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Mental health around pregnancy and child development from early childhood to adolescence

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ABSTRACT

Mental health problems during pregnancy affect around 20% of mothers and may have lasting impacts on children's health, cognitive and socio-emotional skills, educational attainment, and future labour market outcomes. We identify the causal effect of mothers' prenatal mental health on a range of child psychological, socio-emotional and cognitive outcomes. Our methodology exploits shocks to mothers' mental health that are induced by illness of the mother's friends or relatives, whilst accounting for the non-randomness of exposure to illness. We find that mothers' mental health problems negatively affect children's psychological and socio-emotional skills in early childhood, but these fade-out between the ages of 11-13. There is no effect on children's cognitive outcomes. Hence, our findings suggest that maternal prenatal mental health may have a limited direct effect on children's future labour market outcomes.

1. Introduction

It is well-known that adverse shocks that occur *in utero* or early in life can have lasting impacts on later-life outcomes (see for example the reviews in Almond and Currie (2011a,b); Currie (2020); Currie et al. (2010)). Such evidence points to the importance of public policies such as health care provision (e.g., medical intervention for low birth weight babies, Bharadwaj et al. (2013) provision of psychotherapy, Baranov et al. (2020)) as well as the importance of parental investments (e.g., breastfeeding, Del Bono and Rabe (2012) empowering mothers, Lavy et al. (2020)) in determining a child's future outcomes, including health, cognitive and socio-emotional skills, educational attainment, and labour market outcomes.

The majority of studies that consider the effects of prenatal shocks focus primarily on the mother's (or child's) physical health.[1] There is, however, a sparsity of evidence on the impact of maternal mental health during pregnancy on later life outcomes of children.[2] There exist at least two reasons for such a lack of evidence. First, since the vast majority of observational studies only include information on mothers following child birth, maternal mental health is generally only observed *after* (as opposed to *during*) pregnancy. Second, maternal mental health is endogenous to child behaviours and outcomes, and locating plausibly exogenous variation is challenging. Given these two constraints, almost all studies that explore the effects of maternal mental health *in utero* use an Intention-To-Treat (ITT) design, estimating the effects of exogenous variation in the environment *assumed to* induce stress/anxiety in pregnant mothers on their child's later life outcomes.

For example, Black et al. (2016) use the event of the death of the mother's parent in a reduced form framework to identify shocks to mothers during pregnancy. Their model estimates the additional effect on child outcomes from shocks occurring whilst *in utero* from effects due to shocks occurring around pregnancy, and argue that this

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[1] Much of this literature specifies exogenous shocks that exploit the effects of e.g. famines (Almond et al. (2010); Scholte et al. (2012)), flu epidemics (Almond (2006); Almond and Mazumder (2005); Kelly (2011)), exposure to smoke from harvest or forest fires (Rangel and Vogl (2019); Rosales-Rueda (2018)), floods (Rosales-Rueda and Triyana (2018)), temperature during gestation (Bruckner et al. (2014)), and exposure to radiation (Almond et al. (2009); Black et al. (2013)). It should not be ruled out, however, that these events may also impact on a mother's mental health.

[2] Poor mental health or stress around the birth of a child is very common: one in five mothers experience clinical depressive symptoms during pregnancy (Marcus et al. (2003)) whilst postpartum depression is common up to a year after birth (Sohr-Preston and Scaramella (2006)). Note that postpartum depression is different to the less severe "baby blues" which affects around 80% of mothers and tends to occur in the first 10 weeks after birth.

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mitigates concerns over confounding that might operate through the death of a parent. Petra and Rossin-Slater (2018) use a broader measure of the event of a death of a family member, and identify *in utero* exposure to stress by comparing outcomes of children whose mother experienced bereavement during pregnancy with those of children whose mother experienced a bereavement in the year following birth. Others stressful events considered are those linked to terrorist attacks or armed conflicts (Brown (2014); Mansour et al. (2012)), natural disasters such as earthquakes, flooding, or hurricanes (Currie and Rossin-Slater (2013); Karbownik and Wray (2019); Simeonova and Emilia (2009); Torche and Florencia (2011)), or the 2008 financial crisis (Olafsson and Arna (2016)). A summary of the findings is that stress during pregnancy causes poor birth outcomes and childhood obesity, but there are no long-run effects on cognitive ability. However, the extent to which these shocks actually affect mental health of the pregnant mother is often unknown, since reliable data on maternal mental health during pregnancy are rarely available.

Baranov et al. (2020) is an important recent contribution to the literature evaluating the medium-term (around 7 years) impact of treatment for maternal depression on women's mental health, empowerment and parental decision making. Evidence is drawn from pregnant women from rural Pakistan diagnosed at baseline to be suffering from depression and who are randomised to receive either Cognitive Behavioural Therapy or standard access to maternal and health care services. They show positive effects of Cognitive Behavioural Therapy on mother's empowerment and parental decision making. However, effects on the cognitive and socio-emotional development of children at age 7 were small and imprecisely estimated. Sample sizes were too small to make meaningful direct comparisons between mothers who at baseline were depressed and mothers who were not depressed.

As such, poor maternal mental health can have long-term deleterious effects on children's development through changes in children's mental and physical health, as well as cognitive and socio-emotional skills. Poor childhood mental health has been shown to reduce children's human capital outcomes (e.g. reductions in educational attainment or cognitive ability; and increases the probability of grade retention and receiving welfare benefits; see e.g. Currie and Stabile (2006); Currie et al. (2010); Salm and Schunk (2012); Webbink et al. (2011)). Furthermore, Smith and Smith (2010) show a reduction in adult earnings and labour market participation due to poor childhood mental health, and Levere (2021) shows that childhood disability income receipt for mental health disorders reduces adult labour income. This suggests that poor maternal mental health may not only affect mothers, but also their children, both during childhood and their later labour market experiences, shaping within-generation as well as intergenerational social mobility (see also Goodman et al. (2011)).

This paper estimates the causal effect of mothers' mental health around pregnancy, on a wide range of child outcomes including psychological, socio-emotional and cognitive outcomes using data from the UK Avon Longitudinal Study of Parents and Children (ALSPAC).[3] The contribution of our paper is four-fold. First, we directly observe a validated measure of psychological health for a large cohort of mothers during and shortly after pregnancy. Accordingly, we do not rely on events such as earthquakes or the death of a family member as proxies, and estimate the effect of actual changes in mental health, as opposed to the reduced form effect of a shock. Indeed, much less is known about the impact of maternal mental health during pregnancy than stress *per se* on child outcomes.

Second, we measure psychological, socio-emotional and cognitive outcomes of children, from as early as 4 years of age up to age 16, allowing us to look at a wide range of outcomes across the life cycle of child development. This is an important innovation. For example Black et al. (2016) find no long-run effect on cognitive outcomes of stress during pregnancy, whilst Persson and Rosales-Rueda (2018) find that there are long-run effects, but on measures including drug use, treatment for depression and medication for attention deficit hyperactivity disorder. These results suggest that mothers' mental health affects different aspects of child development at different points during childhood. In contrast, we observe psychological and socio-emotional outcomes between ages 7-16 and cognitive outcomes between ages 4-16, allowing us to identify the dynamics of the maternal mental health effect across multiple dimensions of child skills that have been shown to predict children's later life health, economic and labour market success.

Third, mothers' mental health around pregnancy can affect child outcomes through several pathways, including i) a biological channel, via increased maternal and/or children's cortisol levels induced by stress and anxiety; and ii) a behavioural response of the mother, changing her investments in the child.[4] An advantage of our data is that we observe a large range of potential behavioural responses during and after pregnancy, including smoking and alcohol consumption during pregnancy, breastfeeding duration and measures of parenting quality. This allows us to examine whether episodes of poor mental health around pregnancy drive parental investments in early childhood.

Fourth, since maternal mental health is endogenous to child outcomes, our identification strategy relies on a set of instrumental variables constructed from unexpected life events that influence psychological well-being. Our methodology is similar to the literature that exploits family bereavement during pregnancy as an exogenous shock to maternal mental health (Black et al. (2016); Le and Nguyen (2018); Petra and Rossin-Slater (2018)). However, we exploit *illness* of a family friend or relative as an exogenous shock to psychological well-being. We therefore identify the effect of the occurrence of a stressful event that is more commonly experienced than bereavement. The event of illness also mitigates against concerns over potential confounding that might operate through changes in income and resources associated with a death of a family member.

