Multigenerational Effects of Prenatal Exposure to Ramadan

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Abstract

Using administrative tax records, two large-scale nationally representative surveys, and a purpose-built household survey from Pakistan, we estimate the long-run effects of prenatal Ramadan fasting. Maternal fasting reduces birth weight, educational attainment, and adult earnings, with average ATT effects approximately 2.5 times larger than ITT estimates. We document three novel findings. First, exposure and fasting rates do not vary by socioeconomic status, yet impacts concentrate among poorer households, implying a poverty penalty in consequences of early-life shocks. Second, exposure transmits intergenerationally through both maternal and paternal lines. Third, effects vary by trimester in patterns consistent with developmental biology. Back-of-the-envelope calculations suggest this single mechanism permanently lowers Pakistan's steady-state GDP by approximately 1.5% or by $5.6 billion annually.

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I Introduction

Roughly two billion people—a quarter of the world's population—are Muslim (Pew Research Center, 2025). Daytime fasting during Ramadan is a central religious obligation for Muslims, and although pregnant women are conditionally exempt, most observe the fast in practice. Pregnancy is a critical period for human development, and in-utero exposure to maternal fasting can have lasting consequences on exposed children. While we understand these negative consequences for some later-life outcomes (Almond *et al.*, 2018), we do not understand fully how they interact with parental poverty. If the ability to buffer or remediate early-life shocks depends on parental endowment (Cunha & Heckman, 2007; Hoynes *et al.*, 2016), their effects must fall disproportionately on the poor. This distinction is critical as nearly one-third of the world's extreme poor live in Muslim countries (SESRIC, 2015). To the extent that poverty exacerbates these shocks, Ramadan fasting could become a channel through which poverty persists and transmits across generations.

In this paper, we address this gap in the literature. We focus on Pakistan—the world's second-largest Muslim country and home to nearly 11% of the global Muslim population, where roughly 40% of citizens (~92 million people) live below the poverty line (World Bank, 2024). Using a dataset of unusual scope and depth, combining administrative tax records with three household surveys spanning 66 birth cohorts and three generations of Pakistanis, we make four contributions. First, where existing studies often focus on narrow outcomes—such as disability and self-reported health—we trace impacts across the full lifecycle of exposed individuals, from birth weight through educational attainment to earnings. Second, we uncover that the average effects—the primary focus of existing work—conceal a pronounced poverty penalty: although fasting rates do not vary with income, the resulting harmful effects are concentrated on children from poorer households. Third, we extend the analysis to the second generation, showing that the shock propagates forward to children independent of their own in-utero exposure through both paternal and maternal lines. Fourth, we move beyond intention-to-treat estimates by fielding a purpose-built survey that measures actual fasting behavior and allows us to recover the average treatment effect on the treated.

Fasting during Ramadan is one of the five pillars of Islam. It entails complete abstinence from food and water from sunrise to sunset, making it primarily a nutritional shock. Schofield (2020) estimates that caloric intake during Ramadan declines by nearly 600 calories per person per day, with associated shocks such as sleep disruption and dehydration playing a secondary role. Because pregnancy spans approximately nine months, roughly 82% of all births involve some gestational overlap with Ramadan. In our setting, an exposed fetus may experience a deprivation of nutrients and water for an average of 13 hours a day for up to 30 consecutive days. Biomedical literature has documented many channels through which such nutritional deprivation may alter fetal development, with persistent effects on health and human capital (Gluckman & Hanson, 2004; Wadhwa *et al.*, 2009). Crucially, emerging evidence also suggests that such effects can transmit to the next generation through biological and economic channels (Klengel *et al.*, 2016; Almond *et al.*, 2018).

Our research design builds on the seminal contribution in this field by Almond & Mazumder (2011). It exploits the asynchronous rotation of the Islamic lunar calendar through the solar Gregorian calendar. Because the lunar year is approximately 11 days shorter than the solar year, Ramadan cycles through the Gregorian calendar every 33 years, effectively decoupling the timing of the fast from seasonal confounders. We leverage this variation by estimating a specification that includes unrestricted year, month, and district of birth fixed effects. Identification thus solely comes from the variation in pregnancy's overlap with Ramadan induced by the lunar drift within these narrow cells. With 66 birth cohorts spanning two complete lunar cycles, this design absorbs secular cohort trends, seasonal patterns, and spatial heterogeneity while affording sufficient variation to disentangle month-of-birth effects from treatment effects. To explore the poverty penalty in outcomes, we extend this framework by fully interacting exposure measures with parental poverty indicators.

The validity of our research design rests on the assumption that conception timing is not manipulated in anticipation of the fast. We empirically verify this condition through two complementary tests. First, we test for strategic manipulation of conception timing. If parents systematically timed pregnancies to avoid the fast, we would expect to see a decline in birth rates in the corresponding conception months. Using nationally-representative Demographic and Health Survey (DHS), we find no such pattern: birth rates are uniformly distributed across the lunar calendar, with no discernible drop in months that would result in in-utero exposure. Second, we test for selection into exposure on observables. We examine 20 predetermined parental and household characteristics and find no statistically significant or economically meaningful differences between exposed and unexposed children. A joint F-test fails to reject the null of no differential selection. Using our purpose-built household survey, we further establish that the mother's decision to fast is also uncorrelated with household socioeconomic status. Together, these findings underscore the identification advantage of Ramadan-based designs: because its timing is governed by a lunar calendar common to all and the intent to fast is shared across the socioeconomic spectrum, exposure is plausibly orthogonal to household endowment—a key distinction from shocks such as famine or disease, which disproportionately affect the poor.

Having established the validity of our design, we proceed to document the average effects of in-utero exposure across the lifecycle of directly exposed individuals. These effects are already evident at birth: exposed infants weigh 200 grams (6.7%) less than their unexposed counterparts and are 8.5 percentage points (45.6%) more likely to be classified as low birth weight. These gaps persist in subsequent outcomes. Exposed children are 2.8 percentage points (3.9%) less likely to complete middle school and 3.5 percentage points (7.4%) less likely to complete high school. In the labor market, administrative tax data shows that exposed adults earn up to 2.7% less depending on the timing of exposure and are significantly less likely to reach the top half of the earnings distribution. These negative earnings effects are precisely estimated, statistically significant across all trimesters, and especially pronounced for exposure during the second trimester—a period widely viewed as especially sensitive for neurocognitive development (de Graaf-Peters & Hadders-Algra, 2006; Tau & Peterson, 2010).

The intergenerational structure of our data allows us to examine how impacts vary with parental resources. Both our household surveys—the DHS and Pakistan Social & Living Standards Measurement (PSLM)—link children to their parents, enabling a direct test of whether household poverty shapes the consequences of in-utero shocks. The results reveal what we term a poverty penalty: the large, negative effects of in-utero shocks are disproportionately concentrated on children of poor parents. Specifically, exposed children in poor households have on average 700 grams (23%) lower birth weight than their unexposed counterparts, a deficit roughly four times the average effect. By contrast, the reduction in birth weight among exposed children from non-poor households is far weaker. This pronounced gradient endures across the lifecycle. Reductions in middle-school completion and adult earnings, for example, are concentrated among children from poor households, though the earnings effects are estimated with greater uncertainty.

If in-utero exposure compromises the human capital stock of the first generation and the biological endowment of the next, these deficits may propagate across generations through economic and biological channels (Currie & Moretti, 2007; Heckman & Mosso, 2014). We test for such intergenerational transmission by examining whether parental exposure to Ramadan in utero affects the educational attainment of the next generation. Children of exposed fathers are 4.2 percentage points (6.5%) less likely to complete middle school and 3.7 percentage points (9.3%) less likely to complete high school. Children of exposed mothers exhibit a similar pattern. Joint tests reject the null of no effect for paternal exposure on both outcomes and for maternal exposure on high school completion. Similar strongly-negative and statistically significant effects exist in earnings, although we can estimate these effects only for the negatively selected sample of adult children living with their parents. These findings demonstrate that in-utero shocks reverberate across generations: children bear the burden of their parents' exposure, revealing a channel through which adherence to a universal religious norm may perpetuate poverty across generations.

Our results thus far have an intention-to-treat interpretation. We observe whether a pregnancy overlapped with Ramadan but not whether the mother actually fasted. This limitation is common in Ramadan-based designs, as realized fasting is rarely observed alongside long-run outcomes. To move beyond the ITT to LATE, we fielded a purpose-built household survey in Pakistan collecting detailed information on fasting behavior during pregnancy and related practices, norms, and beliefs. The data reveal that nearly 55% of pregnant women fast for at least one day during pregnancy and conditional on fasting they fast for an average of 22 days. Importantly, fasting is uncorrelated with household income and other socioeconomic characteristics, varying significantly only with self-reported religiosity. This absence of selection on household income reinforces the premise underlying the poverty penalty we document: poor and non-poor households are equally likely to fast, yet the poor experience disproportionately large long-run losses. The survey further indicates that fasting decisions reflect private religious convictions rather than social pressure or community norms, as respondents grossly underestimate the prevalence of pro-fasting beliefs among peers. Applying the two-sample, two-stage least squares (TS2SLS) approach (Angrist & Krueger, 1992; Inoue & Solon, 2010), we estimate that the observed fasting rate implies an earnings decline of 3.4% if the mother fasts for at least one day during pregnancy and of 5.0% if the mother fasts for the entire month. Given the absence of always-takers in this setting, these LATE estimates also coincide with the ATT.

Our paper contributes to the literature that estimates the effects of early life shocks on later life outcomes (Almond *et al.*, 2018; Attanasio *et al.*, 2022). Studies in this literature either use negative shocks to the in-utero and early-life environment—such as famines (Scholte *et al.*, 2015), disease (Bleakley, 2007; Bharadwaj *et al.*, 2018), and weather shocks (Maccini & Yang, 2009)—or positive interventions increasing resources to parents during pregnancy and early life of children to estimate these effects (Hoynes *et al.*, 2016; Baird *et al.*, 2016). Within this literature, Ramadan fasting has emerged as an important natural experiment because its timing is calendar-driven and the motivation to fast is shared across the SES distribution (Almond & Mazumder, 2011; van Ewijk, 2011; Majid, 2015). We advance this literature along the four dimensions we note above. Importantly, we uncover a pronounced poverty penalty in these effects, whereby although fasting rates do not vary with income, the resulting harmful effects fall primarily on children of poor households. Moreover, we extend the analysis to the second generation. Despite strong theoretical priors, direct causal evidence on the intergenerational propagation of early-life shocks in humans remains remarkably thin. In their influential survey, Almond *et al.* (2018) note that evidence from animal models makes such transmission "highly likely", potentially offering another reason for "the existence of poverty traps in some disadvantaged areas", yet rigorous evidence from human populations has been slow to materialize. The closest antecedent is Duflo *et al.* (2025), who provide experimental evidence from Ghana showing that maternal receipt of secondary school scholarships improves survival and cognitive development in the next generation—though only through the maternal line, with no positive effects from paternal scholarship receipt. Our findings complement this evidence by documenting intergenerational transmission from both parents in children's human capital outcomes.

Our paper also adds to the literature investigating how religious institutions and practices shape economic development and poverty dynamics (Becker *et al.*, 2024). One theoretical mechanism particularly relevant to our context is the religion as a club good model, wherein individuals engage in costly rituals to signal loyalty and commitment to the group (Iannaccone, 1992). While such costly signaling may enhance individual utility and may facilitate solutions to collective action problems, it can simultaneously impose significant long-run economic costs. Campante & Yanagizawa-Drott (2015), for instance, show that Ramadan observance, while increasing subjective well-being, can reduce labor productivity and hence economic growth. Our study complements this finding by documenting a biological channel through which adherence to religious practice, even if individually rational and utility-enhancing, can inadvertently reinforce poverty and economic disadvantage among adherents.

The remainder of the paper proceeds as follows. Section II surveys the biological and economic pathways through which prenatal fasting may affect offspring outcomes. Section III describes the data. Section IV presents our empirical strategy, including tests ruling out selection into Ramadan fasting. Section V reports the results. Section VI benchmarks our findings against the existing literature and discusses their macroeconomic implications. Section VII concludes.

II Ramadan Fasting as an In-Utero Shock

Ramadan is the ninth month of the Islamic calendar, and observing the fast (*sawm*) during this month is one of the five pillars of Islam. Fasting entails, among other things, abstaining from food and drink from sunrise to sunset and is obligatory for every Muslim apart from those who are exempt, such as children, the sick, and the elderly. Pregnant women are not expressly exempt, but they may defer fasting if they fear harm to themselves or the fetus, in which case they have to make up for the missed days by fasting later in the year.[1] The household survey we run indicates that nearly 40 percent of women are unaware of this conditional exemption (see Figure H.VII). Table I.I summarizes 19 studies that estimate fasting rates in a diverse set of countries, showing that fasting rates during pregnancy typically exceed 50% and remain substantial even in the U.S. and Europe. Consistent with these global estimates, we document a fasting rate of 55% in our sample (see Section V.F for details). Importantly, our survey data also suggests "reverse" pluralistic ignorance: women are not fasting to conform to a perceived oppressive norm; rather, they fast due to strong intrinsic convictions, even while believing society is more lenient than it actually is (see Appendix H.3 for details).

Pregnancy is one of the most critical periods for human development. Fasting during this period can disrupt the supply of nutrients to the fetus: Schofield (2020) estimates that caloric intake during Ramadan declines by nearly 600 calories per person per day. Beyond caloric restriction, altered eating and sleeping patterns can stress the maternal environment through other channels, including glucose fluctuations,

[1] The Encyclopedia of Islam, for example, writes that “The law permits relaxation ... if pregnant or nursing women fear it would be dangerous for them if they should fast” (see “Ṣawm”, in: Encyclopedia of Islam, Second Edition, Edited by: P. Bearman, Th. Bianquis, C.E. Bosworth, E. van Donzel, W.P. Heinrichs).

sleep deprivation, and psychological stress. Our survey corroborates these channels. Women who fast during pregnancy report weakness and exhaustion as the primary effects of fasting, followed by sleep loss, undernourishment, and stress; open-ended responses additionally cite vomiting, hunger, dizziness, and blood-pressure disturbances (see Appendix Figure H.IX). Symmetrically, among women who choose not to fast, the most commonly stated reason for not fasting is feeling too weak to do so (see Appendix Figure H.X).

Biomedical literature has documented many pathways through which fasting during pregnancy may diminish children's health, cognition, and skills endowment at birth. Appendix B.1 details three such mechanisms, focusing especially on the role of metabolic adaptations, epigenetic modifications, and stress hormones. A broad synthesis of this literature suggests that exposure during the first and second trimesters is especially consequential for neurodevelopment and cognitive function, while late-gestation exposure primarily impairs metabolic and endocrine functions, elevating later-life risks for diabetes and cardiovascular disease. These initial shocks may have disproportionately larger effects on children from poorer families. Appendix B.2 describes complementary biological and economic mechanisms responsible for this heterogeneity, which we call poverty penalty in this paper. Finally, there is growing evidence from both animal and human studies that early-life shocks can transmit to the next generation. Appendix B.3 surveys this evidence and describes mechanisms underlying this transmission.

III Data

We use data from the following four sources for our empirical analysis.

(i) Demographic and Health Survey (DHS): We focus on the *ever-married women* part of this nationally-representative survey, pooling together its four rounds carried out in 1990-1991, 2006-2007, 2012-2013, and 2017-2018. The DHS provides information on important birth outcomes, such as birth weight, as well as household characteristics including literacy, education, and assets. The data cover children born between 1952 and 2018 and therefore have a good overlap with our other data sources. Importantly, the DHS allows us to link children with their mothers, helping us rule out selection into fasting and enabling part of our analysis on poverty penalty.

(ii) Pakistan Social and Living Standards Measurement (PSLM): This is a nationally representative household survey conducted periodically to monitor progress in living conditions in Pakistan. We access its seven rounds carried out between 2004 and 2019. The key advantage of the PSLM is that it includes individual-level education and earnings, allowing us to directly measure human capital outcomes in adulthood. Like the DHS, the PSLM also allows linking parents to their children to examine intergenerational effects. We observe earnings in both the PSLM and administrative tax data. The two datasets offer complementary advantages for estimating earnings effects. The PSLM is nationally representative and thus does not have any selection concerns. But as survey data it affords limited power and is susceptible to measurement error. The tax data, by contrast, provide the precision and power needed to detect even modest effects, yet cover only individuals required to file a tax return. The tax-based estimates thus may not be fully representative of the population. An advantage of the PSLM, however, is that we can use it to simulate who should have filed a tax return under the prevailing tax code and thereby quantify selection into the administrative sample directly. This analysis suggests mild, negative selection into the tax data (see Section V.C.4 for details). For both PSLM and DHS, we observe the birth month and birth year for all respondents but the exact date of birth for 82% of respondents in PSLM and 7% in DHS. For the remainder of the sample, our baseline models impute the day of birth as the 15th of the month. We run robustness tests confirming that our estimates are unaffected when we draw the day of birth randomly from a uniform distribution over the month.

(iii) Administrative Tax Data: Our primary data source for earnings and related outcomes is the universe of personal income tax returns filed in Pakistan over the period 2007–2009. Besides tax returns, we also have access to the tax register, which contains information on individual characteristics such as the date and place of birth. We restrict our sample to individuals born between 1924 and 1989. These 66 birth cohorts cover two complete cycles of Ramadan's advance through the solar year, allowing us to control for birth seasonality nonparametrically. In the data, the birth date of individuals who know their year of birth but not the exact day is coded as the 1st January of the birth year. To avoid any measurement error arising from it, we drop all observations where the reported day of birth is the 1st of January. We also drop observations where the date of birth or income is missing. Applying these restrictions leaves us with a sample of around 0.8 million observations for which we observe both the birth date and earnings.

(iv) Self-Conducted Household Survey: A limitation of existing studies in this line of literature is the absence of a clearly identified first stage. Specifically, while these studies observe whether a pregnancy overlaps with the month of Ramadan, they do not directly observe if the mother actually fasted during this period. As a result, these studies can only estimate an intention-to-treat effect. To overcome this limitation and also to understand fasting habits, norms, and first-stage mechanisms underlying our estimates, we fielded a dedicated household survey in Pakistan. The survey focused exclusively on Lahore, the second-largest city in Pakistan. To align our survey closely with our primary earnings data, we defined our survey population as a randomly selected subsample from the administrative tax data. Specifically, we limited our sampling frame to tax filers who submitted at least one tax return during the five-year period 2015-2020 and who were based in Lahore. From this population, we randomly selected households and interviewed ever-pregnant women residing within these households. Survey respondents provided detailed information regarding their fasting habits during Ramadan for each pregnancy. Appendix H.1 details the survey protocol and design, including information on how we controlled for the social desirability bias in responses.

Summary Statistics. Table A.I presents summary statistics across these four data sources. Panel A highlights the stability of our identification strategy: the distribution of potential in-utero exposure is remarkably consistent across all datasets, with the share of individuals exposed in any trimester ranging narrowly between 82% and 84%.[2] Our sample sizes are large, particularly in the administrative tax data. Panel B reports summary statistics for the main outcomes. In the DHS, mean birth weight is 2,992 grams, with 21% of infants classified as low birth weight. The PSLM provides socioeconomic outcomes. Importantly, 27% of individuals live below the poverty line and schooling completion rates, though substantial, are far from universal. The tax records capture higher-income individuals by construction and the filer population is predominantly male (91%). Panel C reports data from our household survey on fasting behavior, beliefs, and norms. Over half of women fasted during pregnancy,

[2] The overlap rate exceeds three-quarters because gestation spans roughly $L \approx 270$ days while Ramadan lasts about $R \approx 30$ days. If conception is uniformly distributed over the year, the probability that an L-day gestation intersects an R-day interval is approximately $(L + R)/365 \approx (270 + 30)/365 \approx 0.82$; equivalently, avoiding any overlap requires conception within a $365 - (L + R) \approx 65$ day window.

averaging 22 days conditional on any fasting, and more than a third reported experiencing weakness or exhaustion. Perhaps most striking is the near-universal belief that pregnant women should fast (97%), combined with a large misperception gap: respondents estimated that only 50% of their peers shared this belief—even as only 58% of respondents were aware of the religious exemption from fasting during pregnancy.

It is important to emphasize that we do not observe the religion of a person in any of our datasets. However, since Pakistan is overwhelmingly Muslim—with more than 97 percent of the population identifying as Muslims (Esposito, 2004)—we treat the entire population as potentially subject to the in-utero exposure.

IV Empirical Strategy

This section describes our research design which builds on the canonical studies in this literature, in particular Almond & Mazumder (2011).

IV.A Potential Exposure

Figure I illustrates how we define exposure of an individual to the shock. We first calculate the conception date of the individual by subtracting the standard gestation length of 266 days from the date of birth. We then bin individuals into twelve groups depending on their conception date relative to Ramadan. The top two panels illustrate these steps for the two consecutive cohorts born in 1974 and 1975. We index relative months by τ. Individuals conceived in the month Ramadan began are indexed as $\tau = 0$; those conceived after the end of one Ramadan and born before the start of the next are indexed as $\tau \in \{-1, -2\}$; and the remainder are indexed $\tau \in \{1, \ldots, 9\}$ based on their gestation age relative to Ramadan.

The top two panels also illustrate that (i) some individuals in boundary months $\tau \in \{0, 9\}$ are not exposed for the full month and (ii) some individuals indexed in months $\tau \in \{-1, -2\}$ might be exposed if the pregnancy is longer than the normal term of 266 days. To account for these considerations, we define potential exposure of an individual i as

$$\text{(1)} \qquad \text{Potential Exposure}_i = \frac{1}{30} \sum_{d \in D_i} p_{di}(B_i).$$

Here p_{di} is the probability that i was in utero on day d of the relevant Ramadan given their birth date B_i and $D_i \in \{29, 30\}$ is the length of the relevant Ramadan. This measure is normalized to the unit interval $[0, 1]$ and equals 1 if the individual was in utero for the entirety of a 30-day Ramadan and 0 if the probability of overlap is zero for all days.

To calculate p_{di}, we assume that the gestation age at live birth follows a normal distribution with the mean of 266 and standard deviation of 11 days (Hoffman *et al.*, 2008; Jukic *et al.*, 2013).[3] Panel C shows the probability p_{di} we obtain under this assumption: it is almost one for up to 244 days before birth and declines sharply after that, approaching zero at 288 days. Feeding this into formula (1), we calculate potential exposure for each individual in our household survey (see Panel D). Specifically, we assign p_{di} implied by their birth date B_i for every day d of the relevant Ramadan and then average it over the whole month. We call it potential exposure as it represents the maximum exposure an individual can receive given the pregnancy's overlap with Ramadan. We later use the observed fasting rate to translate this measure into actual exposure, which serves as the first stage of our empirical framework.

