Early Life Circumstance and Adult Mental Health*

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Abstract

We show that psychological well-being in adulthood varies with circumstance in early life. Combining a time series of real producer prices of cocoa with a nationally representative household survey in Ghana, we find that a one standard deviation rise in the cocoa price in early life decreases the likelihood of severe mental distress in adulthood by 3 percentage points (half the mean prevalence) for cohorts born in cocoa-producing regions relative to those born in other regions. Impacts on related personality traits are consistent with this result. Maternal nutrition, reinforcing childhood investments, and adult circumstance are likely operative channels of impact.

Keywords: mental health, subjective well-being, early life, fetal origins, endowments, commodity prices

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1 Introduction

Mental health disorders account for 13 percent of the overall global disease burden (Collins et al., 2011). The economic losses due to these disorders in low-income countries are staggeringly large: for example, depression is estimated to generate losses of 55.5 million disability-adjusted life years (DALYs) in low- and middle-income countries (Mathers et al., 2008).[1] Despite these costs, both in terms of health and economic development, investment in prevention and treatment remains relatively low (Collins et al., 2011).

It is crucial, then, given the burden of mental health disorders in low-income countries, to gain a better understanding of their origins. Though there has been progress on documenting the determinants of psychological well-being (and relatedly, satisfaction and happiness), open questions still linger (Easterlin, 1974; Stevenson and Wolfers, 2008). Much of the recent work in this literature studies the complex relationship between income and subjective well-being (Haushofer and Shapiro, 2013; Kahneman and Deaton, 2010; Kahneman and Krueger, 2006; Sacks et al., 2012; Stevenson and Wolfers, 2013). A few studies have investigated contemporaneous effects of life choices such as family and career status or residential environment (Bertrand, 2013; Katz et al., 2001). Still, relatively little is known about long-run determinants of mental health.

In this study, we ask: how does circumstance in early life affect psychological distress in adulthood? We focus our attention on early life influences because of the growing evidence that shocks and interventions in the early period of life have large, lasting impacts on health and human capital formation (Almond and Currie, 2011; Heckman, 2006, 2007). We examine this relationship using a nationally representative household survey from Ghana. This includes a module comprising the Kessler Psychological Distress Scale (K10), an internationally validated measure of anxiety-depression spectrum mental distress (Andrews and Slade, 2001; Kessler et al., 2002). We exploit variation in early life conditions induced by changes

[1] In Ghana, where our study is based, Canavan et al. (2013) estimate that the productivity loss associated with mental illness is equivalent to 7 percent of the country's GDP.

in the real producer price of cocoa. Cocoa is Ghana's chief agricultural export commodity, and its price is a key determinant of household incomes in the regions where it is grown.[2]

We show that in cocoa-producing regions of Ghana, low cocoa prices at the time of birth substantially increase the incidence of severe mental distress, as classified by the Kessler Scale. A one standard deviation drop in the cocoa price increases the probability of severe mental distress by 3 percentage points, or nearly 50 percent of mean severe distress incidence, in cohorts born in cocoa-producing regions relative to those born in other regions of Ghana. Effects on related personality traits show remarkable consistency with the Kessler Scale results. The impact on mental health is robust to changes in specification, type of shock, and choice of aggregation of the K10 survey questions.

What drives the long-lasting impacts on mental health we find? Beyond the direct physiological effects of nutritional availability *in utero* (Shonkoff et al., 2012), we examine several additional potential mechanisms. We look first to the most proximate outcome: maternal health. Using Demographic and Health Survey (DHS) data from Ghana, we show that cocoa prices positively predict (contemporaneous) maternal weight and BMI, which are highly correlated with birth weight and health in infancy (Gunnsteinsson et al., 2014). Additionally, we find that parents reinforce these increases in initial endowments by increasing vaccination rates and breastfeeding for longer.[3] Next, we look at adult physical health and economic outcomes. We find mixed evidence here: health stock, as measured by adult height, significantly improves with higher cocoa prices at birth, but indicators of economic circumstance (savings and occupation type) are not significantly higher.

Finally, we check for selective mortality and fertility, and find that both respond to price shocks. On average, child mortality and fertility are both procyclical.[4] We then

[2]Poverty reduces nutritional availability *in utero*, and affects mothers' stress levels and cognitive functioning, all of which in turn affect fetal programming (Almond and Currie, 2011; Mani et al., 2013; Persson and Rossin-Slater, 2014).

[3]This reinforcement behavior is consistent with recent results from both developed and developing countries (Adhvaryu and Nyshadham, 2013; Almond et al., 2009).

[4]The finding that mortality and crop income are positively related is echoed in Miller and Urdinola (2010), who study infant mortality and coffee prices in Colombia.

study, in three ways, the extent to which selective mortality and fertility might be driving our results on mental health. First, we control for maternal and paternal characteristics (educational attainment and occupation dummies) and find no change in the size of the estimated impacts on mental health. Second, we use a household fixed effects strategy to control for unobservable determinants of selection that vary at the (current) household level, and find that the impact on severe mental distress gets stronger after controlling for these effects. Third, to check the extent to which the pattern of results is preserved in the absence of endogenously inflated cohorts due to selection bias, we use frequency weights to deflate the importance of high price, low mental distress observations. After re-weighting observations in this way for various intensities of selection bias, we find similar impact coefficients compared to the baseline results, suggesting that selection bias does not play a major role in our context. However, because we do find that fertility and child mortality are pro-cyclical, and because the price treatments we consider do not have sharp and precise timing, we cannot claim that the role of selection bias is wholly nonexistent.

Our study is closely related to the "fetal origins" literature in economics. The fetal origins hypothesis – that access to nutrition in early life has long-run effects on health and well-being – has been affirmed and extended by a large body of empirical evidence in economics.[5] These studies show that changes in fetal programming can affect a wide variety of outcomes, including physical health (Currie, 2009; Hoynes et al., 2012); IQ, educational performance and attainment (Aizer and Cunha, 2012; Bharadwaj et al., 2013; Bleakley, 2007); and labor market outcomes (Almond, 2006; Bhalotra and Venkataramani, 2012; Bleakley, 2010; Gould et al., 2011).

Apart from being a key determinant of utility and an important endpoint in its own right (Daly et al., 2013; Kahneman and Deaton, 2010), mental health is also a potential mechanism through which some of the previously documented "fetal origins" impacts on health and

[5] Barker's original contributions related to long-term impacts on heart disease, and the subsequent literature in economics and other disciplines, are nicely reviewed in Almond and Currie (2011) and Currie and Vogl (2012).

human capital may arise (Kubzansky et al., 1997, 1998; Whang et al., 2009).[6] Moreover, medical evidence suggests that some components of mental health are coded during fetal development (Shonkoff, 2011; Shonkoff et al., 2012). Changes to the fetal environment, if they alter or disrupt this coding process, may have long-lasting impacts on mental health (Huttunen and Niskanen, 1978; Mednick et al., 1988; Neugebauer et al., 1999).[7]

Few studies in economics have examined the causal impacts of early life stressors on mental health.[8] In economics, perhaps the most closely related work is the recent study by Persson and Rossin-Slater (2014), who study the effects of maternal stress caused by deaths of close family members, finding large increases in prescription drug usage for mental health disorders in childhood and adulthood for exposed individuals. Our study is also related to Almond and Mazumder (2011), who find that the likelihood of mental disability increases with periods of religious fasting during early pregnancy, and Dinkelman (2016), who find that drought shocks can generate long-term mental health effects. Our work complements these previous studies in economics and public health by examining not only the effects of extreme deprivation and acute stress, but also the impacts of moderate exposure to these early insults.

Our focus on psychological well-being is related to a growing literature on the determinants of personality and non-cognitive skill formation (Cunha et al., 2010; Heckman, 2007). Recent studies emphasize the importance of early investments in children for the formation of non-cognitive skills like motivation, perseverance, and dependability (Heckman, 2006). Indeed, these personality traits are linked to economic outcomes independent of traditional cognitive ability measures such as intelligence quotients (Heckman and Rubinstein, 2001;

[6]It bears mentioning, as we discuss later in the paper, that effects may not be due to *in utero* exposure alone; we cannot statistically reject the impacts of prices in the perinatal period more generally.

[7]Stress impacts the "architecture" of the developing brain. In particular, it leads to hypertrophy of the amygdala, and neural connection loss in the hippocampus and prefrontal cortex. These changes, in turn, affect a whole host of outcomes related to psychosocial and executive functioning (Shonkoff et al., 2012).

[8]There is a rich literature in public health studying the impacts of extreme caloric deprivation during gestation on adult mental health: see, e.g., Brown et al. (1995, 2000); Hoek et al. (1998, 1996); Huang et al. (2013); Neugebauer et al. (1999); Pol et al. (2000); Susser and Lin (1992). This evidence focuses overwhelmingly on the impacts of severe famines, particularly on the case of the Dutch Winter Famine of 1944-1945.

Heckman et al., 2006). We add to this work by demonstrating that early life conditions have large (causal) effects on psychological well-being and related personality traits. We also expand the body of evidence in this area by looking in a developing country setting; most previous studies use data from the United States.

As a growing segment of the fetal origins literature has already documented, early life trauma can have outsized impacts in low-income populations whose income smoothing and coping mechanisms are often limited. In particular, smallholder farm households in the developing world are exposed to frequent income fluctuations (Maccini and Yang, 2009; Townsend, 1994). The households we focus on (cocoa farmers), and millions like them, are commodity suppliers to the global market (Deaton, 1999). The wide and persistent price fluctuations that characterize these markets directly affect the livelihoods of smallholder suppliers, leaving households (and young children in particular) vulnerable to the deleterious effects of shocks (Adhvaryu et al., 2013; Benjamin and Deaton, 1993; Cogneau and Jedwab, 2012; Kruger, 2007; Miller and Urdinola, 2010). It is crucial, then, to study whether income shocks and their consequences constitute part of the origins of mental distress in low-income contexts, in order to devise policy solutions that address this problem.[9]

The rest of the paper is organized as follows. In section 2, we outline our empirical strategy. Section 3 describes the cocoa price data and our survey data. Section 4 discusses our results, and section 5 concludes.

2 Empirical strategy

2.1 Intuition

The intuition for our identification strategy is that households in the cocoa-producing regions of Ghana experience changes in the real producer price of cocoa as income shocks,

[9]In this respect, our study is related to recent work documenting the short- and medium-run mental health impacts of natural disasters and crises (Frankenberg et al., 2008; Friedman and Thomas, 2009; Paxson et al., 2012; Rhodes et al., 2010).

while households in regions that do not produce cocoa are unaffected by these fluctuations. Children born into households in cocoa-growing regions during periods of high cocoa prices will have more resources, owing both to the higher incomes of cocoa-producing households and to the dependence of non-agricultural activities in these regions on the cocoa sector. These resource booms could have large and lasting impacts on mental health through their effects during both gestation and infancy.

2.2 Motivation

To motivate this identification strategy, we present a graph in Figure 1 that depicts the correlation between cocoa price shocks during an individual's year of birth and his mental health in later life. The solid line is the 3-year moving average of the log of the real producer price of cocoa. The dotted line is the 3-year moving average of the difference between the incidence of severe mental distress among individuals born in Ghana's cocoa-producing regions and its incidence among individuals born in the rest of Ghana. A clear negative correlation between the two time series is evident. That is, individuals born in the cocoa-producing regions of Ghana when incomes of cocoa-producers are high show low rates of severe mental distress relative to individuals born in the same year but in parts of Ghana that do not grow cocoa. When incomes in cocoa-producing regions fall, the pattern is reversed.

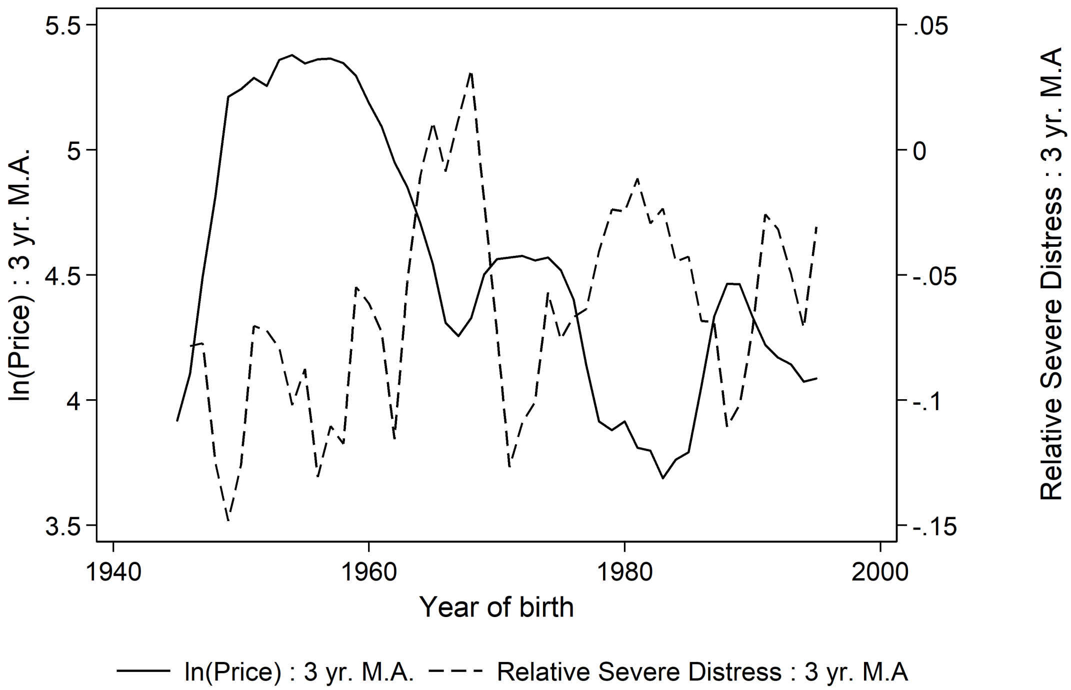
2.3 Specification

To test for the effects of cocoa price fluctuations in the year of birth on later-life mental health, we estimate the following equation:

$$Outcome_{irt} = \alpha + \beta ln(CocoaPrice_t) \times CocoaProducer_r + x'_{irt}\gamma + \delta_r + \eta_t + \epsilon_{irt}. \quad (1)$$

Here, $Outcome_{irt}$ is the outcome for individual i, born in region r in year t. In the

Figure 1: Cocoa Prices at Birth and Severe Distress



main results reported in Table 2 below, we will use either the natural log of the individual's response on the 10-question Kessler Psychological Distress Scale or, in keeping with the convention when analyzing Kessler scores, a dummy for whether the individual's score was 30 or above as an indicator for severe distress. $CocoaPrice_t$ is the real producer price of cocoa in year t. We describe the source of the price data used and how the real producer price is calculated in section 3. $CocoaProducer_r$ is an indicator for whether cocoa is produced in region r. We discuss how this indicator is defined in section 3. β is the coefficient of interest. We anticipate that the effect of beneficial shocks to parental income will reduce adult mental illness, leading to negative estimates of β. Throughout, we will refer to this composite variable $ln(CocoaPrice_t) \times CocoaProducer_r$ as the "Price Shock."

x_{irt} is a vector of controls. In our preferred specification, this will include female, household head, the interaction of female and head, dummies for religion, and dummies for ethnicity. δ_r and η_t are vectors of fixed effects for year and region of birth, respectively; and α

is an intercept. In our baseline, we will cluster standard errors by enumeration area. This is the primary sampling unit of the outcome variables. As robustness checks, we will cluster, alternately, by region of birth or by year of birth. In addition to these, we also report Cameron et al. (2011) standard errors clustered both by enumeration area and year of birth, or alternately by region of birth and year of birth.

Our preferred specification includes an additional vector of controls: $\delta_r \times t$. This set of controls interacts the vector of region of birth fixed effects δ_r with a continuous year of birth variable t to allow for region-of-birth-specific time trends.[10] We also add quadratic region-of-birth-specific time trends in subsequent robustness checks.

We include rainfall and temperature measures in the region as additional controls in a later specification. To the degree that the price shock variable is picking up region-specific fluctuations in temperature or rainfall, effects on mental health outcomes could be due to direct effects of these fluctuations on health of the mother or other members of the household, in addition to household income fluctuations.

3 Data

In this section, we describe the data sources used in the analysis. Additionally, where necessary, we describe the construction of the variables of interest.

3.1 Cocoa prices and production

Our source of data for real producer prices of cocoa is Teal (2002). He calculates these using the following:

$$\frac{P_X^P}{P^C} = \frac{P_X}{P_M}\frac{P_M ER}{P^C}(1-t).$$

[10]These trends are included primarily to absorb irrelevant variation over time in the outcome variable at the region-of-birth level.

