect of education on own mental health, our results suggest that policy measures aimed at improving mental health status should not focus too much on education interventions.

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Compliance with Ethical Standards

The authors declare they have no conflict of interest.

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Appendix

Figure A.1: SF-12 questionnaire of the SOEP.

105. How would you describe your current health?

- Very good ☐
- Good ☐
- Satisfactory ☐
- Poor ☐
- Bad ☐

106. When you have to climb several flights of stairs on foot, does your health limit you greatly, somewhat, or not at all?

- Greatly ☐
- Somewhat ☐
- Not at all ☐

107. And what about other demanding everyday activities, such as when you have to lift something heavy or do something requiring physical mobility: Does your health limit you greatly, somewhat, or not at all?

- Greatly ☐
- Somewhat ☐
- Not at all ☐

108. During the last four weeks, how often did you:

	Always	Often	Some-times	Almost never	Never
• feel rushed or pressed for time?	☐	☐	☐	☐	☐
• feel down and gloomy?	☐	☐	☐	☐	☐
• feel calm and relaxed?	☐	☐	☐	☐	☐
• feel energetic?	☐	☐	☐	☐	☐
• have severe physical pain?	☐	☐	☐	☐	☐
• feel that due to <u>physical health problems</u>					
– you achieved less than you wanted to at work or in everyday activities?	☐	☐	☐	☐	☐
– you were limited in some way at work or in everyday activities?	☐	☐	☐	☐	☐
• feel that due to <u>mental health or emotional problems</u>					
– you achieved less than you wanted to at work or in everyday activities?	☐	☐	☐	☐	☐
– you carried out your work or everyday tasks less thoroughly than usual?	☐	☐	☐	☐	☐
• feel that due to physical or mental health problems you were limited socially, that is, in contact with friends, acquaintances, or relatives?	☐	☐	☐	☐	☐

Source: TNS Infratest Sozialforschung (2014).

Table A.1: Calculation of different standard errors

	All	Daughters	Sons
Panel A: Life satisfaction			
Maternal years of schooling	-0.034	-0.093	-0.028
State of schooling-cohort cluster	(0.088)	(0.163)	(0.075)
Wild cluster bootstrap-t (p-value)	0.65	0.31	0.42
Panel B: MCS score			
Maternal years of schooling	-0.078	-0.258*	0.074
State of schooling-cohort cluster	(0.070)	(0.108)	(0.101)
Wild cluster bootstrap-t (p-value)	0.51	0.07	0.72
Panel C: Being at risk of having a mental disorder			
Maternal years of schooling	-0.021	0.113*	-0.049
State of schooling-cohort cluster	(0.028)	(0.054)	(0.038)
Wild cluster bootstrap-t (p-value)	0.60	0.05	0.56

Notes: SOEPv32 waves 2002 to 2015. 2SLS regressions. All regressions include maternal cohort and state of schooling indicators as in addition to linear maternal state of schooling trends. Additionally, all regression include a second-order polynomial in offspring's age in addition to a linear survey year trend. P-values from wild cluster bootstrap-t procedure are calculated using 999 repetitions and Rademacher weights. Significance stars relate to wild cluster bootstrap p-values and read as follows: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table A.2: Individuals' reform exposure and school track choice

DV: Attending a school track higher than the basic track	Females	Males
Reform Exposure	-0.008	0.017
	(0.018)	(0.020)
N	10586	10892

Notes: SOEPv32. OLS regressions. All regressions include individuals' cohort and state of schooling indicators in addition to linear state of schooling trends. Standard errors in parentheses are clustered on the state of schooling level. The sample is restricted to birth cohorts between 1930 and 1960. Significance: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table A.3: Dropping observations from Hamburg and Schleswig-Holstein

	All	Daughters	Sons
Panel A: Life satisfaction			
Maternal years of schooling	-0.057	-0.083	-0.056
	(0.080)	(0.144)	(0.062)
First stage			
Maternal reform exposure	0.978	0.891	1.009
	(0.093)	(0.069)	(0.135)
F-stat first stage	109.88	167.29	56.22
Individuals	3122	1441	1681
Individual-time observations	23217	10975	12242
Panel B: MCS score			
Maternal years of schooling	-0.073	-0.239**	0.084
	0.072	(0.098)	(0.098)
Panel C: Being at risk of having a mental disorder			
Maternal years of schooling	0.020	0.098**	-0.050
	(0.025)	(0.043)	(0.034)
First stage			
Maternal reform exposure	0.968	0.916	0.967
	(0.089)	(0.067)	(0.128)
F-stat first stage	117.10	186.55	57.46
Individuals	3122	1441	1681
Individual-time observations	11656	5486	6170

Notes: SOEPv32 waves 2002 to 2015. 2SLS regressions. All regressions include maternal cohort and state of schooling indicators in addition to linear maternal state of schooling trends. In addition, a second oder polynomial in the offspring's age and a linear survey year trend are controlled for. Standard errors in parentheses are clustered on the maternal state of schooling level. Significance: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

Table A.4: Dropping all individuals born before 1945

	All	Daughters	Sons
Panel A: Life satisfaction			
Maternal years of schooling	-0.203***	-0.140	-0.289**
	(0.068)	(0.110)	(0.116)
First stage			
Maternal reform exposure	0.906	0.943	0.843
	(0.134)	(0.089)	(0.230)
F-stat first stage	45.58	111.80	13.47
Individuals	2595	1235	1360
Individual-time observations	17770	8814	8956
Panel B: MCS score			
Maternal years of schooling	-0.132**	-0.182**	-0.041
	0.063	(0.087)	(0.091)
Panel C: Being at risk of having a mental disorder			
Maternal years of schooling	0.047*	0.086*	-0.004
	(0.026)	(0.044)	(0.036)
First stage			
Maternal reform exposure	0.894	0.959	0.795
	(0.134)	(0.095)	(0.226)
F-stat first stage	44.36	101.84	12.31
Individuals	2595	1235	1360
Individual-time observations	8885	4388	4497

Notes: SOEPv32 waves 2002 to 2015. 2SLS regressions. All regressions include maternal cohort and state of schooling indicators in addition to linear maternal state of schooling trends. In addition, a second oder polynomial in the offspring's age and a linear survey year trend are controlled for. Standard errors in parentheses are clustered on the maternal state of schooling level. Significance: * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.