IV.B Specification

Following the above evidence on exposure, we treat individuals in month $\tau = -2$ as the reference category (hereinafter referred to as "definitely not exposed") and compare them against others, estimating the following model

$$\text{(2)} \qquad y_{igmt} = \sum_{\tau=-1}^{9} \beta_\tau \, \mathbb{1}(em_i = \tau) + \gamma_g + \eta_m + \lambda_t + \mathbf{\Gamma}'\mathbf{X}_{igmt} + \varepsilon_{igmt}.$$

Here y_{igmt} is the outcome for individual i born in district g, month m, and year t; em_i denotes the pregnancy month of exposure; and $\mathbf{X}_{igmt}$ are a vector of controls. Our

[3] Note that assuming a normal distribution is only an approximation, as the actual distribution has a slight skew, with pre-term births more likely than post-term births. This skew is not important in our setting as ignoring it only results in a conservative definition of exposure.

administrative tax data affords us the power to estimate this exposure-month specification, where we omit the dummy for the baseline category $\tau = -2$ and include separate dummies for the other eleven months. For results based on non-admin data, we have less power and therefore estimate the corresponding dummy and trimester specifications. In these specifications, we drop partially-exposed individuals (exposure month $\tau = -1$) and compare the outcomes of exposed individuals to the definitely unexposed category ($\tau = -2$). The dummy specification pools all individuals with $\tau \in \{0, \ldots, 9\}$ into one category "exposed" and the trimester specification includes individuals exposed in pregnancy months $\tau \in \{0, \ldots, 3\}$ in the first trimester, $\tau \in \{4, \ldots, 6\}$ in the second trimester, and $\tau \in \{7, \ldots, 9\}$ in the third.

Birth seasonality is an important confounder in settings like ours. Because Ramadan follows a lunar calendar, its slow passage through the Western calendar allows us to include the standard month of birth fixed effects into our specifications. Our data include 66 birth cohorts, which lets us control for the birth seasonality using two full lunar-solar cycles.

IV.C Identification

Identification in this setup hinges on the assumption that unobserved determinants of earnings and other outcomes are uncorrelated with our exposure measures. Operationally, it requires that parents do not systematically choose the timing of conception relative to the Hijra calendar so that parental composition does not vary across exposed and unexposed children. In the following section, we use the nationally-representative DHS data to provide direct evidence supporting this assumption.

We begin by first examining the distribution of births across months in both the Gregorian and Hijra calendars. The Gregorian calendar provides a natural benchmark for comparison. Figure II shows that birth rates follow a clear seasonal pattern by Gregorian month, with substantially higher birth rates in summer than in winter. By contrast, no such pattern appears in the Hijra calendar: births are evenly distributed across lunar months, with each month accounting for roughly one-twelfth of annual births. These results use pooled DHS data from all survey waves. Figure C.II shows that conducting the analysis separately by wave yields similar patterns.

A flat birth distribution does not, by itself, rule out sorting of parents by socioeconomic status across Hijra months of conception. To test this, we use data on socioeconomic characteristics of parents from the DHS and examine their correlation with the Hijra timing of conception of children. As a benchmark, we again consider the Gregorian calendar first: Tables C.II-C.III show that parental characteristics are systematically correlated with children's Gregorian quarter of birth, with joint insignificance rejected for 18 of 20 outcomes. By contrast, we find no such correlation for Hijra timing of conception (Table I and Table C.IV). None of the trimester coefficients are individually significant, nor can we reject joint insignificance for any of the 20 outcomes examined. Importantly, these null results are not driven by a lack of statistical power: the estimates are precisely centered around zero, with negligible coefficients and standard errors. Figure III illustrates this visually, showing flat profiles with tight confidence intervals for all outcomes.

We corroborate these findings with two additional tests. First, Table C.V estimates a multinomial logit model testing whether parental education, occupation, or other socioeconomic characteristics can jointly predict the Hijra timing of conception. Consistent with our baseline results, these covariates have no explanatory power. These coefficients are also displayed in Figure C.I, and show marginal effects indistinguishable from zero. Second, we assess sensitivity to heaping: while our baseline specification imputes missing days of birth as the 15th of the month, Tables C.VI-C.VII show that results are insensitive to drawing missing days randomly from a uniform distribution.

V Results

This section presents our results, proceeding from immediate impacts on birth weight through medium-term effects on educational attainment to long-term consequences for earnings. We conclude with intergenerational effects.

V.A Birth Weight

Table II presents our results on birth weight. Columns (1) and (3) report estimates from the dummy and trimester versions of our baseline model (2). The other two columns add a dummy variable *Poor* and its interactions with the exposure dummy or trimester dummies. We define an individual as *Poor* if their household belongs to the bottom two quintiles of household wealth. These two quintiles are categorized as "Poorest" and "Poorer" by the DHS. In comparison, the other three quintiles are categorized as "Middle", "Richer", and "Richest".

Three results in Table II are striking. First, the exposure has a strong negative effect on birth weight: on average, exposed infants have 205 grams lower birth weight than the unexposed. This represents a substantial reduction of 6.7% relative to the mean birth weight of the unexposed of 3,054 grams. Second, this reduction is disproportionately concentrated among poor households, with exposed infants belonging to poor households having 693 grams (22.7%) lower birth weight than their unexposed counterparts. By contrast, the effect on the non-poor is far weaker and only significant marginally for the second trimester exposure. Note that this poverty penalty is unlikely to be driven by differences in fasting rates as our survey data—analyzed later in Section V.F—shows that fasting rates do not vary systematically with household income. Third, the trimester-level specifications indicate that the adverse effect remains relatively uniform across all stages of pregnancy. We cannot reject the null that the three trimester coefficients are equal (for a discussion on trimester-level variation in our results see Section V.E). Moreover, including poverty interactions in the trimester-level specifications yields generally consistent results, indicating that the negative effect on birth weight is primarily concentrated on poor households.

Prior literature has found that the effects on birth weight are stronger in the lower tail of the birth weight distribution (Almond *et al.*, 2018). Table D.I explores this possibility by replicating our analysis using a binary outcome. Following the literature (e.g., Persson & Rossin-Slater, 2018), we define birth weight below 2,500 grams as Low Birth Weight (LBW). The results mirror our earlier findings and also confirm that the adverse effects are indeed driven by the lower tail of the birth weight distribution. Specifically, in-utero exposure increases the probability of LBW by 8.5 percentage points (45.6%). This effect exhibits the same stark poverty penalty and remains relatively uniform across trimesters. Finally, Table D.II explores whether the effect on birth weight varies by child's gender. While the negative effect on birth weight is always stronger for boys, these gender differences are in general not significant.

It is important to emphasize that long-term effects of in-utero shocks need not mediate through proximate outcomes, implying that birth weight may not be a good surrogate for later outcomes (Athey *et al.*, 2025). This distinction is important because proximate metrics often fail to capture underlying adaptations that may manifest only later in the lifecycle (Gluckman & Hanson, 2004). We therefore interpret the impacts on birth weight primarily as evidence linking maternal fasting to fetal development, while acknowledging that these metrics may not fully capture the true long-term impact (Almond *et al.*, 2018).

V.B Educational Attainment

Table III reports the estimated effects of the in-utero shock on educational attainment. The outcome variable here is a binary indicator of whether the respondent completed middle school (Table E.I shows the corresponding results for high school).[4] We use the PSLM data that records individuals' educational outcomes and allows us to link them to their parents. The baseline specification in Column (1) shows that in-utero exposure reduces the probability of completing middle school by 2.8 percentage points (3.9%). Consistent with our broader results, the point estimates are larger for children from poorer households: the implied effect among the poor corresponds to a 4.6% reduction relative to the unexposed mean. The trimester-level specifications in both these tables together suggest that the effect is concentrated in the first two trimesters, especially in the second (see Columns 3-4 of Tables III and E.I). The poverty interactions for educational attainment are not estimated as precisely as those for birth weight. Specifically, the p-value for the interaction term in the second column in Table III is 0.143, and the p-value for the joint significance of the three interaction terms in the fourth column is 0.137, both suggesting meaningful heterogeneity despite reduced statistical precision.

V.C Earnings

We now turn to the next and perhaps the most important labor market outcome in the life cycle of exposed individuals—earnings. We observe earnings in two complementary datasets: administrative tax records and PSLM. We begin our analysis by using the administrative data and later confirm the results using the PSLM. These two datasets, as we mention in Section III, are complementary and together they provide a fuller picture of the effects on earnings.

V.C.1 Earnings Impacts from Admin Data

Figure IV presents nonparametric evidence on the earnings-exposure relationship using administrative data. We group individuals into 52 weekly bins based on conception timing relative to Ramadan: weeks -12 to -4 represent unexposed individuals (conceived after the end of one Ramadan and born before the next), weeks -3 to 0 and

[4]Completing middle school means the respondent has completed at least 10 years of education and completing high school 12 years of education.

36 to 39 are partially exposed, and weeks 1-36 are fully exposed at different gestational ages.[5] The results reveal a striking saucer-shaped profile: earnings decline steadily through early exposure, reach a minimum around week 15, and then gradually recover, finishing at virtually the level they begin from. Not only does this relationship hold for average earnings but also for other moments of the distribution—first quartile, median, and third quartile (see the bottom panel of the figure). The raw evidence thus suggests that Ramadan exposure in utero, particularly during mid-gestation, may have long-run adverse effects on adult earnings.

Figure V refines this analysis by conditioning on month, year, and district of birth. Formally, we estimate a version of equation (2), adding successively each of the three set of fixed effects and plotting the resulting $\hat{\beta}_\tau$ coefficients with 95 percent confidence intervals around them. Including birth covariates, in particular the year of birth fixed effects, flattens the earnings-exposure profile considerably, which is not surprising as we do not measure earnings at a fixed age. Conditional on these fixed effects, no meaningful difference exists within the unexposed individuals or between the unexposed and those exposed in early pregnancy once the year of birth fixed effects are added. Importantly, exposure in the middle period of pregnancy continues to be associated with significantly lower adult earnings regardless of the controls we use.

Table IV formalizes these results by estimating equation (2). We begin with the month-of-exposure level specification. Table V reports the corresponding estimates from the exposure dummy and exposure trimester specifications. For each table, we begin with the most parsimonious specification and successively introduce the month, year, and district of birth fixed effects, building to our preferred specification that includes all three. Both tables utilize data from the period 2007-2009 containing 66 birth cohorts, with standard errors clustered at the individual level.

Unsurprisingly, the regression results confirm the visual evidence, yielding four key insights. First, the coefficients for the no or partial exposure groups $\tau \in \{-1, 0\}$ are indistinguishable from zero (see our preferred specification in the last column of Table IV).[6] These coefficients are built-in placebo tests in our models, and their insignificance shows that the Hijra timing of conception is not systematically correlated

[5]Because a lunar year is shorter than the solar year by roughly 11 days, week 39 in our sample contains only 1 day and week -12 only 3–4 days.

[6]Recall that individuals grouped in month $\tau = -1$ have potential exposure probability of virtually zero and those exposed in month $\tau = 0$ of around 50% (see Figure I-D for details). The coefficient on the first group is always insignificant and on the second is insignificant in specifications with the cohort fixed effects.

with the determinants of earnings, providing additional support for our empirical strategy. Second, we strongly reject the null that exposure has no effect: both month and trimester exposure dummies are jointly significant with p-values near zero in all specifications. Third, exposure in pregnancy months $\tau \in \{3, \ldots, 8\}$ appears to matter the most, a result we discuss further in Section V.E. The final specification, which makes the most granular comparison, shows that individuals exposed in these months earn up to 2.7% less than the certainly unexposed depending on the timing of exposure. Fourth, the coefficient on month $\tau = 9$ is statistically insignificant in all specifications. This is unsurprising given that this being a boundary month is also partially exposed (see Figure I-D).

Finally, we examine the effects of in-utero exposure on within-country inequality. Appendix Table F.XIV reports estimates from equation (2), where the outcomes are indicators for earning above the 50th, 75th, and 90th percentile of the income distribution. Across all specifications, exposure coefficients are consistently negative. The trimester specifications show that exposure in the second trimester seems to matter the most. For example, it reduces the probability of having income above the median and 75th percentile by 1.3 percentage points (2.6%) and 0.7 percentage points (2.8%). Joint significance tests confirm strong effects at the median and 75th percentile and weaker, marginally significant effects at the 90th percentile. While precision declines at the very top of the distribution, the overarching insight remains that prenatal shocks reduce the likelihood of ascending the earnings ladder.

V.C.2 Robustness and Heterogeneity

We conduct extensive robustness checks to verify that our results are not sensitive to alternative specifications. These checks are detailed in Appendix F.1 and include specifications with finer seasonality controls, more granular place-of-birth fixed effects, and replacing year-of-birth fixed effects with flexible polynomial in age. We also estimate the models on each year's data separately instead of pooling together three fiscal years. Across all these specifications, our core results remain stable, with exposure coefficients maintaining similar magnitudes and significance levels.

We also examine heterogeneity in treatment effects by household characteristics and shock intensity. Full details of these results are in Appendix F.2 and F.3. We begin by testing whether effects are stronger for more religious families—where shock intensity is plausibly greater—using an onomastic proxy for religiosity based on the exposed child's first name. We then compare effects across old versus young cohorts and across urban versus rural areas. Finally, we exploit the significant variation in environmental severity—specifically, extreme heat and dryness during the peak months of May and June in Pakistan—to test whether fasting during harsher conditions results in stronger adverse effects. We find that while the interaction coefficients are always of expected sign, they lack statistical precision in our preferred specification across all heterogeneity measures.

V.C.3 Earnings Impacts from PSLM Data

In analyzing earnings with the PSLM, we estimate effects separately for household heads and for children of the household head. Focusing on household heads has the advantage that they are fully established workers and are thus fully active in the labor market. We, however, cannot link them to their parents and therefore cannot include poverty interactions into the equation. By contrast, we can include these interactions for children and can thus explore the poverty penalty in earnings.

Table VI presents results for household heads, revealing three key findings. First, all exposure coefficients are consistently negative across specifications, confirming that exposed individuals earn systematically less than the unexposed. Second, the largest effect arises for second-trimester exposure, which reduces earnings by roughly 3.4% with stable statistical significance. Third, these estimates are less precise than our earlier results, a likely consequence of both measurement error inherent in household survey data and the comparatively smaller sample size. This is precisely the reason we rely on the administrative tax data for our main results on earnings. Nevertheless, it is reassuring that the PSLM-based results, despite their lower precision, still align with our admin-data-based results.

Table F.XV extends the earnings analysis to children of household heads. A key limitation of this analysis is that our children-sample gets mechanically restricted to working-age children who still co-reside with their parents. Figure F.II illustrates the resulting selection: the share of individuals classified as children who live with their parents declines steeply beginning in the mid-twenties and approaches zero at older ages. Consequently, while our sample spans 40 birth cohorts, it is mechanically selected towards younger adults and those with delayed household formation, which may reduce statistical precision and representativeness of these results. We therefore interpret these results cautiously, refraining from drawing strong quantitative conclusions. The main qualitative pattern, however, aligns with our broader findings that the negative earnings effects are concentrated among individuals from poorer households. In particular, the poverty-by-exposure interactions for the first two trimesters are consistently negative, and in our preferred specification the three poverty interaction terms are jointly significant ($p = 0.017$), indicating economically meaningful poverty penalty despite the constraints of the co-residence sample.

V.C.4 Selection into Tax Data

One concern for interpreting our earnings results from the administrative data is selection into tax payment: if the administrative data disproportionately capture individuals who experienced milder shocks or benefited from compensatory investments, our estimates may understate the average treatment effect. Table VII uses the nationally representative PSLM data to test for such selection by simulating taxpayer status based on Pakistan's tax code. The exposure coefficients in our standard specifications (Columns 1-2) are small and statistically insignificant, suggesting minimal selection into tax filing. Because the exemption threshold—the income level above which a person must file a tax return—lies between 75th and 90th percentiles of the income distribution during our sample period, the subsequent columns investigate if exposed individuals are less likely to reach these thresholds. The results suggest lower likelihood of surpassing the 75th percentile but no conclusive evidence for the 90th percentile. We attribute this ambiguity to limited power and precision of survey data at the top of the income distribution and interpret the overall evidence as indicating modest negative selection into the tax data. This conclusion is strengthened by the fact that our earnings results from administrative tax data are similar in magnitude to those from nationally-representative PSLM, ruling out strong negative selection into tax data. But to the extent this selection exists, our results from tax data should be seen as a lower bound on the true effect.

V.D Intergenerational Transmission

Having established the direct effects of early-life shocks on affected individuals, we now examine intergenerational transmission.[7] Because we do not observe the mother's birth date in the DHS data, we cannot study intergenerational effect on birth weight.

[7] See Section B.3 for the biological and economic channels giving rise to these second-generation effects.

We therefore begin by using the PSLM to examine intergenerational impacts on educational attainment. Table VIII reports these results. We estimate equation (2) but instead of examining the effects of self-exposure we examine the effects of parental exposure. When exposure is defined at the father level, the dummy specification yields negative point estimates for both middle- and high-school completion, though these average effects are imprecisely estimated. The trimester specification, however, reveals clear timing heterogeneity: paternal exposure in the third trimester is associated with a 5.8 percentage points (9%) decline in children's middle-school completion and a 6.0 percentage points (15%) decline in high-school completion, and we strongly reject the joint null that paternal trimester coefficients are zero for both schooling outcomes (p-value < 0.001). Maternal exposure produces a similarly consequential pattern, concentrated in high-school completion. The dummy specification indicates that children of exposed mothers are 8.4 percentage points (21%) less likely to complete high school, and the trimester specification attributes this effect primarily to second- and third-trimester maternal exposure (7.1 and 9.5 percentage points, respectively). Consistent with these magnitudes, the joint test rejects the null of no maternal exposure effects for high-school completion (p-value 0.002), while effects on middle-school completion remain smaller and statistically indistinguishable from zero.

Table G.I extends the analysis to the earnings of the second generation. As noted in the previous section, identifying intergenerational earnings links in the PSLM requires restricting the sample to working-age adults who continue to co-reside with their parents. This induces a mechanical selection towards individuals with delayed household formation, potentially capturing a subsample with lower latent earnings potential. We therefore interpret these estimates with caution and treat them as suggestive rather than definitive. Subject to this caveat, the results nonetheless point to sizable intergenerational earnings losses. Paternal exposure reduces offspring earnings by 16.2% (Column 1), while maternal exposure leads to a 15.0% reduction (Column 5). Mirroring the educational results, paternal effects are driven entirely by third-trimester exposure, whereas maternal effects are broader, with significant harms arising from both second and third trimester exposure.

Notably, these intergenerational coefficients are larger in magnitude than the own-exposure effects estimated above. This amplification likely reflects two complementary reasons. First, the selection inherent in our sample implies we are observing the lower tail of the distribution (children who failed to achieve economic independence), where the marginal impact of the shock may be most severe. Second, theoretical models of human capital formation suggest that disadvantage can compound across generations. In these models, the intergenerational transmission of the shock is not merely genetic but environmental, as early-life deficits in one generation translate into a permanent reduction in the parenting resources available to the next (Heckman & Mosso, 2014; Currie, 2011).

In studying intergenerational effects, prior literature has focused mostly on maternal exposure (Duflo *et al.*, 2025). In distinction, we find economically meaningful effects for both father and mother exposure. Our results are consistent with a growing biomedical literature identifying male-line transmission mechanisms, specifically epigenetic modifications in sperm that determine biological inheritance of offspring, including their metabolic phenotypes (Pembrey *et al.*, 2006; Veenendaal *et al.*, 2013).[8] Beyond biological channels, economic mechanisms can also explain paternal transmission: fathers with impaired human capital from prenatal shocks have fewer financial, cognitive, and physical resources to invest in their children's development. These deficits could be exacerbated further by assortative mating. Indeed, we find evidence of assortative mating in our setting: exposed fathers are more likely to pair with exposed mothers, especially those exposed in the second trimester (see Table G.II for details).

V.E Trimester Patterns and Mechanisms

Our analysis uncovers important trimester-specific patterns. Specifically, birth weight effects are relatively uniform across trimesters, whereas educational and earnings effects are concentrated in the first two trimesters. This divergence likely reflects differences in underlying biological mechanisms. Birth weight responds to nutritional shortfalls throughout gestation because somatic growth occurs continuously, and even moderate, recurrent shortfalls can constrain fetal mass accumulation at any stage. By contrast, the concentration of schooling and earnings effects in the first two trimesters, and especially in the second, aligns with the biomedical evidence that mid-gestation is a sensitive period for core neurodevelopmental processes.[9] The dis-

[8]For example, Veenendaal *et al.* (2013) find that offspring of Dutch Hunger Winter-exposed fathers—but not mothers—exhibited increased adiposity.

[9]Neuronal proliferation and migration intensify during the second trimester, with migration activity peaking around weeks 12-20, a window directly relevant to later cognition and skill formation (Rakic, 1995; Tau & Peterson, 2010). Nutritional deficiencies during this window disrupt the formation of cortical architecture in ways that cannot be fully remediated by subsequent catch-up growth (Morgane *et al.*, 1993; Georgieff, 2007).

tinct pattern for intergenerational transmission, where parental exposure in late gestation generates the largest impacts, is also consistent with the biomedical evidence suggesting that nutritional shocks during this period alter the epigenetic programming of the next generation. Under these mechanisms—rooted in biological inheritance rather than cognitive programming—nutritional stress during late gestation can induce epigenetic modifications—particularly in DNA methylation—that transmit developmental disadvantage to offspring (Waterland & Jirtle, 2003).[10]

V.F From ITT To ATT

Our estimates thus far have an intention-to-treat interpretation. This is because while we can identify whether pregnancies overlapped with Ramadan and were therefore potentially exposed, we cannot observe actual exposure—namely, whether mothers fasted during pregnancy.[11] To address this limitation, we conducted a purpose-built household survey in Lahore, the second-largest city in Pakistan. The survey sample was drawn randomly from the population covered in our tax data. Appendix H.1 provides the technical details of the survey design, along with a fuller analysis of the survey data covering fasting behavior, social norms, and social desirability bias. In the following section, we use these data to estimate a local average treatment effect (LATE), which also corresponds to the ATT in our setting.

We first use the survey data to convert our measure of potential exposure, as defined in expression (1), into actual exposure. Figure VI illustrates that in the survey sample approximately 55% of women fast for at least one day during pregnancy (Panel A). Among those who fast, the average number of days fasted is 22, as shown in Panel B. Together, these two panels show that most variation in exposure comes from the mother's decision to fast. Conditional on fasting, mothers typically fast for nearly three-quarters of the Ramadan. Crucially, neither the likelihood nor the intensity of fasting varies systematically with the pregnancy month, suggesting that the differences in treatment effects across pregnancy stages are not driven by variation in fasting behavior but by how exposure affects the biological and neurological

[10] The Dutch Hunger Winter cohort provides direct evidence for this channel: individuals whose mothers were exposed prenatally exhibited altered methylation patterns and adverse health outcomes despite never experiencing famine directly (Lumey *et al.*, 2007; Painter *et al.*, 2008).

[11] This limitation is common in Ramadan-based designs, where long-run outcomes are typically observed in administrative or large household surveys that do not record fasting behavior during pregnancy; as a result, most existing studies recover ITT effects rather than the average treatment effect (ATE) or a local average treatment effect (LATE).

development of the fetus in the particular stage.

To convert the potential exposure into actual exposure, we scale the potential exposure in each gestation month by the unconditional mean share of days fasted during the month, as reported by our survey participants (on average it scales potential exposure by nearly 0.41; Panel A $\times$ Panel B $\approx 0.55 \times \frac{22}{30} \approx 0.41$). The last panel of Figure VI plots the average exposure by gestation month we obtain by following this procedure.[12] The average exposure is close to zero in months $\tau \in \{-2, -1\}$, around 40% in months $\tau \in \{1, \ldots, 8\}$, and much lower (10–20%) in months $\tau \in \{0, 9\}$. Note that the variation in average exposure is primarily driven by the probability of being in-utero during Ramadan (see p_{di} plotted in Figure I), as the fasting rate does not vary much across gestation months (Figure VI-A).