Here, P_X^P is the cedi price received by cocoa producers, which is deflated by P^C, the price of domestic goods. This can be re-expressed as a function of P^X, the export price in foreign currency, P^M, the price of imports in foreign currency, ER, the official exchange rate, and the tax rate t, which encompasses both export duties and the difference between world cocoa prices and the lower prices often set by the monopolistic cocoa board. This real producer price represents a time-varying income opportunity available to households in the cocoa-growing regions of Ghana, but not available in other regions of the country.[11]

In our baseline specification, we interact these price shocks with an indicator variable for whether cocoa is produced in the respondent's region of birth. The data on cocoa production that we use to produce this baseline measure is computed directly from the EGC-ISSER Socioeconomic Panel Survey. These data were collected by the Economic Growth Center at Yale University and the Institute for Statistical, Social and Economic Research at the University of Ghana, Legon.

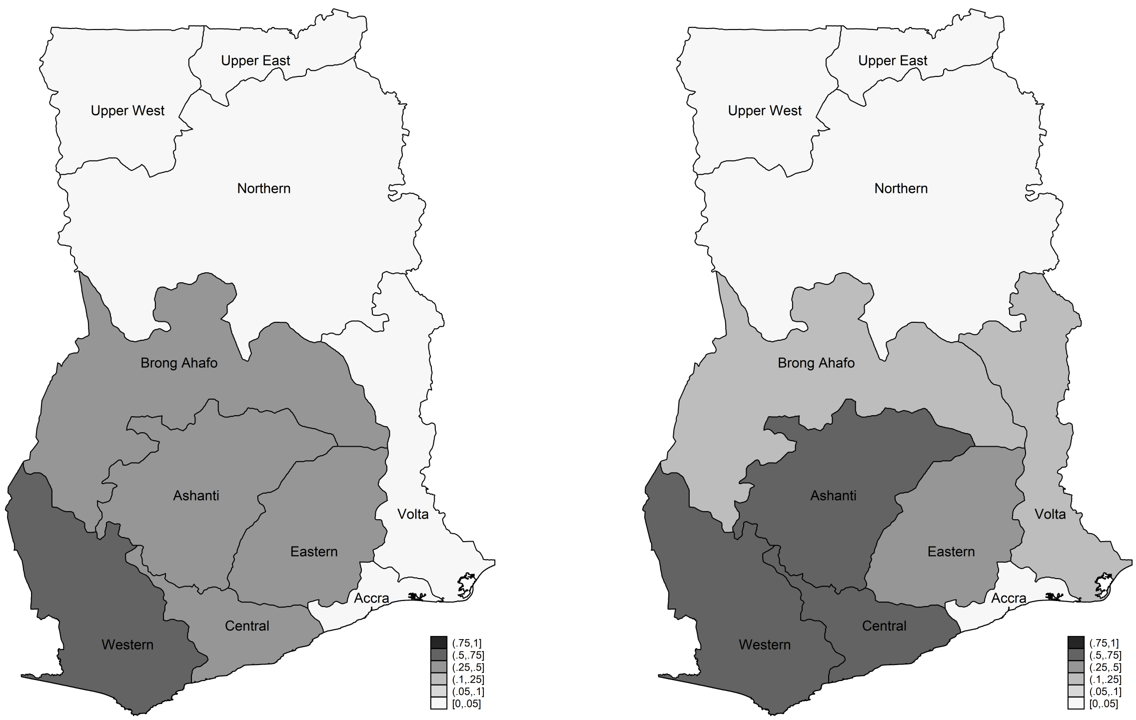
The data consist of a single cross-section, collected between November 2009 and April 2010, covering all of Ghana. Individuals were asked to list all plots of land, and what crops were grown on these plots. In Figure 2 below, we present a map of Ghana in which the 10 regions are shaded according to the percentage of farm acreage devoted to cocoa-growing. As a robustness check, we discard Greater Accra and Volta from the analysis, as less than 20% of farm land in these regions is planted to cocoa.

Our baseline measure is an indicator for the presence of cocoa in a region. This overlaps closely with the area classified as suitable for cocoa production in the 1958 Survey of Ghana Classification Map of Cocoa Soils for Southern Ghana. Produced for the Survey of Ghana, this map classified Ochrosols, Oxysols and Intergrades as suitable for cocoa production, conditional on climatic suitability. We plot the fraction of households in each region that grow cocoa (left panel) and the fraction of land in the region suitable for cocoa production

[11] Note, whereas the global export price of cocoa might be the first choice as a measure of an exogenous determinant of household incomes in cocoa growing regions, such a price series may not reflect the true, farm-gate price faced by cocoa-growing households and is also unavailable for the full range of birth years in the household sample.

(right panel) in Figure 2. The similarity between the maps in the two panels validate our classification of regions to cocoa-producing and non-cocoa-producing.

FIGURE 2: COCOA PRODUCTION AND COCOA SUITABLE SOILS BY REGION



The figure on the left depicts the fraction of land in the EGC-ISSER survey planted to cocoa in each region. The figure on the right depicts the share of all land in the region that is suitable for cocoa.

We focus on regional cocoa production, as opposed to household-level cocoa production measures, for two main reasons. First, as a practical point, while the EGC-ISSER data provide detailed information on cocoa production for the respondent's *current* household, we do not know the extent of cocoa involvement for the respondent's household at the time of his birth. While the extent of cocoa cultivation at the household level is likely very important in determining exposure to price shocks, this unavailability of data prevents us from exploiting (origin) household-level variation.

Second, focusing on regional exposure is relevant because the value chain for cocoa is, due to the nature of the production process, predominantly local (Kaplinsky, 2004). Cocoa must be fermented and dried shortly after harvest, and these immediately downstream steps are done either on or very close to the area of cultivation (Mohammed et al., 2011). Local processors, haulers, and licensed buying companies all participate in subsequent stages of production, before the cocoa is sold to the central marketing board for export. These downstream agents are thus very likely to be "treated" by price changes, through similar channels as cocoa-farming households.

3.2 Mental health

Our principal measure of mental health is computed using the 10-question Kessler Psychological Distress Scale, or K10. These data were collected as part of the EGC-ISSER Socioeconomic Panel Survey, and are described in greater detail by Canavan et al. (2013). The K10 was developed by Ron Kessler and Dan Mroczek in 1992 as a measure of anxiety-depression spectrum mental distress (Kessler et al., 2002). The questionnaire consists of 10 questions about negative emotional states experienced during the past 4 weeks. Respondents give 5-point answers ranging from "none of the time" to "all of the time." In particular, respondents are asked:

1. About how often did you feel tired out for no good reason?
2. About how often did you feel nervous?
3. About how often did you feel so nervous that nothing could calm you down?
4. About how often did you feel hopeless?
5. About how often did you feel restless or fidgety?
6. About how often did you feel so restless you could not sit still?
7. About how often did you feel depressed?
8. About how often did you feel that everything was an effort?
9. About how often did you feel so sad that nothing could cheer you up?
10. About how often did you feel worthless?

The survey methodology was developed and first validated in the United States. It has since been administered in a variety of contexts around the world, including in low-income populations in Australia and South Africa (Kilkkinen et al., 2007; Myer et al., 2008). Responses to the K10 have been shown to correlate with the Composite International Diagnostics Interview and with the probability of a mental disorder as defined by the Diagnostic and Statistical Manual for Mental Disorders (DSM-IV) (Kessler et al., 2003). It is conventional to take a K10 score greater than or equal to 30 as an indicator for severe distress. The K10 measure has been shown to fluctuate with significant life events, such as changes in employment status and childbirth (Wethington and Kessler, 1989). But it is also significantly associated with more long-run components of mental health (see, e.g., Slade et al. (2011)).

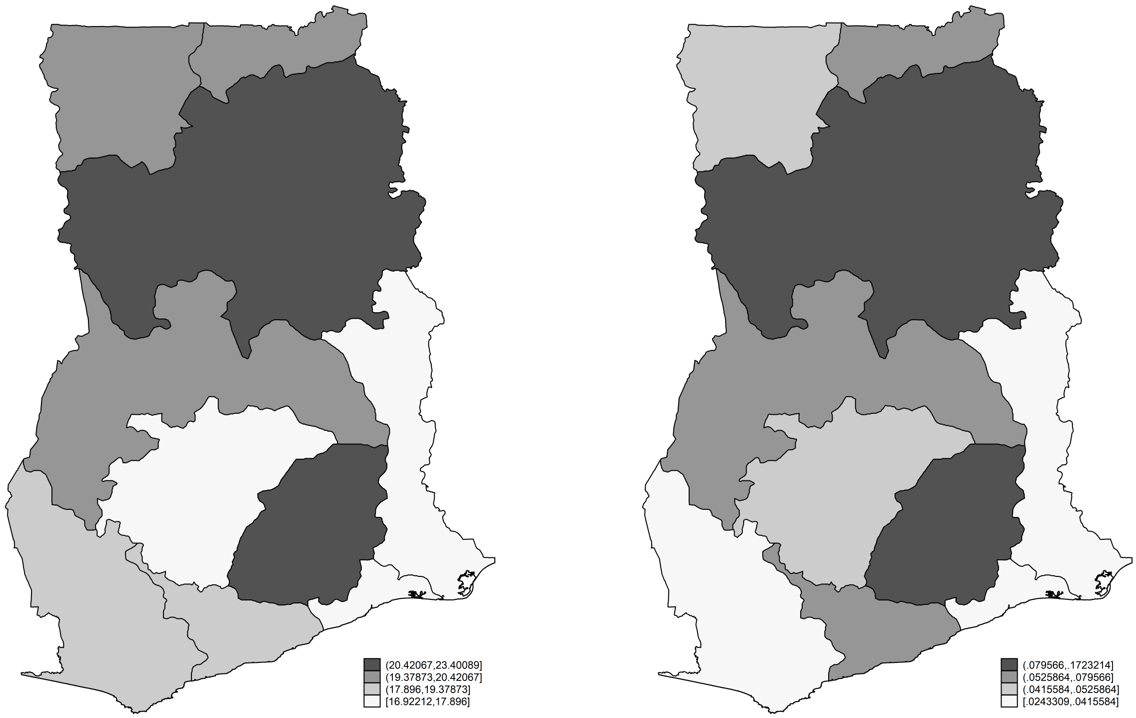
In addition to the K10 questionnaire, individuals were asked several additional questions about their mental state. We use these to validate the K10 measures in section 4. These ask respondents to agree or disagree on a five-point scale with statements such as "I am someone who is depressed, blue" or "I am someone who is relaxed, handles stress well." We show that responses to these alternative measures of mental health follow the same response to early life shocks as the more structured K10.

We retain individuals for our analysis who were born in Ghana, who have non-missing responses on the region of birth and K10 questions, who are aged between 15 and 65 at the time of the survey, and whose self-reported ages are consistent with their self-reported years of birth within 5 years. This leaves us with a base sample of 7,741 individuals. We show means for individuals' $K10$ scores and the indicator for severe distress by region of birth in Figure 3. Greater levels of distress correspond to darker shades of grey.

3.3 Additional controls

The bulk of our additional control variables are taken from the EGC-ISSER data. These include our principal individual controls – fixed effects for region and year of birth, an indicator for female, an indicator for household head, the intersection of female and head,

Figure 3: Mean K10 Score and Severe Distress by Region of Birth



The figure on the left depicts the mean K10 score over individuals in the sample. The figure on the right depicts the fraction of respondents whose scores indicate severe distress.

dummies for religion, and dummies for ethnicity.

In addition to these controls, other variables that we interact with early life shocks are also collected from the EGC-ISSER data. These include indicators for whether an individual's father was in agriculture or whether either of an individual's parents had any education. We test whether early life shocks predict additional outcomes also recorded in the EGC-ISSER data, including height in centimeters, body mass index (BMI), an individual's own education, whether an individual has migrated away from his or her region of birth, and the value of a household's savings.

In a robustness check, we control for rainfall and temperature shocks experienced during a respondent's year of birth. We take data on temperature and rainfall from the standard Willmott and Matsuura series available at `climate.geog.udel.edu/~climate/`. We merge this to the regions of Ghana by taking the average over grid points within a region. Regions not containing a grid point in the climate data are merged to the nearest point.

Finally, we also check for robustness to the inclusion of additional controls for family, economic, and social conditions. These controls, in addition to the age controls subsumed by the birth year fixed effects, are meant to account for changes in mental health measurements due to time-varying socio-demographic factors. The additional controls are number of children in the household, height in cm, general health, and dummies for each type of work, marital status, and ability to read English.

3.4 Demographic and health survey data

In section 4.3, we test several mechanisms that might explain our results. To do this, we make use of additional data from the Ghanaian DHS datasets. These were collected in 1988, 1993, 1998, 2003 and 2008. The data come in three formats:

1. *Individual Recodes*: These are nationally representative cross-sections of women aged 15 through 49 at the time of the survey. The surveys contain information on each woman's year of birth, region of residence, years of education, rural residence, age, occupation, partner's occupation, religion, ethnicity, and anthropometric outcomes such as height and weight.

2. *Births Recodes*: The women surveyed in the individual recodes are asked to provide a complete history of all births. These data include the child's year of birth, birth order, multiple birth, gender, whether the child is still alive and, if not, how long the child lived. In addition to using these birth histories directly, we reshape them into an artificial panel of data for each woman in the data, recording whether she experienced a birth in each year of life up to age 45.

3. *Children's Recodes*: The women surveyed in the individual recodes are also asked a detailed set of questions about all births within the last five years. Like the births recodes, these contain information on the child's year of birth, birth order, multiple

birth, gender, and the child's current age in months. Crucially, mothers are asked about early life investments such as the vaccination histories of these children and how long they were breastfed. Mothers are also asked about prenatal investments, such as visits to doctors, additional vaccines, and the circumstances of the delivery of the child.

3.5 Census data

In section 4.3, we evaluate additional mechanisms that might further account for our results. In particular, we use the 2000 and 2010 waves of the Ghanaian census in order to measure both living standards and human capital. Both surveys were carried out by the Ghana Statistical Service. The population universe for the 2000 census included all persons in households and living quarters in Ghana at midnight on census night; in 2010 this was expanded to include those in institutions and in transit, including the floating population. The 2000 census was conducted by direct enumeration using a single form, while the 2010 census was also conducted by direct enumeration with separate forms for household members and non-members. Both were sampled using a systematic sample of every tenth dwelling, restricted to private dwellings in 2000.

We use the versions of these data made available by Integrated Public Use Microdata Series, i.e. IPUMS-International. These are 10% samples. Together, we have data on 4,360,422 individuals across these two waves of the census, though restricting to those aged 15 to 65, as in our baseline analysis, leaves us with a maximum sample of 2,442,682. Like the EGC-ISSER data, the census reports region of birth and year of birth, allowing us to construct our baseline measure of cocoa price exposure in these samples. Further, these data report the same variables we use as controls in our main analysis: female, head, the interaction of female and year, urban, religion, ethnicity. In addition, we will control for survey year.

The measures of living standards we consider are indicators for whether the respondent's dwelling has electricity and for whether the respondent's dwelling has piped water. We use three measures of human capital; whether the respondent speaks English, whether the respondent is literate, and the respondent's years of schooling.

3.6 Summary statistics

Summary statistics on our variables of interest and principal controls are presented in Table 1. The statistics show that there is a great deal of variability in mental health. The mean respondent has mean K10 score of 19.44, and the mean of the $\ln(K10)$ score is 2.92. The median K10 score is similar, at a corresponding value of 18. This is equivalent to a respondent who replies "a little of the time" to 8 of the 10 items in the questionnaire. 20.14% of our respondents are classified as "moderately distressed" according to the K10. These respondents have a score of at least 25. A respondent who replied "some of the time" to 8 of 10 questions would meet this cutoff. 7.4% of the sample appear severely distressed. These are individuals who score 30 and above. These rates are similar to those experienced in rural Australian and in South Africa (Kilkkinen et al., 2007; Myer et al., 2008). We show a histogram of K10 scores in Figure C.4, in the appendix.

Although cocoa prices in the first decades of the twentieth century were higher than those experienced by our birth cohorts (1943 to 1997), the real producer price of cocoa does fluctuate substantially in our sample. The maximum observed price is 8 times the minimum and more than twice the mean. As indicated in Figure 2, cocoa production is concentrated in Southwestern Ghana.

Our mean respondent was born in 1973. 17% of the sample is Muslim, 7% follows a traditional religion, 5% follows no religion, and the remainder is divided almost entirely among several Christian denominations. Roughly 70% of our respondents ever attended school. 37% report that their fathers had any education, while 20% report the same for their mothers. Two thirds have a father who worked in agriculture. Roughly half the sample was not born in their current village of residence. 82% of people live in their region of birth (of the 10 regions of Ghana). Of those born in a non-cocoa region, 85% are still in a non-cocoa region. Of those born in a cocoa region, 98% are still in a cocoa region.