We observe maternal mental health at two points around pregnancy – at 18 weeks gestation and at 8 weeks postpartum – and combine these measures using factor analysis to create a latent variable capturing mental health *around* pregnancy.[5] This is our main variable of interest. We then create an indicator for experiencing friends'/relatives' illness between the start of pregnancy and 8 weeks post-birth. This forms our instrumental variable which strongly predicts mothers' mental health.[6] We interpret this as the event inducing a shock to maternal psychological well-being during and/or immediately after pregnancy, which may have long-term effects on child development. The timing of shocks to health have been used elsewhere to derive exogenous variation (see for example, Jones et al. (2020)).

The illness of friends and/or relatives may not occur randomly in the population of mothers. Families whose friends and relatives experience an illness around pregnancy may be different to families not experiencing such events. They may have lower human capital in terms of health, education and socio-economic status more generally, and these traits are likely to be correlated with child outcomes. Moreover, even if the illness of a friend or relative around pregnancy is exogenous, if the illness lasts for a prolonged period beyond pregnancy, it may directly affect child outcomes.

To ensure that our strategy does not pick up variation in mental health driven by unobservable family traits, we control for the experience of illness outside of the period of pregnancy measured by any *new* events of friends'/relatives' illness, occurring between child birth and 8 months post birth. Consequently we identify the effect of mothers' men-

[3] Throughout we use psychological health interchangeably with mental health. These cover the impact of stress/anxiety and depression on the well-being of a mother.

[4] Section 2 lays out concisely the evidence for the potential channels.

[5] We refer to mental health *around* and *during* pregnancy interchangeably.

[6] Note that the instrument cannot be decomposed into illness of a friend and illness of a relative.

tal health during pregnancy on children, by exploiting the incidence of the illness of a friend or relative during pregnancy, conditional on the likelihood of experiencing illness of friends or relatives more generally. In addition, we control for a wide set of socio-economic characteristics of both mothers and grandparents.

A potential further violation of the exclusion restriction is that an illness may reduce interaction time between the child and the friend or relative, which can independently drive child outcomes. For example, if a grandparent was ill around the mothers' pregnancy they may continue to be ill or even die before the child outcomes are observed, reducing time investments of the grandparent. We investigate this indirectly by exploring whether our estimates differ across samples that vary in the quantity of childcare provided by grandparents or friends when the child is aged ~1 $\frac{1}{2}$–3 $\frac{3}{4}$ years. These estimates are statistically indistinguishable, suggesting that this does not drive our results.

Finally, it may be that our first stage regressions are driven by those with a pre-existing mental health issue. We address this concern by controlling for the mother's mental health *before* pregnancy in all benchmark specifications. Furthermore, we show that the conclusions of our paper persist when we restrict the sample to only include mothers *without* pre-existing mental health problems.

Our results show two main findings. First, we find sizeable effects of maternal mental health around pregnancy on children's psychological and socio-emotional outcomes when these are measured early in the child's development. However, the IV estimates suggest that the effects fade out over time, becoming insignificantly different from zero by the time the child is 11–13 years old. This therefore indicates that maternal mental health around pregnancy may have limited direct effects on children's future labour market outcomes.

We find no significant effects on cognitive skills; neither in the short-run, nor in the longer-run. The fact that children's cognitive scores do not respond to changes in maternal mental health due to illness of friends or relatives while other outcomes do respond, suggests that our instrument is not solely reflecting some measure of the socio-economic environment that would be expected to be correlated with all outcomes. Instead, this suggests that our approach to identification performs well in picking up the effect of a deterioration in mental health driven by illness of a friend or relative.

Second, we find little evidence of behavioural responses to poor mental health around pregnancy on a wide range of parenting investments. The measures of parental investments are informed by insights from the biological literature about maternal behavioural responses to stress and range from health behaviours during pregnancy, scores representing the quantity and quality of mother-child interactions, and monetary investments in children. The lack of evidence for non-biological channels suggests that biological effects may be driving the treatment effects in early life.

2. Literature relating prenatal stress to child outcomes

This section presents evidence on the channels through which maternal mental health problems around pregnancy may have a meaningful impact on the development of her child. A predisposition to mental health has a genetic component, so a mother's mental health may directly affect her child's psychological and socio-emotional well-being. During pregnancy, however, evidence also suggests a strong biological and behavioural response to maternal mental health.

A biological mechanism exists when, during pregnancy, stress of the mother raises her levels of cortisol. Maternal cortisol has been linked to many child outcomes, including lower fetal growth and birth weight (e.g. Gitau et al. (1998)), and lower years of education potentially exacerbating intergenerational persistence of poverty (Aizer et al. (2016)). There is accumulating evidence that elevated maternal cortisol concentrations during pregnancy are associated with fetal brain anatomy. For example, Li et al. (2011), find evidence of reduced fetal brain growth following late gestation increases in maternal serum cortisol. Other studies report an increased risk of stress, social anxiety and internalizing behaviours of newborns and infants (for example, Davis (2007); Davis et al. (2011); LeWinn et al. (2009) report lowered cognitive skills in offspring.

More recent interest has focused on the particular role of the amygdala in a child's development, since this regulates a variety of emotions including fear, depression and anxiety. The amygdala develops at an early embryonic stage, is associated with a range of neurodevelopmental and psychopathological disorders, and is believed to be sensitive to elevated levels of cortisol. Buss et al. (2012) find that higher maternal cortisol concentrations in early gestation are associated with larger right amygdala volume in girls at 7 years, which is consistent with the suggestion that negative emotions are predominantly processed in the right hand side of the amygdala. It has also been found that exposure to high levels of stress in early postnatal life is associated with an altered and enlarged amygdala and an increase in anxiety disorders (for example, in children reared in orphanages; Tottenham et al. (2010)). Similarly, maternal postnatal depression has also been found to be associated with larger amygdala volumes in preadolescent children (Lupien et al. (2011)).

In additional to any observed biological link, both pre- and post-natal maternal mental health problems may also lead to *behavioural* responses, where mothers are less likely to seek prenatal care (Miller (1992)), gain less weight during pregnancy (Walker et al. (1999)), use more drugs and alcohol, smoke more and feel more stressed (Zuckerman et al. (1989)). These factors can independently drive later outcomes of the child, either directly via health endowments or indirectly through child investments. We explore this empirically in more detail below.

3. Data

Our data are from a cohort of children born in one geographic area (Avon) of England. Women resident in Avon with an expected delivery date between 1st April 1991 and 31st December 1992 were invited to take part in the population-based Avon Longitudinal Study of Parents and Children (ALSPAC). The initial number of pregnancies enrolled is 14,541. Of these initial pregnancies, there were 14,676 foetuses, resulting in 14,062 live births and 13,988 children who were alive at 1 year of age. ALSPAC is a cohort; there is no systematic data collection on siblings. As is common in longitudinal surveys, attrition leads to fewer individuals being observed over time. Depending on the outcome of interest, the final sample varies but includes a maximum of 7,773 mother-child pairs.

The Avon area has approximately 1 million inhabitants and is broadly representative of the UK as a whole, although slightly more affluent than the general population (for more information, see Boyd et al. (2013); Fraser et al. (2013)).[7]

3.1. Maternal mental health

One advantage of the ALSPAC data is the detailed information available *during* pregnancy, as most other birth cohorts start data collection after the child has been born, and therefore need to rely more on retrospective data collection (von Hinke and Jones (2015)). We exploit this

[7] See http://www.bristol.ac.uk/alspac for a more detailed description of the data. The study website contains details of all data that are available through a fully searchable data dictionary and variable search tool see http://www.bristol.ac.uk/alspac/researchers/our-data/. At age 7, an attempt was made to bolster the initial sample with eligible cases who had failed to join the study originally. As a result, when considering variables from age 7 onwards, there are data available for more than the 14,541 pregnancies mentioned above. However, this does not apply here, as our analysis requires us to observe maternal mental health around pregnancy, which is only available for those who joined the study originally.

by specifically investigating the effect of maternal mental health measured around pregnancy; we use mental health measured at 18 weeks gestation and 8 weeks post-birth.[8] While these capture mental health at specific points in time, we do not know for how long between these points a mother has experienced problems. Accordingly, we combine the two measures into one latent factor measuring mental health around pregnancy. In the factor analysis, the factor loading for mental health at 18 weeks gestation was normalised to one and the factor loading for mental health at 8 weeks postpartum took the value of 0.567.