We also examine heterogeneity in average exposure across key household characteristics (see Figure H.III). Maternal religiosity is the strongest predictor of fasting, while other characteristics—notably household income—exhibit no meaningful association. A potential concern here is that religiosity may correlate with socioeconomic status, which could confound our estimates. To investigate this further, we divide households into income deciles and employ the nonparametric binscatter procedure of Cattaneo *et al.* (2024). We find no systematic correlation between household income and religiosity (Figure H.IV), supporting the interpretation that the poverty penalty in long-run outcomes we document above is unlikely to be driven by differential selection into fasting.

Table IX presents our two-sample two-stage least squares (TS2SLS) estimates. We follow Angrist & Krueger (1992) to combine a first stage from our household survey with the reduced-form relationship from the administrative data. The instrument is an indicator for whether the pregnancy overlapped with Ramadan. In the first stage, we estimate the relationship between this overlap and two endogenous variables: (i) an indicator for any maternal fasting, and (ii) a continuous fasting-intensity measure (the realized exposure index combining overlap duration with reported fasting days, as depicted in Figure I-D). Standard errors are computed using the method of Inoue & Solon (2010), extended to accommodate heteroskedasticity by Pacini & Windmeijer (2016). Appendix H.6 provides further technical details of our approach, including the two additional assumptions required for identification beyond the standard IV.

[12] Because the survey records fasting behavior only when pregnancy overlapped with Ramadan, we do not observe fasting rates for months $\tau = -2$ and $\tau = -1$. We impute these using the average rate for months $\tau \in \{1, \ldots, 8\}$. This imputation is unlikely to result in any significant measurement error as the probability of being in-utero (p_{di}) for these individuals is virtually zero (see Figure I-D).

The results reveal substantial negative effects of in-utero fasting on long-run earnings. Our preferred specifications, which include birth-month and birth-year fixed effects, imply that adult earnings decline by 3.4% if the mother fasts for at least one day during pregnancy and by 5.0% if the mother fasts for the entire month.[13] Because only mothers whose pregnancies overlap with Ramadan can fast—a setting with one-sided noncompliance—these LATE estimates correspond to the ATT. The magnitudes of these ATT estimates are approximately 2.5 times the corresponding ITT estimates, consistent with the first-stage scaling factor of roughly 0.40.

VI Discussion

VI.A Comparison With Existing Literature

Figure I.I benchmarks our estimates against the existing literature on prenatal Ramadan exposure. We plot standardized effect sizes (in percent) across multiple outcomes, distinguishing between our ITT estimates (red circles), our ATT estimates (red diamonds), and the corresponding estimates from previous studies (gray markers). Because previous studies report only ITT estimates, we rescale them by our estimated compliance rate, so that the gray diamond markers can be interpreted on an approximately common "per-fasting" basis. Three features of our results stand out. First, our estimates span a broader set of outcomes: we cover the whole life cycle of exposed individuals, studying impacts on birth weights, educational attainment, earnings, and position in the income distribution. Second, our estimates are systematically larger than those in the existing literature. For birth weight, for example, Almond & Mazumder (2011) find that prenatal Ramadan fasting among Muslim women in Michigan reduces birth weight by approximately 40 grams (1.2%); our estimate is nearly an order of magnitude larger. This difference likely reflects the poverty penalty we document in the paper: mothers in our Pakistani sample are substantially poorer than those in high-income country settings, leaving fewer resources available for compensatory investments that might buffer the fetus against nutritional disruption. Third, our estimates are more precisely estimated, with notably tighter confidence intervals than the rescaled literature benchmarks. This precision reflects the

[13] Note that we cannot include the district of birth fixed effects in our model as the survey was carried out in only one district, Lahore. Our ITT estimates in all previous sections, however, show that including the district of birth fixed effects does not change the results materially.

statistical power afforded by our datasets, especially the administrative tax records.

VI.B Macroeconomic Implications

How much output do Pakistan and the world in general lose because of Ramadan fasting during pregnancy? A back-of-the-envelope calculation suggests that such losses are substantial. Specifically, combining (i) the mechanical probability that a pregnancy overlaps with Ramadan $(270 + 30)/365 \approx 0.82$; (ii) our survey-based compliance rate that roughly 55% of pregnant women fast at least one day; and (iii) our TS2SLS estimate that maternal fasting reduces adult earnings by about 3.4%, we can calculate an average long-run earnings loss of roughly $0.82 \times 0.55 \times 0.034 \approx 1.5\%$ per birth cohort in steady state. Assuming a standard production function where capital adjusts in the long run to effective labor units, this translates to an equivalent 1.5% reduction in steady-state output.[14] For an economy like Pakistan, with a nominal GDP of approximately $375 billion and a PPP GDP approaching $1.6 trillion, this implies an annual output loss of roughly $5.6 billion in nominal terms and $24 billion in PPP terms. Extrapolating globally, with roughly 600 million Muslims in the labor force and a conservatively assumed average fasting rate of 40%, the implied steady-state output loss is approximately 1.1% ($0.82 \times 0.40 \times 0.034$), amounting to roughly $55-70 billion annually in forgone output.[15]

VII Conclusion

Leveraging administrative tax records combined with two large-scale nationally representative surveys and a purpose-built household survey from Pakistan, this pa-

[14] In a neoclassical growth model with Cobb-Douglas production, $Y = K^{\alpha}(AL)^{1-\alpha}$, a permanent reduction in labor-augmenting human capital A initially reduces output by only the labor share $(1 - \alpha)$ times the shock. However, lower labor productivity reduces the marginal product of capital, depressing investment until the capital-labor ratio returns to its steady-state level. In the long run, both capital and output fall proportionally with effective labor, so a 1.5% reduction in human capital implies a 1.5% reduction in steady-state GDP.

[15] Importantly, our back-of-the-envelope is not vulnerable to the "missing intercept" critique emphasized in recent micro-to-macro work (Wolf, 2023). Our treatment operates as a persistent contraction in labor-augmenting human capital rather than a demand shock whose aggregate effect is mediated by policy rules or equilibrium price movements. Our calculation does not infer aggregate losses from cross-sectional income gaps; instead, following the accounting logic used in related micro-to-macro applications (e.g. Finkelstein *et al.*, 2025), we apply an identified treatment-induced change in effective labor to observed aggregates. If anything, general-equilibrium spillovers would attenuate our micro contrasts.

per provides comprehensive evidence on the long-run consequences of prenatal Ramadan fasting. We document substantial negative effects on birth weight, educational attainment, and adult earnings, with effect sizes that are large relative to the existing literature. Three findings merit particular emphasis. First, we uncover a pronounced poverty penalty: identical prenatal shocks generate substantially different lifecycle outcomes depending on parental socioeconomic status, with children from low-SES households bearing disproportionate costs. This heterogeneity suggests that the capacity for compensatory investment—not just the initial biological shocks—shapes the long-run return to early-life health. Second, we document intergenerational transmission through both maternal and paternal lines, a finding consistent with recent biomedical evidence but unusual in the economics literature. Third, by collecting original data on actual fasting behavior, we move beyond ITT estimation to recover ATT—revealing that the causal impact of maternal fasting is approximately 2.5 times larger than what ITT estimates suggest.

Our findings carry implications for both policy and research. On the policy side, back-of-the-envelope calculations indicate that prenatal fasting reduces Pakistan's steady-state output by approximately 1.5%—losses on the order of $5.6 billion annually in nominal terms. Because the relevant counterfactual involves exercising a well-defined religious exemption rather than abandoning religious practice, the policy margin is actionable and aligns with established jurisprudence. Low-cost information interventions clarifying both the exemption and the medical evidence could, in principle, correct widespread misperceptions and thus yield high returns (our survey reveals that most women incorrectly believe fasting during pregnancy is obligatory and harmless). However, because religious beliefs often function as protected values resistant to updating (Bénabou & Tirole, 2011), the effectiveness of such interventions remains an open empirical question warranting careful experimental evaluation.

More broadly, our results underscore that the fetal origins of inequality operate not only through biological channels but also through the economic and social resources available to buffer early-life shocks. Understanding how these channels interact—and how policy can strengthen the capacity for compensation among disadvantaged households—remains a first-order question for research on human capital and intergenerational mobility.

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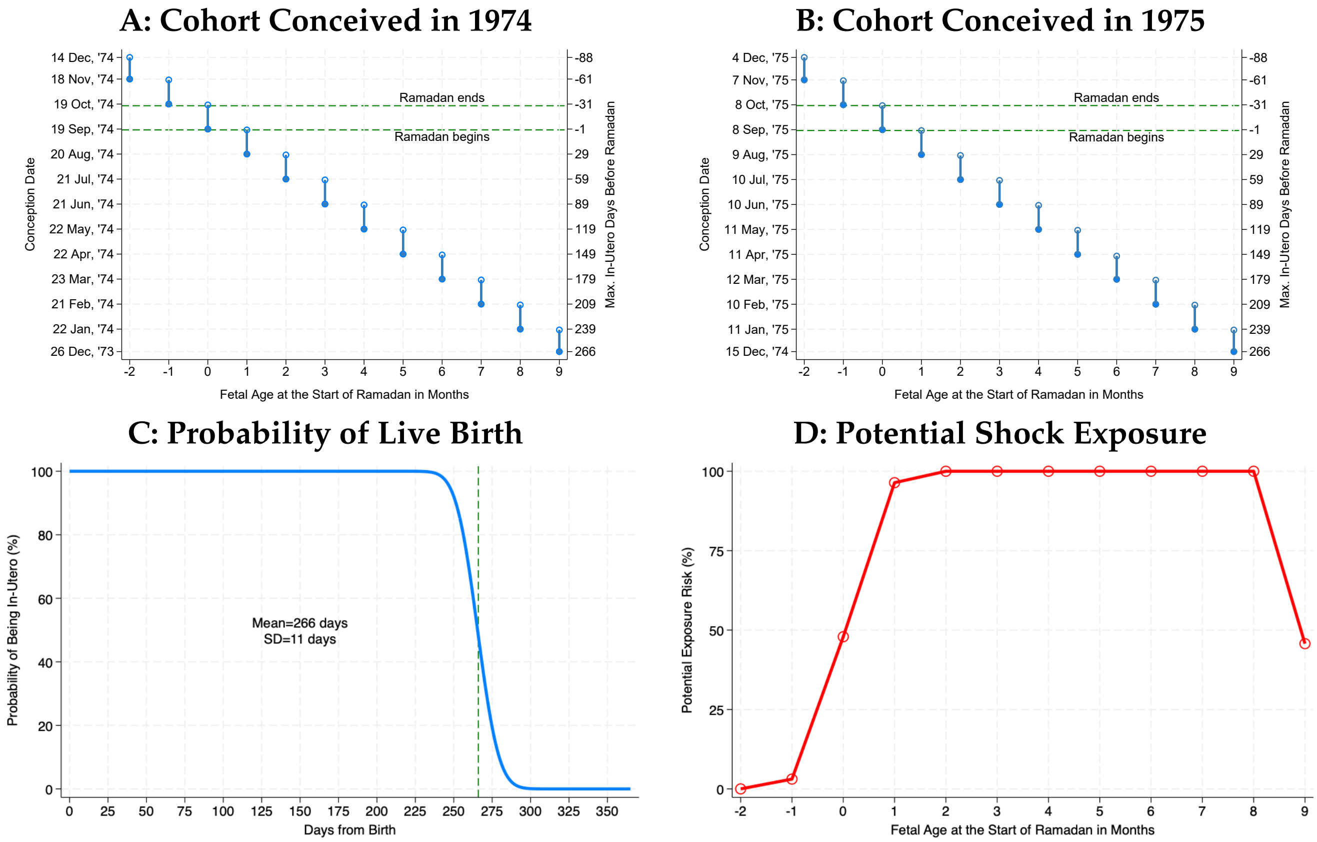
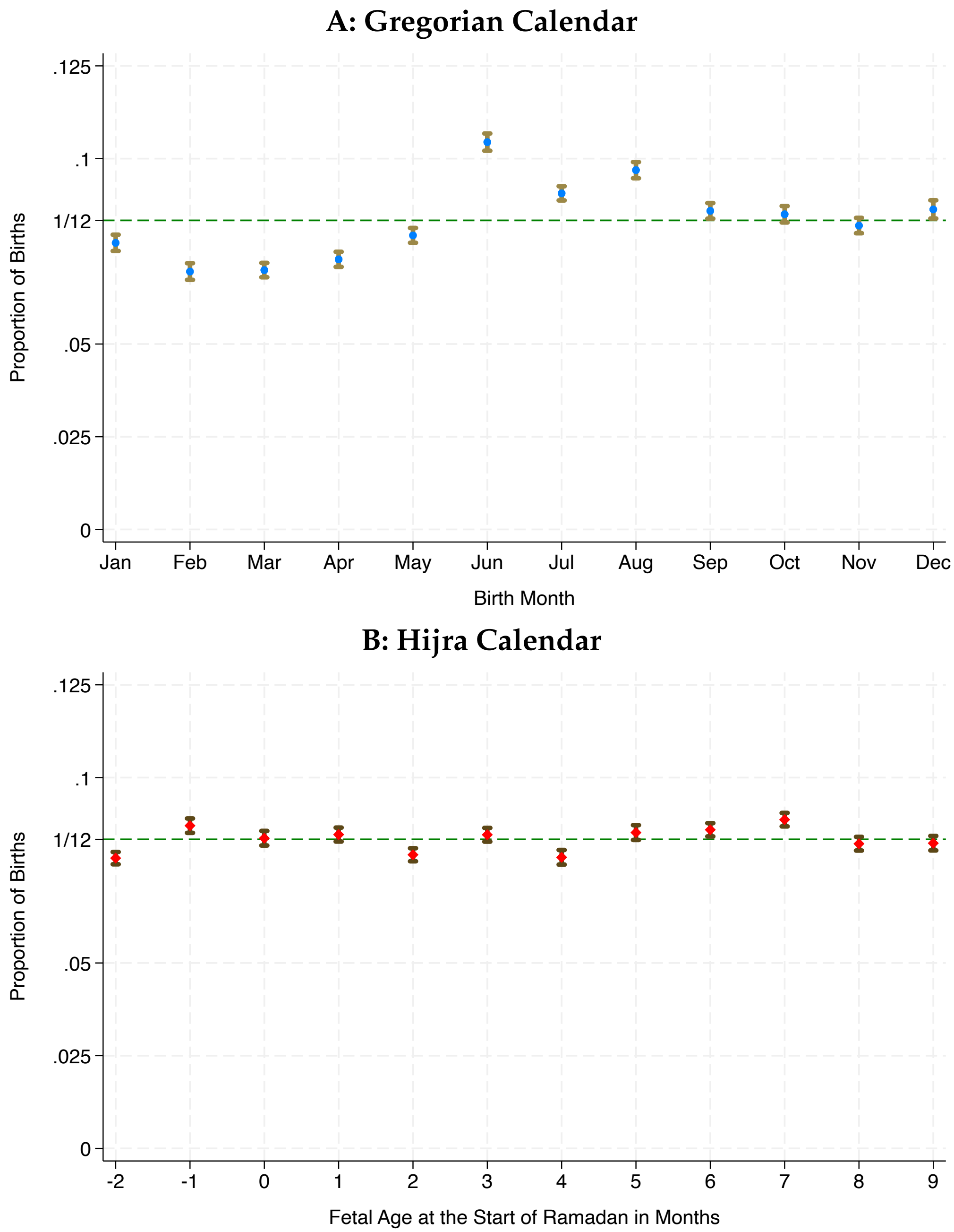
FIGURE I: **IN-UTERO RAMADAN EXPOSURE**



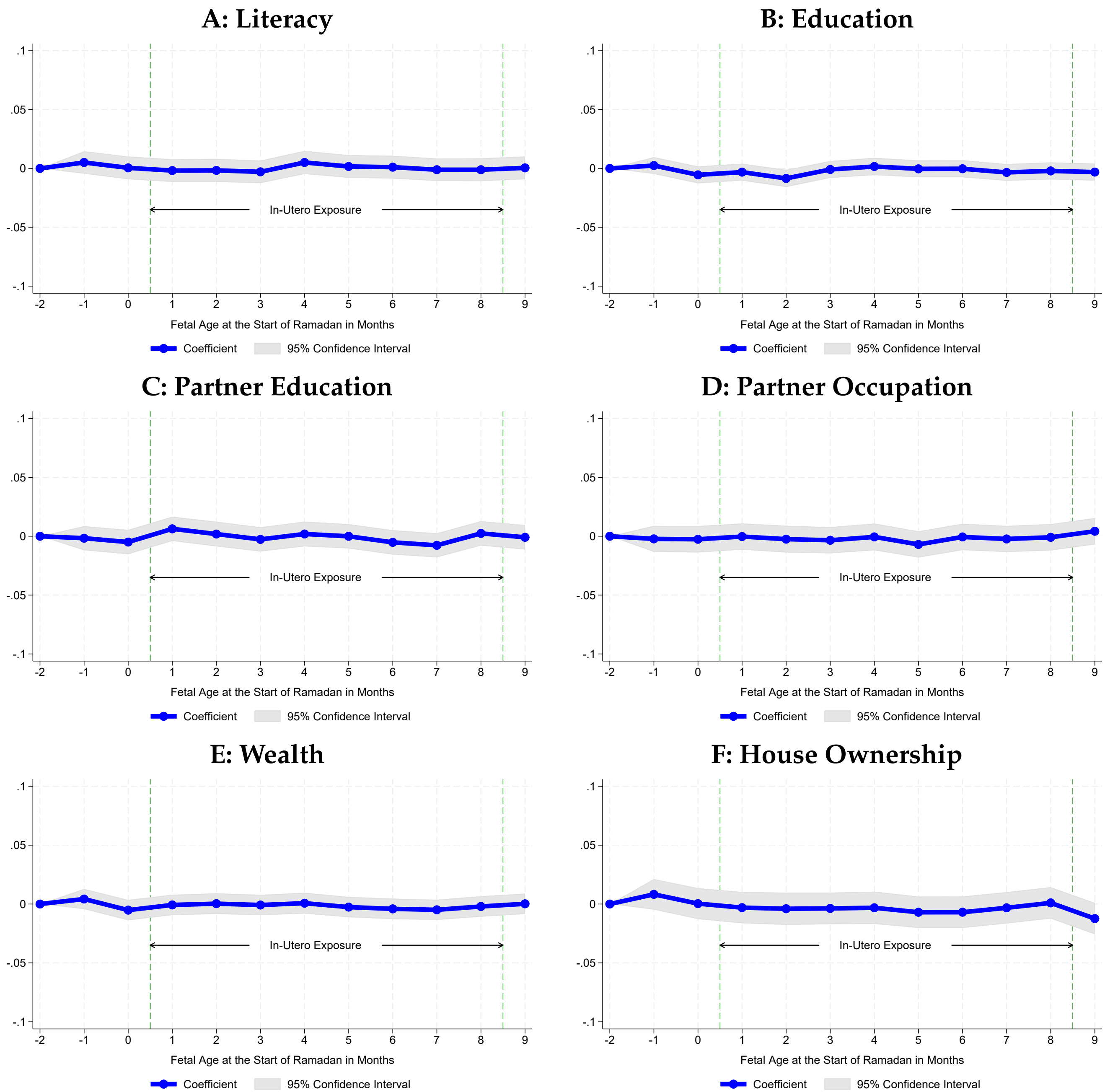
Notes: The figure illustrates how we define our Ramadan exposure measure. Panel A shows the cohort born between 7th September 1975 and 26th August 1976. The left y-axis indicates the conception date of these individuals, which is defined as the birth date minus the normal gestation length of 266. The relevant Ramadan for this cohort began on 7th September and ended on the 7th October of 1975. The right y-axis indicates the maximum days the individual has been in utero at the beginning of Ramadan. Panel B repeats the exercise for the next cohort. Panel C plots the probability that a fetus is in utero d days before birth assuming that gestational age at live birth is normally distributed with a mean of 266 days and a standard deviation of 11 days (Hoffman *et al.*, 2008; Jukic *et al.*, 2013). Panel D translates this timing into potential risk of exposure to the in-utero shock as a function of fetal age (in months) at Ramadan's start. We assign each individual in our household survey data a potential exposure value using expression (1) and then plot the average potential exposure for each gestation month.

Figure II: **Birth Seasonality**



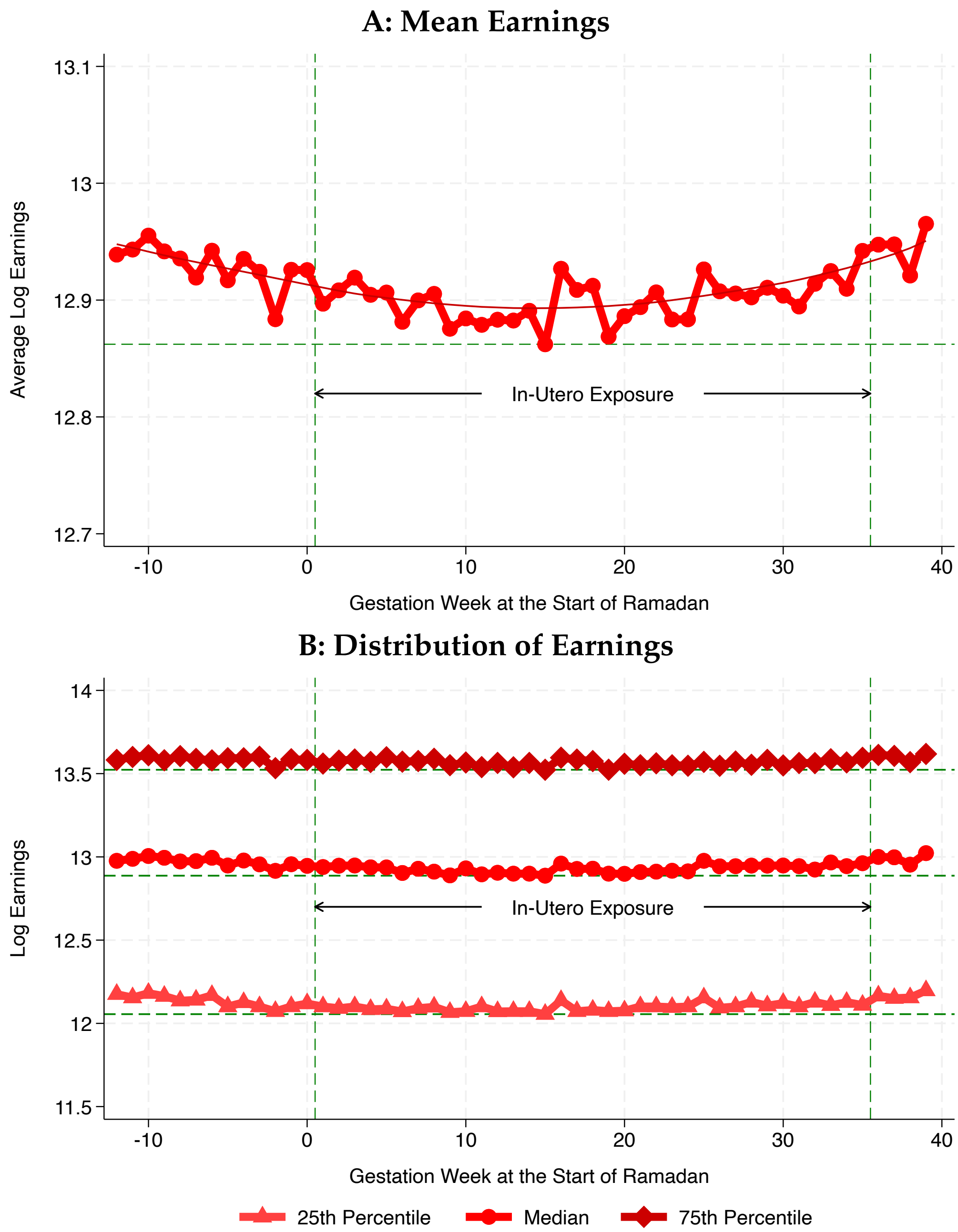
Notes: The figure explores birth seasonality under both the Gregorian and Islamic (Hijra) calendars. Using DHS data, we estimate the share of births in each month by regressing an indicator for birth in month m on a constant, separately for each month, and plotting the resulting point estimates and 95% confidence intervals. All regressions are weighted by DHS sampling weights, so the estimates are nationally representative. The top panel defines month of birth as the Gregorian calendar month. The bottom panel defines month of birth as the Hijra calendar month. For comparability across calendars, we group births into the same twelve lunar-month bins used in Figure I. In particular, month $\tau = 0$ corresponds to births whose conception occurred in the Hijra month in which Ramadan begins. The sample includes all DHS waves used in the analysis: 1990–1991, 2006–2007, 2012–2013, and 2017–2018.