4 Results

4.1 Mental Distress

We report our main estimates of equation (1) in Table 2. There is a negative impact of the price shock at birth on the log of the respondent's K10 score in adulthood. This effect is statistically significant once time trends are added for each region-of-birth and survives the addition of individual controls. The negative effect of cocoa prices on severe mental distress is robust across specifications. As discussed in section 2, the most rigorous specifications including region-of-birth time trends and individual controls, reported in columns 3 and 6, are the preferred specifications.[12]

The magnitudes of the effects on the log of the $K10$ score are moderate, while the impacts on severe distress are large. The real producer price of cocoa has varied widely over time, and a one standard deviation increase in the log price is equivalent to 0.55 log points. In column (3), this would reduce the log K10 score for an individual born in a cocoa-producing region by $-0.045 \times 0.55 = 0.025$. This is roughly 0.08 standard deviations, or 1% of the mean.[13] For severe distress, a one-standard-deviation price shock leads to a roughly 3 percentage point reduction in severe distress, which is nearly half the mean.[14] Finally, in columns 7 through 9, we report marginal effects from a logit specification for severe distress. The estimates are roughly half the magnitude of those from the linear probability model, but are still significant

[12]The addition of region-of-birth trends likely has two competing effects on precision. On the one hand, these trends absorb noise over time at the region-of-birth level; this should increase precision. On the other hand, they further restrict the variation used to identify treatment effects; this should decrease precision. In our context, the latter effect seems somewhat to outweigh the former.

[13]As a comparison, this is about a quarter of the magnitude of the psychological impact of transitioning to full-time non-employment ("homemaker") status for women in a developed country context (Wethington and Kessler, 1989).

[14]Indeed, this impact accounts for essentially the entire difference in the outcome mean across cocoa and non-cocoa regions.

and relatively large as compared to the mean levels of severe distress.

We also report in Table 2 alternative estimates of the standard error of the impact of the price shock. First, we report standard errors clustered by region of birth or by year of birth. Second, we report Cameron et al. (2011) standard errors clustered by both enumeration area and year of birth or by both region of birth and year of birth. Third, because the number of possible regions of birth is small, we report Moulton-corrected standard errors clustered by year of birth or by region of birth (see Angrist and Pischke (2008)). Lastly, we also calculate wild cluster bootstrap standard errors (Cameron et al., 2008). Our estimates of the standard error do not change noticeably across specifications and do not impact which coefficients are significant at conventional levels.

4.2 Personality Outcomes

We show in Table 3 that, in addition to mental health as measured by the K10 questionnaire, the impact of early life cocoa price shocks is apparent for a variety of similar outcomes. This helps establish the validity of the K10 as a measure of mental illness, and the statistical robustness of our results.

First, individuals who received beneficial cocoa price shocks in their year of birth are less likely to report that they are the sort of person who is depressed, or "blue." Similarly, they are more likely to self-identify as relaxed. They are less likely to state that they tend to start quarrels, are disorganized, are moody, or are cold and aloof. These are traits we would expect from individuals who are less likely to experience mental distress as a result of favorable early life events. These personality results are generally robust across specifications and are statistically significant at conventional levels, particularly in the preferred specification reported in columns 3 and 6.

Similarly, although the K10 questions are designed to be used as an index, we show in Appendix B.2 that several individual components respond to early-life cocoa prices. That is, the results on K10 scores and severe distress are not driven primarily by responses to singular questions.

4.3 Mechanisms

4.3.1 Agricultural Incomes

First, we provide additional evidence that the year-of-birth price shocks are indeed operating through parental income at the time of birth. We show evidence in Table 4 that the impacts of the price shock on adult mental health are strongest among children of farmers and among the Akan, the ethnic group most associated with cocoa-production. In columns 1 and 2 of Table 4, we also explore heterogeneity by gender, and find little difference in effects across male and female respondents.

4.3.2 Mediation Analysis: Wealth, Occupation, Literacy, and Health

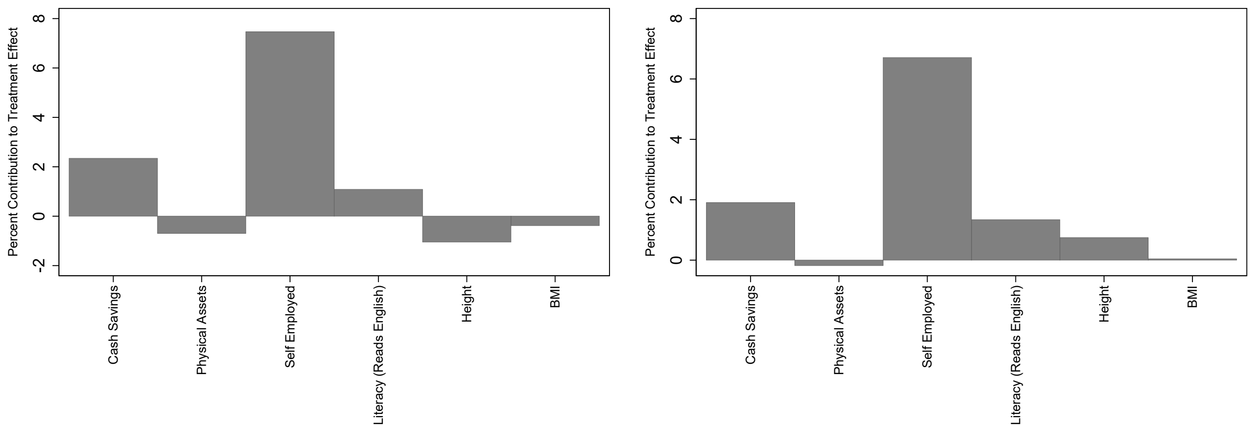
Next, we focus our attention on the set of related outcomes available in the main EGC-ISSER data. The candidate mediating factors available to us in the EGC-ISSER are cash savings, value of physical assets (e.g., durables, livestock), self employment, english literacy, BMI, and height. Though measures for some candidate mediators like investment in childhood and adolescence are missing from this set of variables (as they are not measured for the adult sample for whom we have mental health outcomes), the candidate mediators we are able to explore cover many of the most obvious channels of impact such as economic welfare, education and both stock and acute health. These are all measured at the same point in adulthood as mental distress outcomes.

The mediation analysis presented here (following the methodology developed in Heckman et al. (2013) with an application of inverse probability weighting suggested by Huber (2014)) involves estimating the degree to which impacts of shocks on mental distress vary by values of the candidate mediating factors, in addition to the impacts of shocks on these factors directly.[15]

[15]The three key assumptions for identifying the indirect effect of a treatment through mediators as well as

Using these two sets of estimation results along with the main results for impacts of shocks on mental distress (all reported in Table 5), we can calculate the contribution of each mediating factor to the total treatment effects on the two mental distress outcomes.[16]

Figure 4: Mediation Analysis



This figure depicts contributions of mediating factors to total treatment effects as calculated from regression results reported in Table 5. The methodology for conducting this meditation analysis follows a special case of the procedure presented in Heckman et al. (2013) along with an application of inverse probability weighting developed in Huber (2014) to address concerns of endogeneity in the mediating variables.

Figure 4 shows the percent contribution of each of the mediating factors to the total treatment effects on severe distress and the log K10 score.[17] All non-binary mediating

the direct effect are outlined and discussed clearly in Huber et al. (2016). They are: conditional independence of treatment, conditional independence of the mediator, and common support. While the first assumption is the same as the identifying assumption in the main analysis of this paper (i.e., that the cocoa price shock at the time of birth is exogenous), the conditional independence of the mediator does not necessarily hold. To address this concern, we follow Huber (2014) in constructing inverse probability weights from the probability of treatment as a function of the full set of candidate mediators and any covariates. We use these weights in all regressions reported in Table 5. Finally, we check that none of the mediators perfectly predicts treatment to ensure that common support is not violated.

[16]Note we abstract away from the treatment of measurement error that is well developed in Heckman et al. (2013), primarily because we lack the multiple measures for each factor required to implement the three-step procedure developed in that study. Of the several frontier mediation analysis methods reviewed in Huber et al. (2016), the inverse probability weighting procedure we employ performs as well or better than other methods under the circumstances presented in our context.

[17]Note that the contributions reported in Figure 4 correspond to total treatment effects reported in Table 5, but differ from the main results reported in Table 2. This is because we transform the shock variable to a binary for the shock falling above its region specific mean in order to simplify the calculation of the contribution of each mediating factor.

variables (i.e., cash savings, physical assets, BMI, and height) are standardized for ease of comparison.

The results indicate that economic mediators such as cash savings and self-employment seem to contribute more to the total treatment effect than do literacy and health mediators such as adult height and BMI. Additionally, cash savings mediates the effect on mental distress more strongly than does the value of physical assets. Self employment appears to contribute to treatment effects on mental distress most strongly with a percentage contribution equivalent to a more than 3 SD change in cash savings. The contribution of self employment is also the most precisely estimated (one-sided p-values are reported in parentheses under estimated contributions in Table 5).

All six mediating factors together account for roughly 10% of total treatment effects on both severe distress and ln(K10). The remaining 90% of the treatment effects on mental distress outcomes is either the direct effect of the shock or mediated through other channels not measured for this adult sample in the EGC-ISSER data. On the one hand, restricting attention to additional outcomes in the same dataset as our main results allows us to calculate the contribution of each of these additional outcomes to the total treatment effect estimated on mental distress, in addition to ensuring the strongest internal validity of the relationship implied between these outcomes by such a mediation analysis.

However, the EGC-ISSER data precludes us from commenting on a broader array of mechanisms (e.g., parental health, early-life investments) which are not measured for the adult cohort for whom we have mental health outcomes. Additionally, the relatively small sample size of the EGC-ISSER (as compared to the DHS and Ghanaian Census which are between 1.5 and 300 times as large) limits the power of this empirical exercise.

Accordingly, we also explore below impacts of price shocks on parental health and early-life investments using the DHS and on assets and education using the 2000 and 2010 waves of the Ghanaian Census. These alternative datasets allow us to explore additional outcomes and potential channels of impacts, but lack the main mental distress outcomes of interest and so cannot be used to expand the formal mediation analysis presented above. While the Ghanaian Census in fact does attempt to measure mental distress, the questions pertain only to extreme conditions and are reported to be present in less than one percent of the population. These measures are not comparable to those in the EGC-ISSER and show no clear pattern of impact from the shock. This is potentially due to the fact that the price shock we study offers more moderate variation than, e.g., extreme caloric deprivation, so impacts on extreme mental health outcomes may be small.

4.3.3 Investments

We next investigate the role of early life investments in the child as a mediating mechanisms for our main results. Adult outcomes of children born during cocoa booms might be affected by investments made after birth such as duration of breastfeeding and vaccinations. Investments, even if they are made after birth, might be affected by year-of-birth price shocks by way of the household's intertemporal budget. Investments may also respond to the child's endowment, which was augmented by the year-of-birth shock.[18]

We are able to use the DHS children's recodes to test whether a broad number of investments respond to cocoa price shocks. We estimate:

$$Investment_{irt} = \alpha + \beta ln(CocoaPrice_t) \times CocoaProducer_r + x'_{irt}\gamma + \delta_r + t_r + \eta_t + \epsilon_{irt}. \quad (2)$$

Here, $Investment_{irt}$ is a measure of investment in child i, living in region r, and born in year t. Examples include the duration of breastfeeding and receipt of various vaccinations. The cocoa price shock variable $ln(CocoaPrice_t) \times CocoaProducer_r$ is measured in the child's year of birth. δ_r is a vector of region fixed effects. t_r is a vector of region-specific time trends. η_t is a vector of year-of-birth fixed effects and α is an intercept. The vector of controls x_{irt}

[18] Indeed, parents may have strong incentives to reinforce shifts in infants' endowments if the production of child health exhibits complementarities in investments across different periods of childhood (Heckman, 2007).

includes both maternal characteristics (years of education, rural, mother's age, mother's age squared, dummies for religion, and dummies for ethnicity) and predetermined child characteristics (birth order, multiple birth, female, and age in months at time of survey).[19] Standard errors are clustered by region.

In our selection of investment outcomes, we follow Adhvaryu and Nyshadham (2013), who find evidence that parents reinforce stronger endowments with more investment. Specifically, we estimate impacts of year-of-birth price shocks on the number of vaccination doses received and duration of breastfeeding. We also consider at-birth and antenatal investments such as doctor-attended delivery and 0 dose vaccinations. The results of these regressions are reported in the top two panels of Table 6.

Consistent with the results in Adhvaryu and Nyshadham (2013), we find evidence of reinforcing investment in terms of breastfeeding and vaccinations in early life, but little evidence of effects on at-birth or prenatal investments. Estimates reported in the middle panel of Table 6 show both increases in vaccinations and breastfeeding. The bottom panel shows some weak evidence of an increase in the likelihood of a prenatal doctor visit, but no evidence of an increased likelihood of receiving at-birth vaccination doses (i.e. polio 0 and BCG) nor an increased likelihood of the child being delivered under formal health care. We find no evidence that prenatal investments respond to cocoa prices in the year before birth (not reported).

This pattern is consistent with, but not exclusive to, impacts on investments being driven by responses to improved endowments rather than by budget constraint slackness during cocoa booms.[20] We propose this mechanism based on the following logic. If investment effects were driven by increased incomes, we would expect at-birth and prenatal investments to reflect the increased household income in the child's year of birth. Although not reported here for the sake of brevity, we also regress the same early life investment outcomes on price

[19] In Table A15 in the appendix, we show results are similar omitting mother controls.

[20] We note, however, that due to lack of data on 1) objective measures of child health and 2) reliable household income measures, we cannot directly test this hypothesis.

shocks in the first and second year of the child's life and find no evidence of effects. We interpret this as further evidence against effects on investments being driven by increased incomes.

4.3.4 Parental Health

We then explore the role of parental health during gestation as another mediating mechanism for our results. That is, it is possible that children born during cocoa booms receive greater nutrition during gestation by way of greater maternal nutrition. As discussed above, to test whether early life price shocks operate through greater maternal health, we cannot use the EGC-ISSER data, because it is a single cross-section. Instead, we test whether the observable health outcomes of women in the DHS individual recodes respond to contemporary price shocks. We estimate:

$$Outcome_{irt} = \alpha + \beta ln(CocoaPrice_t) \times CocoaProducer_r + x'_{irt}\gamma + \delta_r + t_r + \eta_t + \epsilon_{irt}. \quad (3)$$

Here, $Outcome_{irt}$ is a health outcome (weight or BMI) for woman i, living in region r in year t. The cocoa price shock variable $\beta ln(CocoaPrice_t) \times CocoaProducer_r$ is measured in year t. δ_r is a vector of region fixed effects. t_r is a vector of region-specific time trends. η_t is a vector of year fixed effects and α is an intercept. The vector of controls x_{irt} includes years of education, rural, age, age squared, dummies for religion, and dummies for ethnicity. Standard errors are clustered by region. We discard outliers with reported weights outside the range 35-140kg and BMIs outside the range 15-35. Columns 1 and 2 of the third panel in Table 6 show that parental weight and BMI are improved by contemporaneous positive cocoa-price shocks. We interpret these results as evidence of maternal nutrition as a channel for the estimated impacts of early life income shocks on adult mental health.

4.3.5 Other Adult Outcomes

Finally, we also explore the degree to which impacts on mental health and personality traits are accompanied by impacts on other outcomes in adulthood. Having explored the set of related adult outcomes available in the EGC-ISSER in the mediation analysis above, we investigate adult outcomes in additional datasets here to enrich the analysis. Specifically, we regress respondent's occupation from the DHS and measures of assets (dwelling quality) and human capital accumulation (English, literacy, and years of schooling) from the Ghanaian census on the same year-of-birth price shocks using the same specification as reported in columns 3 and 6 of Tables 2 and 3. These results are reported in the bottom two panels of Table 6 and indeed show impacts on all of these categories of outcomes. Specifically, we find (imprecisely estimated) positive impacts on labor force participation, and substantial impacts on agricultural self-employment for respondents in the DHS. The census results show positive impacts on both dwelling quality (electricity and piped water) and human capital accumulation (English, literacy and years of schooling). We interpret these results as supporting evidence that impacts on mental health are coincident with improved labor, education and economic outcomes.

As discussed above, though these estimates from alternative datasets of impacts of shocks on related outcomes and candidate mediating factors help to reinforce the evidence from the mediation analysis conducted above, they cannot themselves be included in a proper mediation analysis. As broad surveys such as the Ghanaian Census begin to collect measures of mental health, we hope that future research can extend significantly the meditation analysis conducted here.