Our measure of maternal mental health is the Crown-Crisp Experiential Index (CCEI) that captures a broad definition of mental health.[9] A high value of the CCEI indicates that mothers are more affected, or have worse mental health. The CCEI consists of three subscales: anxiety, depression and somaticism. Each measures specific attributes of mental health, though our analysis uses the overall score as a more general measure of the mother's state of mental health. The clinical threshold to indicate depression is a score of 8 for each subscale, or 24 for the combined score (e.g. Capron et al. (2015); Heron et al. (2004); Taylor-Robinson et al. (2011)). Figure A.1 shows the unstandardised CCEI distribution at 18 weeks gestation and 8 weeks post birth, with the vertical line indicating the threshold at 24. In the sample, 9.7% of mothers have a CCEI score above the threshold at 18 weeks gestation and 4.75% at 8 weeks post birth. This suggests that the proportion of mothers suffering from depression, whilst not large, is not trivial either.

In addition to these two measures of maternal mental health, we observe a binary indicator that measures whether the mother ever had depression prior to pregnancy. We include this in the analyses, aiming to capture and account for a maternal baseline measure of mental health.

3.2. Child outcomes

ALSPAC contains an extremely rich set of child outcomes, including psychological, socio-emotional, cognitive and behavioural outcomes. We focus on child depression measured at 9 $\frac{1}{2}$ (115 months) and 11 $\frac{3}{4}$ (140 months) as a measure of psychological outcomes.[10] This is based on the short Mood and Feelings Questionnaire (MFQ), a validated measure of depressive symptoms (Angold et al. (1995) also see Messer et al. (1995)). The measure, based on a 13-item checklist, is designed for the evaluation of core depressive symptomatology. Parents complete the questionnaire on behalf of their children. Higher scores imply greater depressive symptoms. Figure A.2 shows the distribution of the MFQ at ages 9 $\frac{1}{2}$ and 11 $\frac{3}{4}$. According to the Child Outcomes Research Consortium guidelines[11], a score of 12 or higher may indicate the presence of depression in children. Applying this cut-off, 2.14% and 2.29% of our sample experience depression at age 9 $\frac{1}{2}$ and 11 $\frac{3}{4}$ respectively, suggesting that relative few children are clinically depressed.

We use the Strengths and Difficulties Questionnaire (SDQ) as a measure of socio-emotional development. The SDQ is a well recognised and validated measure of child behaviour, developed as a mental health screening instrument (see Goodman (1997)). The parent is asked 20 questions in total, which are grouped into dimensions of behaviour including emotional problems, conduct problems, hyperactivity, and peer problems. We use the total score which is a composite of these four dimensions. Of particular use to our study is that SDQ is measured across a range of child ages; at 6 $\frac{3}{4}$, 9 $\frac{1}{2}$, 11 $\frac{3}{4}$, 13, and 16 $\frac{1}{2}$.[12]

Table 1
Descriptive statistics for mothers' mental health, child outcomes, instrumental variables.

	N	Mean	Standard Deviation
Mothers' mental health			
Mothers mental health (CCEI) 18 weeks gestation	7,773	-0.086	0.956
Mothers mental health (CCEI) 8 weeks post birth	7,773	-0.067	0.936
Mothers mental health (CCEI) during pregnancy	7,773	0.000	0.956
Child outcomes			
Depression 9 $\frac{1}{2}$ years	5,607	-0.018	0.974
Depression 11 $\frac{1}{2}$ years	5,155	-0.018	0.984
Strengths difficulties 6 $\frac{3}{4}$ years	6,101	-0.030	0.987
Strengths difficulties 9 $\frac{1}{2}$ years	5,588	-0.043	0.973
Strengths difficulties 11 $\frac{1}{2}$ years	5,180	-0.035	0.977
Strengths difficulties 13 years	4,289	-0.030	0.988
Strengths difficulties 16 $\frac{1}{2}$ years	3,208	-0.020	0.984
Entry school assessment	5,307	0.149	0.960
Key stage 1 (age 7)	6,955	0.167	0.940
Key stage 2 (age 11)	6,627	0.171	0.908
Key stage 3 (age 14)	6,075	0.190	0.934
Key stage 4 (age 16)	6,720	0.183	0.922
Instrumental Variable			
Friend/relative ill	7,773	0.297	0.455

The child's cognitive outcomes are measured by their performance on different tests of academic achievement. First, we use an entry assessment test, taken by all pupils about to start primary school (age 4). Although there were no compulsory national assessment tests at this time, the Local Education Authorities covering the ALSPAC area used the same tests, which is available for 80% of (not privately owned) schools. In addition, we use four nationally set examinations taken at ages 7, 11, 14 and 16 (also known as the Key Stage 1 (KS1), Key Stage 2 (KS2), Key Stage 3 (KS3) and Key Stage 4 (KS4, or GCSE) examinations, respectively). With the exception of the age 16 test score outcomes (KS4), the examinations are low-stakes for the children as they are all entitled to progress to the next stage of schooling irrespective of the results. However, schools are judged on the performance of all test scores and consequently the test scores are high stakes to some degree for teachers and schools. KS4 is high stakes for the children also, as it determines whether the children are able to progress into higher education up to age 18.

Children's scores are obtained from the National Pupil Database, a census of all pupils in England within the state school system, which is matched into ALSPAC. For each of the Key Stage tests (1-4), we use an average score for the child's mandatory subjects.[13] All child outcomes have been standardised on the full sample of children for whom data are available, with mean 0 and standard deviation 1. Descriptive statistics for mother mental health and child outcomes are reported in Table 1.

3.3. Instrumental variables

We create an instrumental variable for mothers' mental health around pregnancy by combining two questions on the illness of a friend or relative. At 18 weeks gestation mothers provided information on whether they had experienced the illness of a friend or relative *since she became pregnant*. Similarly, at 8 weeks post birth the mother was asked whether, *since the middle of her pregnancy*, she had experienced the event of a friend or relative being ill. The timing of the events align

[8] The data also includes a measure of maternal mental health at 32 weeks gestation. We do not use this in our analysis, since the period between the start of pregnancy and 8 weeks post-birth overlaps perfectly with the period covered by the instrumental variable.

[9] This is a validated measure; see e.g. Birtchnell J et al. (1988); Ross and Hafner (1990)). Our results are similar if we specify mothers' Edinburgh Postnatal Depression Score (EPDS), capturing the extent to which the mother is at risk of perinatal depression, as the measure of mental health (Murray and Carrothers (1990)).

[10] ALSPAC specifies ages in months, but for ease of interpretation, we refer to them in year equivalents, rounded to the nearest quarter.

[11] Child Outcome Research Consortium.

[12] We also explore the individual sub-scores below.

[13] For KS1, this is an average of the child's reading, writing, spelling and mathematics scores; KS2 includes reading, writing, science and mathematics. For KS3 and KS4, the final score is an average of the child's English, mathematics and science.

Table 2
Descriptive statistics for covariates.

	N	Mean	Standard Deviation
Mother level variables			
Mother had depression	7,773	0.072	0.259
Mother age	7,773	28.868	4.504
Mum education: CSE/none	7,773	0.126	0.332
Mum education: Vocational/O level	7,773	0.460	0.498
Mum education: A level	7,773	0.261	0.439
Mum education: Degree	7,773	0.153	0.360
Mum class: I - Professional	7,773	0.062	0.241
Mum class: II - Managerial	7,773	0.323	0.468
Mum class: IIInm - Skilled non-manual	7,773	0.435	0.496
Mum class: IIIm - Skilled manual	7,773	0.072	0.258
Mum class: IV - Semi-skilled / unskilled	7,773	0.108	0.310
Pregnancy intentional	7,773	0.756	0.429
Friend/relative ill 8 months	7,773	0.208	0.406
Child level variables			
Child female dummy variable	7,773	0.483	0.500
Birth order	7,773	2.130	2.248
Month of birth	7,773	5.608	3.692
Child non-white	7,773	0.038	0.191
Additional variables			
Grandparent childcare age 1 $\frac{1}{2}$	7,323	0.456	0.498
Grandparent childcare age 2	7,027	0.451	0.498
Grandparent childcare age 3 $\frac{1}{4}$	6,986	0.439	0.439

Notes: CSE and O-levels refer to examinations taken at age 16, and A-levels at age 18.

with our measure of maternal mental health. Hence, we construct a binary indicator, which takes the value 1 if the mother experienced either of these and 0 otherwise. The questions ask about *any* friend or relative. Hence, this may include anyone from a friendly neighbour to a great-aunt, meaning that many mothers may experience such an event. Table 1 shows that 30% of mothers experienced a friend or relative who was ill between the start of pregnancy and 8 weeks post birth.[14]

To account for the fact that experiencing a friend's or relative's illness may be non-random, we condition additionally on the same event occurring between birth and 8 months post-birth. Specifically, we control for a dummy variable indicating that the mother experienced the new event of a friend's or relative's illness between birth and 8 months later. Approximately 21% experienced such an event (see Table 2). This period overlaps with the period measured at 8 *weeks* post birth - the overlapping period is between birth and 8 weeks post birth. Once we condition on the event at 8 months, our instruments are identifying the effects of maternal mental health *around* pregnancy from illnesses that occurred *during* pregnancy.