FIGURE III: **SELECTION INTO IN-UTERO RAMADAN EXPOSURE?**



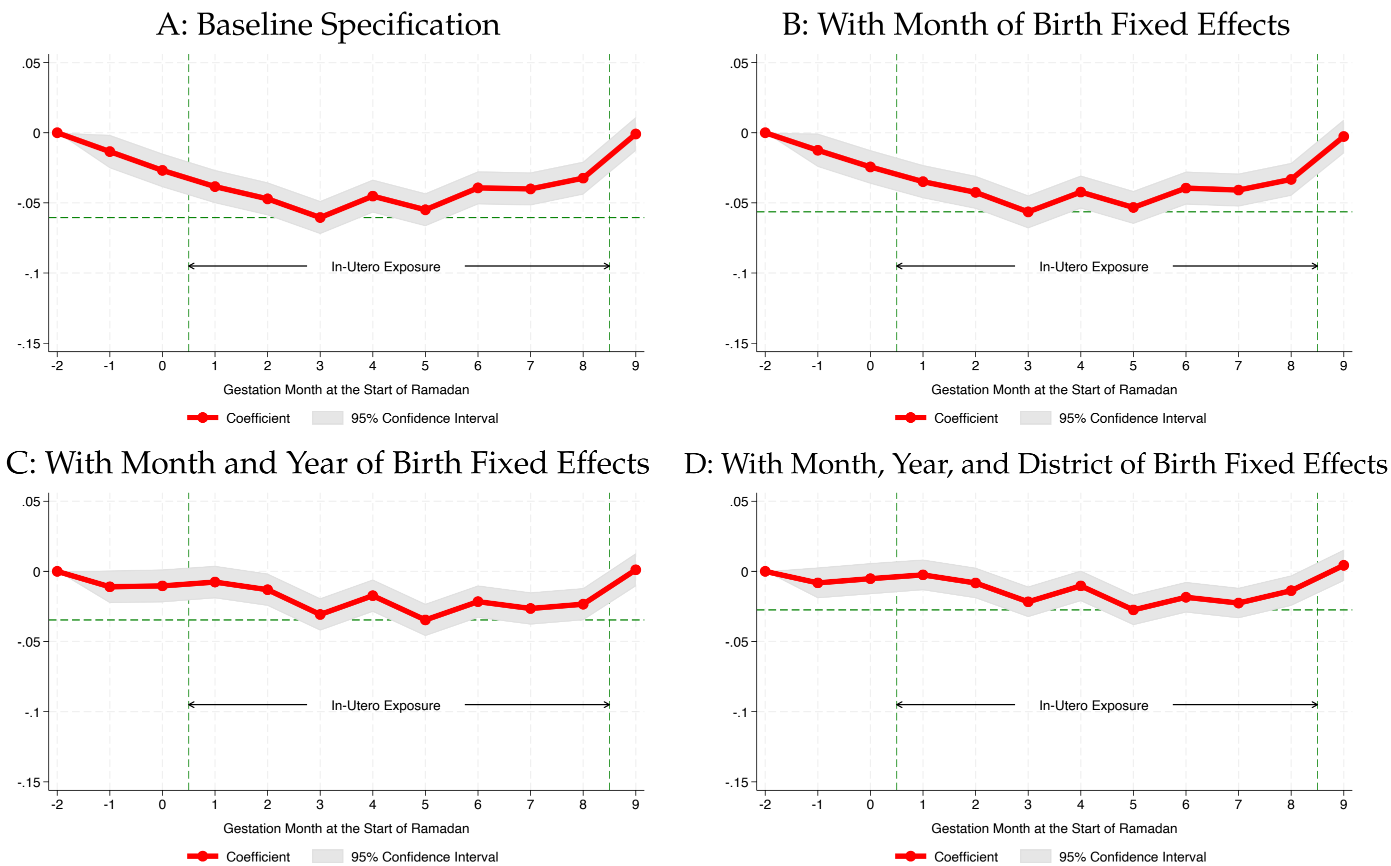
Notes: The figure tests for parental sorting across the Hijra months of conception of children. We estimate equation (2) using the DHS data and plot the coefficients $\hat{\beta}_{\tau}$'s along with the 95% confidence intervals around them. The dependent variable in each regression is indicated in the heading of each column. All regressions are weighted by DHS sampling weights, so the estimates are nationally representative. All specifications include district-of-birth, month-of-birth, and year-of-birth fixed effects. Variable definitions are provided in Appendix A. The sample includes all DHS waves used in the analysis: 1990–1991, 2006–2007, 2012–2013, and 2017–2018. See Figure I for the construction of the conception-timing months. The omitted category is certainly unexposed individuals, defined as those conceived two months after Ramadan (month $\tau = -2$).

FIGURE IV: **IN-UTERO RAMADAN EXPOSURE AND EARNINGS**



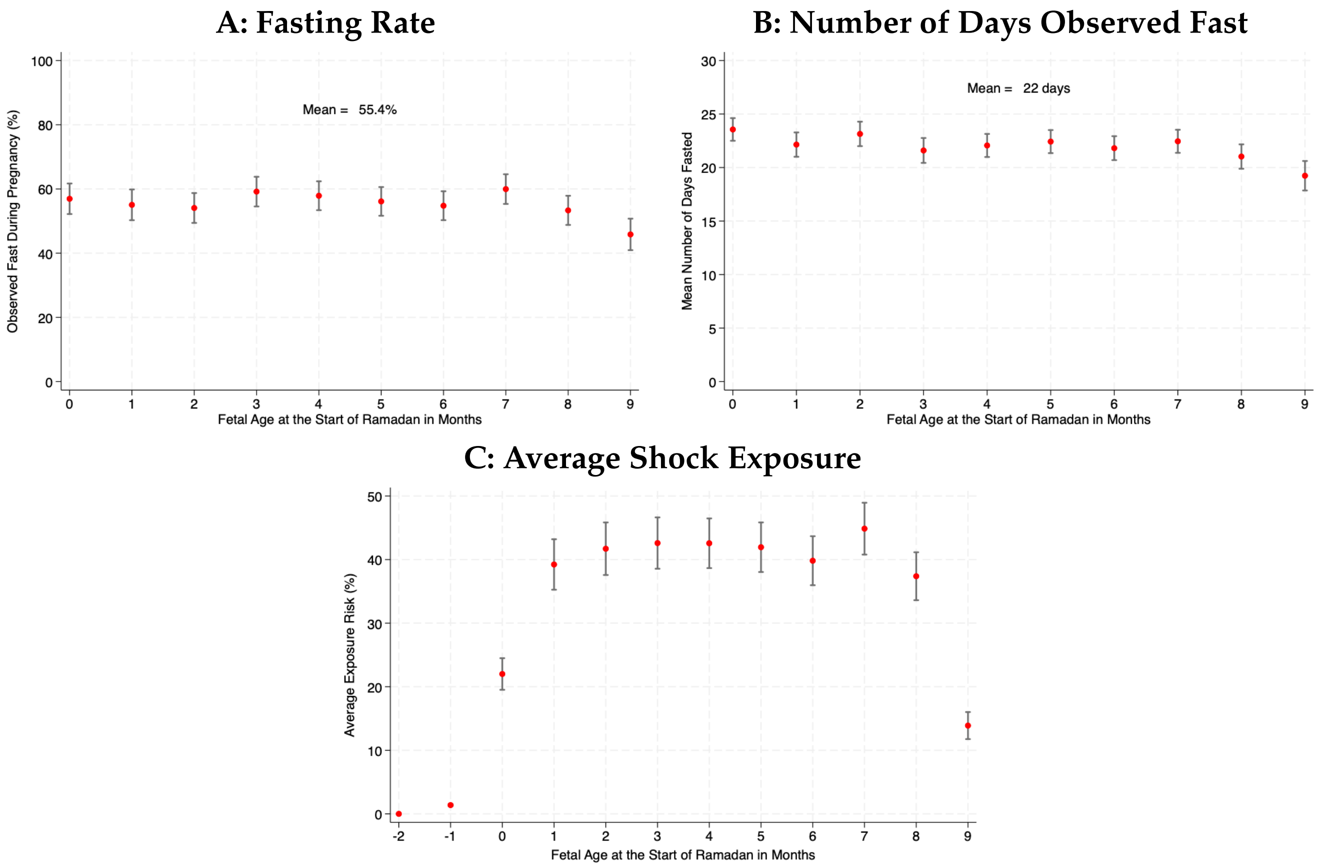
Notes: The figure shows the non-parametric relationship between earnings and in-utero exposure. We divide individuals into 52 groups depending upon the gestation week in which they experience Ramadan. Individuals in week 0 are conceived in the same week Ramadan began in. For example, in 1975 Ramadan began on the 8th of September. Individuals conceived between 8–14 September are included in group 0. We find the conception date by subtracting the normal gestation length of 266 days from the exact date of birth. Individuals in weeks $[1, 36]$ are exposed, in weeks $[-3, 0]$ and $[37, 40]$ are partially exposed, and others are not exposed. Panel A reports average earnings within each group, while Panel B plots distributional moments (25th percentile, median, 75th percentile). Earnings are defined as income reported on the income tax returns filed by the individual.

FIGURE V: **IN-UTERO RAMADAN EXPOSURE AND EARNINGS**



Notes: The figure explores the relationship between earnings and in-utero Ramadan exposure. We divide individuals into 12 groups depending upon the gestation month in which they experience Ramadan. Individuals in month $\tau = 0$ are conceived in the same month Ramadan began in. For example, in 1975 Ramadan began on 8 September. Individuals conceived between 8 September and 7 October are included in group $\tau = 0$. We find the conception date by subtracting the normal gestation length of 266 days from the exact date of birth. We estimate a version of equation (2) and plot the coefficients $\hat{\beta}_\tau$'s along with the 95% confidence interval around them. We start with the baseline specification in Panel A and then sequentially add our three main sets of control in each panel: month of birth fixed effects in Panel B; month and year of birth fixed effects in Panel C; and month, year, and district of birth fixed effects in Panel D. The sample includes all tax returns filed in 2007–2009. The horizontal dashed line indicates the most negative $\hat{\beta}_\tau$ from the regression, showing the gestation month of exposure for which we estimate the largest earnings reduction.

FIGURE VI: **AVERAGE SHOCK EXPOSURE**



Notes: This figure uses our household survey data to convert the potential exposure measure displayed in Figure I (Panel D) into realized (actual) exposure, which serves as the first stage of our empirical framework. Panel A reports the fasting rate during pregnancy by fetal age at the start of Ramadan: about 55% of pregnant women fast at least one day, with no systematic variation across gestational months. Panel B reports the fasting intensity among fasters: the mean of 22 days implies that, conditional on fasting, women typically fast for nearly three-quarters of Ramadan. Panel C combines these margins to plot average realized exposure (fasting rate $\times$ the potential exposure): exposure is close to zero for conceptions in months $\tau = -2$ and $\tau = -1$, around 40% for months $\tau \in \{1, \ldots, 8\}$, and lower (10-20%) for months $\tau = 0$ and $\tau = 9$, reflecting partial overlap. Together, Panels A-B indicate that most variation in realized exposure arises from the decision to fast (extensive margin) rather than the number of days fasted (intensive margin). The absence of gestational patterning in A-B suggests that trimester differences in outcomes primarily reflect stage-specific fetal biology rather than differences in fasting behavior. Error bars denote 95% confidence intervals.

Table I: Selection into In-Utero Ramadan Exposure?

In-Utero Ramadan Exposure in	Literacy (1)	Literacy (2)	Education (3)	Education (4)	Partner Education (5)	Partner Education (6)	Partner Occupation (7)	Partner Occupation (8)	Wealth (9)	Wealth (10)
Dummy Specification:										
Exposed	0.002		-0.002		0.002		-0.004		-0.003	
	(0.005)		(0.003)		(0.005)		(0.006)		(0.004)	
Trimester Specification:										
First Trimester		-0.001		-0.006		0.000		-0.006		-0.003
		(0.005)		(0.004)		(0.005)		(0.006)		(0.004)
Second Trimester		0.003		0.001		0.004		-0.004		-0.003
		(0.005)		(0.004)		(0.006)		(0.006)		(0.004)
Third Trimester		0.003		-0.001		0.003		-0.001		-0.002
		(0.005)		(0.004)		(0.006)		(0.006)		(0.004)
Observations	152,468	152,468	152,563	152,563	150,755	150,755	150,718	150,718	152,563	152,563
Joint test, coefficients on trimesters 1-3 equal 0:										
p-value	0.724	0.640	0.477	0.036	0.675	0.749	0.502	0.640	0.443	0.850
Mean Value	0.261	0.261	0.118	0.118	0.297	0.297	0.368	0.368	0.194	0.194
Fixed Effects:										
Month of Birth	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
District of Birth	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year of Birth	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table tests for parental sorting across Hijra months of conception. We estimate equation (2) using the DHS data. The outcome variable in each of these regressions is indicated in the heading of each column. We weight the regressions by sampling weights, so the estimates are nationally representative. Odd-numbered columns estimate the dummy specification, whereas the even-numbered columns estimate the trimester specification. The mean of the dependent variable is reported in the row above the fixed-effects indicators. The row labeled 'p-value' reports the p-value from an F-test of the null hypothesis that the exposure coefficients are jointly zero. For details of the variables used here see Appendix A. The sample includes all DHS waves used in the analysis (1990–1991, 2006–2007, 2012–2013, and 2017–2018). Individuals exposed to Ramadan in months 0–3 of their pregnancy are included in the First Trimester group, in months 4–6 in the Second Trimester group, and in months 7–9 in the Third Trimester group. Certainly unexposed individuals, i.e., those who were conceived two months after Ramadan (month $\tau = -2$), are the omitted category, and partially-exposed individuals grouped in months $\tau = -1$ are dropped. Standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE II: **IMPACTS OF THE EXPOSURE ON BIRTH WEIGHT**

Outcome:	Birth Weight (Grams)			
	(1)	(2)	(3)	(4)
Exposure	-205.3***	-102.2		
	(66.7)	(66.7)		
	[-6.7%]	[-3.3%]		
Exposure × Poor		-693.1***		
		(247.4)		
		[-22.7%]		
First Trimester			-206.6***	-104.1
			(68.3)	(67.9)
			[-6.8%]	[-3.4%]
Second Trimester			-237.7***	-134.2*
			(77.2)	(79.4)
			[-7.8%]	[-4.4%]
Third Trimester			-192.2**	-89.8
			(75.3)	(75.3)
			[-6.3%]	[-2.9%]
First Trimester × Poor				-677.8***
				(264.0)
				[-22.2%]
Second Trimester × Poor				-725.1***
				(267.1)
				[-23.7%]
Third Trimester × Poor				-689.7***
				(267.3)
				[-22.6%]
Observations	5,100	5,100	5,100	5,100
Mean Value of Unexposed	3054.9	3054.9	3054.9	3054.9

Notes: The table estimates the effects of in-utero shock on the birth weight. We regress birth weight in grams on our exposure measures and exposure × poor interaction. Certainly unexposed individuals, who were conceived two months after Ramadan (month $\tau = -2$), are the omitted category; partially exposed individuals (conceived one month after Ramadan, month $\tau = -1$) are excluded. The sample pools DHS waves 2006–2007, 2012–2013, and 2017–2018. Numbers in square brackets express the treatment effect as a percentage of the mean birth weight of the unexposed group. All specifications include non-parametric controls for mother's education, partner's education, and household wealth. Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE III: **IMPACTS OF THE EXPOSURE ON EDUCATIONAL ATTAINMENT**

Outcome:	𝟙(Middle School Completed)			
	(1)	(2)	(3)	(4)
Dummy Specification:				
Exposure	-0.028*	-0.011		
	(0.015)	(0.019)		
	[-3.9%]	[-1.6%]		
Exposure × Poor		-0.033		
		(0.028)		
		[-4.6%]		
Trimester Specification:				
First Trimester			-0.035**	-0.016
			(0.016)	(0.021)
			[-4.8%]	[-2.2%]
Second Trimester			-0.032*	-0.031
			(0.018)	(0.023)
			[-4.5%]	[-4.3%]
Third Trimester			-0.021	0.004
			(0.017)	(0.021)
			[-3.0%]	[0.6%]
First Trimester × Poor				-0.039
				(0.030)
				[-5.4%]
Second Trimester × Poor				-0.005
				(0.032)
				[-0.6%]
Third Trimester × Poor				-0.052*
				(0.031)
				[-7.2%]
Observations	15,045	15,045	15,045	15,045
Mean Value of Unexposed	0.723	0.723	0.723	0.723

Notes: The table estimates the effects of in-utero shock on the educational attainment of exposed children. We regress the outcome variable—a dummy variable indicating that the individual completed the middle school—on our exposure measures and exposure × poor interactions. Certainly unexposed individuals, who were conceived two months after Ramadan (month $\tau = -2$), are the omitted category; partially exposed individuals (individuals conceived one month after Ramadan; month $\tau = -1$) are excluded. The sample pools PSLM waves from 2004–2019. Numbers in square brackets express the treatment effect as a percentage of the average birth weight of the unexposed group. All specifications include non-parametric controls for mother's education, partner's education, and household wealth. Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE IV: **IMPACTS OF THE EXPOSURE ON EARNINGS**

Gestation Month at the Start of Ramadan	Outcome: Log Earnings					
	(1)	(2)	(3)	(4)	(5)	(6)
-1	-0.013	-0.012	-0.010	-0.010	-0.012	-0.008
	(0.010)	(0.010)	(0.009)	(0.009)	(0.010)	(0.009)
0	-0.027***	-0.024**	-0.021**	-0.020**	-0.013	-0.005
	(0.010)	(0.010)	(0.009)	(0.009)	(0.010)	(0.009)
1	-0.038***	-0.035***	-0.032***	-0.030***	-0.011	-0.002
	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
2	-0.047***	-0.042***	-0.042***	-0.039***	-0.018*	-0.008
	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
3	-0.060***	-0.056***	-0.051***	-0.049***	-0.035***	-0.022**
	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
4	-0.045***	-0.042***	-0.037***	-0.037***	-0.020**	-0.010
	(0.010)	(0.010)	(0.009)	(0.009)	(0.010)	(0.009)
5	-0.055***	-0.053***	-0.046***	-0.046***	-0.037***	-0.027***
	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
6	-0.039***	-0.039***	-0.035***	-0.036***	-0.021**	-0.018**
	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
7	-0.040***	-0.041***	-0.036***	-0.037***	-0.025***	-0.022**
	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
8	-0.032***	-0.033***	-0.023**	-0.023**	-0.022**	-0.014
	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
9	-0.001	-0.002	0.001	0.001	0.003	0.005
	(0.010)	(0.010)	(0.009)	(0.009)	(0.010)	(0.009)
Observations	829,998	829,998	829,831	829,831	829,998	829,831
Joint test, coefficients on months 1-9 equal 0:						
p-value	0.000	0.000	0.000	0.000	0.000	0.002
Fixed Effects:						
Month of Birth	-	✓	-	✓	-	✓
District of Birth	-	-	✓	✓	-	✓
Year of Birth	-	-	-	-	✓	✓

Notes: The table reports estimates from equation (2). We regress the outcome variable—log of income—on eleven Ramadan exposure dummies, omitting the reference category—certainly unexposed individuals, who were conceived two months after Ramadan (month $\tau = -2$). Please see Figure I on how we define these exposure dummies. The sample consists of the universe of personal income tax returns filed in 2007–2009. Standard errors are in parentheses and clustered at the individual level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table V: **Impacts of the Exposure on Earnings**

In-Utero Ramadan	Outcome: Log Earnings									
Exposure in	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dummy Specification:										
Exposure	-0.039***		-0.037***		-0.036***		-0.020***		-0.015**	
	(0.007)		(0.007)		(0.007)		(0.007)		(0.007)	
Trimester Specification:										
First Trimester		-0.044***		-0.040***		-0.040***		-0.019**		-0.012*
		(0.008)		(0.008)		(0.007)		(0.008)		(0.007)
Second Trimester		-0.047***		-0.045***		-0.043***		-0.026***		-0.021***
		(0.008)		(0.008)		(0.008)		(0.008)		(0.007)
Third Trimester		-0.025***		-0.026***		-0.021***		-0.015*		-0.012*
		(0.008)		(0.008)		(0.008)		(0.008)		(0.007)
Observations	763,958	763,958	763,958	763,958	763,885	763,885	763,958	763,958	763,885	763,885
Joint test, coefficients on trimesters 1-3 equal 0:										
p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.005	0.007	0.026	0.025
Fixed Effects:										
Month of Birth	-	-	✓	✓	-	-	-	-	✓	✓
District of Birth	-	-	-	-	✓	✓	-	-	✓	✓
Year of Birth	-	-	-	-	-	-	✓	✓	✓	✓

Notes*:* The table reports estimates from equation (2). The odd-numbered columns report results from the dummy version of the specification and the even-numbered from the trimester version. Individuals exposed to Ramadan in months 0–3 of their pregnancy are included in the First Trimester group, in months 4–6 in the Second Trimester group, and in months 7–9 in the Third Trimester group. Please see Figure I on how we define the gestation month of exposure. Certainly unexposed individuals, who were conceived two months after Ramadan (month $\tau = -2$), are the omitted category. Partially-exposed individuals grouped in months $\tau = -1$ are excluded. The last row reports the p-value from an F-test of the null hypothesis that the exposure coefficients are jointly zero. The sample includes the universe of personal income tax returns filed in 2007–2009. Standard errors are in parentheses and are clustered at the individual level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE VI: **IMPACTS OF THE EXPOSURE ON EARNINGS**