5 Conclusion

In this study, we estimate the impacts of early life income shocks on adult mental health. We show, among a nationally representative sample of households in Ghana, that a one standard deviation increase in the cocoa price in early life reduces the likelihood of severe mental distress in later life by roughly 3 percentage points for individuals born in cocoa-producing regions relative to the same birth cohort born in other regions. This is nearly half the mean.

Recent studies from the mental health literature have estimated that treatment, either by medication or therapy, can reduce the prevalence of mental distress by between 70 and 93.5 percent in low-income countries (Patel et al., 2007). We show that insulating households from a one standard deviation drop in agricultural output prices can reduce the prevalence of mental distress by roughly 50 percent. Given that studies have calculated the cost effectiveness ratio of treatment of mental disorders in low-income settings to be US$500-1000 per averted disability-adjusted life-year (Patel et al., 2007), roughly commensurate with treatment and prevention of chronic illness such as HIV/AIDS and diabetes, policies to protect households against income fluctuations as a means of preventing later-life mental distress might prove relatively more cost-effective. This argument is likely made even stronger once the economic costs of mental distress are more comprehensively measured.

In addition to being direct determinants of utility, psychological traits have been linked to physical and economic health and decision-making in adults (Heckman and Rubinstein, 2001; Heckman et al., 2006). That is, psychological well-being is important both as an outcome in its own right, as well as as a potential mechanism for some of the large, later-life impacts of early life factors measured in the literature to date. As Heckman (2006, 2007) and Cunha et al. (2010) have recently shown, personality outcomes are formed early and are likely greatly influenced by factors in early life. We demonstrate that these impacts continue to persist in the long-run, suggesting that intervention at early ages to blunt the effects of trauma (economic and otherwise) could spare some fraction of adults from severe psychological distress and its economic consequences.

Our results suggest two mutually compatible interpretations of the impacts on mental health. First, mental health could be a mechanism that partly explains other economic impacts of early life shocks. Second, mental health is a final outcome that depends partly on these economic and health-related outcomes. In this case, the indirect effects of health and economic welfare on mental illness would be in addition to direct effects we have estimated. Previous measurements of the welfare importance of early life factors, while already large, are underestimated to the degree that they do not include mental health.

This study complements a growing set of studies in the development economics literature that regard economic outcomes as incomplete measurements of welfare (Angelucci et al., 2013; Baird et al., 2013; Devoto et al., 2012; Haushofer and Shapiro, 2013). Measures such as mental health add to the richness of research on the efficacy of welfare interventions and should be increasingly measured, reviewed, and addressed in policy recommendations. This is particularly true in developing contexts where these dimensions of welfare have received less attention and where resources for improving mental health are most limited.

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Table 1. Summary Statistics

	(1) Mean	(2) s.d.	(3) Min	(4) Max	(5) N
Mental Health					
ln K10	2.92	0.31	2.30	3.91	7,815
Severe distress	0.074	0.26	0	1	7,815
Cocoa Price Shocks					
ln(Cocoa price) X Region any cocoa: Year of birth	3.30	2.01	0	5.52	7,741
Controls					
Female	0.55	0.50	0	1	7,815
Year of birth	1,973	13.7	1,943	1,997	7,815
Head	0.49	0.50	0	1	7,815
Female X Head	0.15	0.36	0	1	7,815
Real Producer Price Series (1943-1997)					
Real Cocoa Price	105	60.1	31.1	251	55
ln(Cocoa Price)	4.50	0.55	3.44	5.52	55
Fraction of Farm Area Under Cocoa, By Region					
Ashanti	44.36%				
Brong Ahafo	31.80%				
Central	34.51%				
Eastern	26.20%				
Greater Accra	0.09%				
Northern	0.00%				
Upper East	0.00%				
Upper West	0.00%				
Volta	4.38%				
Western	53.95%				

Source: EGC-ISSER Socioeconomic Panel Survey and Teal (2002).

Table 2. Impacts of Y.O.B. Price Shock on Mental Distress

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		ln(K10)			*Severe Distress*			*Severe Distress (Logit Marginal Effects)*	
Price shock (Y.O.B.)	-0.023	-0.045**	-0.045**	-0.052***	-0.061***	-0.062***	-0.021**	-0.033***	-0.031**
	(0.016)	(0.022)	(0.022)	(0.016)	(0.022)	(0.022)	(0.010)	(0.013)	(0.012)
S.E. clustered by									
R.O.B.	(0.021)	(0.016)	(0.016)	(0.005)	(0.014)	(0.013)			
Y.O.B.	(0.012)	(0.019)	(0.018)	(0.013)	(0.018)	(0.019)			
C.G.M.: E.A. & Y.O.B.	(0.014)	(0.020)	(0.019)	(0.013)	(0.019)	(0.020)			
C.G.M.: R.O.B. & Y.O.B.	(0.021)	(0.019)	(0.019)	(0.005)	(0.021)	(0.018)			
Moulton: R.O.B.	(0.015)	(0.020)	(0.020)	(0.013)	(0.018)	(0.018)			
Moulton: Y.O.B.	(0.015)	(0.020)	(0.020)	(0.013)	(0.018)	(0.018)			
Wild cluster bootstrap	(0.019)	(0.025)	(0.027)	(0.027)	(0.034)	(0.034)			
Observations	7,741	7,741	7,741	7,741	7,741	7,741	7,740	7,740	7,710
Y.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Trends	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes	No	No	Yes
Outcome Mean: Non-cocoa regions	3.04	3.04	3.04	0.12	0.12	0.12	0.12	0.12	0.12
Outcome Mean: Cocoa regions	2.88	2.88	2.88	0.060	0.060	0.060	0.060	0.060	0.060

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. Number of enumeration areas per region range between 12 and 60. All regressions are OLS. Controls are female, head, female X head, ethnicity dummies, and religion dummies, unless otherwise indicated. Y.O.B. Indicates year of birth (accounts for both non-linear trends in unobservable at the country level and well-established associations between age at survey and mental health. R.O.B. Indicates region of birth. C.G.M. Indicates Cameron, Gelbach and Miller standard errors.

Table 3. Impacts of Y.O.B Price Shock on Related Personality Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	I am someone who is depressed, blue			*I am someone who is relaxed, handles stress well.*		
Price shock (Y.O.B.)	-0.223***	-0.278***	-0.280***	0.168***	0.121	0.142*
	(0.065)	(0.090)	(0.089)	(0.060)	(0.077)	(0.074)
Observations	6,973	6,973	6,973	7,005	7,005	7,005
Outcome mean	2.01	2.01	2.01	3.73	3.73	3.73
	I am someone who starts quarrels with others			*I am someone who tends to be disorganized*		
Price shock (Y.O.B.)	-0.085**	-0.087*	-0.100*	-0.235***	-0.134*	-0.141*
	(0.039)	(0.052)	(0.052)	(0.058)	(0.078)	(0.077)
Observations	6,998	6,998	6,998	6,987	6,987	6,987
Outcome mean	1.29	1.29	1.29	1.66	1.66	1.66
	I am someone who can be moody			*I am someone who can be cold and aloof*		
Price shock (Y.O.B.)	-0.254***	-0.261***	-0.275***	-0.248***	-0.288***	-0.292***
	(0.068)	(0.092)	(0.091)	(0.064)	(0.092)	(0.092)
Observations	6,989	6,989	6,989	7,000	7,000	7,000
Outcome mean	2.12	2.12	2.12	2.18	2.18	2.18
Y.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Trends	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. All regressions are OLS. Controls are female, head, female X head, ethnicity dummies, and religion dummies, unless otherwise indicated.

Table 4. Heterogeneous Impacts on Mental Distress

	(1)	(2)	(3)	(4)
	ln(K10)	*Severe Distress*	*ln(K10)*	*Severe Distress*
	Interact with "Female"		*Interact with "Father in Agriculture"*	
Price shock (Y.O.B.)	-0.046**	-0.063***	-0.042*	-0.052**
	(0.022)	(0.022)	(0.022)	(0.023)
Shock X Interaction	0.000	0.005*	-0.010	-0.009*
	(0.003)	(0.003)	(0.006)	(0.005)
Observations	7,741	7,741	7,047	7,047
Mean of Interaction Variable	0.552	0.552	0.655	0.655
Outcome mean	2.92	0.074	2.92	0.074
Additional Regressors	Y.O.B & R.O.B. Fixed Effects; R.O.B. Trends; Controls			
	Interact with "Akan"		*Baseline specification*	
Price shock (Y.O.B.)	-0.034	-0.048**	-0.045**	-0.062***
	(0.023)	(0.022)	(0.022)	(0.022)
Shock X Interaction	-0.020*	-0.021***		
	(0.012)	(0.008)		
Observations	7,719	7,719	7,741	7,741
Mean of Interaction Variable	0.444	0.444		
Outcome mean	2.92	0.074	2.92	0.074
Additional Regressors	Y.O.B & R.O.B. Fixed Effects; R.O.B. Trends; Controls			

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. All regressions are OLS and include the uninteracted "Interaction" variable. Controls are female, head, female X head, ethnicity dummies, and religion dummies, unless otherwise indicated.

Table 5. Impacts of Y.O.B Price Shocks (Binary) on Adult Outcomes and their Contribution to Total Treatment Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(K10)	*Severe Distress*	*Cash Savings*	*Physical Assets*	*Self Employed*	*Literacy*	*Height*	*BMI*
Binary Shock X Cash Savings	-0.0220***	-0.0243***						
	(0.0067)	(0.0058)						
Binary Shock X Physical Assets	0.0013	0.0045						
	(0.0087)	(0.0074)						
Binary Shock X Self Employed	-0.0659***	-0.0658***						
	(0.0244)	(0.0226)						
Binary Shock X Literacy	-0.0301	-0.0219						
	(0.0263)	(0.0222)						
Binary Shock X BMI	0.0011	-0.0086						
	(0.0113)	(0.0117)						
Binary Shock X Height	-0.0056	0.0070						
	(0.0110)	(0.0096)						
Binary Price shock (Y.O.B.)	0.0922	-0.0838	0.0552	0.0880	0.0650**	0.0284	0.0855	-0.0255
	(0.2235)	(0.2012)	(0.0855)	(0.0874)	(0.0308)	(0.0363)	(0.0729)	(0.0547)
Observations	7,324	7,324	7,324	7,324	7,324	7,324	7,324	7,324
Outcome mean	2.92	0.074	0.195	0.389	0.637	0.470	5.352	5.352
Additional Regressors	Y.O.B & R.O.B. Fixed Effects; R.O.B. Trends; Controls							
	Total Treatment Effect							
	ln(K10)	*Severe Distress*	*Cash Savings*	*Physical Assets*	*Self Employed*	*Literacy*	*Height*	*BMI*
Binary Price shock (Y.O.B.)	-0.0638**	-0.0573**						
	(0.0251)	(0.0263)						
Percent Contribution to Total			1.9055	-0.1810	6.7090*	1.3386	0.7447	0.0425
Effect on ln(K10)			(0.2865)	(0.9425)	(0.0805)	(0.261)	(0.326)	(0.4625)
Percent Contribution to Total			2.3423	-0.6976	7.4688*	1.0855	-1.0475	-0.3818
Effect on Severe Distress			(0.29)	(0.8165)	(0.0785)	(0.2595)	(0.2685)	(0.358)
Observations	7,324	7,324						
Outcome mean	2.92	0.074						
Additional Regressors	Y.O.B & R.O.B. Fixed Effects; R.O.B. Trends; Controls							

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. For analytical tractibility in the mediation analysis, the Y.O.B. price shock has been transformed to a binary variable taking value 1 if the price in the region of birth fell above the region-specific mean during the individual's Y.O.B. Cash savings, physical assets, height in cm, and BMI are transformed into standard deviation units for ease of comparison. Self employed and literacy are binary variables and are not transformed. All regressions are OLS. Controls are female, head, female X head, ethnicity dummies, and religion dummies, along with all mediator variables wherever these variables are not outcomes.

Table 6. Other Adult Outcomes, Maternal Health, and Investment Responses (DHS and Ghanaian Census)

	(1)	(2)	(3)	(4)	(5)
	Early-life Investments (DHS: Child Recode)				
	No. of Polio doses received	*No. of DPT doses received*	*Received Measles Vaccination*	*No. of Total Vaccinations*	*Months of Breastfeeding*
Price shock (Y.O.B.)	0.218**	0.317***	0.034	0.528***	0.989*
	(0.076)	(0.063)	(0.054)	(0.137)	(0.511)
Observations	11,903	11,829	11,809	11,725	13,134
Outcome mean	2.25	2.24	0.65	5.14	14.9
	Antenatal and At-birth Investments (DHS: Child Recode)				
	Prenatal Doctor Visit	*Received BCG Vaccination*	*Received Polio 0 dose*	*Home Delivery*	*Doctor Attended Delivery*
Price shock (Y.O.B.)	0.085*	-0.034	-0.010	0.028	-0.017
	(0.042)	(0.042)	(0.107)	(0.066)	(0.021)
Observations	9,582	11,886	9,067	11,101	11,090
Outcome mean	0.22	0.85	0.49	0.55	0.073
	Maternal Health (DHS: Individual Recode)			*Occupation (DHS: Individual Recode)*	
	Weight (no outliers)	*BMI (no outliers)*		*Not Working (DHS)*	*Agricultural Self Employment*
Price shock (Y.O.B.)				-0.036	0.065***
				(0.021)	(0.008)
Contemporaneous Price Shock	3.538***	1.044***			
	(0.861)	(0.319)			
Observations	14,411	14,022		19,831	19,831
Outcome mean	57.5	22.5		0.23	0.294
	Other Outcomes (Ghanaian Census: 2000 & 2010)				
	Dwelling Has Electricity	*Dwelling Has Piped Water*	*Speaks English*	*Literate*	*Years of Schooling*
Price shock (Y.O.B.)	0.011**	0.012**	0.015***	0.017***	0.462***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.048)
Observations	2,367,613	2,367,687	2,410,404	2,410,404	2,410,407
Outcome mean	0.55	0.43	0.63	0.68	6.51
Additional Regressors	Y.O.B & R.O.B. Fixed Effects; R.O.B. Trends; Controls				

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by region in parentheses, unless otherwise indicated. All regressions are OLS. Controls in the individual recode are years of education, rural, age, age squared, dummies for religion, and dummies for ethnicity. Controls in the child recode with year of birth shocks are maternal characteristics (years of education, rural, mother's age, mother's age squared, dummies for religion, and dummies for ethnicity) and predetermined child characteristics (birth order, multiple birth, female, and age in months at time of survey). Controls in the individual recode with contemporary shocks are years of education, rural, age, age squared, dummies for religion, and dummies for ethnicity. Controls in the Ghanaian Census specifications are dummies for gender, household head, gender by head, urban, religion, ethnicity and census year. Standard errors in these census specifications are clustered by district of survey.

Appendix: Not for publication.

A Robustness and Selection

A.1 Robustness

We demonstrate the statistical robustness of our main result in Table A1. First, we add quadratic trends by region of birth to the linear trends, and show that there is little change in the results. Second, we discard the Volta and Greater Accra regions from our data. Cocoa is grown in both regions, but in small amounts, making it unclear whether these can be cleanly included in the treatment or control groups. This too does little to diminish our main result.

Third, we find that replacing the price shock in an individual's year of birth with the three-year moving average of the price of cocoa neither improves nor worsens the precision of our main result. Fourth, following on Figures C.5 and C.6, we show that cocoa prices averaged over the year of birth and first two years of an individual's life predict later-life mental health.

Fifth, including other early life shocks that might be correlated with cocoa prices does not diminish our main results. Here, we control for region-level rainfall and temperature experienced in an individual's year of birth. Rainfall and temperature in the year of birth are themselves exogenous. While rainfall and temperature might have effects on a child's later-life outcomes through mechanisms other than parental income (e.g. maternal health), the stability in the effect of the price shock after controlling for rainfall and temperature supports the interpretation of the price shock as working through parental income. We show below that controlling for changes in Ghanaian political economy over time does not affect our results.

Sixth, because individuals may only know their ages imprecisely, we discard all individuals whose ages are divisible by 5. This reduces the sample by a third, and leads the estimated impact on the log K10 score to become insignificant, though the sign and magnitude of the coefficient are similar the main results in Table 2. Estimates of the effect of price shocks on severe mental distress, by contrast, remain significant and become larger in magnitude than in the baseline.