3.4. Covariates

We exploit the richness of the data and include a wide range of control variables, aiming to capture baseline mental health, as well as other maternal and household characteristics such as socio-economic status of the child's parents and grandparents. These are important, as they may be correlated with the measures of maternal mental health and they may independently drive child outcomes. Furthermore, they will absorb some of the unobserved residual heterogeneity that may otherwise be correlated with the instrumental variables and the child outcomes. For example, it may be that only mothers of low socio-economic status have friends or relatives who became ill during pregnancy. This source of correlation is controlled for by including a quadratic in maternal age at birth, dummy variables for maternal education, the mother's mother's and mother's father's education, dummy variables for the mother's social class and additionally a dummy variable indicating whether the pregnancy was intentional. Finally, we account for a set of variables relating to the child, including the child's gender, their month of birth, and an indicator for being non-white.

Table 2 presents the descriptive statistics of all covariates, showing that 7.2% of mothers reported ever having had depression in the past and mothers are on average 28.8 years old at the birth of the child. The majority of mothers have achieved O-levels[15] (46%) and are in the skilled non-manual social class (44%); 76% of pregnancies were intentional. Furthermore, looking at the covariates relating to the children, we see that just under half of all cohort members are girls, and 3.8% are non-white. We additionally include birth order to control for potential effects on cognitive (e.g. Black et al. (2005)) and non-cognitive (e.g. child delinquent behaviours, Breining et al. (2020)) outcomes.

A schematic timeline of the occurrence of key events for ALSPAC mothers and children, together with the timings of outcomes, measurement of controls and maternal mental health, is provided in Fig. 1.

The final three variables in Table 2 report the statistics for three variables used in the sensitivity analysis, namely indicator variables for whether grandparents provide childcare when the cohort member was aged 1 $\frac{1}{2}$; 2 years and 3 $\frac{1}{4}$. Around 44-46% of grandparents provide such care.

4. Methodology

We consider the following model for child outcomes

$$y_i = \alpha + \beta MH_i + \delta X_i + u_i, \tag{1}$$

where y_i denotes a set of outcomes for child i, including psychological, socio-emotional and cognitive outcomes. MH_i denotes the mothers' mental health around pregnancy, constructed as a latent factor as described in Section 3.1. X_i denotes a set of covariates, including an indicator for the illness of a friend or relative occurring between birth and 8 months post birth, mothers' depression before pregnancy, child gender, ethnicity, dummies for month of birth, birth order, a quadratic in the mothers' age at birth, mother's education and social class, grandparents' education, and an indicator for whether the pregnancy was planned. u_i denotes an idiosyncratic error term. We report heteroscedasticity robust standard errors throughout.

The mental health of mothers is likely to be endogenous. For example, environmental factors such as socio-economic position may drive the mental health of mothers and may persist to drive the outcomes of children. Similarly, mental health is likely to be measured with error, leading to an attenuation bias.

To deal with both sources of endogeneity, we use an instrumental variables approach. For this, we adopt an identification strategy similar to Black et al. (2016) and Petra and Rossin-Slater (2018) and exploit the fact that ALSPAC mothers were asked to provide details on certain 'life events' that occurred at specific times during and shortly after pregnancy. Rather than using the *death* of a relative we use information on whether a mother experiences an *illness* of a friend or relative; an event more common during pregnancy. We use an indicator for experiencing illness of a friend or relative between the start of pregnancy and 8 weeks post birth as an instrumental variable for maternal mental health around pregnancy.[16]

[14] Data records whether the mother's partner experienced the illness of a friend or relative. Whilst the occurrence of the mother's friends'/relatives' illness is a plausible shock-inducing event affecting a mother's mental health, we expect a similar event related to the mother's partner to have a smaller impact due to a weaker relationship bond to the individual experiencing the illness. Our analysis confirms a far weaker relationship between mothers' mental health and illness of the partner's friends/relatives than illness information directly related to the mother. We therefore do not exploit information on partner's friends/relatives in the IV strategy.

[15] These are examinations comparable to the current GCSE exams taken at age 16.

[16] We show below that the instrumental variables satisfy the relevance assumptions as they are strong predictors of mothers' mental health around pregnancy.

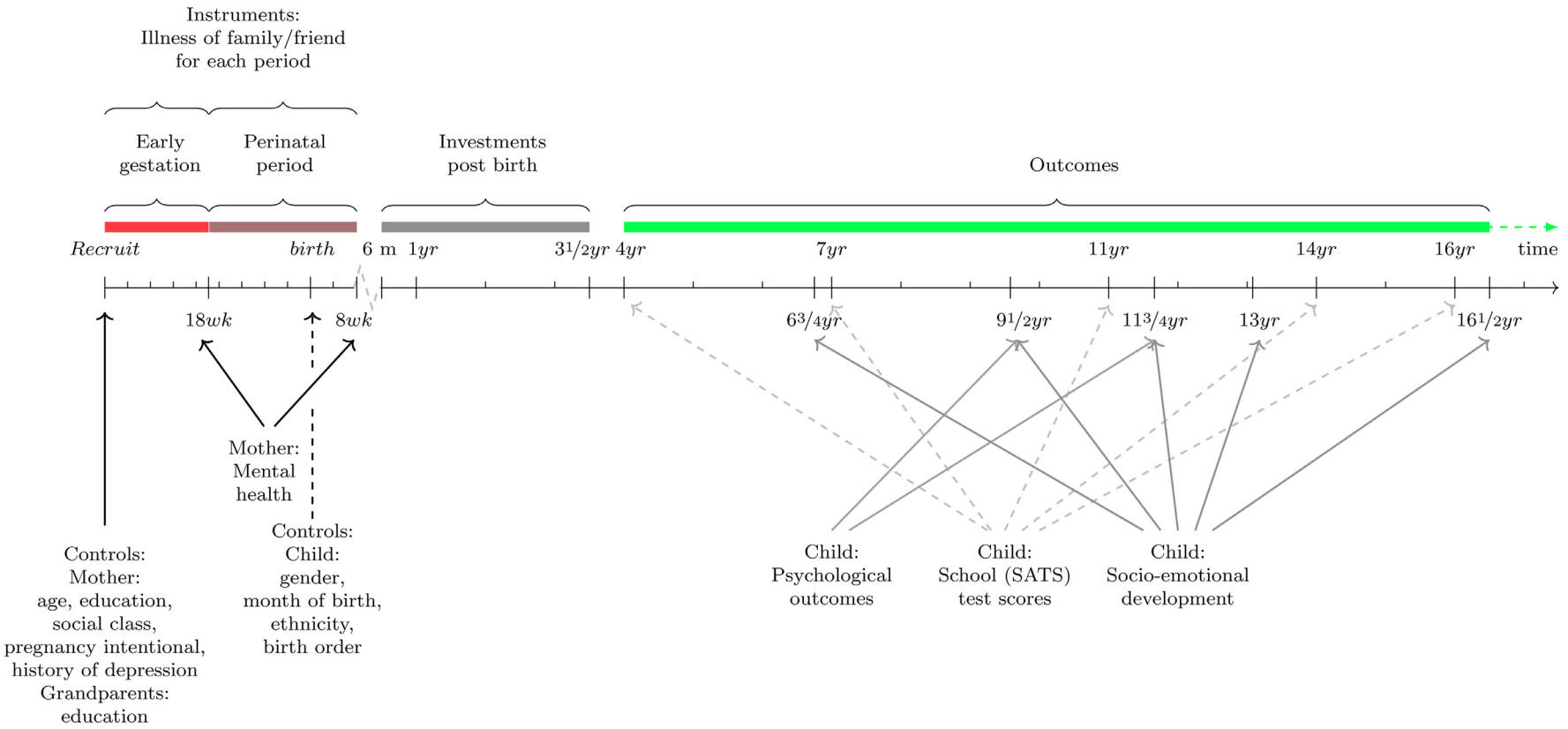

Fig. 1. Timeline (months then years) of main outcomes, controls and measures of maternal mental health.