In-Utero Ramadan	Outcome: Log Income					
Exposure in	(1)	(2)	(3)	(4)	(5)	(6)
Dummy Specification:						
Exposed	-0.028		-0.028		-0.019	
	(0.018)		(0.018)		(0.017)	
Trimester Specification:						
First Trimester		-0.028		-0.027		-0.018
		(0.019)		(0.019)		(0.018)
Second Trimester		-0.042**		-0.044**		-0.034*
		(0.020)		(0.020)		(0.019)
Third Trimester		-0.014		-0.015		-0.007
		(0.021)		(0.021)		(0.019)
Observations	41,385	41,385	41,385	41,385	41,385	41,385
p-value	0.123	0.110	0.123	0.090	0.267	0.160
Fixed Effects:						
District of Birth	-	-	-	-	✓	✓
Month of Birth	-	-	✓	✓	✓	✓
Year of Birth	✓	✓	✓	✓	✓	✓

Notes*:* The table reports estimates from equation (2). The odd-numbered columns report results from the dummy version of the specification and the even-numbered from the trimester version. The sample here consists of household heads in the PSLM data, where we pool waves from 2004–2019. To align this sample with the administrative data, we restrict attention to the same 66 birth cohorts (1924–1989). Individuals exposed to Ramadan in months 0–3 of their pregnancy are included in the First Trimester group, in months 4–6 in the Second Trimester group, and in months 7–9 in the Third Trimester group. Please see Figure I on how we define the gestation month of exposure. Certainly unexposed individuals, who were conceived two months after Ramadan (month $\tau = -2$), are the omitted category; partially-exposed individuals grouped in months $\tau = -1$ are excluded. The last row reports the p-value from an F-test of the null hypothesis that the exposure coefficients are jointly zero. Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE VII: **SELECTION INTO ADMINISTRATIVE TAX DATA?**

In-Utero Ramadan Exposure in	Taxpayer		Income > 75th Percentile		Income > 90th Percentile	
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy Specification:						
Exposed	0.007		-0.013		0.008	
	(0.008)		(0.011)		(0.008)	
Trimester Specification:						
First Trimester		0.007		-0.020*		0.009
		(0.008)		(0.011)		(0.008)
Second Trimester		0.001		-0.023*		0.003
		(0.009)		(0.012)		(0.008)
Third Trimester		0.013		0.004		0.013
		(0.009)		(0.012)		(0.009)
Observations	39,360	39,360	44,703	44,703	44,703	44,703
Joint test, coefficients on trimester 1-3 $\times$ poor interactions equal 0:						
p-value	0.357	0.244	0.217	0.001	0.278	0.358
Fixed Effects:						
District of Birth	✓	✓	✓	✓	✓	✓
Month of Birth	✓	✓	✓	✓	✓	✓
Year of Birth	✓	✓	✓	✓	✓	✓

Notes*:* The table reports estimates from equation (2). The odd-numbered columns report results from the dummy version of the specification and the even-numbered from the trimester version. The outcome variable for the first two columns is a dummy indicating that the individual must have filed a tax return under the Pakistani tax code applicable in the corresponding year. The outcome variable in the next four columns is a dummy indicating that the individual has income above the x-th percentile of the income distribution for the year, with x indicated in the heading of the column. The sample consists of individuals classified as household heads in the PSLM data, where we pool waves from 2004–2019. To align this sample with the administrative data, we restrict attention to the same 66 birth cohorts (1924–1989). Individuals exposed to Ramadan in months 0–3 of their pregnancy are included in the First Trimester group, in months 4–6 in the Second Trimester group, and in months 7–9 in the Third Trimester group. Please see Figure I on how we define the gestation month of exposure. Certainly unexposed individuals, who were conceived two months after Ramadan (month $\tau = -2$), are the omitted category; partially-exposed individuals grouped in months $\tau = -1$ are dropped. The last row reports the p-value from an F-test of the null hypothesis that the exposure coefficients are jointly zero. Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table VIII: **Intergenerational Effects of In-Utero Shocks on Educational Attainment**

In-Utero Ramadan Exposure in	Father is Exposed				Mother is Exposed			
	Middle School		High School		Middle School		High School	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dummy Specification:								
Exposed	-0.042		-0.037		-0.015		-0.084**	
	(0.030)		(0.031)		(0.039)		(0.039)	
Trimester Specification:								
First Trimester		-0.001		0.017		0.013		-0.036
		(0.032)		(0.034)		(0.042)		(0.043)
Second Trimester		-0.008		0.019		-0.011		-0.071*
		(0.032)		(0.034)		(0.041)		(0.041)
Third Trimester		-0.058*		-0.060*		-0.021		-0.095**
		(0.030)		(0.031)		(0.039)		(0.040)
Observations	16,910	16,910	16,910	16,910	17,195	17,195	17,195	17,195
Joint test, coefficients on trimesters 1-3 equal 0:								
p-value	0.155	0.000	0.228	0.000	0.692	0.216	0.033	0.002
Mean Value of Unexposed	0.642	0.642	0.399	0.399	0.622	0.622	0.392	0.392
Fixed Effects:								
District of Birth	✓	✓	✓	✓	✓	✓	✓	✓
Month of Birth	✓	✓	✓	✓	✓	✓	✓	✓
Year of Birth	✓	✓	✓	✓	✓	✓	✓	✓

Notes*:* The table reports estimates from equation (2). Odd-numbered columns report results from the dummy version of the specification and even-numbered columns from the trimester version of the specification. The sample consists of individuals classified as the children of household head in the PSLM data, where we pool waves from 2004–2019. In Columns (1)-(4), the exposure indicator equals one if the individual's father was exposed in utero to Ramadan during any gestational month in odd-numbered columns and during the first, second, or third trimester in even-numbered columns. Columns (5)-(8) report the parallel estimates in which exposure is defined analogously using the mother's in-utero exposure. The outcome variables are dummies indicating that the individual has completed the middle or high school indicated in the heading of each column. To align this sample with the administrative data, we restrict attention to the same 66 birth cohorts (1924–1989). Parents exposed to Ramadan in months 0–3 of their pregnancy are included in the First Trimester group, in months 4–6 in the Second Trimester group, and in months 7–9 in the Third Trimester group. Please see Figure I on how we define the gestation month of exposure. Certainly unexposed parents, who were conceived two months after Ramadan (month $\tau = -2$), are the omitted category. Partially-exposed Parents conceived in months $\tau = -1$ are excluded. The last row reports the p-value from an F-test of the null hypothesis that the exposure coefficients are jointly zero. Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE IX: **IMPACTS OF THE EXPOSURE ON EARNINGS – TS2SLS ESTIMATES**

	Outcome: Log Earnings		
	(1)	(2)	(3)
Reduced Form (ITT)			
Exposed	-0.039***	-0.037***	-0.019***
	(0.007)	(0.007)	(0.007)
TS2SLS Estimates (ATT)			
Fasted (instrumented)	-0.071***	-0.067***	-0.034***
	(0.014)	(0.014)	(0.013)
Fasting Intensity (Instrumented)	-0.106***	-0.100***	-0.050***
	(0.021)	(0.021)	(0.019)
Mean of dependent variable	12.907	12.907	12.907
Observations	762,035	762,035	762,035
Fixed Effects:			
Month of Birth	-	✓	✓
Year of Birth	-	-	✓

Notes: This table presents Two-Sample Two-Stage Least Squares (TS2SLS) estimates of the impact of prenatal fasting on adult earnings. The dependent variable is the log of taxable income. The top panel reports the Reduced Form (Intention-to-Treat) estimate from the administrative tax sample. The bottom panel reports structural Average Treatment on the Treated (ATT) estimates, where we instrument maternal fasting behavior using pregnancy's overlap with Ramadan. Since fasting status is unobserved in the tax data, we estimate the first-stage relationship using the household survey data and combine it with the reduced form following the TS2SLS framework. Rows labeled 'Fasted' and 'Fasting Intensity' correspond to separate specifications defining the endogenous variable as an indicator for any maternal fasting and a continuous fasting-intensity measure based on realized exposure index combining overlap duration with reported fasting days. Column (1) presents the baseline specification; Column (2) adds month of birth fixed effects; and Column (3) adds month and year of birth fixed effects. Standard errors are clustered at the individual level and adjusted for the two-sample estimation step following Inoue & Solon (2010) and Pacini & Windmeijer (2016). ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

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A Definition of Variables

(*i*) **Earnings.** Annual income from all sources reported in the survey or on the tax return.

(*ii*) **Conception Date.** The exact date of birth minus 266 days.

(*iii*) **Literacy.** Takes the value 1 if the DHS surveyor classifies the mother as "can read easily" in distinction to "reads with difficulty" or "cannot read".

(*iv*) **Education.** Takes the value 1 if the mother has completed secondary school or higher.

(*v*) **Partner Education.** Takes the value 1 if the mother's partner has completed secondary school or higher.

(*vi*) **Partner Occupation.** Takes the value 1 if the mother's partner is employed in one of the following four relatively skilled occupations: (1) professional, technical or managerial; (2) clerical; (3) sales; and (4) services.

(*vii*) **Wealth.** The DHS data divide households into five categories based on a composite measure of their cumulative living standard: (1) poorest; (2) poorer; (3) middle; (4) richer; and (5) richest. The dummy variable Wealth indicates that the household belongs to the top category.

(*viii*) **Owns Home (etc).** Takes the value 1 if the mother lives in an owned or rent-free house as opposed to a rented mortgaged house. Other such variables such as "Owns Television" are self-explanatory.

(*ix*) **Middle School.** The variable is from the PSLM data indicating that the respondent has completed at least ten years of education, obtaining classification called "Matriculation" in Pakistan.

(*x*) **High School.** The variable is from the PSLM data indicating that the respondent has completed at least twelve years of education.

Table A.I: Summary Statistics

(a) Exposure to Shock

	Share (%)			
	DHS	PSLM	Tax Data	Survey
Exposed in Any Trimester	82	83	84	84
Exposed in First Trimester	33	35	33	33
Exposed in Second Trimester	24	24	26	27
Exposed in Third Trimester	26	25	25	25
Maximum Exposure Risk	75	74	76	75
N	169,078	108,219	830,107	5,954

(b) Demographics and Outcomes

	Mean	St. Dev.	Observations
DHS			
Died at Birth (%)	3	54	5,100
Birth Weight (grams)	2,992	924	5,100
Low Birth Weight (%)	21	41	5,100
PSLM			
Earnings (PKR)	178,097	332,057	9,693
Taxpayer (%)	12	33	12,095
Below Poverty Line (%)	27	44	18,118
Completed Middle School (%)	70	46	15,835
Completed High School (%)	46	50	15,835
Tax Data			
Earnings (PKR)	763,251	1,675,422	829,893
Male (%)	91	29	829,445

(c) Beliefs, Norms, and Mechanisms

	Mean	St. Dev.	Observations
Household Survey			
Fasted During Pregnancy (%)	55.39	49.71	4,414
Days Fasted (Unconditional) (%)	12.19	12.86	4,414
Days Fasted (Conditional) (%)	22.01	9.07	2,445
Experienced Weakness During Fast (%)	36.62	48.19	2,455
Experienced Exhaustion During Fast (%)	35.27	47.79	2,455
Not Fasted Because of Weakness (%)	83.50	37.13	1,842
Believes Pregnant Women Should Fast (%)	96.86	17.43	1,690
Guessed Share Believing Pregnant Women Should Fast (%)	49.80	25.22	1,690
Misperception Gap (Actual - Guessed) (%)	47.07	29.85	1,690
Believes Fasting is Not Harmful to Fetus (%)	50	7.06	4,586
Aware of Religious Exemption (%)	58.11	49.35	1,690

Notes: This table presents summary statistics of our data. Panel A reports the share of individuals whose gestation overlaps Ramadan in each trimester, constructed from the date of birth and an assumed gestation length of 266 days. "Exposed in any trimester" equals one if any portion of the inferred gestational window overlaps Ramadan. "Maximum exposure risk" is the fraction of pregnancies exposed to the full duration of the relevant Ramadan. Panel B reports means, standard deviations, and the number of observations for the main variables used in the analysis; monetary amounts are in Pakistani rupees (PKR). "Taxpayer" indicates an individual who, based on PSLM information and prevailing provisions of the tax code, would be required to file an income tax return; Administrative tax earnings are taken from personal income tax returns filed in 2007-2009. Panel (c) reports fasting behavior, symptoms, beliefs, and social norm perceptions from our household survey. "Days Fasted (Conditional)" conditions on any fasting during pregnancy. "Not Fasted Because of Weakness" indicates respondents who reduced fasting due to experienced symptoms. "Believes Pregnant Women Should Fast" is corrected for social desirability bias using a list experiment (see Appendix H.1). "Guessed Share" reports respondents' incentivized estimates of the proportion of other respondents who believe pregnant women should fast. "Misperception Gap" is the difference between actual agreement (96.9%) and guessed agreement (49.8%). Sample sizes vary mainly depending upon whether the question relates to mother or to pregnancy.

B Pathways Linking In-Utero Shocks to Later Outcomes

B.1 Biological Mechanisms Triggered by Fasting During Pregnancy

In this section, we detail three complementary biological mechanisms through which a shock to the in-utero environment can influence the development trajectory of the child.

B.1.1 Placental Function as Mediator

The placenta acts as the physiological gatekeeper, regulating maternal inputs to the fetus. It not only supplies oxygen and nutrients but also serves as a barrier against harmful exposures. Adverse shocks to the maternal environment can disrupt this placental architecture and transport function, directly affecting fetal programming (Jansson & Powell (2007)). Mechanistically, this operates through two distinct channels relevant to fasting. First, the placenta adapts to nutritional scarcity. In nutrient-deprived pregnancies, the placenta can upregulate specific transport proteins to shield the fetus. For example, biological evidence indicates that intrauterine growth restriction triggers increased expression of the GLUT3 glucose transporter, an adaptive response to maintain fetal nutrition amidst scarcity (Janzen *et al.* (2013)). These compensatory mechanisms help prioritize essential organ development ("brain sparing"); however, if maternal malnutrition is severe or prolonged, this capacity is overwhelmed, resulting in compromised somatic growth and latent metabolic deficits at birth (Miller *et al.* (2016)). Second, the placenta protects the fetus from excessive maternal stress signals via enzymatic shielding. The enzyme 11β-HSD2 inactivates maternal cortisol before it crosses to the fetus. Studies indicate that chronic maternal stress downregulates placental 11β-HSD2 expression (Bronson & Bale (2016)). This degradation of the placental "glucocorticoid barrier" allows fetal overexposure to cortisol at critical developmental periods, which is known to reprogram the hypothalamic-pituitary-adrenal (HPA) axis and alter the maturation of neurobiological systems (Bronson & Bale (2016)).

B.1.2 Endocrine Programming and Metabolic Trade-offs

Shocks to maternal environment alter fetal development through the following hormonal and metabolic channels. First, stress activates the maternal HPA axis, elevating cortisol. While essential for maturation, untimely glucocorticoid exposure accelerates differentiation at the expense of proliferation, permanently reducing cell counts in the brain and periphery (Bronson & Bale (2016)). This signaling is amplified by placental corticotropin-releasing hormone (CRH), which creates a positive feedback loop under stress, intensifying the fetal exposure (Kassotaki *et al.* (2021)).

Second, nutritional restriction drives metabolic programming. Maternal hypoglycemia reduces fetal insulin and IGF availability. In response, the fetus adopts a "brain-sparing" strategy: it hemodynamically redirects blood flow toward vital organs (brain, heart) at the expense of the liver, kidneys, and pancreas (Faa *et al.* (2024)). While this preserves immediate survival, it results in asymmetric growth—neonate brains may be relatively preserved while metabolic organs remain underdeveloped.

Finally, these adaptations permanently calibrate the offspring's metabolism, a mechanism called the "thrifty phenotype" (Vaag *et al.* (2012)). By downregulating insulin sensitivity and metabolic rate to survive in utero, the fetus anticipates a harsh postnatal environment. Crucially, if the postnatal environment is nutrient-rich, this programming becomes maladaptive, predisposing the individual to obesity, diabetes, and metabolic syndrome in adulthood (Gluckman & Hanson (2004)). The specific constellation of deficits depends on the critical window of exposure: early shocks (during neurogenesis) compromise cognitive endowment, while later shocks primarily impair metabolic regulation (Wu *et al.* (2024)).

B.1.3 Epigenetic Modifications

Adverse in-utero environments can reprogram fetal gene expression via epigenetic modifications as well. These mechanisms provide a molecular memory of prenatal conditions, linking early exposures to long-term gene regulation. Studies now show that maternal nutritional deprivation or stress can induce specific epigenetic changes in both placental and fetal tissues, thereby programming fetal physiology. For example, maternal starvation during the Dutch Hunger Winter (1944-45) led to persistent DNA methylation differences in adults who were in utero at that time (Heijmans *et al.* (2008)). Genome-wide analyses have since identified numerous differentially methylated regions (DMRs) associated with prenatal famine exposure, particularly in genes involved in growth, metabolism, and vascular development (Tobi *et al.* (2014)). One study found that prenatal malnutrition left epigenetic marks at genes like INSR (insulin receptor) and CPT1A (a fat metabolism enzyme), with these methylation changes correlating with lower birth weight and altered cholesterol levels in offspring (Tobi *et al.* (2014)). Such findings support the idea that epigenetic metabolic memory of an adverse womb environment contributes to the child's reduced health endowment at birth and elevated disease risks later (Faa *et al.* (2024)).

Even at birth, some consequences of epigenetic programming are evident. For instance, one study found newborns with very low birth weight had a distinctive DNA methylation signature in their cord blood, reflecting in-utero growth restriction (Tobi *et al.* (2014)). Such signatures may be linked to immediate issues like immature immunity or organ function. Moreover, adverse intrauterine conditions can affect the epigenome of the placenta itself, reducing its functional capacity. Exposure to maternal famine or stress has been shown to alter methylation of placental genes that regulate nutrient transport and vascular development, potentially compounding the fetal growth impairment (Bronson & Bale (2016)).

B.2 Poverty Penalty in Early-Life Shocks

There are several complementary reasons why in-utero shocks may have larger effects on children born to poorer parents. First, the biological weathering hypothesis implies that low-SES mothers typically enter pregnancy with poorer nutrition and health. This baseline disadvantage means any additional shock hits an already vulnerable system with no buffer against the shock (Aizer & Currie (2014)). Second, even when the initial harm is similar, poorer parents typically have less scope to offset it through remedial investment, leaving the child to suffer the full developmental consequences (Cunha & Heckman (2007); Fan & Porter (2020)). Third, the relationship between an input shock and outcomes could be non-linear: a given caloric shortfall may be largely inframarginal for a well-nourished fetus but could be devastating for a malnourished fetus. Fourth, for low-SES individuals an in-utero shock often coincides with other shocks before and after birth, compounding the initial damage. Aslim *et al.* (2025), for example, find that Afghan girls prenatally exposed to conflict exhibit lower school attendance and math scores—deficits that reflect not only the initial shock but also its interaction with gender discrimination and degraded schooling in war-affected areas.

B.3 Intergenerational Transmission of Early Life Shocks

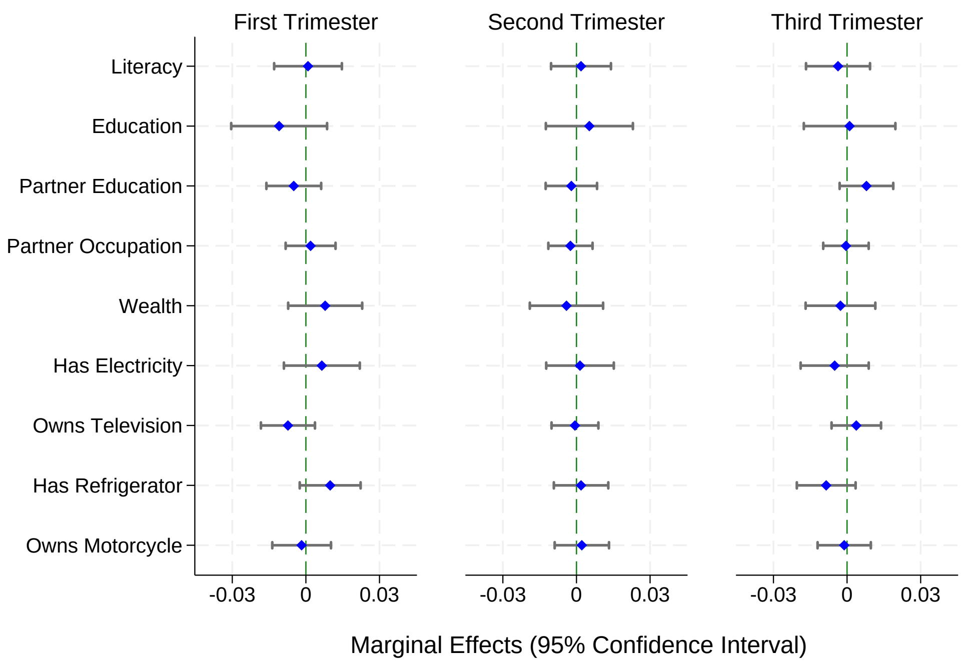
There is growing evidence from animal studies that early-life shocks can transmit into the next generation. Importantly, this transmission can occur through non-genetic channels including epigenetic reprogramming and altered maternal physiology (Klengel *et al.* (2016)). Controlled experiments, for example, show that male mice subjected to maternal separation stress as neonates develop depressive behaviors in adulthood. Strikingly, these effects are also observed in their offspring despite no direct exposure of the offspring. The mechanism for this transmission has been traced to epigenetic changes: the early stress induces abnormal DNA methylation patterns in the father's sperm, with the same methylation marks and gene expression changes later found in the brains of the offspring (Franklin *et al.* (2010)). Similarly, female mice that experience adversity in early life often show long-term changes in their endocrine and metabolic state, which can affect the uterine environment of their offspring (King *et al.* (2025)).

In humans, economic factors can complement these biological mechanisms. Parents who suffered a shock in infancy tend to grow up with poorer health, education, and earnings, which in turn undermines the resources and care they can give to their offspring. For example, Currie & Moretti (2003) find that better-educated mothers have healthier babies likely because of increased prenatal care, less smoking, and higher likelihood of marriage. Conversely, when a parent's early-life shock leaves them less healthy or financially strained, their children tend to start life at a disadvantage. For example, children of cohorts exposed to China's 1959-61 Great Famine (especially daughters) were found to perform worse cognitively, an outcome traced partly to tight household budgets and reduced parental investment (Chen & Zhou (2007)). Similarly, Norwegian men exposed to nuclear fallout in utero were found to have lower IQs and earnings, an effect that extended to the next generation with their sons showing reduced cognitive scores (Black *et al.* (2019)).