Finally, we check that impacts of year of birth shocks persist after controlling for exposure to shocks in years adjacent to the year of birth. We see in the bottom right panel of Table A1 that once we control for exposure to shocks in the 5 adjacent years before and the 5 adjacent years after the year of birth, the coefficient on the year of birth shock is of the same magnitude as in the main results, and is still significant at conventional levels for the severe distress outcome. Though the corresponding coefficients are not reported in Table A1, adjacent year shock exposures are statistically insignificant at conventional levels in the specification including all 11 years of exposure as regressors. We therefore cannot conclude whether adjacent year exposures have incremental impacts on adult mental distress. This imprecision is likely due to the high serial correlation in the underlying price time series. Raw pairwise correlations between year of birth shocks and shocks in the 5 years before and after the year of birth are between .975 and .995; while residual pairwise correlations after accounting for the full set of controls, trends and fixed effects in the main specification are between .077 and .794.

Below, we show that early-life shocks do not predict incomplete responses on the K10. We also show that we can find no evidence that migration into cocoa-producing regions responds to cocoa price shocks.[21]

[21] We conduct additional robustness checks that are not reported here. These results are available upon request. Our main results survive when we remove individuals born before 1960 or those who are less than 18 years old at the time of the survey. Using the real producer price without taking its logarithm still yields a significant effect on adult mental health. Defining cocoa-producing regions as those containing land suitable for cocoa changes little. Greater Accra moves to the control group, and the estimated effects are similar to the baseline. Similarly, if we interact the log real producer price of cocoa with the fraction of land in a region that is planted to cocoa, we find a negative coefficient; cocoa prices matter more where cocoa is more important. As additional solutions to possible age heaping, we collapse ages into three-year bins and use 2009 minus self-reported year of birth as an alternative to self-reported year of birth. Our results survive including fixed effects for each region intersected with birth during the rule of Jerry Rawlings. We find significant effects of early life cocoa price shocks even if the estimation is performed separately for household heads and non-heads. The effect on severe distress remains significant even when controlling for household fixed effects that compare two members of the same household.

A.2 Selection

Finally, we investigate the degree to which fertility decisions and infant mortality are affected by cocoa price shocks. Specifically, we might suspect that cohort size varies with caloric availability, as shown in Dyson (1991); that babies born during cocoa booms are born to different families than those born during busts, as shown in Dehejia and Lleras-Muney (2004); or that infant mortality varies with economic conditions, as shown in Miller and Urdinola (2010) and Baird et al. (2011).[22]

A.2.1 Mortality

Using the DHS birth recodes, we test for selective mortality by estimating

$$Mortality_{irt} = \alpha + \beta ln(CocoaPrice_t) \times CocoaProducer_r + x'_{irt}\gamma + \delta_r + t_r + \eta_t + \epsilon_{irt}. \quad (4)$$

Here, $Mortality_{irt}$ is an indicator for whether child i, who was born in year t, and whose mother lives in region r died in the first five years of life. The cocoa price shock variable $\beta ln(CocoaPrice_t) \times CocoaProducer_r$ is measured in the child's year of birth. δ_r is a vector of region fixed effects. t_r is a vector of region-specific time trends. η_t is a vector of year-of-birth fixed effects and α is an intercept. The vector of controls x_{irt} includes both maternal characteristics (years of education, rural, mother's year of birth, mother's year of birth squared, dummies for religion, and dummies for ethnicity) and predetermined child characteristics (birth order, multiple birth, and female). Standard errors are clustered by region. Results are reported in columns 1 through 4 of Table A2.

We find only limited evidence of selective mortality in childhood. Greater cocoa prices predict greater mortality, an effect that appears to be weaker among children of farmers.[23]

[22] These concerns are in line with the type of selection bias highlighted in the recent work by Lumey and van Poppel (2013) and Xu et al. (2016).

[23] This procyclical mortality impact is akin to, for example, the main result in Miller and Urdinola (2010). The most likely explanation for this phenomenon is that when the opportunity cost of time increases (e.g.,

We replace child mortality with infant mortality in Table A12, below. We find no evidence of selective infant mortality, nor of heterogeneity in the relationship between infant mortality and cocoa prices. We investigate the degree to which selection on individual or parental characteristics in child mortality might explain our main results in Table A3, described below. We further show that our main shock variable does little to predict pre-determined characteristics of these children in Table A8, also described below.

A.2.2 Fertility

To test whether fertility responds to cocoa prices, we construct an artificial panel of women using the births recodes of the DHS. Each observation is a year of a woman's life, up to age 45. We use OLS to estimate the following (where the outcome is a dummy for whether the mother gave birth that year):

$$Birth_{irat} = \alpha + \beta ln(CocoaPrice_t) \times CocoaProducer_r + x'_{irat}\gamma + t_r + \theta_a + \eta_t + \lambda_m + \epsilon_{irat}. \quad (5)$$

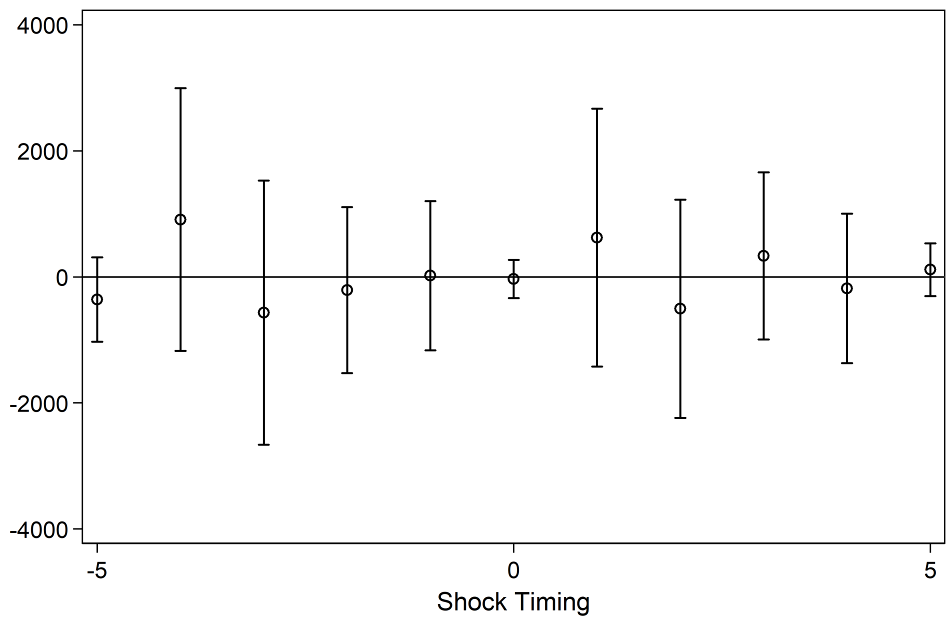
Here, λ_m is a fixed effect for mother. We are, then, restricting our identification to comparisons of fertility outcomes within the life of the same woman. These fixed effects will absorb our usual region fixed effects (δ_r) and many of the usual controls in x_{irat}.

We find, as shown in column 5 of Table A2, that higher cocoa prices predict greater fertility. We confirm this result using an exercise similar to that in Maccini and Yang (2009). We count the number of respondents in the EGC-ISSER data born in each region in each year, and test whether the number of individuals from cocoa-growing regions rises when cocoa prices are higher. These results (not reported) also suggest a similar increase in fertility in response to greater cocoa prices.

when the price of farm output is high), time spent farming likely increases and time spent childrearing likely decreases. If the production of child health is sensitive enough to time inputs, and/or if the price shock has a substantial enough impact on the value of time spent farming, it is possible that the negative impacts of time spent away from childrearing outweigh the positive effects of greater income.

We then include, as in columns 2 through 4, variables measuring parental economic status and their interactions with the price shock in order to investigate the degree to which this pro-cyclicality in fertility is uniform or concentrated among specific demographic groups. The results reported in columns 6 through 8 of Table A2 suggest that those self-employed in agriculture are more likely to time their fertility to correspond with high cocoa price periods.

FIGURE A.1: OBSERVATION COUNT BY SHOCK TIMING



This figure depicts impacts on the number of adults in the sample born in various years relative to shock timing. The timing of the shock is in years relative to the year of birth.

Figure A.1 depicts observation counts in the sample by the timing of shocks relative to birth year using data from the Ghanaian census. These counts represent, in a sense, the composite impacts of shocks on fertility and mortality. Two conclusions are apparent. First, there is no clear pattern in which the relative number of observations corresponding to individuals born in cocoa-growing regions responds to the producer price of cocoa. Second, although we are able to find a significant coefficient for prices precisely in the year of birth, this is driven by a contraction in the standard errors, and not the existence of a coefficient that is greater for this year than for others.

Last, we conduct and empirical exploration of the degree to which selection on individual or parental characteristics might explain our main results. That is, are our estimates of the impacts of year-of-birth price shocks on adult mental health generated in part through selective mortality and fertility? We attempt to address this question in three ways: 1) we directly include the parental characteristics from Table A2 (i.e. parents' education and occupations) as observable determinants of selection in the regression specification; 2) we re-estimate the main results including, in addition to our baseline controls, a (current) household fixed effect, under the assumption that adults living in the same household now (most likely spouses) were also likely to be born into households with similar unobservables; and 3) we reestimate our main results after applying corrective frequency weights to specific segments of the sample that are most likely to contribute to potential selection bias due to endogenous selection.

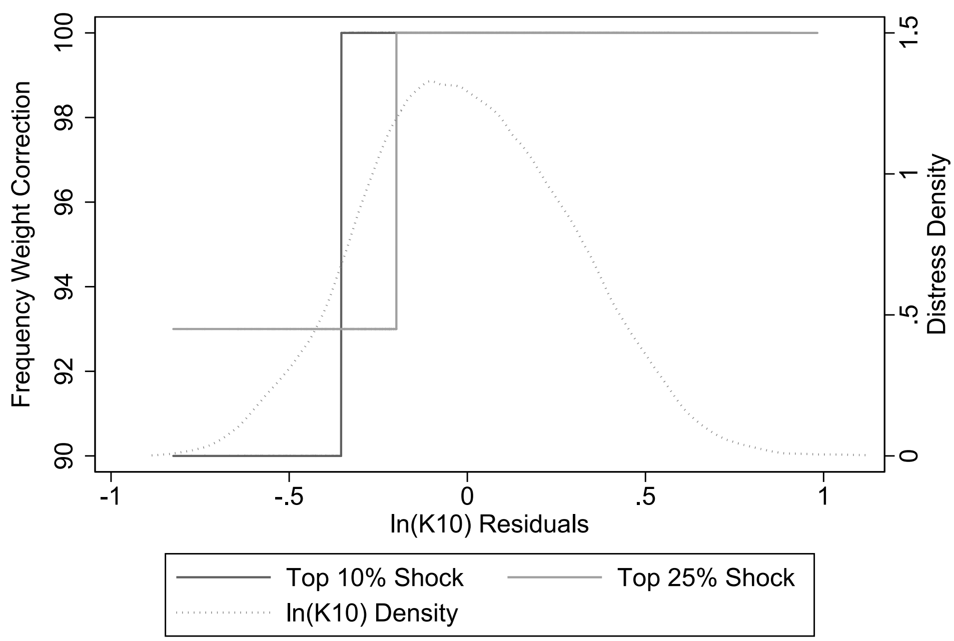
The results from all three robustness checks are reported in Table A3. In the top panel, columns 1 and 2 show that our main results are robust to the inclusion of controls for parents' education and occupations.[24] Estimates reported in columns 6 and 8 of the top panel of Table A3 also generally show robustness to the inclusion of current household (i.e., essentially spousal) fixed effects, though the estimate from the log(K10) specification is no longer significant at conventional levels. Columns 5 and 7 present the results from the original specification (without the additional fixed effects) estimated on the subsample of households with multiple respondents required to identify the household fixed effects in columns 4 and 6. These results are reported to check that the main results are largely preserved on this restricted subsample (with the caveat that the impact coefficient in the $ln(K10)$ regression

[24] Using these results from regressions with added controls, we also estimate the "implied ratio" of selection on unobservables to selection on the included observables (both individual and parental characteristics) that would be necessary to fully account for our main results (Altonji et al., 2005). We calculate two sets of implied ratios using the procedure developed by Altonji et al. (2005): 1) the necessary contribution of unobservable vis-a-vis observable individual controls (-61.1 for ln(K10) and -144.5 for severe distress), and 2) the analogous "implied ratio" of unobservable to observable parental controls (-8.8 for ln(K10) and 13.8 for severe distress). The implied ratios indicate that selection on unobservables would have to be between 8.7 and 144.5 times as large as selection on observables in order to fully explain our main results. These ratios do not tell us *how much* of the estimated impact is explained by selection, however.

becomes more diffuse).

In the bottom panel of Table A3, we report regression results analogous to the baseline results but with frequency weights used to correct for potential selection bias due to endogenous selective fertility and mortality. The results shown in Table A2 reveal that more individuals are born and survive infancy during high cocoa price periods. While we do not find evidence that these selective fertility and mortality impacts are differential by household demographic characteristics such as occupation and education, we might still be concerned this sample selection is confounding the analysis. Specifically, if the additional individuals in high price period cohorts are less likely to have mental distress, the selection might be contributing to the estimated impacts of price shocks on mental health.

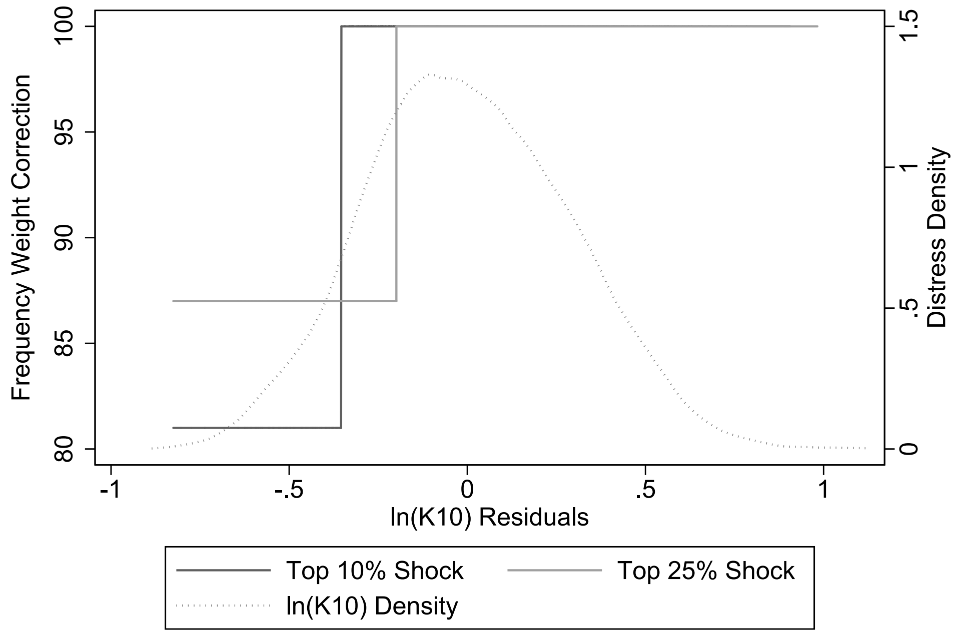
Figure A.2: Selection (6%) Frequency Weight Correction



Spread estimated 6% selection re-weighting over bottom 10 and 25% of K10 scores and top 10 and 25% of shock.
Re-weighting procedures are applied to shock and lnK10 residuals from regression with full FEs, controls and trends.

This figure depicts frequency weight corrections for subsample of individuals with high shock values and low mental distress. Two different sets of weights are depicted: one assuming individuals with top 10% shock values and bottom 10% ln(K10) scores are subsample impacted by selection (1% of full sample), and another assuming the affected subsample is larger including individuals with top 25% shock values and bottom 25% ln(K10) scores (6.6% of full sample). Selection is assumed to be 6% per unit shock as estimated in Table A2. Weights are scaled by 100 and rounded to the nearest integer to conform to weight requirements imposed by regression analysis.

Figure A.3: Selection (12%) Frequency Weight Correction



Spread estimated 12% selection re-weighting over bottom 10 and 25% of K10 scores and top 10 and 25% of shock.
Re-weighting procedures are applied to shock and lnK10 residuals from regression with full FEs, controls and trends.

This figure depicts analogous weights to those shown in Figure A.2, but with 12% selection per unit shock assumed rather than 6%.