The exclusion restriction assumes that the instrumental variables do not directly drive child outcomes except through the pathway of mothers' mental health. Using family bereavement as an instrument, Black et al. (2016) point out several potential violations of this assumption. They indicate that an early death of a grandparent may increase financial resources through inheritance, indicate poor health endowments, and/or may lead to a reduction in childcare.

In our case, using *illness* of a friend or relative as an instrument, the potential threats to identification are slightly different. First, we do not have to worry about any income shock through inheritance that may affect child investments. However, the biggest difference is that illness experienced during pregnancy may be a prolonged event which affects child development long after pregnancy. For example, if a grandparent became ill during pregnancy and passed away several years later, the illness can not be viewed as a shock that affects mothers during pregnancy only. Instead, the prolonged illness may affect mothers' time investments in children long after the pregnancy, and therefore drive child human capital. In addition, similar to the instrumental variable of bereavement, families whose friends and relatives experience an illness during pregnancy may be different to other families not experiencing such events. They may have lower human capital in terms of health, education and socio-economic status more generally and these traits are likely to be correlated with child outcomes.

To minimise the extent of these issues, our benchmark specification conditions on many measures of the mothers' socio-economic status, including her social class and education, as well as the education of her parents. Additionally we control for a new event of the incidence of illness of a friend or relative *after* pregnancy, measured at 8 months post birth. This aims to capture potential unobserved differences between families who experience illness and those who do not. To reassure that the illness of a friend or relative during pregnancy is not related to differences in the childcare provision, we stratify our regressions by whether grandparents or friends provided childcare when the child was between $1\frac{1}{2}$ and $3\frac{1}{4}$ years old. The ability of the grandparent or friend to provide this care will be reduced if their illness episode continued after pregnancy. Our results are not statistically different across groups, suggesting that a potential reduction in childcare is not driving our findings.

5. Results

5.1. Main findings

Our main results are provided in Tables 3 and 4. For each outcome, we report OLS results in the first row and IV results in the second row. In addition, each panel reports the coefficient on the instrument from the first stage equation as well as the F-statistic for instrument relevance.

5.1.1. Child psychological health

The first set of outcomes, presented in Table 3, include measures of child psychological health. The OLS results indicate that a standard deviation increase in (i.e. deterioration of) mothers' mental health around pregnancy raises the depression score of their child by 22.5% and 23.8% of a standard deviation at ages $9\frac{1}{2}$ years and $11\frac{3}{4}$ respectively. When moving to the IV estimates, the effect of mothers' mental health persists. The IV estimates exhibit less precision than OLS results. However, they remain statistically significant, where a standard deviation increase in mothers' mental health problems raises child depression by 86.0% of a standard deviation at age $9\frac{1}{2}$. However, by age $11\frac{3}{4}$, the coefficient has fallen to 39.6% of a standard deviation. The first stage F-statistics are all at a level to suggest they are strong predictors of mothers' mental health around pregnancy.[17]

Table 3
Effect of mothers' mental health on child depression.

	(1)	(2)
Child age	$9\frac{1}{2}$ years	$11\frac{3}{4}$ years
OLS	0.225***	0.238***
	(0.016)	(0.017)
IV	0.860***	0.396*
	(0.278)	(0.234)
Observations	5,592	5,145
F statistic 1st Stage	19.47	19.84
First stage coefficient	0.121***	0.128***
	(0.027)	(0.029)

All regressions control for child gender, birth order, ethnicity, dummies for month of birth, a quadratic in mothers' age at birth, mothers' education and social class, grandparents' education, whether mother had experienced depression in the past, whether the pregnancy was planned, and illness of friend or relative between child delivery and 8 months post birth. Instrumental variable: illness of a friend or relative between the start of pregnancy and 8 weeks post birth. *, **, and *** indicate $p < 0.10$, 0.05 and 0.01 respectively.

Although this appears a large effect, it does not mean that maternal prenatal mental health problems drive many children to depression. Indeed, to gauge the magnitude of the shift, we need to relate the changes in the child depression score to the mean as well as to the threshold indicating depression (i.e., a score over 12, as defined by the Child Outcome Research Consortium; see Section 3). The unstandardised mean (standard deviation) depression score at age $9\frac{1}{2}$ and $11\frac{3}{4}$ is 2.5 (3.1) and 2.5 (3.2). Table 3 shows that worsening mothers' mental health by one standard deviation raises the depression score by 0.86 and 0.40 of a standard deviation at ages $9\frac{1}{2}$ and $11\frac{3}{4}$, respectively. This implies an increase in the average depression score to 5.2 at age $9\frac{1}{2}$ and to 3.8 at age $11\frac{3}{4}$ (both from a mean of 2.5). These values are still far below the threshold of 12, suggesting that maternal mental health problems around pregnancy have quantitatively relatively small effects on child psychological health.

One advantage of our methodology and dataset compared to the existing literature is the ability to measure mothers' mental health around pregnancy. Rather than relying on an ITT reduced form estimation of a stressful event on child outcomes, we construct an instrumental variable for stressful events from which we can identify the Local Average Treatment Effect (LATE) of mothers' mental health. We report the estimated coefficients of the instrumental variable from the first stage regressions in Tables 3 and 4. The first-stage coefficients in Table 3 suggest that exposure to illness of a friend or relative during pregnancy worsens a mother's mental health by between 0.121 to 0.128 standard deviations. The corresponding raw (i.e., unstandardised) means (and standard deviations in parentheses) of the CCEI scores are 13.0 (7.4) and 9.1 (6.7) respectively at 18 weeks gestation and 8 weeks post birth. This suggests that the incidence of illness moves mothers from the average CCEI to a score of around 14 (from 13) and 10 (from 9.1) at 18 weeks gestation and 8 weeks post birth respectively. Using the threshold of 24 to define depression and estimate a linear probability model, we find that experiencing illness of a friend or family increases the probability of depression by 2.6 percentage points (not shown here, but available upon request).

For both columns, the IV estimate is larger than the OLS estimate. Analysis in Section 5.2 strongly suggests the reason is that we identify

The death or illness of a relative or spouse has also been used as an exogenous shock in other contexts. For example, Oswald et al. (2015) show that individuals who report to have experienced recent tragedies (defined as bereavement or illness in the family) are disproportionately the ones who report lower happiness and who had lower productivity. They interpret these events as "unhappiness shocks".

[17] The first stage F statistic for the IV regressions varies across specifications, mainly due to small differences in the sample size (the covariates and instruments employed in the first-stage regression are constant across models within each panel, but the outcomes differ).

Table 4
Effect of mothers' mental health on child socio-emotional (SDQ) and cognitive outcomes.

A: Socio-emotional outcomes	(1)	(2)	(3)	(4)	(5)
Child age	6 $\frac{3}{4}$ years	9 $\frac{1}{2}$ years	11 $\frac{3}{4}$ years	13 years	16 $\frac{1}{2}$ years
OLS	0.249***	0.239***	0.227***	0.203***	0.192***
	(0.014)	(0.015)	(0.015)	(0.018)	(0.021)
IV	0.649***	0.645***	0.426*	0.261	0.261
	(0.232)	(0.242)	(0.228)	(0.358)	(0.330)
Observations	6,080	5,573	5,170	4,279	3,204
F statistic 1st Stage	22.93	20.29	21.55	9.197	11.11
First stage coefficient	0.127***	0.124***	0.133***	0.094***	0.118***
	(0.027)	(0.027)	(0.029)	(0.031)	(0.035)
B: Cognitive outcomes	(1)	(2)	(3)	(4)	(5)
Child age	School Assessment	KS1	KS2	KS3	KS4
Outcome	Entry	7	10	13	15/16
OLS	-0.048***	-0.030**	-0.033***	-0.043***	-0.045***
	(0.013)	(0.012)	(0.012)	(0.012)	(0.012)
IV	0.336	0.094	0.005	0.173	0.080
	(0.242)	(0.221)	(0.220)	(0.272)	(0.236)
Observations	5,287	6,928	6,600	6,049	6,693
F statistic 1st Stage	16.51	17.27	15.80	11.57	15.49
First stage coefficient	0.118***	0.104***	0.103***	0.092***	0.101***
	(0.029)	(0.025)	(0.026)	(0.027)	(0.026)

All regressions contain the set of controls listed in Table 3.

a LATE from more vulnerable mothers whose mental health response to the shock during pregnancy is relatively large. We come back to this below.