C Additional Results and Robustness Tests on Selection

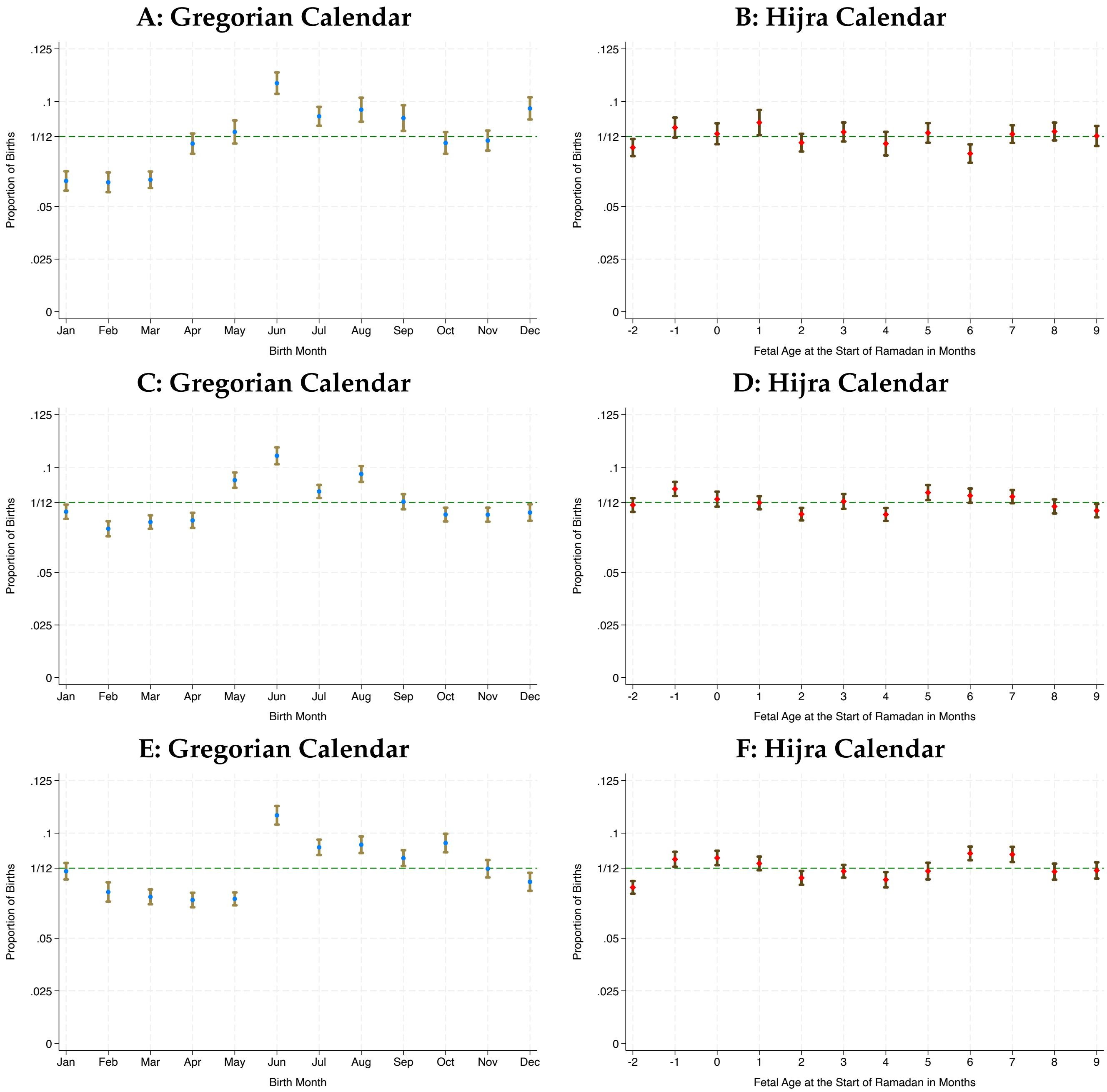
This section presents additional results and robustness tests ruling out selection into treatment. These results are described and referenced in the main text.

FIGURE C.I: **SELECTION INTO IN-UTERO EXPOSURE? MARGINAL EFFECTS**



Notes: This figure tests for parental sorting across trimesters of in-utero exposure. We estimate a multinomial logistic model where the outcome variable is the trimester of exposure as defined in Figure I. We exclude the unexposed and partially exposed individuals (exposure months $\tau = -2$, and $\tau = -1$,) and classify the remainder into three mutually exclusive groups: First Trimester (exposure in months 0-3), Second Trimester (exposure in months 4-6), and Third Trimester (exposure in months 7-9). We plot the estimated marginal effects together with their 95% confidence intervals. The model's coefficients and tests of joint significance are reported in Table C.V. For details of the explanatory variables used here see Appendix A. The model also includes the month of birth, district of birth, and year of birth fixed effects. The sample comprises 33,856 observations and pools all four DHS rounds (1990–91, 2006–07, 2012–13, and 2017–18).

FIGURE C.II: **BIRTH SEASONALITY**



Notes: The figure replicates the analysis in Figure II separately by DHS waves. The top panels use the 1990–1991 wave, the middle panels the 2006–2007 wave, and the bottom panels the 2012–2013 wave. Within each wave, each panel regresses a month-of-birth indicator on a constant; the resulting coefficients therefore report the (weighted) share of births occurring in each month. We estimate one regression per month and plot the coefficients with 95% confidence intervals. All regressions are weighted by DHS sampling weights to obtain nationally representative estimates. The left panels define month of birth using the Gregorian calendar; the right panels define month of birth using the Islamic Hijra calendar. For consistency with Figure I, Hijra timing is grouped into the same twelve conception-month categories (e.g., month $\tau = 0$ denotes conceptions occurring in the Hijra month in which Ramadan begins).

TABLE C.II: **SEASONALITY IN GREGORIAN QUARTER OF BIRTH**

Gregorian Quarter of Birth	Literacy (1)	Literacy (2)	Education (3)	Education (4)	Partner Education (5)	Partner Education (6)	Partner Occupation (7)	Partner Occupation (8)	Wealth (9)	Wealth (10)
Second Quarter	-0.012***	-0.006	-0.008**	-0.004	-0.010**	-0.007*	-0.008*	-0.008**	-0.001	-0.004
	(0.005)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
Third Quarter	-0.010**	-0.006	-0.008***	-0.006**	-0.003	-0.002	-0.007	-0.007*	-0.001	-0.003
	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
Fourth Quarter	-0.003	-0.001	0.000	0.001	0.001	0.001	-0.006	-0.005	-0.002	-0.003
	(0.004)	(0.004)	(0.003)	(0.002)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
Observations	167,049	167,049	167,151	167,151	165,165	165,165	165,123	165,123	167,151	167,151
Joint test, coefficients on trimesters 1-3 equal 0:										
p-value	0.021	0.261	0.006	0.032	0.099	0.275	0.197	0.175	0.945	0.689
Mean Value	0.262	0.262	0.118	0.118	0.297	0.297	0.367	0.367	0.195	0.195
Fixed Effects:										
District of Birth	-	✓	-	✓	-	✓	-	✓	-	✓
Year of Birth	-	✓	-	✓	-	✓	-	✓	-	✓

Notes: The table tests for parental sorting across Gregorian months of birth. We estimate a version of our equation (2), regressing the outcome indicated in the heading of each column on three Gregorian quarter of birth dummies, omitting the first quarter as the omitted category. Second Quarter dummy, for example, equals one for births in April–June. We use the DHS data and weight the regressions by sampling weights so that the results are nationally representative. Even-numbered columns additionally include district-of-birth and year-of-birth fixed effects. The mean of the dependent variable is reported above the fixed-effects rows. For details of the variables used here see Appendix A. The sample pools four DHS waves (1990-1991, 2006-2007, 2012-2013, and 2017-2018). Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE C.III: **SEASONALITY IN GREGORIAN QUARTER OF BIRTH**

Gregorian Quarter	Owns Home		Has Electricity		Has Television		Has Refrigerator		Has Motorcycle	
of Birth	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Second Quarter	0.010*	0.013	0.001	0.000	-0.006	-0.006	0.001	0.002	-0.014***	-0.009*
	(0.006)	(0.000)	(0.004)	(0.003)	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)	(0.005)
Third Quarter	-0.003	0.000	0.001	0.000	-0.003	-0.004	-0.001	-0.000	-0.003	-0.003
	(0.006)	(0.000)	(0.004)	(0.003)	(0.005)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)
Fourth Quarter	0.007	0.005	0.003	0.001	-0.000	-0.003	0.005	0.002	0.002	-0.000
	(0.005)	(0.000)	(0.004)	(0.003)	(0.005)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)
Observations	39,049	39,049	167,092	167,092	167,090	167,090	167,084	167,084	167,052	167,052
Joint test, coefficients on trimesters 1-3 equal 0:										
p-value	0.061	.	0.898	0.993	0.705	0.583	0.602	0.904	0.022	0.279
Mean Value	0.890	0.890	0.881	0.881	0.544	0.544	0.410	0.410	0.300	0.300
Fixed Effects:										
District of Birth	-	✓	-	✓	-	✓	-	✓	-	✓
Year of Birth	-	✓	-	✓	-	✓	-	✓	-	✓

Notes: The table explores parental sorting across different Gregorian months of birth. We estimate a version of our equation (2), regressing the outcome indicated in the heading of each column on three quarter of birth dummies, dropping the first as the omitted category. Second Quarter dummy, for example, includes individuals born in calendar months April to June. We use the DHS data for this purpose and weight the regressions by sampling weights so that the results are nationally representative. Even-numbered columns include the district and year of birth as controls. Mean value of the outcome variable is indicated in the row above fixed effects.. For details of the variables used here see Appendix A. The sample here includes all three waves of the DHS that occurred in 1990, 2006, 2012, and 2017. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE C.IV: **SELECTION INTO IN-UTERO RAMADAN EXPOSURE?**

In-Utero Ramadan	Owns Home		Has Electricity		Has Television		Has Refrigerator		Has Motorcycle	
Exposure in	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dummy Specification:										
Exposed	-0.004		-0.002		-0.004		0.000		-0.005	
	(0.007)		(0.004)		(0.005)		(0.005)		(0.005)	
Trimester Specification:										
First Trimester		-0.004		-0.001		-0.004		0.001		-0.007
		(0.008)		(0.004)		(0.006)		(0.006)		(0.006)
Second Trimester		-0.003		-0.002		-0.003		0.002		-0.003
		(0.007)		(0.004)		(0.006)		(0.006)		(0.006)
Third Trimester		-0.004		-0.003		-0.004		-0.004		-0.005
		(0.007)		(0.004)		(0.006)		(0.006)		(0.006)
Observations	35,564	35,564	152,507	152,507	152,506	152,506	152,501	152,501	152,472	152,472
Joint test, coefficients on trimesters 1-3 equal 0:										
p-value	0.552	0.932	0.630	0.878	0.478	0.908	0.991	0.481	0.354	0.617
Mean Value of DV	0.889	0.889	0.881	0.881	0.544	0.544	0.410	0.410	0.300	0.300
Fixed Effects:										
Month of Birth	-	✓	-	✓	-	✓	-	✓	-	✓
District of Birth	-	✓	-	✓	-	✓	-	✓	-	✓
Year of Birth	-	✓	-	✓	-	✓	-	✓	-	✓

Notes: The table tests for parental sorting across Hijra months of conception. We estimate equation (2) using the DHS data. The outcome variable in each of these regressions is indicated in the heading of each column. We weight the regressions by sampling weights, so the estimates are nationally representative. Odd-numbered columns estimate the dummy specification, whereas the even-numbered columns estimate the trimester specification. The mean of the dependent variable is reported in the row above the fixed-effects indicators. The row labeled 'p-value' reports the p-value from an F-test of the null hypothesis that the exposure coefficients are jointly zero. For details of the variables used here see Appendix A. The sample includes all DHS waves used in the analysis (1990–1991, 2006–2007, 2012–2013, and 2017–2018). Individuals exposed to Ramadan in months 0–3 of their pregnancy are included in the First Trimester group, in months 4–6 in the Second Trimester group, and in months 7–9 in the Third Trimester group. Certainly unexposed individuals, i.e., those who were conceived two months after Ramadan (month $\tau = -2$), are the omitted category, and partially-exposed individuals grouped in months $\tau = -1$ are dropped. Standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE C.V: **SELECTION INTO IN-UTERO RAMADAN EXPOSURE? A MULTINOMIAL LOGISTIC MODEL**

	Coefficients		
	First Trimester (1)	Second Trimester (2)	Third Trimester (3)
Literacy	-0.008	-0.004	-0.025
	(0.055)	(0.059)	(0.057)
Education	-0.083	-0.033	-0.048
	(0.084)	(0.084)	(0.090)
Partner Education	-0.004	0.002	0.040
	(0.045)	(0.047)	(0.048)
Partner Occupation	-0.006	-0.021	-0.013
	(0.040)	(0.041)	(0.042)
Wealth	0.035	-0.003	0.002
	(0.059)	(0.064)	(0.059)
Has Electricity	0.052	0.039	0.014
	(0.058)	(0.061)	(0.059)
Owns Television	-0.068	-0.050	-0.033
	(0.042)	(0.044)	(0.044)
Has Refrigerator	0.066	0.045	0.005
	(0.050)	(0.052)	(0.052)
Owns Motorcycle	-0.013	0.000	-0.012
	(0.048)	(0.050)	(0.050)
Joint test, above coefficients equal 0 (by column):			
F-test	0.609	0.278	0.237
p-value	0.790	0.981	0.989
Joint test, above coefficients equal 0 (all columns):			
F-test	0.473		
p-value	0.990		
Observations	87,659		

Notes: This table tests for parental sorting across trimesters of in-utero exposure. We estimate a multinomial logistic model where the outcome variable is the trimester of exposure as defined in Figure I. We exclude the unexposed and partially exposed individuals (exposure months $\tau = -2$, and $\tau = -1$,) and classify the remainder into three mutually exclusive groups: First Trimester (exposure in months 0-3), Second Trimester (exposure in months 4-6), and Third Trimester (exposure in months 7-9). We report the coefficients from the model for these three categories. Figure C.I reports the marginal effects from this model. For details of the explanatory variables used here see Appendix A. The model also includes the month of birth, district of birth, and year of birth fixed effects. The sample pools all four DHS rounds (1990–91, 2006–07, 2012–13, and 2017–18). Standard errors are in parenthesis. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table C.VI: **Selection into In-Utero Ramadan Exposure? An Alternative Imputation**

In-Utero Ramadan	Literacy		Education		Partner Education		Partner Occupation		Wealth	
Exposure in	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dummy Specification:										
Exposed	0.005		-0.001		0.004		-0.001		-0.003	
	(0.005)		(0.003)		(0.005)		(0.006)		(0.005)	
Trimester Specification:										
First Trimester		0.004		-0.004		0.003		-0.002		-0.002
		(0.005)		(0.004)		(0.005)		(0.006)		(0.005)
Second Trimester		0.006		0.002		0.005		-0.003		-0.003
		(0.006)		(0.004)		(0.006)		(0.006)		(0.005)
Third Trimester		0.005		-0.001		0.005		0.001		-0.004
		(0.005)		(0.004)		(0.006)		(0.006)		(0.005)
Observations	152,468	152,468	152,563	152,563	150,755	150,755	150,718	150,718	127,707	127,707
Joint test, coefficients on trimesters 1-3 equal 0:										
p-value	0.320	0.775	0.740	0.238	0.407	0.826	0.800	0.731	0.523	0.905
Mean Value	0.261	0.261	0.118	0.118	0.297	0.297	0.368	0.368	0.178	0.178
Fixed Effects:										
Month of Birth	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
District of Birth	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year of Birth	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes: The table tests for parental sorting across Hijra months of conception. We estimate equation (2) using the DHS data. The outcome variable in each of these regressions is indicated in the heading of each column. Where we do not observe the exact day of birth of an individual, we impute it by drawing a random day of birth from a uniform distribution. We weight the regressions by sampling weights, so the estimates are nationally representative. Odd-numbered columns estimate the dummy specification, whereas the even-numbered columns estimate the trimester specification. The mean of the dependent variable is reported in the row above the fixed-effects indicators. The row labeled 'p-value' reports the p-value from an F-test of the null hypothesis that the exposure coefficients are jointly zero. For details of the variables used here see Appendix A. The sample includes all DHS waves used in the analysis (1990–1991, 2006–2007, 2012–2013, and 2017–2018). Individuals exposed to Ramadan in months 0–3 of their pregnancy are included in the First Trimester group, in months 4–6 in the Second Trimester group, and in months 7–9 in the Third Trimester group. Certainly unexposed individuals, i.e., those who were conceived two months after Ramadan (month $\tau = -2$), are the omitted category, and partially-exposed individuals grouped in months $\tau = -1$ are dropped. Standard errors are reported in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE C.VII: **SELECTION INTO IN-UTERO RAMADAN EXPOSURE? AN ALTERNATIVE IMPUTATION**

In-Utero Ramadan	Owns Home		Has Electricity		Has Television		Has Refrigerator		Has Motorcycle	
Exposure in	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dummy Specification:										
Exposed	-0.004		-0.002		-0.004		0.000		-0.005	
	(0.007)		(0.004)		(0.005)		(0.005)		(0.005)	
Trimester Specification:										
First Trimester		-0.004		-0.001		-0.004		0.001		-0.007
		(0.008)		(0.004)		(0.006)		(0.006)		(0.006)
Second Trimester		-0.003		-0.002		-0.003		0.002		-0.003
		(0.007)		(0.004)		(0.006)		(0.006)		(0.006)
Third Trimester		-0.004		-0.003		-0.004		-0.004		-0.005
		(0.007)		(0.004)		(0.006)		(0.006)		(0.006)
Observations	35,564	35,564	152,507	152,507	152,506	152,506	152,501	152,501	152,472	152,472
Joint test, coefficients on trimesters 1-3 equal 0:										
p-value	0.552	0.932	0.630	0.878	0.478	0.908	0.991	0.481	0.354	0.617
Mean Value	0.889	0.889	0.881	0.881	0.544	0.544	0.410	0.410	0.300	0.300
Fixed Effects:										
Month of Birth	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
District of Birth	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year of Birth	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Notes*:* The table rules out parental sorting across different Hijra months of conception. We estimate equation (2) using the DHS data. The outcome variable in each of these regressions is indicated in the heading of each column. Where we do not observe the exact day of birth of an individual, we impute it by drawing a random day of birth from a uniform distribution. We weight the regressions by sampling weights so that the results are nationally representative. Odd-numbered columns estimate the dummy specification, whereas the even-numbered columns estimate the trimester specification. Mean value of the outcome variable is indicated in the row above fixed effects. P-value of the hypothesis that the exposure coefficients are jointly zero is provided in the row before that. For details of the variables used here see Appendix A. The sample here includes all three waves of the DHS that occurred in 1990, 2006, 2012, and 2017. Individuals exposed to Ramadan in months 0–3 of their pregnancy are included in the First Trimester group, in months 4–6 in the Second Trimester group, and in months 7–9 in the Third Trimester group. Certainly unexposed individuals, i.e., those who were conceived two months after Ramadan (month -2), are the omitted category, and individuals grouped in months -1 are dropped. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

D Additional Results on Birth Weight

This section presents additional results and robustness tests on birth weight. These results are described and referenced in the main text.

TABLE D.I: **IMPACTS OF THE EXPOSURE ON BIRTH WEIGHT**

Outcome:	$\mathbb{1}$(Low Birth Weight)			
	(1)	(2)	(3)	(4)
Exposure	0.085***	0.059**		
	(0.026)	(0.027)		
	[45.6%]	[31.5%]		
Exposure × Poor		0.176**		
		(0.078)		
		[94.6%]		
First Trimester			0.087***	0.062**
			(0.028)	(0.029)
			[46.6%]	[33.5%]
Second Trimester			0.104***	0.072**
			(0.031)	(0.033)
			[56.2%]	[38.9%]
Third Trimester			0.076***	0.056*
			(0.030)	(0.031)
			[40.9%]	[29.9%]
First Trimester × Poor				0.159*
				(0.088)
				[85.5%]
Second Trimester × Poor				0.257***
				(0.085)
				[138.2%]
Third Trimester × Poor				0.131
				(0.087)
				[70.8%]
Observations	5,100	5,100	5,100	5,100
Mean Value of Unexposed	0.186	0.186	0.186	0.186

Notes: The table estimates the effects of in-utero shock on the birth weight. The outcome variable here is a dummy indicating that the birth weight of the child is less than 2,500 grams. We regress this outcome on our exposure measures and exposure × poor interaction. Certainly unexposed individuals, who were conceived two months after Ramadan (month $\tau = -2$), are the omitted category; partially exposed individuals (conceived one month after Ramadan, month $\tau = -1$) are excluded. The sample pools DHS waves 2006–2007, 2012–2013, and 2017–2018. Numbers in square brackets express the treatment effect as a percentage of the mean birth weight of the unexposed group. All specifications include non-parametric controls for mother's education, partner's education, and household wealth. Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE D.II: **IMPACTS OF THE EXPOSURE ON BIRTH WEIGHT BY GENDER**

Outcome:	Birth Weight (Grams)			
	(1)	(2)	(3)	(4)
Exposure	-205.3***	-112.9		
	(66.7)	(90.1)		
	[-6.7%]	[-3.7%]		
Exposure × Male		-187.8		
		(136.3)		
		[-6.1%]		
First Trimester			-206.6***	-130.5
			(68.3)	(95.5)
			[-6.8%]	[-4.3%]
Second Trimester			-237.7***	-102.4
			(77.2)	(100.2)
			[-7.8%]	[-3.4%]
Third Trimester			-192.2**	-124.5
			(75.3)	(101.0)
			[-6.3%]	[-4.1%]
First Trimester × Male				-157.3
				(145.7)
				[-5.1%]
Second Trimester × Male				-268.5*
				(147.2)
				[-8.8%]
Third Trimester × Male				-136.4
				(145.8)
				[-4.5%]
Observations	5,100	5,100	5,100	5,100
Mean Value of Unexposed	3054.9	3054.9	3054.9	3054.9

Notes: The table explores whether the effects of in-utero shock on the birth weight differ by the gender of the child. We regress the outcome variable—birth weight in grams—on measures of exposure and exposure × male interaction. Certainly unexposed individuals, who were conceived two months after Ramadan (month $\tau = -2$), are the omitted category; partially exposed individuals (conceived one month after Ramadan, month $\tau = -1$) are excluded. The sample pools DHS waves 2006–2007, 2012–2013, and 2017–2018. Numbers in square brackets express the treatment effect as a percentage of the mean birth weight of the unexposed group. All specifications include non-parametric controls for mother's education, partner's education, and household wealth. Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

E Additional Results on Educational Attainment

This section presents additional results and robustness tests on educational attainment. These results are described and referenced in the main text.

TABLE E.I: **IMPACTS OF THE EXPOSURE ON EDUCATIONAL ATTAINMENT**

Outcome:	$\mathbb{1}$(High School Completed)			
	(1)	(2)	(3)	(4)
Dummy Specification:				
Exposure	-0.035**	-0.038		
	(0.017)	(0.024)		
	[-7.4%]	[-8.1%]		
Exposure × Poor		0.007		
		(0.034)		
		[1.5%]		
Trimester Specification:				
First Trimester			-0.049**	-0.041
			(0.020)	(0.025)
			[-10.4%]	[-8.7%]
Second Trimester			-0.055**	-0.067**
			(0.022)	(0.033)
			[-11.6%]	[-14.3%]
Third Trimester			-0.018	-0.025
			(0.019)	(0.025)
			[-3.8%]	[-5.2%]
First Trimester × Poor				-0.013
				(0.036)
				[-2.8%]
Second Trimester × Poor				0.027
				(0.043)
				[5.6%]
Third Trimester × Poor				0.013
				(0.036)
				[2.7%]
Observations	15,833	15,833	15,833	15,833
Mean Value of Unexposed	0.473	0.473	0.473	0.473

Notes: The table estimates the effects of in-utero shock on the educational attainment of exposed children. We regress the outcome variable—a dummy variable indicating that the individual completed the high school—on our exposure measures and exposure × poor interactions. Certainly unexposed individuals, who were conceived two months after Ramadan (month $\tau = -2$), are the omitted category; partially exposed individuals (individuals conceived one month after Ramadan; month $\tau = -1$) are excluded. The sample pools PSLM waves from 2004–2019. Numbers in square brackets express the treatment effect as a percentage of the average birth weight of the unexposed group. All specifications include non-parametric controls for mother's education, partner's education, and household wealth. Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

F Additional Results on Earnings

This section presents additional results and robustness tests on earnings. Some of these results are described in the main text and the rest are described below. All these results are referenced in the main text.