In order to check the extent to which the pattern of results is preserved in the absence of these endogenously inflated cohorts, we use frequency weights to deflate the importance of high price, low mental distress observations. Assuming 6% selection per unit shock (the sum of the estimated selective mortality and fertility impacts in Table A2), we adjust downward the relative frequency weights of the subsample of observations with the highest shock values (top 10 or 25% of values) *and* lowest ln(K10) scores (bottom 10 and 25%) by $1/(1.06) = .943$ for every unit deviation of shock values from the mean of the shock variable.[25] That is, for the subsample of individuals with top 10% shock values and bottom 10% ln(K10) values, we adjust relative frequency weights to roughly .89 because shock values among this sample

[25]We use residual distributions of shock and distress values from regressions of these variables on the full set of controls, trends, and fixed effects to determine the selected subsamples. Note that while we focus the discussion of robustness to selection on the possibility of sample selection bias due to endogenous fertility and mortality, these robustness results also speak to endogenous misreporting of year of birth, which might produce non-classical measurement error in the shock variable as well.

are roughly 2 units higher than the mean shock value in the full sample. We construct an additional set of weights for 12% selection per unit shock to check robustness to the possibility of endogenous selection of the twice the magnitude estimated in Table A2. These 4 sets of weights are depicted in Figures A.2 and A.3.

Taking the specifications whose results are reported in columns 3 and 6 of Table 2 and reestimating with the addition of these frequency weights, we present selection-corrected results in the bottom panel of Table A3. The results show that both the magnitude and (for severe distress) significance of estimates are generally preserved across all 4 sets of corrective weights. Indeed, tests of statistical equivalence (not reported here for brevity) confirm that these coefficients do not differ statistically from the estimated impacts in our baseline results. We interpret the results from Table A3 as evidence that our baseline estimates do not suffer from significant selection bias.

B Additional Results

B.1 Controlling for political economy shocks

We demonstrate that our results survive controlling for additional shocks that might affect Ghana's regions unevenly in an individual's year of birth. First, we show that controlling for whether an individual shared the ethnicity of Ghana's political leader in his year of birth does not diminish our main result.[26] Results are presented in Table A4. Although there is some evidence that individuals benefit from sharing the ethnicity of the leader in early life (Franck and Rainer, 2012), this does not diminish our main result.

Similarly, we show that our result survives including political regime fixed effects specific to each region. This allows us to remove any unobserved heterogeneity due to favoritism

[26] We select the ethnic group of the leader who held power for the longest period of time during any given year. In particular, we assign "British" to years before 1960, "Akan" for 1960 through 1965, "Ga-Dangbe" for 1966 through 1968, "Akan" for 1969 through 1979, "Grusi" for 1980 and 1981, "Ewe" for 1982 through 2000, and "Akan" for years after 2001.

towards specific regions at different periods in Ghana's political history.[27] Results are presented in Table A4. The effect of early life cocoa prices on adult mental health remains.

B.2 Incomplete responses and specific indicators

It is possible that those individuals suffering most from mental distress are simply unable to complete the K10 questionnaire. To test for this, create a dummy variable indicating whether an individual was administered the K10 but did not complete it. We show in Table A5 that non-response does not diminish in response to early-life beneficial price shocks.

We also use Table A5 to show that it is not only one specific item in the K10 questionnaire that responds to early-life income shocks. We find that both indicators of nervousness, restlessness, and the feeling of being unable to cheer up all are improved by beneficial early-life price shocks. Although other indicators move insignificantly in response to price shocks, 9 of 10 have negative signs.

In addition, we use Table A5 to show that parental migration into cocoa-growing regions cannot explain our main result. As a proxy for parental migration, we create a dummy for "misplaced ethnic group" that equals one if an individual was not born in the modal Ghanaian region for his ethnic group. If ethnic groups from non-cocoa-growing regions migrated into cocoa-growing regions in response to cocoa price shocks, we would expect a positive effect of price shocks on our misplaced ethnic group measure. The coefficient estimate we find is negative and insignificant.

B.3 Alternate Price Series

Table A6 presents results from instrumental variables specifications in which the price shocks from our primary specifications is instrumented using alternate price series. Columns 2 and 5

[27] In particular, we code years before 1960 as "Colonial," years from 1960 to 1965 as "First Republic," years from 1966 to 1969 as "First Military Rule," years from 1970 to 1971 as "Second Republic," years from 1972 to 1979 as "Second Military Rule," years from 1980 to 1981 as "Third Republic," years from 1982 to 1992 as "Third Military Rule," and years from 1993 on as "Fourth Republic."

report results using global export prices and columns 3 and 6 report results using Ivory Coast export prices, both converted to real cedis. The results indicate robustness to instrumenting for cocoa prices in Ghana using alternate export price series in the case of severe distress. Using the continuous K10 measure, the estimated coefficients are larger than in the baseline, but marginally insignificant ($t = 1.51$ and $t = 1.38$, respectively). We interpret these results as support of the exogeneity of the Ghanaian cocoa price variations used in the main results.

B.4 Placebo Outcomes

Table A7 presents regression results from "placebo" variables we would not expect to be impacted by economic shocks at birth. The specifications are identical to those from the main results without controls, and the specific outcomes regressed are dummies for various ethnicities and religions. Overall, the estimates are small in magnitude and insignificantly different from 0.

Table A8 reports a similar exercise using data from the DHS births recodes. Using the same sample of births that we used when estimating equation (5), we estimate:

$$X_{irt} = \alpha + \beta ln(CocoaPrice_t) \times CocoaProducer_r + \delta_r + t_r + \eta_t + \epsilon_{irt}. \tag{6}$$

Here, all variables are defined as in (5), except that X_{irt} now reports a predetermined characteristic for child i, born in region r in year t. One at a time, we include all variables in X_{irt} that were treated as controls in (5). Further, we use factor analysis to build the first principal component of the child characteristics and the first principal component of the mother characteristics that were previously used as controls. Results in Table A8 show that very few of these former controls are correlated with our main shock variable. Of the three that are, two are dummies for ethnic groups that form less than 3% of the sample each (Guan and Mande). The third is maternal education, which responds negatively to the shock variable. This effect is, however, quite small relative to the standard deviation of maternal education, and its negative sign contrasts with the beneficial effects we find in our main results on adult mental health.

B.5 Price Changes and Spline Specifications

Table A9 investigates specifications for price changes as opposed to price level effects and non-linearities in the price level effects in separate specifications. The first row of Table A9 shows that positive price changes do not produce the same significant impacts on mental distress as do price level increases. The spline results reported on the bottom of Table A9 shows evidence that price impacts on mental distress are slightly larger at lower price levels. That is, we find some evidence of diminishing impacts of positive economic shocks in early-life on mental distress.

B.6 Alternate Mental Distress Outcome Definitions

Table A10 reports results from regressions of alternate definitions of mental distress outcomes. Specifically, we conduct a principal component analysis on the K10 score and then regress both the normalized first principal component and the calculated mean effect from the principal components on the same regressors as in the main results specification. The results using these alternate outcome definitions portray the same pattern as the main results.

B.7 Decile Effects and Climate Controls

Table A11 reports results from decile spline specifications with and without controls for deciles of the rain and temperature distribution. These estimates provide suggestive evidence that the main are derived largely by prices rising above their median levels, but not entirely from extreme right tail price events. These patterns persist even after controlling flexibly for non-linearities in weather events.

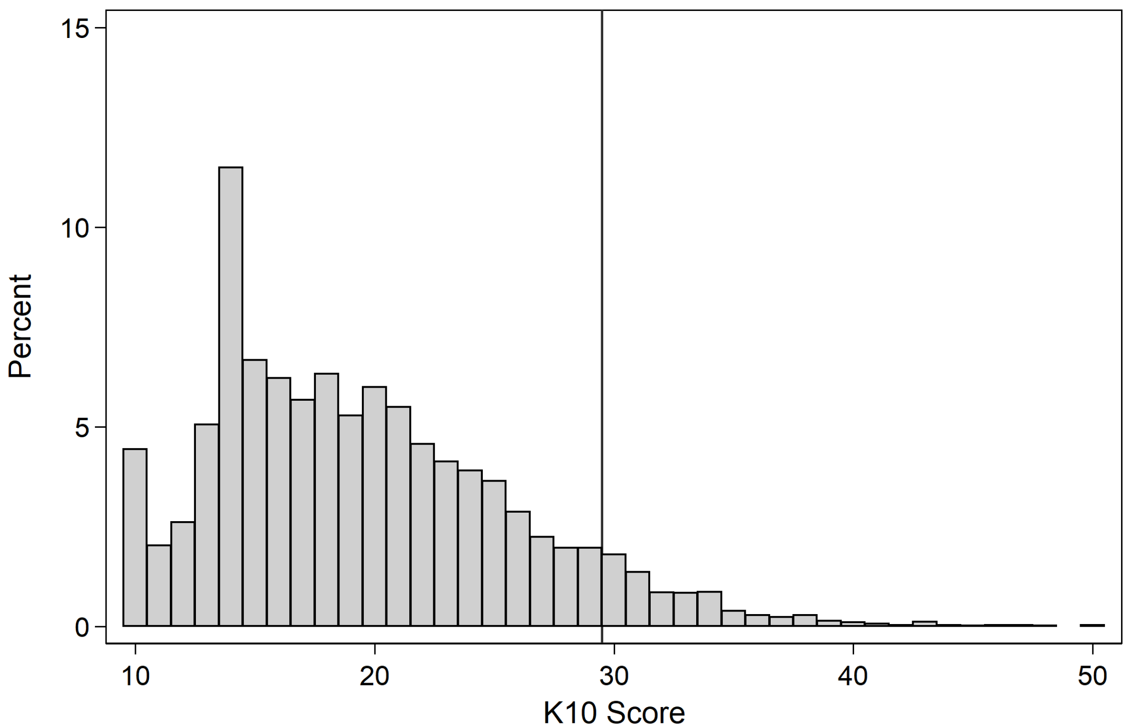
B.8 Heterogeneous Infant Mortality

Table A12 test for significant heterogeneity in infant mortality by maternal characteristics. There is little evidence of such heterogeneity in the data.

C Additional Figures

C.1 Histogram of K10 scores

Figure C.4: Histogram of K10 scores

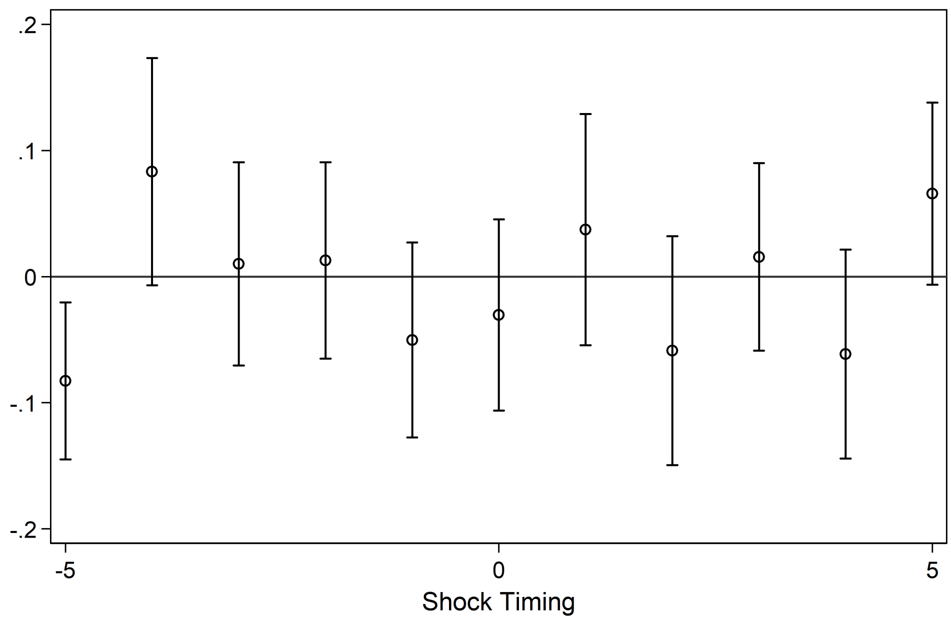


The vertical line in the figure represents the cutoff for severe distress.

Figure C.4 presents a histogram of the raw K10 scores. The vertical line depicts the cutoff for severe distress as defined in the main analysis. About 7.5 percent of observations lie above the cutoff.

C.2 Timing of Impact

FIGURE C.5: EFFECTS ON LN(K10) BY SHOCK TIMING

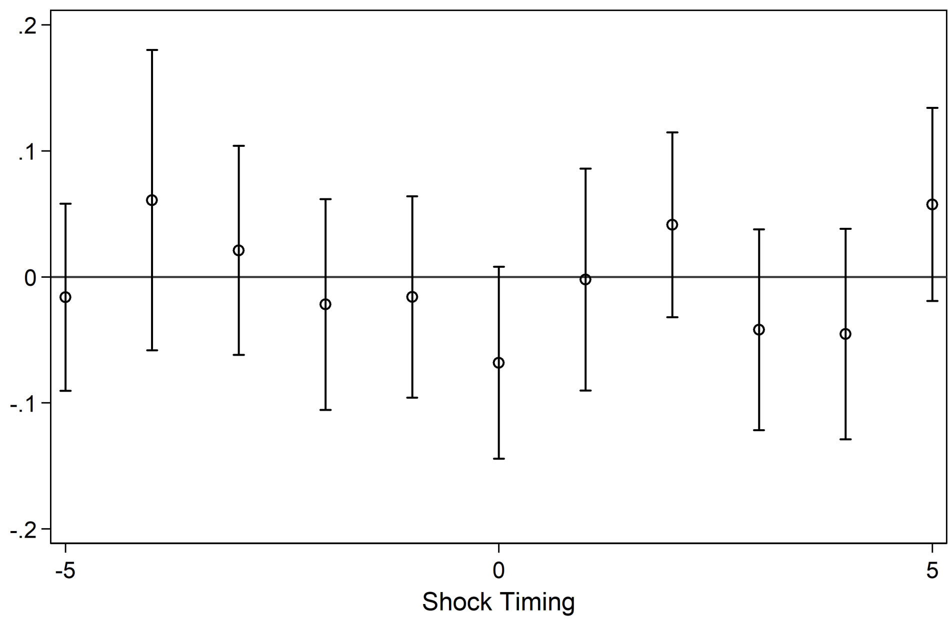


This figure depicts coefficient estimates and the 95% confidence interval for the main specification with price shocks in 5 adjacent years added, along with region trends, controls, and standard errors clustered by enumeration area. The timing of the shock is in years relative to the year of birth.

In Figures C.5 and C.6, we re-estimate our baseline specification including both region-specific trends and controls, with one change. In addition the year-of-birth shock, we also include price shocks experienced in years preceding and years after birth (the year of birth is denoted as year 0 in the figures). We report point estimates and 95% confidence intervals for the impact of price shocks in each year on $ln(K10)$ and the severe distress dummy in Figures C.5 and C.6, respectively.

The results are different in both the size/pattern of coefficients as well as precision (the size of standard error bands). In our estimation, both changes make sense. Including all years from -5 to +5 in one specification has the advantage that effects can be discerned from the partial as opposed to the absolute correlations (the additional impact of shifts in the cocoa price in any given year, conditional on price movements in all the other years), but of

Figure C.6: Effects on Severe Distress by Shock Timing



This figure depicts coefficient estimates and the 95% confidence interval for the main specification with price shocks in 5 adjacent years added, along with region trends, controls, and standard errors clustered by enumeration area. The timing of the shock is in years relative to the year of birth.

course has the drawbacks that precision will likely decline, and that autocorrelation in prices may result in the fact that residual variation is too small to identify effects well.

These figures, which harness this new specification with all consecutive-year impacts from -5 to +5 included, appear to reflect the drawbacks mentioned above. Specifically, precision suffers, as none of the estimated impact coefficients is significantly different from 0 at the 5 percent level (though the year of birth impact is significantly different from 0 at the 10 percent level in Figure C.6, where severe psychological distress is the outcome). Moreover, this lack of precision generates a case in which we cannot reject that the year of birth impact is the same as any other year from -5 to +5.

We cannot argue based on available evidence that there is a unique *in utero* impact of income shocks on adult mental health, distinguishable from impacts from shocks in the few years prior to gestation and the first few years of early life. In our view, this is not necessarily the argument we want to put forward. With regard to shocks in years prior to gestation, income shocks likely have longer lasting effects than a one-year horizon. This is for two reasons. First, an income-generated nutrition shock to a woman?s health stock in one year likely carries forward to impact health stock in subsequent years. Second, a shock to income in one year might generate persistent effects in the presence of credit market imperfections, which are rife in low-income contexts. For both these reasons, income in years prior to the gestation period could affect maternal, and thus fetal, health, and carry forward to adult mental health through the *in utero* channel.