5.1.2. Child socio-emotional skills

Deming (2017) argues that socio-emotional skills are increasingly predictive for longer run labour market achievement. We explore the effect of mothers' mental health around pregnancy on a range of socio-emotional skills of children, captured by the Strengths and Difficulty Questionnaires (SDQ) measured between ages 6 $\frac{3}{4}$ and 16 $\frac{1}{2}$. The results in Panel A of Table 4 again suggest very strong and consistent effects of mothers' mental health around pregnancy on SDQ scores in the OLS regressions.

The first stage coefficients on the instrument are comparable to those reported in Table 3: experiencing illness of a friend or relative during pregnancy worsens mothers' mental health score by 9-13% of a standard deviation. For the IV analyses, the effect of mothers' mental health shows a clear fade-out across child age. At 6 $\frac{3}{4}$ and 9 $\frac{1}{2}$ years, a one standard deviation increase in mothers' mental health issues raises child SDQ problems by 64.9% and 64.5% of a standard deviation, respectively, dropping to 42.6% of a standard deviation at age 11 $\frac{3}{4}$. The magnitude of this effect falls further as children age, with the coefficient no longer statistically significant at age 13 years onwards.

Given that the child's SDQ score is assessed by their parent, it is possible that the observed fade-out with child age reflects a change in reporting behaviour as a mother recovers from poor mental health during pregnancy. However, this is unlikely given that fade-out appears to start when the child is approximately 10 years of age; far beyond the typical improvement in maternal mental health post childbirth. Alternatively, selective sample attrition as the cohort ages may be responsible for the observed fade-out effect. We explore this in Section 6.2.1 and provide evidence that the estimates pick up a reduction in the effect size as children age, rather than potential non-random sample attrition.

We analyse SDQ as an aggregate measure. It is, however, constructed from four sub-scores: hyperactivity, emotional symptoms, conduct problems and peer problems. Table A.1 reports results using each of the component scores separately. The results are broadly in line with those reported in Table 4 in that IV results fade out as children age. There are two notable differences. For conduct problems, the IV estimates remain large across the full age range, and for peer problems, the IV estimates fade out much more rapidly and is only large and significant at age 6 $\frac{3}{4}$.

5.1.3. Child cognitive outcomes

Finally, we consider cognitive outcomes of the children in Panel B of Table 4. OLS estimates suggest that mothers' poor mental health is associated with lower cognitive achievement across all compulsory schooling ages, with a 1 standard deviation deterioration in mental health reducing children's test scores by 3-5% of a standard deviation. However, once we instrument for maternal mental health, the estimates are no longer statistically significant. The general finding of no significant effect on cognitive outcomes is in line with Black et al. (2016) who find no effect of bereavement during pregnancy on later cognitive outcomes of children.

In addition, the fact that children's cognitive scores do not respond to changes in maternal mental health due to illness of friends or relatives also suggests that our instruments are not solely picking up some measure of the socio-economic environment. Indeed, if they were, given that all child outcomes used here have been shown to be positively correlated with socio-economic position of the household, we would expect the IV estimates to show a similar negative effect of maternal mental health on all child outcomes - psychological, socio-emotional, as well as cognition - and across measures recorded at different child ages. We do not find this, suggesting that the IV estimates perform well in picking up the effect of a deterioration in maternal mental health driven by illness among mothers' relatives/friends.[18]

5.2. LATE interpretation

The IV estimates of the effect of mothers' mental health during pregnancy on child depression and socio-emotional skills outcomes are larger than the OLS estimates. This may indicate measurement error in our measure of mothers' mental health. Or it may be a consequence of estimating a LATE, where the estimate is identified off a specific group of mothers who have a different treatment effect from the general population of mothers. We explore this in more detail in two ways. First, it may be that mothers who are more likely to respond to the instrumental variable may be those with inherent mental health issues. Whilst we do control for the mothers' baseline mental health, we repeat our

[18] We provide further evidence on the validity of the instrument and the relationship between the instrument and the mothers' socio-economic characteristics in Section 6.

analysis dropping mothers with previous experience of depression pre-pregnancy. As only 7.2% of mothers report having experienced depression in the past (see Table 2), Table A.2 shows that excluding these mothers does not substantially change our results.

To explore the possibility that we are estimating a LATE, we next characterise the sample of 'compliers' by estimating the first stage coefficients across different subsamples of mothers.[19] This will illustrate heterogeneity in the maternal prenatal mental health response to illness of a friend or relative. Table A.3 reports the first stage coefficients, showing that mothers with low education; an unintended pregnancy or previous incidence of depression have higher first stage coefficients compared to the full sample. However, when comparing child sub-samples, such as girls or boys, we observe very similar first stage coefficients. These results suggest that more vulnerable mothers are at a greater risk of mental health problems when experiencing illness of a friend or relative around pregnancy. Therefore, combined with treatment effect heterogeneity, the IV estimates are likely higher than the OLS because they estimate a LATE with a relatively high weight for these policy relevant groups of vulnerable mothers.

5.3. Reduced form estimates

Table A.4 estimates the reduced form estimates of the instrumental variable, the illness of a friend or relative between the date of the child delivery and 8 months post birth, on the full range of child outcomes. The results are in line with our benchmark estimates. Illness increases the likelihood of child depression at both 9 $\frac{1}{2}$ and 11 $\frac{3}{4}$ years and has an effect on socio-emotional outcomes which is largest at 6 $\frac{3}{4}$ years but falls across child age. Similarly to the IV estimates, the reduced form effect is no longer statistically significant from age 13 and the magnitude of the coefficient is much reduced. There is no statistically significant reduced form effect of the incidence of friend or family illness on child cognitive outcomes.

5.4. Heterogeneity by gender

There is evidence from studies of fetal brain development that elevated maternal cortisol concentrations are associated with enlarged right amygdala volume in girls (Buss et al. (2012)). This suggests child gender differences in the impact of maternal mental health and stress. We test this assumption by repeating the analysis on outcomes where an interaction term is added between mothers' mental health and an indicator for a female child. The results are reported in Tables A.5-A.6. In all regressions, both the main effect and interaction terms for maternal mental health are instrumented. The interaction term is significant at the 10% level for socio-emotional outcomes at ages 6 $\frac{3}{4}$ and 9 $\frac{1}{2}$ years, suggesting slightly larger effects for girls than boys, at least for socio-emotional outcomes.

6. Robustness analysis

6.1. Validity and interpretation of instrumental variables method

Our instrumental variable is an indicator for the mother experiencing the illness of a friend or relative during pregnancy. In this section we explore the validity and interpretation of the instrumental variables estimates.

6.1.1. Exogeneity of the instrument

One potential threat to the validity of the instrument is that illness of a friend or relative may not occur randomly across mothers, and might be confounded with unobservable factors. While this cannot be tested directly, we offer reassurance that this is unlikely to be the case in our data. Should illness be non-randomly experienced, it is likely that less educated, lower social class mothers experience a greater proportion of illness events than their more educated, higher social class counterparts, and that their children will face poorer later life outcomes. We explore this in more detail in Table A.7, presenting the results of two sets of regressions. In the first column illness is regressed separately on each of the control variables specified in our main model. This shows that the instrument is correlated with the socio-economic status of mothers. The second column is a regression of illness jointly on all the control variables. Few of the variables remain significant when controlling for the full set of controls suggesting that mothers who experience illness during pregnancy tend not to be systematically different to those who do not experience illness, conditional on covariates.[20]

As a further check on the influence of socio-economic controls on the instrument, Table A.8 reports the coefficient on the instrument from the first stage regression in the benchmark model, including our full set of controls (column 1) and in a regression with no controls (column 2). Column 3 reports t-statistics for the hypothesis that the coefficients in columns 1 and 2 are equivalent and column 4 reports the number of observations. The results show that first stage coefficients from the two models are very similar and not statistically different. This provides further supportive evidence of the validity of our instrumental variable, suggesting that it is not highly correlated with the set of controls and therefore that mothers who experience illness during pregnancy are not systematically different from mothers who do not experience illness.

6.1.2. Grandparent time inputs

Another potential threat to the validity of our instrumental variables strategy would occur if the exclusion restriction failed and the instruments directly affected child outcomes. One channel in particular would be if the friend or relative of the mother spent less time with the child in the period between birth and the outcome being measured, as a result of their illness during pregnancy. For example, if a grandparent was ill during pregnancy, this may create mental health problems for the mother but also reduce the interaction time between the child and their grandparent. In the extreme, the grandparent who was ill during pregnancy may have a higher probability of death and which could independently drive child outcomes. However, including the death of a relative or friend measured at 8 months post birth as an additional covariate does not change our results, ruling out this immediate channel (not shown here, but available upon request).