F.1 Robustness

Leveraging the richness of our administrative data, we implement a series of robustness tests to address potential identification and inference concerns regarding our earnings estimates. To contextualize these checks, Section IV.C established that parents do not time pregnancies to avoid or limit their children's exposure to Ramadan. Nor does parental composition differ significantly across the exposed and unexposed groups. This conclusion is reinforced by the built-in placebo tests embedded in our estimating equations: coefficients for certainly unexposed and partially exposed groups are consistently small and statistically indistinguishable from zero, indicating no systematic differences in earnings by Hijra conception timing in the absence of substantive exposure (see section V.C.1). In this section, we present results from additional robustness checks.

In our main results, we controlled for birth seasonality using the month of birth fixed effects. Our sample contains 66 birth cohorts so that we can experiment with finer seasonality controls. We can also experiment with more granular spatial controls. Tables F.I and F.II do this, showing that our results are not sensitive to these alternative specifications. Table F.III shows that the results are also robust to replacing the cohort fixed effects with flexible controls for age. The results in the last three columns, where we progressively add age and its higher-order terms into the model, are similar to ones with the cohort fixed effects. Finally, our baseline specification pools data for all three years (2007–2009), clustering standard errors at the individual level. Tables F.IV–F.IX show that similar results are obtained if we estimate our models on each year's data separately. We report these year-wise results for both our pregnancy month and trimester measures of exposure.

F.2 Exposure Intensity

Figure VI establishes that a majority of women in our sample fast during pregnancy. Figure H.III further demonstrates that the propensity to fast is strongly and positively correlated with self-reported religiosity. To the extent that the fasting rate is higher among more religious segments of the population, the effect size must also be higher. We, however, cannot test this directly as we do not observe religiosity in our tax data.

We therefore follow an alternative strategy to proxy for the religiosity of a family using the individual's given name. Specifically, we define a family as more religious if the individual's given name is *Muhammad*. We presume that religious families are more likely to choose a religious name for their children and mothers in these families are more likely to fast during pregnancy. Because the child's name is chosen at the time of birth, it captures religiosity of the family close to the event of interest—the pregnancy. Bifurcating our sample on the basis of this criterion, we estimate the effect size for the two groups separately.

It is important to emphasize that naming a child *Muhammad* carries specific doctrinal weight distinct from religious names in other traditions. Islamic texts and traditions actively encourage the use of the Prophet Muhammad's name. A well-known Hadith states: "Name yourselves by my name..." (Source: Sahih al-Bukhari 110; Sahih Muslim). This explicit religious endorsement ensures that the name *Muhammad* serves as a credible signal of familial religious observance rather than mere cultural preference.

The results, plotted in Figure F.I, are consistent with our a priori reasoning. The point estimates for the group with higher predicted compliance (the "*Muhammad*" subsample) are consistently larger in magnitude, although the difference between the two subgroups is not always statistically significant.[16] Since *Muhammad* is exclusively a male name, we restrict the sample for this analysis to males only. The positive relationship between exposure intensity and the effect size strengthens our causal story, linking the exposure during pregnancy to earnings in the later life.

[16]This finding is consistent with a similar result in Majid (2015) showing that the negative effect of in-utero Ramadan exposure is stronger for more religious families.

F.3 Heterogeneity

We explore heterogeneity in the treatment effect along three dimensions. First, the epigenetic mechanisms we discussed in Appendix B.1 reprogram the body so that it remains at its prime at least until the reproductive age. As a result, some adverse effects of the Predicted Adaptive Responses (PARs) are not expected to appear until late in life (Gluckman & Hanson (2004)). In our setup, this means that the negative effect of in-utero Ramadan exposure is likely to be worse among older cohorts, especially if health is an important channel through which the earnings effect mediates. Table F.X tests this hypothesis. We estimate an augmented version of equation (2), adding interactions of the Ramadan exposure dummies and an indicator that the individual belongs to an older cohort. Various columns of the table look at cohorts aged above 40 to above 65 using our preferred specification that includes all three types of birth fixed effects. The point estimates of the interaction terms are almost always negative, economically meaningful, and increase with age, but these differences are statistically indistinguishable from zero. We therefore cannot rule out that the negative earnings effect of Ramadan exposure is the same for both young and old cohorts.

The length and severity of the Ramadan fast vary across meteorological seasons. Pakistan is located around 2,000 miles north of the equator and, accordingly, its day length does not vary as much over the year as it does in other countries.[17] In contrast to day length, the variation in temperature across summer and winter months is unusually large in Pakistan, with temperature reaching 50°C (or 122°F) in some parts of the country in summer. Fasting during such extreme weather is likely to have more pronounced effects than during other months.[18] Table F.XII explores heterogeneity along this dimension. We indicate individuals whose in-utero exposure to Ramadan was in months May and June—the two hottest and driest months in the country—with the dummy variable *Ext Weather*. As expected, the coefficients on the interaction terms are negative and meaningful, but as earlier we cannot rule out if

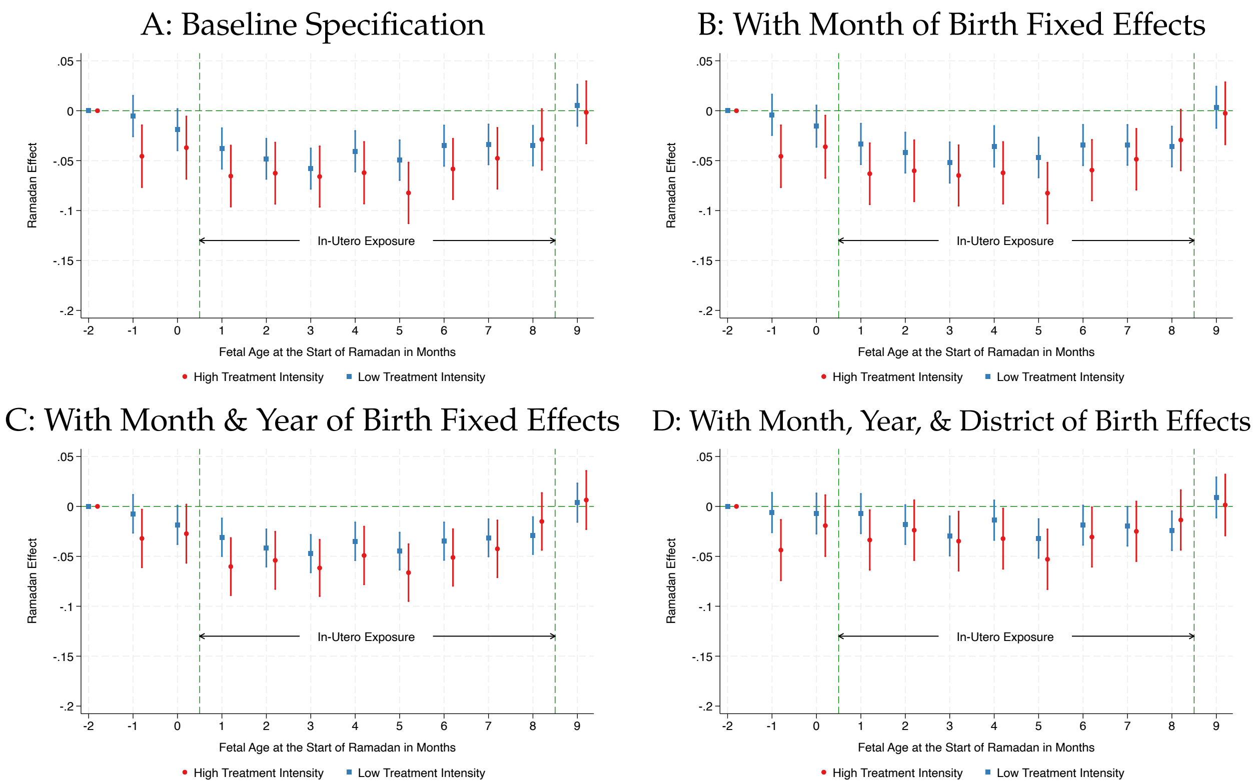
[17] Pakistan's latitude is 30.3753° N. The day length remains constant at 12 hours at a latitude of 0° and varies between 0 and 24 hours at a latitude of 80°. In Pakistan, the day length varies by around 3–4 hours over the year. For example, in 2021, the day length was 13 hours 41 minutes on the 21st of June and 10 hours 36 minutes on the 21st of December in Karachi (data from the website https://www.timeanddate.com/, retrieved on June 27, 2021).

[18] Note that Ramadan fasting involves abstaining from both food and water from sunrise to sunset. In hot and dry months, abstaining from water and other liquids becomes more important, causing dehydration and other related concerns.

they are indistinguishable from zero in our preferred specification.

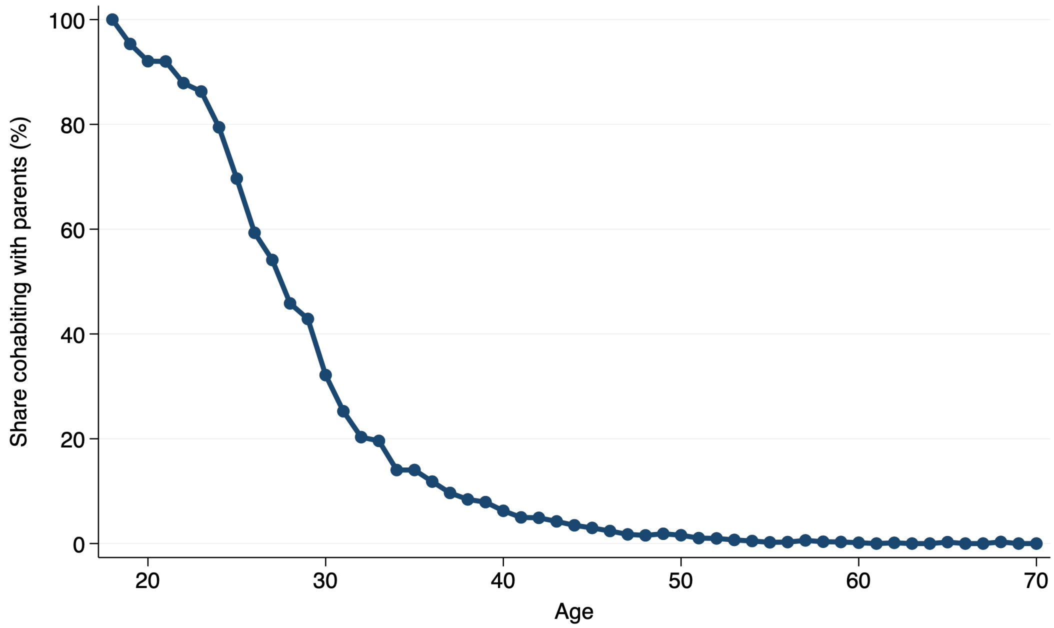
Parental investment, as we note earlier, can offset the negative effects of prenatal shocks. We do not observe parental income in our data and instead proxy for it using the place of birth of the individual. The dummy *Major City* in Table F.XIII indicates that the individual was born in one of the three richest cities of Pakistan—Karachi, Lahore, and Islamabad. To the extent that parental income is on average higher for this group of individuals, any differential effect could capture the role of parental investment. Clearly, incomes of these individuals are on average higher than others (see the results of the specifications where we do not control for the place of birth fixed effects) and the point estimates of the interaction terms are of the expected sign, but again these differences are statistically insignificant in more granular comparisons.

FIGURE F.I: **IMPACTS OF THE EXPOSURE ON EARNINGS BY TREATMENT INTENSITY**



Notes: The figure investigates if the effect size varies with the intensity of exposure. We divide our sample into two groups. The first group, which we call the high exposure intensity group, comprises individuals whose given name is Muhammad. We treat the name as a proxy for the religiousness of the family, arguing that mothers of these individuals are more likely to have fasted during pregnancy. The second group comprises all other individuals. Since Muhammad is a male name, we restrict the sample here to males only to make the two groups compatible. We estimate equation (2) separately for the two groups and plot the coefficients $\hat{\beta}_{\mu}$'s along with the 90% confidence interval around them from these regressions. We progressively introduce our three main sets of control: month of birth fixed effects in Panel B; month and year of birth fixed effects in Panel C; and month, year, and district of birth fixed effects in Panel D. We treat all English variants of the Urdu name Muhammad—Mohammad, Muhammed, and Mohammed—as the same.

FIGURE F.II: **SHARE OF CHILDREN COHABITING WITH PARENTS**



Notes: The figure plots, by age, the share of individuals classified as children of the household head who cohabit with their parents, using PSLM household roster data. "Child" is defined by the roster relationship to the head, so co-residence is observed even when the child is an adult. The series shows that cohabitation is nearly universal in the late teens, declines steeply through the 20s, and persists for a nontrivial fraction into the 30s and early 40s, before approaching zero at older ages.

TABLE F.I: **ALTERNATIVE BIRTH SEASONALITY CONTROLS**

Fetal Age (Months) at the Onset of Ramadan	Outcome: Log Earnings (1)	(2)	(3)	(4)	(5)
-1	-0.013	-0.013	-0.013	-0.011	-0.014
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
0	-0.027***	-0.025**	-0.024**	-0.023**	-0.027***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
1	-0.038***	-0.035***	-0.035***	-0.034***	-0.039***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
2	-0.047***	-0.043***	-0.043***	-0.042***	-0.047***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
3	-0.060***	-0.057***	-0.056***	-0.055***	-0.062***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
4	-0.045***	-0.042***	-0.042***	-0.041***	-0.045***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
5	-0.055***	-0.054***	-0.053***	-0.052***	-0.054***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
6	-0.039***	-0.040***	-0.039***	-0.038***	-0.039***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
7	-0.040***	-0.041***	-0.041***	-0.039***	-0.040***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
8	-0.032***	-0.034***	-0.033***	-0.033***	-0.032***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
9	-0.001	-0.002	-0.003	-0.001	-0.002
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Observations	830,107	830,107	830,107	830,107	830,107
Joint test, coefficients on months 1-9 equal 0:					
p-value	0.000	0.000	0.000	0.000	0.000
Fixed Effects:					
Quarter of Birth	-	✓	-	-	-
Month of Birth	-	-	✓	-	-
Week of Birth	-	-	-	✓	-
Day of Birth	-	-	-	-	✓

Notes*:* The table reports estimates from equation (2). The first column replicates the specification in the first column of Table IV. The rest of the columns introduce successively more granular birth seasonality controls. In each column, the outcome variable is the log of taxable income and as earlier we report coefficients on eleven Ramadan exposure dummies, omitting the reference category (month −2). Please see Figure I on how we define these exposure dummies. The sample here includes all three years 2007–2009. Standard errors are in parentheses and clustered at the individual level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE F.II: **ALTERNATIVE SPATIAL CONTROLS**

Fetal Age (Months) at the Onset of Ramadan	Outcome: Log Earnings (1)	(2)	(3)	(4)	(5)
-1	-0.013	-0.014	-0.011	-0.011	-0.008
	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)
0	-0.027***	-0.027***	-0.022**	-0.022**	-0.020**
	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)
1	-0.038***	-0.040***	-0.032***	-0.032***	-0.030***
	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
2	-0.047***	-0.051***	-0.042***	-0.042***	-0.040***
	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
3	-0.060***	-0.062***	-0.051***	-0.051***	-0.049***
	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
4	-0.045***	-0.046***	-0.037***	-0.037***	-0.035***
	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
5	-0.055***	-0.052***	-0.046***	-0.046***	-0.043***
	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
6	-0.039***	-0.040***	-0.035***	-0.036***	-0.034***
	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
7	-0.040***	-0.040***	-0.036***	-0.036***	-0.034***
	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
8	-0.032***	-0.029***	-0.023***	-0.023***	-0.021**
	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
9	-0.001	0.000	0.001	0.001	0.002
	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)
Observations	830,107	830,068	829,940	829,858	829,806
Joint test, coefficients on months 1-9 equal 0:					
p-value	0.000	0.000	0.000	0.000	0.000
Fixed Effects:					
Province of Birth	-	✓	-	-	-
District of Birth	-	-	✓	-	-
Tehsil of Birth	-	-	-	✓	-
UC of Birth	-	-	-	-	✓

Notes*:* The table reports estimates from equation (2). The first column replicates the specification in the first column of Table IV. The rest of the columns introduce successively more granular place of birth controls. In each column, the outcome variable is the log of taxable income and as earlier we report coefficients on eleven Ramadan exposure dummies, omitting the reference category (month -2). Please see Figure I on how we define these exposure dummies. The sample here includes all three years 2007–2009. Standard errors are in parentheses and clustered at the individual level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE F.III: **ALTERNATIVE AGE CONTROLS**

In-Utero Ramadan	Outcome: Log Earnings									
Exposure in	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dummy Specification:										
Exposure	-0.039***		-0.031***		-0.018***		-0.013*		-0.013*	
	(0.007)		(0.007)		(0.007)		(0.007)		(0.007)	
Trimester Specification:										
First Trimester		-0.044***		-0.035***		-0.020***		-0.007		-0.007
		(0.008)		(0.007)		(0.007)		(0.007)		(0.007)
Second Trimester		-0.047***		-0.039***		-0.022***		-0.019***		-0.020***
		(0.008)		(0.007)		(0.007)		(0.007)		(0.007)
Third Trimester		-0.025***		-0.020***		-0.012*		-0.014*		-0.014*
		(0.008)		(0.007)		(0.007)		(0.007)		(0.007)
Observations	762,035	762,035	761,876	761,876	761,876	761,876	761,876	761,876	761,876	761,876
Joint test, coefficients on trimesters 1-3 equal 0:										
p-value	0.000	0.000	0.000	0.000	0.007	0.010	0.053	0.016	0.050	0.013
Controls:										
Month of Birth	-	-	✓	✓	✓	✓	✓	✓	✓	✓
District of Birth	-	-	✓	✓	✓	✓	✓	✓	✓	✓
Age	-	-	-	-	✓	✓	✓	✓	✓	✓
Age^2	-	-	-	-	-	-	✓	✓	✓	✓
Age^3	-	-	-	-	-	-	-	-	✓	✓

Notes*:* The table reports estimates from equation (2). The first two columns replicate the specifications in the first two columns of Table V. The rest of the columns introduce age and its higher-order terms successively into the model. The outcome variable in each specification is the log of taxable income and as usual we report the coefficients on exposure dummies by successively adding more stringent controls for age. Individuals exposed to Ramadan in months 0–3 of their pregnancy are included in the First Trimester group, in months 4–6 in the Second Trimester group, and in months 7–9 in the Third Trimester group. Please see Figure I on how we define the gestation month of exposure. Certainly unexposed individuals, who were conceived two months after Ramadan (month -2), are the omitted category. Individuals grouped in months -1 are dropped. P-value of the hypothesis that the exposure coefficients are jointly zero is provided in the last row. The sample here includes all three years 2007–2009. Standard errors are in parentheses and clustered at the individual level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE F.IV: **IMPACTS OF THE EXPOSURE ON EARNINGS – 2007**

Fetal Age (Months) at the Onset of Ramadan	Outcome: Log Earnings (1)	(2)	(3)	(4)	(5)	(6)
-1	-0.010	-0.011	-0.008	-0.008	-0.015	-0.011
	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.012)
0	-0.023*	-0.024*	-0.022*	-0.022*	-0.018	-0.014
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)
1	-0.046***	-0.047***	-0.045***	-0.045***	-0.021	-0.020
	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.012)
2	-0.060***	-0.060***	-0.060***	-0.060***	-0.031**	-0.028**
	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.012)
3	-0.071***	-0.070***	-0.064***	-0.063***	-0.039***	-0.028**
	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.012)
4	-0.054***	-0.053***	-0.050***	-0.050***	-0.017	-0.011
	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.012)
5	-0.065***	-0.064***	-0.054***	-0.054***	-0.037***	-0.024**
	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.012)
6	-0.057***	-0.057***	-0.052***	-0.052***	-0.029**	-0.024**
	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.012)
7	-0.052***	-0.050***	-0.046***	-0.045***	-0.033***	-0.027**
	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.012)
8	-0.044***	-0.042***	-0.034***	-0.033***	-0.035***	-0.024**
	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.012)
9	-0.010	-0.010	-0.009	-0.009	-0.006	-0.005
	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.012)
Observations	165,737	165,737	165,703	165,703	165,737	165,703
Joint test, coefficients on months 1-9 equal 0:						
p-value	0.000	0.000	0.000	0.000	0.018	0.162
Fixed Effects:						
Month of Birth	-	✓	-	✓	-	✓
District of Birth	-	-	✓	✓	-	✓
Year of Birth	-	-	-	-	✓	✓

Notes*:* The table reports estimates from equation (2). The sample here includes tax returns filed in the tax year 2007 only. We regress the outcome variable—log of taxable income—on eleven Ramadan exposure dummies, omitting the reference category—certainly unexposed individuals, who were conceived two months after Ramadan (month -2). Please see Figure I on how we define these exposure dummies. Standard errors are in parentheses and clustered at the individual level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE F.V: **IMPACTS OF THE EXPOSURE ON EARNINGS – 2008**

Fetal Age (Months) at the Onset of Ramadan	Outcome: Log Earnings (1)	(2)	(3)	(4)	(5)	(6)
-1	-0.018	-0.017	-0.016	-0.016	-0.015	-0.012
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.010)
0	-0.030***	-0.027**	-0.023**	-0.022**	-0.014	-0.004
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.010)
1	-0.039***	-0.034***	-0.032***	-0.030***	-0.008	0.002
	(0.011)	(0.011)	(0.010)	(0.010)	(0.011)	(0.010)
2	-0.049***	-0.043***	-0.041***	-0.038***	-0.018*	-0.005
	(0.011)	(0.011)	(0.010)	(0.010)	(0.011)	(0.010)
3	-0.064***	-0.058***	-0.053***	-0.051***	-0.037***	-0.021**
	(0.011)	(0.011)	(0.010)	(0.010)	(0.011)	(0.010)
4	-0.047***	-0.043***	-0.037***	-0.036***	-0.022**	-0.009
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.010)
5	-0.059***	-0.057***	-0.052***	-0.053***	-0.040***	-0.033***
	(0.011)	(0.011)	(0.010)	(0.010)	(0.011)	(0.010)
6	-0.041***	-0.041***	-0.038***	-0.040***	-0.023**	-0.021**
	(0.011)	(0.011)	(0.010)	(0.011)	(0.011)	(0.010)
7	-0.042***	-0.043***	-0.038***	-0.039***	-0.027**	-0.024**
	(0.011)	(0.011)	(0.010)	(0.010)	(0.011)	(0.010)
8	-0.031***	-0.032***	-0.021**	-0.022**	-0.019*	-0.011
	(0.011)	(0.011)	(0.010)	(0.010)	(0.011)	(0.010)
9	-0.004	-0.007	-0.002	-0.003	0.001	0.001
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.010)
Observations	312,418	312,418	312,291	312,291	312,418	312,291
Joint test, coefficients on months 1-9 equal 0:						
p-value	0.000	0.000	0.000	0.000	0.000	0.002
Fixed Effects:						
Month of Birth	-	✓	-	✓	-	✓
District of Birth	-	-	✓	✓	-	✓
Year of Birth	-	-	-	-	✓	✓