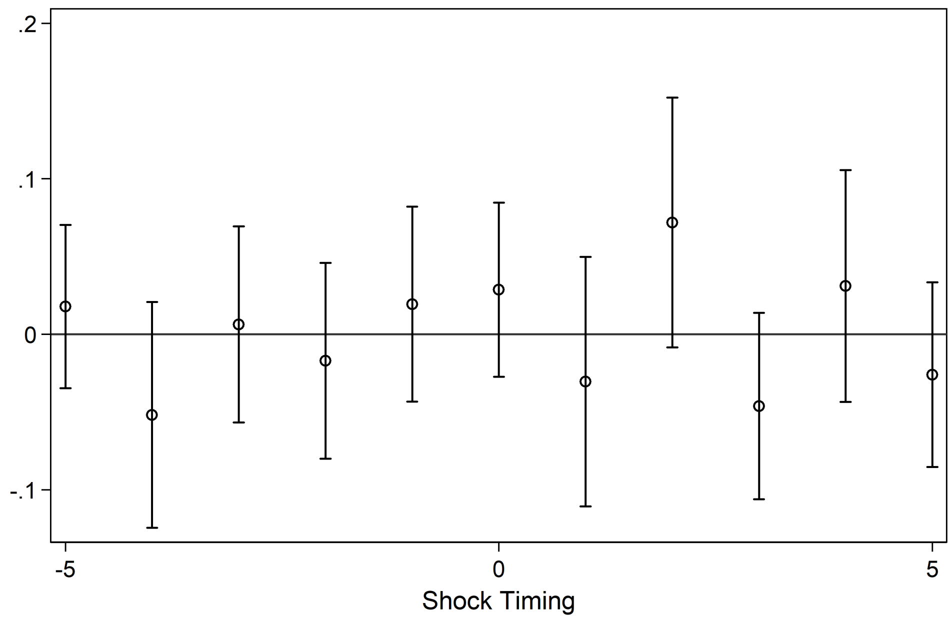
With regard to shocks in years after gestation (in early childhood), these shocks may well have independent effects on adult mental health through the inter-temporal nature of the child health production function (which are potentially different from impacts generated by analogous *in utero* exposure, but the event study figure does not allow us to determine this, unfortunately).

We also learn, despite the general lack of precision, that even after including all exposure variables from year -5 to year 5, the coefficient on the year-of-birth exposure in Figure C.6 remains significantly different from 0 at the 10 percent level and with roughly the same magnitude as the original estimate. This is of note, because it shows that, while there is a degree of autocorrelation in prices (and/or potential temporal economic spillovers in income effects for the household from year to year), including adjacent-year effects does not dampen the size of the year of birth impact.

C.3 Impacts in Non-cocoa-producing Regions

Figures C.7 and C.8 present impacts of shocks relative to year of birth for individuals from non-cocoa-producing regions only. Both figures reveal no evidence of significant impacts in non-cocoa-producing regions. Indeed, in both figures coefficient estimates are relatively small compared to the baseline estimates on mental health impacts, lending credibility to the idea underlying our identification strategy, that incomes in cocoa-producing regions are sensitive to the world price, while incomes in non-cocoa regions are not.

Figure C.7: Effects on ln(K10) by Shock Timing (Control Only)

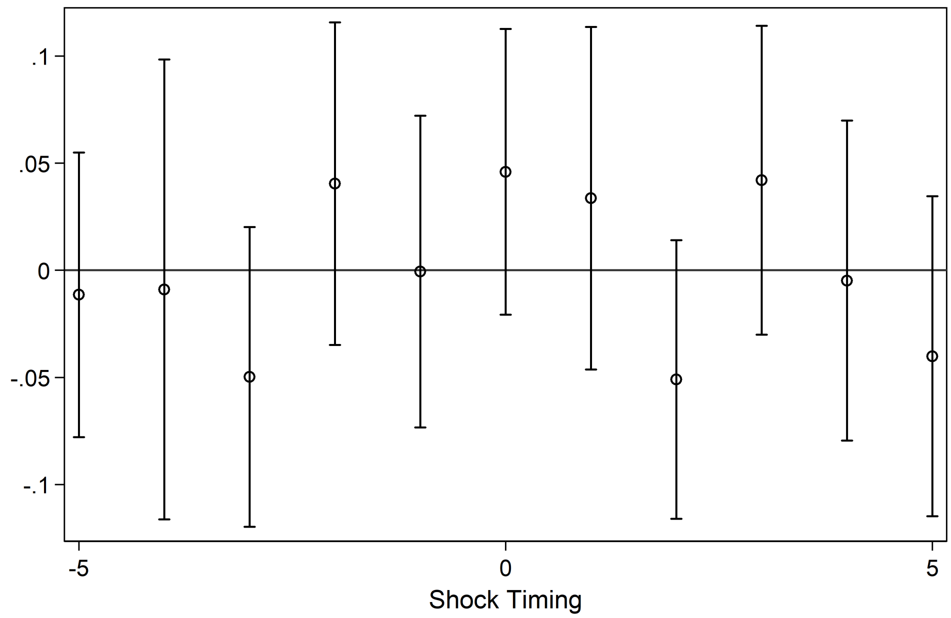


This figure depicts coefficient estimates and the 95% confidence interval for the main specification with price shocks in 5 adjacent years added, along with region trends, controls, and standard errors clustered by enumeration area. The timing of the shock is in years relative to the year of birth.

C.4 Robustness of Main Results to Using Variation in Intensity of Cocoa Cultivation

Here we examine the robustness of our results to using variation in the intensity of cocoa farming across regions (of individuals' births). In particular, we use the percent of each region's area under cocoa cultivation as the source of cross-sectional variation. We present the results in Table A13. These results are in line with the baseline results; the impact on severe distress is precisely estimated in all specifications (columns 4-6), and when controls are added to the ln(K10) specification, the impact becomes slightly more imprecise (just below levels of conventional significance, columns 1-3).

Figure C.8: Effects on Severe Distress by Shock Timing (Control Only)



This figure depicts coefficient estimates and the 95% confidence interval for the main specification with price shocks in 5 adjacent years added, along with region trends, controls, and standard errors clustered by enumeration area. The timing of the shock is in years relative to the year of birth.

C.5 Impact of Cocoa Price on Current Household Crop Revenues

Finally, to validate our presumed "first stage" we examine impacts of cocoa price fluctuations on current households? crop revenues. We have limited variation in the cocoa price because the EGC-ISSER data set is a cross section, but there is nevertheless variation month-to-month that is generated by the timing of survey. We use this variation to construct averages of the cocoa price in the 12 months before survey, as this is the window about which households are asked about crop revenues in the survey. We then interact this moving average with indicators or intensities of cocoa production both at the region as well as at the household level. Main effects and other controls are included in the specifications but their coefficient estimates are not reported. This analysis allows us to estimate the impact of cocoa price movements on crop revenues for current households as a way to check that the implied first stage in our main analysis is plausible.

Results are reported in Table A14. The first panel shows interactions of this 12-month average price with variables related to cocoa production defined at the region level, namely a dummy for any cocoa production in the region (akin to our baseline specification) and the percent of area under cocoa cultivation in the region. Both show significant and economically substantial impacts. (Standardized) cocoa price fluctuations have large effects, using both these cross-sectional variables, on crop revenues in the 12 months before survey.

In the next panel, we look instead at cross-sectional variation at the household level, since for the current household, we have good data on farming practices, particularly the intensity of cultivation. We construct an analogous dummy for any cocoa farmed for each household, as well as the percent of crop area under cocoa at the household level. The results on the interaction terms show that price fluctuations matter greatly for these households. Moreover, the coefficient magnitudes are larger by a factor of 2 than the regional-level analysis in the previous panel, indicating that households actually farming cocoa are affected more than other households in the region, which seems plausible even if other households are indirectly involved in cocoa production through their roles in other parts of the commodity supply chain.

The last panel reports results that combine these two sources of cross-sectional variation – at the regional and household level – and show interactions of variables at both these levels with the 12-month average cocoa price. We find that, as suspected, impacts on cocoa-farming households are driving the majority of the regional level result. However, we also note that when looking at the intensity of cocoa farming, we still find impacts on households in cocoa regions who do not farm cocoa, suggesting that non-cocoa-farming households in cocoa regions, through their involvement in the local value chain, are indeed impacted by changes in the cocoa price.

Table A1. Robustness of Impacts on Mental Distress

	(1)	(2)	(3)	(4)
	ln(K10)	*Severe Distress*	*ln(K10)*	*Severe Distress*
	Quadratic region trends		*Drop regions with 0-25% of farmland under cocoa*	
Price shock (Y.O.B.)	-0.046**	-0.059***	-0.043*	-0.064***
	(0.021)	(0.022)	(0.023)	(0.023)
Observations	7,741	7,741	6,243	6,243
Outcome mean	2.92	0.074	2.92	0.074
	Price measured as 3 year moving average		*Price averaged over ages 0-2*	
Price shock (Y.O.B.)	-0.042*	-0.042**	-0.056**	-0.066***
	(0.022)	(0.021)	(0.027)	(0.024)
Observations	7,741	7,741	7,741	7,741
Outcome mean	2.92	0.074	2.92	0.074
	Control for rainfall and temperature shocks		*Discard possible age heaping*	
Price shock (Y.O.B.)	-0.050**	-0.062***	-0.036	-0.096***
	(0.022)	(0.022)	(0.024)	(0.026)
Observations	7,741	7,741	5,375	5,375
Outcome mean	2.92	0.074	2.92	0.074
	Control for current family and social conditions		*Control for 5 year Lead/Lag Shocks*	
Price shock (Y.O.B.)	-0.051**	-0.060**	-0.030	-0.068*
	(0.022)	(0.024)	(0.039)	(0.039)
Observations	7,498	7,498	7,741	7,741
Outcome mean	2.92	0.074	2.92	0.074
Additional Regressors	Y.O.B & R.O.B. Fixed Effects; R.O.B. Trends; Controls			
	Baseline specification			
Price shock (Y.O.B.)	-0.045**	-0.062***		
	(0.022)	(0.022)		
Observations	7,741	7,741		
Outcome mean	2.92	0.074		
Additional Regressors	Y.O.B & R.O.B. Fixed Effects; R.O.B. Trends; Controls			

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. All regressions are OLS. Controls are female, head, female X head, ethnicity dummies, and religion dummies, unless otherwise indicated. Controls for current family and social conditions are number of kids in the HH, height in cm, general health, and dummies for each type of work, marital status, and reads English.

Table A2. Selection Checks (DHS)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Selective Mortality (Births Recode)				*Selective Fertility (Constructed Birth Panel)*			
	Died as child				*Birth*			
Price shock	0.017*	0.013	0.012	0.015	0.040***	0.041***	0.031***	0.025***
	(0.009)	(0.008)	(0.008)	(0.008)	(0.006)	(0.005)	(0.009)	(0.007)
Shock X Any Education		0.010				-0.003		
		(0.006)				(0.005)		
Any Education		-0.056*						
		(0.026)						
Shock X Agr. Self-Employed Occupation			-0.024**				0.036***	
			(0.010)				(0.006)	
Agr. Self-Employed Occupation			0.111**					
			(0.045)					
Shock X Professional Occupation				0.031				0.004
				(0.029)				(0.012)
Professional Occupation				-0.150				
				(0.125)				
Observations	67,356	67,356	53,037	53,037	452,110	452,110	349,103	349,103
Outcome mean	0.13	0.13	0.13	0.13	0.14	0.14	0.14	0.14
Additional Regressors	Y.O.B & R.O.B. Fixed Effects; R.O.B. Trends; Controls				Mother, Age, Year & Region Fixed Effects; Region Trends; Controls			

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by region in parentheses, unless otherwise indicated. All regressions are OLS. Controls are stock of past male births, stock of past female births, years of education, rural, dummies for religion, and dummies for ethnicity, unless otherwise indicated. Controls in the births recode are maternal characteristics (years of education, rural, mother's year of birth, mother's year of birth squared, dummies for religion, and dummies for ethnicity) and predetermined child characteristics (birth order, multiple birth, and female).

Table A3. Robustness of Mental Health Impacts to Selection Controls and Sample Corrections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(K10)	*Severe Distress*	*ln(K10)*	*Severe Distress*	*ln(K10)*		*Severe Distress*	
Price shock (Y.O.B.)	-0.051**	-0.058***	-0.045**	-0.062***	-0.029	-0.026	-0.065***	-0.086**
	(0.021)	(0.022)	(0.022)	(0.022)	(0.024)	(0.024)	(0.024)	(0.041)
Observations	7,741	7,741	7,741	7,741	5,964	5,964	5,964	5,964
Outcome mean	2.92	0.074	2.92	0.074	2.92	2.92	0.074	0.074
Selection Controls	Parents' Education & Occupation		Baseline Specification		Household Fixed Effects		Household Fixed Effects	
Spousal Fixed Effects	No	No	No	No	No	Yes	No	Yes
	ln(K10)	*Severe Distress*	*ln(K10)*	*Severe Distress*	*ln(K10)*	*Severe Distress*	*ln(K10)*	*Severe Distress*
Price shock (Y.O.B.)	-0.040*	-0.060***	-0.035	-0.059***	-0.034	-0.059***	-0.025	-0.057**
	(0.021)	(0.022)	(0.021)	(0.022)	(0.021)	(0.022)	(0.021)	(0.022)
Outcome mean	2.92	0.074	2.92	0.074	2.92	0.074	2.92	0.074
Observations	7,741	7,741	6,019	6,019	6,019	6,019	6,019	6,019
Assumed Selection / Unit Shoc	6%	6%	12%	12%	6%	6%	12%	12%
Subsample Affected by Selecti	Top 10% Shock, Bottom 10% ln(K10)				Top 25% Shock, Bottom 25% ln(K10)			
Percent of Sample Reweighted	1.01%	1.01%	1.01%	1.01%	6.63%	6.63%	6.63%	6.63%
Additional Regressors	Y.O.B & R.O.B. Fixed Effects; R.O.B. Trends; Controls		Y.O.B & R.O.B. Fixed Effects; R.O.B. Trends; Controls		Y.O.B & R.O.B. Fixed Effects; R.O.B. Trends; Controls		Y.O.B & R.O.B. Fixed Effects; R.O.B. Trends; Controls	

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. All regressions are OLS and include the uninteracted "Interaction" variable. Controls are female, head, female X head, ethnicity dummies, and religion dummies, unless otherwise indicated. Spousal Fixed Effects Sample differs from the sample used in the main results specification in terms of sample size and means of some covariates. Accordingly, we report results from our main specifications on this amended sample to check that estimates are generally preserved.

Table A4. Controlling for early life political economy

	(1)	(2)	(3)	(4)	(5)	(6)
		ln(K10)			*Severe Distress*	
	Control for ethnic match with president in year of birth					
Price shock (Y.O.B.)	-0.026	-0.047**	-0.048**	-0.052***	-0.062***	-0.062***
	(0.016)	(0.022)	(0.022)	(0.016)	(0.022)	(0.022)
Ethnic match with president (Y.O.B.)	-0.059***	-0.057***	-0.049***	-0.007	-0.007	-0.005
	(0.011)	(0.011)	(0.011)	(0.008)	(0.009)	(0.009)
	Control for regime X region fixed effects					
Price shock (Y.O.B.)	-0.051**	-0.057**	-0.057**	-0.049**	-0.058**	-0.059**
	(0.025)	(0.026)	(0.025)	(0.025)	(0.025)	(0.026)
Observations	7,741	7,741	7,741	7,741	7,741	7,741
Y.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Trends	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. All regressions are OLS. Controls are female, head, female X head, ethnicity dummies, and religion dummies, unless otherwise indicated.

Table A5. Incomplete responses, specific survey items, and migration

	(1)	(2)	(3)	(4)	(5)	(6)
	K10 Incomplete	A1 About how often did you feel tired out for no good reason	A2 About how often did you feel nervous	A3 About how often did you feel so nervous that nothing could calm you down	A4 About how often did you feel hopeless	A5 About how often did you feel restless or fidgety
Price shock (Y.O.B.)	0.010	-0.041	-0.112	-0.159***	-0.051	-0.101
	(0.011)	(0.074)	(0.070)	(0.059)	(0.077)	(0.065)
Observations	9,902	8,109	8,100	8,093	8,097	8,032
	A6 About how often did you feel so restless you could not sit still	A7 About how often did you feel depressed	A8 About how often did you feel that everything was an effort	A9 About how often did you feel so sad that nothing could cheer you up	A10 About how often did you fell worthless	Misplaced ethnic group
Price shock (Y.O.B.)	-0.153**	-0.086	0.047	-0.164**	-0.056	-0.023
	(0.071)	(0.073)	(0.097)	(0.069)	(0.077)	(0.020)
Observations	8,080	8,092	8,073	8,038	8,027	7,442
Y.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Trends	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. All regressions are OLS. Controls are female, head, female X head, ethnicity dummies, and religion dummies, unless otherwise indicated.