Furthermore, the data contain information on the childcare provided by grandparents and friends at different child ages. Table A.9 shows the results where we split the sample by whether or not a grandparent carried out childcare when the child was aged 1 $\frac{1}{4}$, 2, and 3 $\frac{1}{4}$, respectively. The table reports estimated coefficients for the outcomes for which we found significant effects of mothers' mental health (i.e., child depression and socio-emotional outcomes; we do not report the estimates on child cognitive outcomes, but these are available upon request). We report a *t*-statistic for the test of equality of coefficients for the sample where the grandparent does and does not provide childcare. In all cases we cannot reject the null that the coefficients are equal.[21] This therefore strengthens our belief that the instrumental variable does not pick up a direct effect through changes in time investments made by friends or grandparents.

[19] Nicoletti et al. (2022) used a similar methodology. Due to not having a binary instrument, we cannot carry out the complier analysis suggested by Angrist and Pischke (2008).

[20] Note that in column 2 some variables were dropped from the regression owing to collinearity.

[21] Splitting the sample by whether a friend provides childcare at age 1 $\frac{1}{4}$, 2, and 3 $\frac{1}{4}$ also shows no statistically significant differences in the estimates (results available upon request).

6.1.3. Restricting the sample to households experiencing illness

As a further robustness check, we restrict our analysis sample to those who experienced illness of a friend or relative at any point between the start of pregnancy and age 2 $\frac{3}{4}$ (33 months) post-birth, and we estimate the effect of mothers mental health during pregnancy on child outcomes, using the same instrument as the main analysis: illness of a friend or relative between the start of pregnancy and 8 weeks post-birth. Restricting the sample in this way allows us to compare mothers who experience illness of a friend or relative during, or very soon after pregnancy with those who suffer the same event in the period after birth up to the child's age 2 $\frac{3}{4}$. An assumption here is that the timing of the event occurs randomly across these intervals, similar in spirit to Black et al. (2016) and Petra and Rossin-Slater (2018).

Table A.10 reports the results. Panel A shows the full sample (benchmark) results, and Panel B shows results for the restricted sample of households defined above. As expected, imposing this restriction reduces the sample size to around 60% of the benchmark specification, and leads to a reduction in the first-stage F-statistics. To ensure consistency in model specifications, in addition to controlling for illness of a friend or relative between pregnancy and 8 months post birth (the benchmark model), we include dummy variables indicating whether the mother experienced illness of a relative or friend between 8 months post-birth and age 1 $\frac{3}{4}$ (21 months) and between age 1 $\frac{3}{4}$ and age 2 $\frac{3}{4}$ (33 months).[22]

Compared with the benchmark, the first stage coefficients on the instrument are similar when considering the restricted sample. This suggests that the incidence of illness during pregnancy affects mental health during pregnancy for mothers who experience illness between the onset of pregnancy and age 2 $\frac{3}{4}$ post birth to a similar degree as our benchmark estimates where some households (around 40%) do not experience such illness. Furthermore, the point estimates of the effect of mothers mental health on child depression are almost identical to our benchmark, albeit with less precision due to a reduction in the sample size. The t-statistics reported in the table relate to the hypothesis that the restricted sample estimates are equivalent to our benchmark estimates. We fail to reject this hypothesis. For the socio-emotional outcomes, while the point estimates are lower in the restricted sample, they are within the confidence intervals of our benchmark estimates. Again, we fail to reject the null hypothesis that the restricted sample coefficients are equal to our benchmark estimates. Taken as a whole, the results support the validity of our main (benchmark) results.

6.2. Sample attrition

Sample sizes decrease across two of the three sets of outcomes as children age.[23] Sample attrition is common in cohort or panel surveys as they mature over time. It would be a concern if attrition is related to both mothers' mental health and child outcomes, or if attrition is driven by the instrument. We explore both possibilities below.

6.2.1. Sample attrition and fade-out

Table 4 shows that attrition is particularly prevalent for the socio-emotional skills measured at age 16 $\frac{1}{2}$, where the sample size is around 3,200 compared to around 6,100 at age 6 $\frac{3}{4}$. One concern therefore, is that, rather than the effect of maternal mental health fading out over time, the reduction in the estimates may be driven by non-random sample attrition. To test this, Table A.11 estimates the IV regressions of mothers' mental health, restricting the sample to mother-child pairs with non-missing SDQ scores at age 16 $\frac{1}{2}$. Selecting on this smaller sample, the conclusions of our benchmark specification remain unchanged with the effect of mothers' mental health fading out as the child ages. For the SDQ outcomes, we find a similar pattern as that above: statistically significant effects of maternal mental health for the earliest SDQ measures (albeit with reduced precision), which reduce as the child ages and is no longer significant from age 11 $\frac{3}{4}$ onwards.

6.2.2. Sample attrition and illness of friends and relatives

There is a further concern that attrition may be driven by the instrument and that the instrument may therefore induce changes in the sample composition. For example, sample attrition may be higher among children whose mother experienced illness during pregnancy, or illness of a friend or relative may differentially affect attrition for high-educated versus low-educated mothers. We address this in two ways. First, we consider whether the proportion of mothers experiencing illness during pregnancy differs depending on whether the child outcome with the greatest attrition - the SDQ score at age 16 $\frac{1}{2}$ - is missing or not. From the full sample of 7773 households, 3208 have a missing SDQ score at this age and 4565 children have valid scores. The proportion of mothers experiencing illness during pregnancy is 30% in the sample with non-missing SDQ scores, and 28.8% in the sample with missing SDQ scores, suggesting that attrition is not driven by the instrument.

Second, we extend the above descriptive analysis of attrition by regressing the instrument, illness, on an indicator for the child's socio-emotional outcome measured at age 16 $\frac{1}{2}$ being non-missing for different sub-groups of mothers, defined by mothers' education (low and high), and occupation (professional, managerial, skilled or unskilled). Table A.12 presents the results. Across the sub-groups there are no statistically significant relationships between illness and the SDQ outcome being observed. Again, this suggests that attrition is not correlated with our instrumental variable.

7. Mechanisms: Biological or behavioural

The literature examining the effect of stress during pregnancy - proxied by the death of a parent, an earthquake or other event - tends to find that stress leads to worse child outcomes at birth. However, it is rare to find negative *long-run effects* from stress during pregnancy (see Black et al. (2016) and Li et al. (2011)). Our analysis finds statistically significant effects of mothers' mental health on child outcomes up to 11 $\frac{3}{4}$ years but these effects do not persist into adolescence. We explore whether any effect of poor mental health around pregnancy fades out in the long-run because parents' behaviour compensates for poor mental health during pregnancy by raising investments in children during pregnancy and early childhood years.

Table A.13 provides descriptive statistics for a set of parental investments observed when the child was aged 6 to 42 months; these are informed by the biological literature about behavioural responses to stress during pregnancy (see, for example, Miller (1992); Zuckerman et al. (1989)).

The measures of parental investments in our data include indicators for smoking and drinking during pregnancy; maternal breastfeeding; scores reflecting the quality of mothers' parenting measured between child age $\frac{1}{2}$ and 3 $\frac{1}{2}$; scores reflecting the extent to which mothers engaged in teaching their child certain skills measured between child age 1 $\frac{1}{2}$ and 3 $\frac{1}{2}$; scores reflecting the activities mothers engage in with their children measured between child age $\frac{1}{2}$ and 3 $\frac{1}{2}$; and finally two toy scores which measure the quality of the home learning environment, measured at child age 2 and 3 $\frac{1}{2}$. Table A.14 lists the set of questions used to create the parenting, teaching, activity, and toy score scores.

Table 5 presents the regression coefficients on mothers' mental health around pregnancy from separate regressions, where the dependent variable is given by the different parental investment measures listed in the rows. The estimates are derived from IV regressions using the set of controls in our benchmark specification. It is apparent from Table 5 that, with the exception of the toy score (indicating monetary

[22] Our results are very similar when we do not include these additional controls.

[23] Sample attrition is not an issue for the cognitive scores, as these are derived from administrative data.

Table 5
Maternal mental health on post-birth parental investments.