Table A6. Robustness to Instrumenting with Alternate Price Series

	(1)	(2)	(3)	(4)	(5)	(6)
		ln(K10)			*Severe Distress*	
	OLS	*IV: Bazzi-Blattman*	*IV: Williamson*	*OLS*	*IV: Bazzi-Blattman*	*IV: Williamson*
ln(Cocoa price) X Region any cocoa: Year of birth	-0.042	-0.053	-0.055	-0.091***	-0.098***	-0.088**
	(0.026)	(0.035)	(0.040)	(0.029)	(0.037)	(0.040)
Observations	5,642	5,642	5,642	5,642	5,642	5,642
Y.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Trends	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
KP Wald		1522	1155		1522	1155

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. All regressions are OLS. Controls are female, head, female X head, ethnicity dummies, and religion dummies, unless otherwise indicated. Y.O.B. Indicates year of birth (accounts for both non-linear trends in unobservable at the country level and well-established associations between age at survey and mental health. R.O.B. Indicates region of birth. C.G.M. Indicates Cameron, Gelbach and Miller standard errors. Instrument prices in columns 2 and 5 are taken from Bazzi and Blattman (2014); prices in columns 3 and 6 are taken from faostat.fao.org.

Table A7. Impacts of Y.O.B Price Shock on Placebo Ethnicity and Religion Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Ewe		*Ga-Dangbe*		*Grusi*		*Guan*		*Guma*		*Mande*		*Mole-Dagbani*		*Missing*	
Price shock (Y.O.B.)	0.007	0.016	0.006	-0.005	0.008	0.030*	0.016	0.012	0.020	0.020	0.000	-0.004	-0.075***	-0.067**	-0.000	-0.003
	(0.009)	(0.014)	(0.007)	(0.011)	(0.015)	(0.018)	(0.010)	(0.014)	(0.023)	(0.029)	(0.005)	(0.005)	(0.024)	(0.030)	(0.003)	(0.005)
Observations	7,741	7,741	7,741	7,741	7,741	7,741	7,741	7,741	7,741	7,741	7,741	7,741	7,741	7,741	7,741	7,741
	Religion 2		*Religion 3*		*Religion 4*		*Religion 5*		*Religion 6*		*Religion 7*		*Religion 8*		*Religion 9*	
Price shock (Y.O.B.)	0.006**	0.005	0.010	0.020	0.010	0.001	-0.010	0.001	-0.004	-0.005	0.004	0.018	-0.023	-0.039	-0.016	0.021
	(0.003)	(0.007)	(0.009)	(0.014)	(0.008)	(0.012)	(0.014)	(0.020)	(0.005)	(0.007)	(0.017)	(0.022)	(0.027)	(0.032)	(0.022)	(0.026)
Observations	7,741	7,741	7,741	7,741	7,741	7,741	7,741	7,741	7,741	7,741	7,741	7,741	7,741	7,741	7,741	7,741
Y.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Trends	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Controls	No	No	No	No	No	No	No	No	No	No	No	No	No	No	No	No

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. All regressions are OLS.

Table A8. Impacts of Y.O.B Price Shock on Placebo Outcomes in the DHS Births Recode

	(1)	(2)	(3)	(4)	(5)	(6)
	Birth order	*Multiple*	*Female*	*Resp. Education*	*Rural*	*Mother's year of birth*
Price shock (Y.O.B.)	-0.013	0.009	0.015	-0.467***	-0.001	-0.007
	(0.078)	(0.006)	(0.015)	(0.064)	(0.013)	(0.221)
Observations	67,676	67,676	67,676	67,609	67,676	67,676
Outcome Mean	3.188	0.0355	0.489	3.856	0.712	1961
Outcome s.d.	2.139	0.185	0.500	4.500	0.453	10.26
	Muslim	*Other*	*Other Christian*	*Protestant*	*Traditional/Spiritualist*	
Price shock (Y.O.B.)	0.004	0.007	-0.003	0.009	-0.008	
	(0.017)	(0.009)	(0.009)	(0.016)	(0.029)	
Observations	67,627	67,627	67,627	67,627	67,627	
Outcome Mean	0.150	0.102	0.303	0.198	0.0921	
Outcome s.d.	0.357	0.303	0.459	0.398	0.289	
	Ewe	*Ga-Adangbe*	*Grusi*	*Guan*	*Gurma*	
Price shock (Y.O.B.)	0.001	-0.003	-0.004	0.007**	0.015	
	(0.003)	(0.002)	(0.005)	(0.003)	(0.012)	
Observations	67,472	67,472	67,472	67,472	67,472	
Outcome Mean	0.128	0.0662	0.0331	0.0236	0.0422	
Outcome s.d.	0.335	0.249	0.179	0.152	0.201	
	Mande	*Mole-Dagbani*	*Other*	*Mother Factor*	*Child Factor*	
Price shock (Y.O.B.)	0.001*	-0.011	0.003	0.049	0.031	
	(0.000)	(0.008)	(0.006)	(0.037)	(0.034)	
Observations	67,472	67,472	67,472	67,356	67,676	
Outcome Mean	0.00105	0.196	0.0542	-4.30e-10	2.61e-09	
Outcome s.d.	0.0324	0.397	0.226	1.467	1.051	
Y.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Trends	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	No	No	No	No	No

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by region in parentheses, unless otherwise indicated. All regressions are OLS.

Table A9. Price Changes and Spline Specifications

	(1)	(2)	(3)	(4)	(5)	(6)
		ln(K10)			*Severe Distress*	
Price is greater than previous year	0.009	0.010	0.011	-0.022	-0.023	-0.022
	(0.014)	(0.014)	(0.013)	(0.015)	(0.015)	(0.015)
Observations	7,815	7,815	7,815	7,815	7,815	7,815
Splines						
Ln Price X Region Any Cocoa X Ln Price Between 3 And 4	-0.097***	-0.130***	-0.121***	-0.106***	-0.120***	-0.117***
	(0.036)	(0.043)	(0.042)	(0.038)	(0.043)	(0.043)
Ln Price X Region Any Cocoa X Ln Price Between 4 And 5	-0.084***	-0.114***	-0.106***	-0.098***	-0.110***	-0.107***
	(0.032)	(0.038)	(0.037)	(0.034)	(0.039)	(0.039)
Ln Price X Region Any Cocoa X Ln Price Between 5 And 6	-0.072***	-0.102***	-0.096***	-0.088***	-0.101***	-0.098***
	(0.026)	(0.033)	(0.033)	(0.028)	(0.033)	(0.033)
Observations	7,741	7,741	7,741	7,741	7,741	7,741
Y.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Trends	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. All regressions are OLS. Controls are female, head, female X head, ethnicity dummies, and religion dummies, unless otherwise indicated. Y.O.B. Indicates year of birth (accounts for both non-linear trends in unobservable at the country level and well-established associations between age at survey and mental health. R.O.B. Indicates region of birth. C.G.M. Indicates Cameron, Gelbach and Miller standard errors.

Table A10. Robustness to Alternate Mental Distress Outcome Definitions

	(1)	(2)	(3)	(4)	(5)	(6)
	Norm. 1st Principal Component of K10			*K10 Mean Effect*		
Price shock	-0.140***	-0.164**	-0.173**	-0.075**	-0.098**	-0.100**
	(0.054)	(0.070)	(0.068)	(0.033)	(0.044)	(0.043)
Observations	7,741	7,741	7,741	7,741	7,741	7,741
Y.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Trends	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. All regressions are OLS. Controls are female, head, female X head, ethnicity dummies, and religion dummies, unless otherwise indicated. Y.O.B. Indicates year of birth (accounts for both non-linear trends in unobservable at the country level and well-established associations between age at survey and mental health. R.O.B. Indicates region of birth. C.G.M. Indicates Cameron, Gelbach and Miller standard errors.

Table A11. Decile Effects and Climate Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(K10)				*Severe Distress*			
Price Xtile 2 X Cocoa Region	-0.033	-0.032	-0.033	-0.040	-0.028	-0.023	-0.024	-0.021
	(0.032)	(0.031)	(0.030)	(0.032)	(0.030)	(0.030)	(0.030)	(0.030)
Price Xtile 3 X Cocoa Region	0.032	0.033	0.029	0.021	-0.003	-0.001	-0.005	0.002
	(0.028)	(0.027)	(0.026)	(0.028)	(0.023)	(0.023)	(0.023)	(0.022)
Price Xtile 4 X Cocoa Region	0.005	-0.010	-0.012	-0.017	-0.033	-0.037	-0.037	-0.026
	(0.037)	(0.038)	(0.037)	(0.038)	(0.032)	(0.033)	(0.033)	(0.033)
Price Xtile 5 X Cocoa Region	-0.031	-0.038	-0.040	-0.043	-0.032	-0.034	-0.037	-0.031
	(0.033)	(0.033)	(0.032)	(0.034)	(0.033)	(0.033)	(0.033)	(0.034)
Price Xtile 6 X Cocoa Region	-0.067**	-0.077**	-0.076**	-0.086**	-0.071*	-0.074**	-0.076**	-0.071*
	(0.032)	(0.033)	(0.032)	(0.033)	(0.036)	(0.037)	(0.037)	(0.037)
Price Xtile 7 X Cocoa Region	-0.001	-0.017	-0.017	-0.023	-0.035	-0.040	-0.040	-0.038
	(0.028)	(0.031)	(0.031)	(0.032)	(0.032)	(0.034)	(0.034)	(0.036)
Price Xtile 8 X Cocoa Region	-0.040	-0.074	-0.082*	-0.082*	-0.087**	-0.101**	-0.102**	-0.085*
	(0.037)	(0.047)	(0.047)	(0.048)	(0.038)	(0.045)	(0.045)	(0.044)
Price Xtile 9 X Cocoa Region	-0.021	-0.058	-0.060	-0.056	-0.037	-0.055	-0.056	-0.044
	(0.036)	(0.044)	(0.044)	(0.045)	(0.040)	(0.044)	(0.044)	(0.047)
Price Xtile 10 X Cocoa Region	-0.040	-0.076	-0.076	-0.072	-0.116***	-0.132**	-0.133**	-0.122**
	(0.040)	(0.048)	(0.046)	(0.048)	(0.044)	(0.052)	(0.052)	(0.052)
Observations	7,815	7,815	7,815	7,741	7,815	7,815	7,815	7,741
Y.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Trends	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Controls	No	No	Yes	Yes	No	No	Yes	Yes
Rain and Temperature Deciles	No	No	No	Yes	No	No	No	Yes

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. All regressions are OLS. Controls are female, head, female X head, ethnicity dummies, and religion dummies, unless otherwise indicated. Y.O.B. Indicates year of birth (accounts for both non-linear trends in unobservable at the country level and well-established associations between age at survey and mental health. R.O.B. Indicates region of birth. C.G.M. Indicates Cameron, Gelbach and Miller standard errors.

Table A12. Heterogeneous Infant mortality

	(1)	(2)	(3)	(4)
	Selective Mortality (Births Recode)			
	Died as infant			
Price shock	0.003	0.001	0.004	0.005
	(0.009)	(0.008)	(0.010)	(0.010)
Shock X Any Education		0.005		
		(0.006)		
Any Education		-0.028		
		(0.026)		
Shock X Agr. Self-Employed Occupation			-0.010	
			(0.009)	
Agr. Self-Employed Occupation			0.047	
			(0.038)	
Shock X Professional Occupation				0.018
				(0.024)
Professional Occupation				-0.082
				(0.100)
Observations	67,356	67,356	53,037	53,037
Additional Regressors	Y.O.B & R.O.B. Fixed Effects; R.O.B. Trends; Controls			

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by region in parentheses, unless otherwise indicated. All regressions are OLS. Controls are maternal characteristics (years of education, rural, mother's year of birth, mother's year of birth squared, dummies for religion, and dummies for ethnicity) and predetermined child characteristics (birth order, multiple birth, and female).

Table A13. Impacts of Y.O.B. Price Shock on Mental Distress

	(1)	(2)	(3)	(4)	(5)	(6)
		ln(K10)			*Severe Distress*	
Y.O.B. Price X Pct area cocoa in R.O.B.	-0.071**	-0.080	-0.073	-0.083***	-0.112**	-0.111**
	(0.036)	(0.050)	(0.049)	(0.032)	(0.044)	(0.044)
Observations	7,741	7,741	7,741	7,741	7,741	7,741
Y.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R.O.B. Trends	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. Number of enumeration areas per region range between 12 and 60. All regressions are OLS. Controls are female, head, female X head, ethnicity dummies, and religion dummies, unless otherwise indicated. Y.O.B. Indicates year of birth (accounts for both non-linear trends in unobservable at the country level and well-established associations between age at survey and mental health. R.O.B. Indicates region of birth. C.G.M. Indicates Cameron, Gelbach and Miller standard errors.

Table A14. Impacts of contemporary prices on income

	(1)	(2)	(3)	(4)
	Revenue from crops: Last 12 months			
12 Month Moving Average from Survey X Any cocoa in current region	4,685.145***	3,496.275**		
	(1,558.921)	(1,563.048)		
12 Month Moving Average from Survey X Pct cocoa in current region			12,850.184***	11,136.110***
			(3,986.142)	(3,846.295)
Observations	5,009	5,009	5,009	5,009
	Revenue from crops: Last 12 months			
12 Month Moving Average from Survey X Any cocoa in household	8,876.151**	8,878.311**		
	(3,866.165)	(3,884.988)		
Any cocoa in household	990.831***	962.956***		
	(109.021)	(106.590)		
12 Month Moving Average from Survey X Pct area cocoa in household			12,696.841***	12,594.995***
			(4,262.376)	(4,357.569)
Pct area cocoa in household			1,225.530***	1,190.108***
			(137.552)	(135.786)
Observations	5,009	5,009	5,009	5,009
	Revenue from crops: Last 12 months			
12 Month Moving Average from Survey X Any cocoa in current region	1,977.057*	706.312		
	(1,105.735)	(1,125.517)		
12 Month Moving Average from Survey X Any cocoa in household	8,464.142**	8,734.990**		
	(3,815.011)	(3,836.436)		
Any cocoa in household	990.160***	962.755***		
	(109.087)	(106.663)		
12 Month Moving Average from Survey X Pct cocoa in current region			6,975.538**	5,313.221*
			(3,066.701)	(2,878.247)
12 Month Moving Average from Survey X Pct area cocoa in household			11,331.210***	11,562.509***
			(4,175.433)	(4,266.253)
Pct area cocoa in household			1,226.389***	1,190.956***
			(137.507)	(135.707)
Observations	5,009	5,009	5,009	5,009
Month of Survey Fixed Effects	Yes	Yes	Yes	Yes
Region of Survey Fixed Effects	Yes	Yes	Yes	Yes
HH Head Controls	No	Yes	No	Yes

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Prices refer to log prices, re-centered to have mean zero. Standard errors clustered by enumeration area in parentheses, unless otherwise indicated. Number of enumeration areas per region range between 12 and 60. All regressions are OLS. Controls are female, head, female X head, ethnicity dummies, and religion dummies, unless otherwise indicated. Y.O.B. Indicates year of birth (accounts for both non-linear trends in unobservable at the country level and well-established associations between age at survey and mental health. R.O.B. Indicates region of birth. C.G.M. Indicates Cameron, Gelbach and Miller

Table A15. DHS Investment Responses With No Mother Controls

	(1)	(2)	(3)	(4)	(5)
	Early-life Investments (DHS: Child Recode)				
	No. of Polio doses received	*No. of DPT doses received*	*Received Measles Vaccination*	*No. of Total Vaccinations*	*Months of Breastfeeding*
Price shock (Y.O.B.)	0.196*	0.296***	0.025	0.481**	0.927
	(0.101)	(0.069)	(0.064)	(0.189)	(0.519)
Observations	11,963	11,888	11,868	11,783	13,196
	Antenatal and At-birth Investments (DHS: Child Recode)				
	Prenatal Doctor Visit	*Received BCG Vaccination*	*Received Polio 0 dose*	*Home Delivery*	*Doctor Attended Delivery*
Price shock (Y.O.B.)	0.091*	-0.038	-0.014	0.033	-0.016
	(0.044)	(0.049)	(0.128)	(0.058)	(0.022)
Observations	9,635	11,946	9,090	11,160	11,149
Additional Regressors	Y.O.B & R.O.B. Fixed Effects; R.O.B. Trends; Controls				

Notes: ***Significant at 1%, **Significant at 5%, *Significant at 10%. Standard errors clustered by region in parentheses, unless otherwise indicated. All regressions are OLS. Controls in the child recode with year of birth shocks are predetermined child characteristics (birth order, multiple birth, female, and age in months at time of survey).