Dependent variable	
Smoked during pregnancy	0.106
	(0.096)
Drank alcohol during pregnancy	0.040
	(0.121)
Ever breastfed: $\frac{1}{2}$ year	0.086
	(0.104)
Breastfed for long period	0.142
	(0.112)
Mother parenting score: $\frac{1}{2}$ year	0.527
	(0.327)
Mother parenting score: $1\frac{1}{2}$ years	-0.693
	(0.663)
Mother parenting score: 2 years	0.796
	(0.521)
Mother parenting score: $3\frac{1}{4}$ years	0.928
	(0.669)
Mother parenting score: $3\frac{1}{2}$ years	−0.669
	(1.020)
Mother teaching score: $1\frac{1}{2}$ years	0.483
	(0.306)
Mother teaching score: $2\frac{1}{2}$ years	0.431*
	(0.261)
Mother teaching score: $3\frac{1}{2}$ years	0.489*
	(0.275)
Mother activity score: $\frac{1}{2}$ year	0.763
	(0.583)
Mother activity score: $2\frac{1}{2}$ years	0.029
	(0.681)
Mother activity score: $3\frac{1}{2}$ years	0.472
	(0.618)
Toy score: 2 years	1.701**
	(0.780)
Toy score: $3\frac{1}{2}$ years	-0.083
	(0.145)

The table reports separate IV estimates of the effect of mothers' mental health around pregnancy on each parental investment shown in the rows. All regressions contain the set of controls listed in Table 3. Maternal mental health is instrumented in all regressions using illness of a friend or relative between the start of pregnancy and 8 weeks post birth. *, **, and *** indicate $p < 0.10$, 0.05 and 0.01 respectively.

investments in children) and a marginally significant effect on the teaching score at child age 2 $\frac{1}{2}$ and 3 $\frac{1}{2}$, that the coefficients are not statistically significant. In general, therefore, we find little evidence that an increase in maternal mental health problems around pregnancy leads to a behavioural response in terms of her health habits during pregnancy, breastfeeding patterns soon after pregnancy, and the quality or quantity of her parenting in the subsequent early childhood years. Analysis available on request shows the same is true when the parental investments are defined at more extreme levels, for example being in the top or bottom decile of parenting quality; we find no evidence that these behaviours respond to mothers' mental health around pregnancy.

This result may be surprising, given the impacts of poor mental health on individuals' well-being. Looking outside the economics literature, research suggests that poor maternal mental health may affect maternal sensitivity towards children (NICHD et al. (1999)), potentially leading to negative interactions or disengagement (Belsky (1984); Kiernan and Huerta (2008); McLearn et al. (2006)), as well as increased parenting stress (Lovejoy et al. (2000)); may reduce time and financial investments in children, or can act as a mediator for the income–child development relationship (see e.g., Bigatti et al. (2001); Frech and Kimbro (2011); Gershoff et al. (2007); Yeung et al. (2002)). However, there are two main points that distinguishes our analysis from these papers. First, this literature tends to focus on low-income families, where mothers are perhaps more vulnerable to common stressors, and where children - on average - are behind in their development compared to high-income families. Second, this literature explores simple correlations between maternal mental health and child investments, not accounting for the endogeneity of the former. Indeed, if poverty leads to worse maternal mental health as well as fewer child investments, the effect of maternal mental health will be overestimated. In contrast, our analysis aims to address this endogeneity and estimate the causal impact of maternal mental health.

The investment measures included in our analysis were informed by the scientific literature. However, as we show that these are not systematically affected by mothers' mental health, this suggests one of two explanations: either the effect of mother's mental health around pregnancy is driven by *other* behaviours that are not observed in our data, or the true mechanism for our estimated effects is *biological*, e.g., increased maternal cortisol levels causing a biological response in the fetus that in turn affects childhood development.

8. Conclusion

It is well established that adverse early life events can have serious long-term consequences, affecting child development and human capital accumulation (see e.g., Currie (2020)). Such negative events or shocks may occur *in utero*, impacting on fetal development (for example, Almond and Currie (2011a,b); Currie (2011)). To date, much of the literature that focuses on shocks during pregnancy considers shocks to the physical health of mothers and the resulting impact on their children. The impact of experiences of poor mental health during pregnancy has received less attention, despite this being a crucial time due to the rapid development of children *in utero*. In particular, poor maternal mental health and stress during pregnancy has been associated with increased levels of cortisol and linked to a number of effects on a child's development ranging from lower fetal growth and birth weight to an increased risk of affective disorders and lower cognitive skills. Poor childhood mental health has in turn been shown to reduce children's human capital and labour market outcomes, suggesting intergenerational consequences of maternal mental well-being. Poor mental health around pregnancy may also have important behavioural consequences in terms of a mother's investments in her children.

Due to a general paucity of data that directly measure maternal mental health around pregnancy, studies that have considered its impact on a child's later life outcomes have tended to rely on proxy measures such as a death of a family member (e.g. Black et al. (2016)) or natural disasters (e.g. Currie and Rossin-Slater (2013)) and an intention-to-treat identification strategy. The extent to which such shocks actually affect a mother's mental health, however, is often unknown. This paper contributes to the literature on early life development and later life outcomes by examining the effects of maternal mental health around pregnancy on key psychological, socio-emotional and cognitive skills of a child and into adolescence. We use a binary variable indicating whether the mother experienced illness of a friend or relative during pregnancy as an instrument for maternal mental health. We find that this strongly predicts mental health, worsening mental health by 0.12-0.14 standard deviations. In addition, our analysis suggests that more vulnerable mothers are more at risk of mental health problems when experiencing illness, highlighting the need for additional support for these mothers.

Our results show that poor maternal mental health around pregnancy raises the risk of child depression and negatively impacts socio-emotional skills in childhood. These are shown for both OLS estimates and our favoured IV results. Although the effects that we estimate are significant, they are not necessarily large. Indeed, although a one standard deviation worsening of maternal mental health during pregnancy increases the child depression score, the average score remains far below the clinical threshold that indicates depression, suggesting that maternal mental health problems around pregnancy have quantitatively relatively small effects on child psychological health.

We find larger effects of maternal mental health on children's socio-emotional skills, as measured by children's SDQ scores. The existing literature investigating whether interventions affect children's behaviour (as measured by the SDQ) generally find improvements in response to parenting interventions, though there are exceptions (e.g. Edwards et al. (2014)). For example, Lindsay and Strand (2013) and Lakes et al. (2011) find that parenting interventions improved children's SDQ by 0.5-0.7 standard deviations. Using a randomized trial, O'Farrelly et al. (2021) and Bierman et al. (2021) show that a parenting intervention and learning programme improved SDQ scores by 0.2-0.3 standard deviations. For comparison to our analysis, there are two important points to make. First, the majority of studies focus on children in lower social classes, in poverty, or those with existing behavioural problems, rather than the general population. Second, many studies report the effect of a particular intervention (i.e. the absence of presence of a parenting programme). We instead examine the effect of a deterioration in maternal mental health (a continuous score). Furthermore, as the child ages, we observe a fade-out in the estimates of mothers' mental health, suggesting that the effect sizes differ depending on the child's age. Hence, it is difficult to compare the actual magnitude of their and our estimates in detail.

Similar to the existing literature, we do not find an impact on children's cognitive abilities. Hence, while we cannot rule out detriments to human capital accumulation, our results suggest that maternal mental health during pregnancy may have a limited direct effect on children's future labour market outcomes, both due to a null effect on children's cognitive skills and a fade-out of effects on children's non-cognitive skills.

We provide evidence suggesting that mothers' mental health around pregnancy does not impact on investments made by mothers in the early years of her child's life. Specifically, her breastfeeding behaviour, ability to interact with her child and the home learning environment does not respond to the incidence of mental health problems. This perhaps suggests that the dominant mechanism for the effect of mothers' mental health stems from biological channels, such as changes in the level of cortisol and in the amygdala.

Given the high incidence of clinical depressive symptoms around pregnancy, a greater understanding of the long term consequences on a child's development is warranted to inform public policy. This may be particularly so around the management and care of maternal depression and stress, but also in enabling appropriate behaviours and investments in infants and young children where maternal mental health problems are present. For example, this might consist of additional specialist care and support for women at home or in maternity units through advice on medication and lifestyles and providing counselling and cognitive behavioural therapy where appropriate. Similar policies have been found to be effective in a lower middle income country setting (Baranov et al. (2020)), and while targeted at supporting mothers around pregnancy, have been shown to have persistent positive impacts on mental health together with increases in time and financial parental investments. Accordingly, such policies are likely to have important benefits to children's socio-emotional and psychological development, which in turn affect children's subsequent health, human capital, and labour market outcomes.

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Supplementary material

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