Notes: The table reports estimates from equation (2). The sample here includes tax returns filed in the tax year 2008 only. We regress the outcome variable—log of taxable income—on eleven Ramadan exposure dummies, omitting the reference category—certainly unexposed individuals, who were conceived two months after Ramadan (month -2). Please see Figure I on how we define these exposure dummies. Standard errors are in parentheses and clustered at the individual level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE F.VI: **IMPACTS OF THE EXPOSURE ON EARNINGS – 2009**

Fetal Age (Months) at	Outcome: Log Earnings					
the Onset of Ramadan	(1)	(2)	(3)	(4)	(5)	(6)
-1	-0.014	-0.012	-0.010	-0.010	-0.010	-0.006
	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)
0	-0.029***	-0.025**	-0.022**	-0.021**	-0.010	-0.001
	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)
1	-0.040***	-0.035***	-0.031***	-0.028***	-0.010	0.001
	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)
2	-0.048***	-0.042***	-0.040***	-0.037***	-0.014	-0.003
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
3	-0.059***	-0.054***	-0.049***	-0.047***	-0.032***	-0.020**
	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)
4	-0.045***	-0.042***	-0.037***	-0.037***	-0.022**	-0.013
	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)
5	-0.049***	-0.048***	-0.039***	-0.041***	-0.033***	-0.024**
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
6	-0.037***	-0.037***	-0.031***	-0.032***	-0.021**	-0.017*
	(0.010)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)
7	-0.035***	-0.037***	-0.033***	-0.033***	-0.021**	-0.021**
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
8	-0.030***	-0.032***	-0.022**	-0.022**	-0.019*	-0.013
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
9	0.004	0.002	0.006	0.006	0.007	0.008
	(0.011)	(0.011)	(0.010)	(0.010)	(0.010)	(0.010)
Observations	351,952	351,952	351,748	351,748	351,952	351,748
Joint test, coefficients on months 1-9 equal 0:						
p-value	0.000	0.000	0.000	0.000	0.001	0.005
Fixed Effects:						
Month of Birth	-	✓	-	✓	-	✓
District of Birth	-	-	✓	✓	-	✓
Year of Birth	-	-	-	-	✓	✓

Notes: The table reports estimates from equation (2). The sample here includes tax returns filed in the tax year 2009 only. We regress the outcome variable—log of taxable income—on eleven Ramadan exposure dummies, omitting the reference category—certainly unexposed individuals, who were conceived two months after Ramadan (month -2). Please see Figure I on how we define these exposure dummies. Standard errors are in parentheses and clustered at the individual level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE F.VII: **IMPACTS OF THE EXPOSURE ON EARNINGS – 2007**

In-Utero Ramadan	Outcome: Log Earnings									
Exposure in	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dummy Specification:										
Exposure	-0.049***		-0.048***		-0.044***		-0.026***		-0.020**	
	(0.010)		(0.010)		(0.009)		(0.010)		(0.009)	
Trimester Specification:										
First Trimester		-0.050***		-0.051***		-0.048***		-0.027***		-0.022**
		(0.010)		(0.010)		(0.010)		(0.010)		(0.010)
Second Trimester		-0.059***		-0.058***		-0.052***		-0.027***		-0.019*
		(0.011)		(0.011)		(0.010)		(0.010)		(0.010)
Third Trimester		-0.036***		-0.035***		-0.030***		-0.025**		-0.019*
		(0.011)		(0.011)		(0.010)		(0.010)		(0.010)
Observations	152,028	152,028	152,028	152,028	151,997	151,997	152,028	152,028	151,997	151,997
Joint test, coefficients on trimesters 1-3 equal 0:										
p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.006	0.056	0.026	0.150
Fixed Effects:										
Month of Birth	-	-	✓	✓	-	-	-	-	✓	✓
District of Birth	-	-	-	-	✓	✓	-	-	✓	✓
Year of Birth	-	-	-	-	-	-	✓	✓	✓	✓

Notes*:* The table reports estimates from equation (2). The sample here includes tax returns filed in the tax year 2007 only. The odd-numbered columns report results from the dummy version of the specification and the even-numbered from the trimester version. Individuals exposed to Ramadan in months 0–3 of their pregnancy are included in the First Trimester group, in months 4–6 in the Second Trimester group, and in months 7–9 in the Third Trimester group. Please see Figure I on how we define the gestation month of exposure. Certainly unexposed individuals, who were conceived two months after Ramadan (month -2), are the omitted category. Individuals grouped in months -1 are dropped. P-value of the hypothesis that the exposure coefficients are jointly zero is provided in the last row. Standard errors are in parentheses and are clustered at the individual level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table F.VIII: **Impacts of the Exposure on Earnings – 2008**

In-Utero Ramadan	Outcome: Log Earnings									
Exposure in	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dummy Specification:										
Exposure	-0.041***		-0.038***		-0.034***		-0.021**		-0.012	
	(0.008)		(0.008)		(0.008)		(0.008)		(0.008)	
Trimester Specification:										
First Trimester		-0.046***		-0.040***		-0.037***		-0.019**		-0.007
		(0.009)		(0.009)		(0.008)		(0.009)		(0.008)
Second Trimester		-0.049***		-0.046***		-0.042***		-0.028***		-0.021**
		(0.009)		(0.009)		(0.009)		(0.009)		(0.008)
Third Trimester		-0.026***		-0.027***		-0.021**		-0.015*		-0.011
		(0.009)		(0.009)		(0.009)		(0.009)		(0.008)
Observations	286,808	286,808	286,808	286,808	286,685	286,685	286,808	286,808	286,685	286,685
Joint test, coefficients on trimesters 1-3 equal 0:										
p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.012	0.012	0.106	0.031
Fixed Effects:										
Month of Birth	-	-	✓	✓	-	-	-	-	✓	✓
District of Birth	-	-	-	-	✓	✓	-	-	✓	✓
Year of Birth	-	-	-	-	-	-	✓	✓	✓	✓

Notes*:* The table reports estimates from equation (2). The sample here includes tax returns filed in the tax year 2008 only. The odd-numbered columns report results from the dummy version of the specification and the even-numbered from the trimester version. Individuals exposed to Ramadan in months 0–3 of their pregnancy are included in the First Trimester group, in months 4–6 in the Second Trimester group, and in months 7–9 in the Third Trimester group. Please see Figure I on how we define the gestation month of exposure. Certainly unexposed individuals, who were conceived two months after Ramadan (month -2), are the omitted category. Individuals grouped in months -1 are dropped. P-value of the hypothesis that the exposure coefficients are jointly zero is provided in the last row. Standard errors are in parentheses and are clustered at the individual level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE F.IX: **IMPACTS OF THE EXPOSURE ON EARNINGS – 2009**

In-Utero Ramadan	Outcome: Log Earnings									
Exposure in	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dummy Specification:										
Exposure	-0.037***		-0.034***		-0.030***		-0.018**		-0.010	
	(0.008)		(0.008)		(0.007)		(0.008)		(0.007)	
Trimester Specification:										
First Trimester		-0.044***		-0.038***		-0.035***		-0.017**		-0.005
		(0.008)		(0.008)		(0.008)		(0.008)		(0.008)
Second Trimester		-0.044***		-0.041***		-0.035***		-0.025***		-0.018**
		(0.009)		(0.009)		(0.008)		(0.008)		(0.008)
Third Trimester		-0.021**		-0.022**		-0.017**		-0.011		-0.009
		(0.009)		(0.009)		(0.008)		(0.008)		(0.008)
Observations	323,100	323,100	323,100	323,100	322,909	322,909	323,100	323,100	322,909	322,909
Joint test, coefficients on trimesters 1-3 equal 0:										
p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.022	0.013	0.160	0.044
Fixed Effects:										
Month of Birth	-	-	✓	✓	-	-	-	-	✓	✓
District of Birth	-	-	-	-	✓	✓	-	-	✓	✓
Year of Birth	-	-	-	-	-	-	✓	✓	✓	✓

Notes: The table reports estimates from equation (2). The sample here includes tax returns filed in the tax year 2009 only. The odd-numbered columns report results from the dummy version of the specification and the even-numbered from the trimester version. Individuals exposed to Ramadan in months 0–3 of their pregnancy are included in the First Trimester group, in months 4–6 in the Second Trimester group, and in months 7–9 in the Third Trimester group. Please see Figure I on how we define the gestation month of exposure. Certainly unexposed individuals, who were conceived two months after Ramadan (month -2), are the omitted category. Individuals grouped in months -1 are dropped. P-value of the hypothesis that the exposure coefficients are jointly zero is provided in the last row. Standard errors are in parentheses and are clustered at the individual level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE F.X: **HETEROGENEITY IN TREATMENT EFFECT – OLD VS. YOUNG COHORTS**

In-Utero Ramadan	Old Cohort: Age >					
Exposure in	40	45	50	55	60	65
	(1)	(2)	(3)	(4)	(5)	(6)
Exposed	-0.005	-0.009	-0.011	-0.011	-0.011	-0.011*
	(0.009)	(0.008)	(0.007)	(0.007)	(0.007)	(0.007)
Exposed × Trait	-0.014	-0.009	-0.007	-0.019	-0.052	-0.070
	(0.012)	(0.012)	(0.015)	(0.021)	(0.034)	(0.049)
Observations	761,876	761,876	761,876	761,876	761,876	761,876
Joint test, coefficients on three Trimester × Old Cohort equal 0:						
p-value	0.258	0.453	0.608	0.369	0.124	0.149
Fixed Effects:						
Month of Birth	✓	✓	✓	✓	✓	✓
District of Birth	✓	✓	✓	✓	✓	✓
Year of Birth	✓	✓	✓	✓	✓	✓

Notes: The table reports estimates from an augmented version of our dummy specification corresponding to equation (2), where we add a double interaction term interacting the exposure dummy with the dummy variable Old Cohort, which takes the value 1 if the age of individual i on July 1, 2007 exceeds the cutoff indicated in the heading of each column. Certainly unexposed individuals, who were conceived two months after Ramadan (month -2), are the omitted category. Individuals grouped in months -1 are dropped, and the rest of individuals are defined as Exposed. The sample here includes tax returns filed in the tax years 2007–2009. The outcome variable is the log of taxable income of the individual, and all specifications include the full set of month, district and year of birth fixed effects. Standard errors are in parentheses and clustered at the individual level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE F.XI: **HETEROGENEITY IN TREATMENT EFFECT – OLD VS. YOUNG COHORTS**

In-Utero Ramadan	Old Cohort: Age >					
Exposure in	40	45	50	55	60	65
	(1)	(2)	(3)	(4)	(5)	(6)
1st Trimester	0.001	-0.003	-0.005	-0.006	-0.007	-0.008
	(0.009)	(0.008)	(0.008)	(0.007)	(0.007)	(0.007)
2nd Trimester	-0.021	-0.018	-0.019	-0.035	-0.060*	-0.060
	(0.013)	(0.014)	(0.016)	(0.022)	(0.035)	(0.052)
3rd Trimester	-0.017*	-0.018**	-0.021***	-0.020***	-0.017**	-0.018**
	(0.010)	(0.009)	(0.008)	(0.007)	(0.007)	(0.007)
1st Trimester × Trait	-0.002	0.000	0.010	0.008	-0.041	-0.058
	(0.014)	(0.014)	(0.017)	(0.025)	(0.039)	(0.053)
2nd Trimester × Trait	-0.006	-0.009	-0.009	-0.010	-0.009	-0.009
	(0.010)	(0.009)	(0.008)	(0.007)	(0.007)	(0.007)
3rd Trimester × Trait	-0.009	-0.005	-0.010	-0.017	-0.052	-0.118**
	(0.014)	(0.014)	(0.016)	(0.024)	(0.040)	(0.057)
Observations	761,876	761,876	761,876	761,876	761,876	761,876
Joint test, coefficients on three Trimester × Old Cohort equal 0:						
p-value	0.200	0.342	0.125	0.077	0.385	0.182
Fixed Effects:						
Month of Birth	✓	✓	✓	✓	✓	✓
District of Birth	✓	✓	✓	✓	✓	✓
Year of Birth	✓	✓	✓	✓	✓	✓

Notes: The table reports estimates from an augmented version of our trimester specification corresponding to equation (2), where we add double interaction terms interacting each exposure trimester dummy with the dummy variable Old Cohort, which takes the value 1 if the age of individual i on July 1, 2007 exceeds the cutoff indicated in the heading of each column. Individuals exposed to Ramadan in months 0–3 of their pregnancy are included in the First Trimester group, in months 4–6 in the Second Trimester group, and in months 7–9 in the Third Trimester group. Please see Figure I on how we define the gestation month of exposure. Certainly unexposed individuals, who were conceived two months after Ramadan (month -2), are the omitted category. Individuals grouped in months -1 are dropped. The sample here includes tax returns filed in the tax years 2007–2009. The outcome variable is the log of taxable income of the individual, and all specifications include the full set of month, district and year of birth fixed effects. Standard errors are in parentheses and clustered at the individual level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE F.XII: **HETEROGENEITY IN TREATMENT EFFECT – WEATHER**

In-Utero Ramadan	Outcome: Log Earnings					
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy Specification:						
Exposure	-0.028***		-0.022***		-0.012*	
	(0.007)		(0.007)		(0.007)	
Exposure × Trait	-0.051***		-0.055***		-0.006	
	(0.006)		(0.005)		(0.013)	
Trimester Specification:						
1st Trimester		-0.037***		-0.033***		-0.010
		(0.008)		(0.007)		(0.007)
2nd Trimester		-0.005		-0.006		0.000
		(0.009)		(0.009)		(0.014)
3rd Trimester		-0.032***		-0.026***		-0.016**
		(0.008)		(0.008)		(0.008)
1st Trimester × Trait		-0.068***		-0.073***		-0.015
		(0.011)		(0.011)		(0.015)
2nd Trimester × Trait		-0.009		-0.002		-0.010
		(0.008)		(0.008)		(0.008)
3rd Trimester × Trait		-0.104***		-0.109***		-0.006
		(0.012)		(0.011)		(0.015)
Observations	762,035	762,035	761,876	761,876	761,876	761,876
Joint test, coefficients on three Trimester × Ext Weather equal 0:						
p-value	0.000	0.000	0.000	0.000	0.618	0.677
Fixed Effects:						
Month of Birth	✓	✓	✓	✓	✓	✓
District of Birth	-	-	✓	✓	✓	✓
Year of Birth	-	-	-	-	✓	✓

Notes*:* The table reports estimates from an augmented version of equation (2), where we add double interaction terms interacting the exposure dummies with the dummy variable Trait. The dummy variable takes the value 1 if individual i was exposed to Ramadan while in-utero in the months May and June. These two months are the harshest months in Pakistan in terms of weather. Temperature during these two months is at its peak, reaching the level of 50 degree centigrade (or 122°F) on some days. The sample here includes tax returns filed in the tax years 2007–2009. The outcome variable is the log of taxable income of the individual. Standard errors are in parentheses and clustered at the individual level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE F.XIII: **HETEROGENEITY IN TREATMENT EFFECT – LOCATION**

In-Utero Ramadan	Outcome: Log Earnings					
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy Specification:						
Exposure	-0.035***		-0.031***		-0.013*	
	(0.007)		(0.007)		(0.007)	
Exposure × Trait	-0.165***		-0.015		0.002	
	(0.018)		(0.059)		(0.056)	
Trimester Specification:						
1st Trimester		-0.037***		-0.034***		-0.010
		(0.008)		(0.007)		(0.007)
2nd Trimester		-0.159***		-0.011		0.017
		(0.028)		(0.062)		(0.059)
3rd Trimester		-0.042***		-0.038***		-0.019**
		(0.008)		(0.008)		(0.007)
1st Trimester × Trait		-0.183***		-0.030		-0.022
		(0.035)		(0.065)		(0.063)
2nd Trimester × Trait		-0.024***		-0.020***		-0.011
		(0.008)		(0.008)		(0.007)
3rd Trimester × Trait		-0.154***		-0.007		0.004
		(0.033)		(0.065)		(0.062)
Observations	762,035	762,035	761,876	761,876	761,876	761,876
Joint test, coefficients on three Trimester × Major City equal 0:						
p-value	0.000	0.000	0.793	0.950	0.972	0.824
Fixed Effects:						
Month of Birth	✓	✓	✓	✓	✓	✓
District of Birth	-	-	✓	✓	✓	✓
Year of Birth	-	-	-	-	✓	✓

Notes: The table reports estimates from an augmented version of equation (2), where we add double interaction terms interacting exposure dummies with the dummy variable Trait. The dummy variable takes the value 1 if the district of birth of individual i is one of the three major cities of Pakistan in terms of per-capita income—Karachi, Lahore, and Islamabad. The sample here includes tax returns filed in the tax years 2007–2009. The outcome variable is the log of taxable income of the individual. Standard errors are in parentheses and clustered at the individual level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table F.XIV: **Impacts of the Exposure on Income Inequality**

In-Utero Exposure in	Income >											
	Median				75th Percentile				90th Percentile			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dummy Specification:												
Exposure	-0.024***		-0.010***		-0.011***		-0.003		-0.004*		-0.001	
	(0.003)		(0.003)		(0.003)		(0.003)		(0.002)		(0.002)	
Trimester Specification:												
First Trimester		-0.026***		-0.010***		-0.009***		0.001		-0.004		0.001
		(0.003)		(0.003)		(0.003)		(0.003)		(0.002)		(0.002)
Second Trimester		-0.029***		-0.013***		-0.015***		-0.007**		-0.006**		-0.002
		(0.004)		(0.003)		(0.003)		(0.003)		(0.002)		(0.002)
Third Trimester		-0.015***		-0.008**		-0.008**		-0.003		-0.004		-0.002
		(0.004)		(0.003)		(0.003)		(0.003)		(0.002)		(0.002)
Observations	762,035	762,035	761,876	761,876	762,035	762,035	761,876	761,876	762,035	762,035	761,876	761,876
Joint test, coefficients on trimesters 1-3 equal 0:												
p-value	0.000	0.000	0.001	0.001	0.001	0.000	0.382	0.002	0.050	0.126	0.757	0.064
Fixed Effects:												
Month of Birth	-	-	✓	✓	-	-	✓	✓	-	-	✓	✓
District of Birth	-	-	✓	✓	-	-	✓	✓	-	-	✓	✓
Year of Birth	-	-	✓	✓	-	-	✓	✓	-	-	✓	✓

Notes: The table reports estimates from equation (2). The outcome variable for each column is a dummy indicating that the individual earns more than the threshold given in the heading of the column. The odd-numbered columns report results from the dummy version of the specification and the even-numbered from the trimester version. Individuals exposed to Ramadan in months 0–3 of their pregnancy are included in the First Trimester group, in months 4–6 in the Second Trimester group, and in months 7–9 in the Third Trimester group. Please see Figure I on how we define the gestation month of exposure. Certainly unexposed individuals, who were conceived two months after Ramadan (month -2), are the omitted category. Individuals grouped in months -1 are dropped. P-value of the hypothesis that the exposure coefficients are jointly zero is provided in the last row. The sample here includes tax returns filed in the tax years 2007-2009. Standard errors are in parentheses and are clustered at the individual level. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE F.XV: **IMPACTS OF THE EXPOSURE ON EARNINGS**

In-Utero Ramadan	Outcome: Log Income					
Exposure in	(1)	(2)	(3)	(4)	(5)	(6)
Dummy Specification:						
Exposed	0.087		0.076		0.034	
	(0.066)		(0.065)		(0.058)	
Exposed × Poor	-0.070		-0.071		-0.014	
	(0.088)		(0.088)		(0.084)	
Trimester Specification:						
First Trimester		0.133*		0.111		0.049
		(0.076)		(0.077)		(0.066)
Second Trimester		0.076		0.052		0.026
		(0.070)		(0.071)		(0.065)
Third Trimester		0.038		0.054		0.017
		(0.069)		(0.069)		(0.060)
First Trimester × Poor		-0.175*		-0.170*		-0.093
		(0.098)		(0.097)		(0.090)
Second Trimester × Poor		-0.022		-0.026		-0.018
		(0.098)		(0.099)		(0.096)
Third Trimester × Poor		0.031		0.020		0.097
		(0.093)		(0.093)		(0.085)
Observations	9,693	9,693	9,693	9,693	9,693	9,693
Joint test, coefficients on trimesters 1-3 equal 0:						
p-value	0.185	0.256	0.245	0.530	0.551	0.886
Joint test, coefficients on trimester 1-3 × poor interactions equal 0:						
p-value	0.427	0.019	0.416	0.028	0.870	0.017
Fixed Effects:						
District of Birth	-	-	-	-	✓	✓
Month of Birth	-	-	✓	✓	✓	✓
Year of Birth	✓	✓	✓	✓	✓	✓

Notes*:* The table reports estimates from equation (2). The odd-numbered columns report results from the dummy version of the specification and the even-numbered from the trimester version. Each specification includes the exposure × poverty interaction. The sample consists of individuals classified as the children of household head in the PSLM data, where we pool waves from 2004–2019. To align this sample with the administrative data, we restrict attention to the same 66 birth cohorts (1924–1989). Individuals exposed to Ramadan in months 0–3 of their pregnancy are included in the First Trimester group, in months 4–6 in the Second Trimester group, and in months 7–9 in the Third Trimester group. Please see Figure I on how we define the gestation month of exposure. Certainly unexposed individuals, who were conceived two months after Ramadan (month $\tau = -2$), are the omitted category. Partially-exposed individuals conceived in months $\tau = -1$ are excluded. The last row reports the p-value from an F-test of the null hypothesis that the exposure coefficients are jointly zero. Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

G Additional Results on Intergenerational Transmission

This section presents additional results on intergenerational transmission of shocks. These results are described and referenced in the main text.

TABLE G.I: **IMPACTS OF THE EXPOSURE ON EARNINGS OF NEXT GENERATION**

In-Utero Ramadan	Outcome: Log Income of Children							
Exposure in	Father is Exposed				Mother is Exposed			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dummy Specification:								
Exposed	-0.162**				-0.150**			
	(0.081)				(0.070)			
Trimester Specification:								
First Trimester		-0.063	-0.065	-0.053		-0.005	-0.004	-0.039
		(0.113)	(0.114)	(0.081)		(0.093)	(0.092)	(0.081)
Second Trimester		-0.094	-0.100	-0.080		-0.200**	-0.199**	-0.170**
		(0.120)	(0.120)	(0.083)		(0.083)	(0.083)	(0.079)
Third Trimester		-0.194*	-0.201*	-0.205**		-0.120	-0.122	-0.170**
		(0.117)	(0.117)	(0.083)		(0.074)	(0.074)	(0.071)
Observations	10,323	10,323	10,323	10,323	10,504	10,504	10,504	10,504
Joint test, coefficients on trimesters 1-3 equal 0:								
p-value		0.007	0.004	0.000		0.016	0.018	0.013
Fixed Effects:								
District of Birth	✓	-	-	✓	✓	-	-	✓
Month of Birth	✓	-	✓	✓	✓	-	✓	✓
Year of Birth	✓	✓	✓	✓	✓	✓	✓	✓

Notes*:* The table reports estimates from equation (2). Columns (1) and (5) report results from the dummy version of the specification, whereas the rest of the columns report results from the trimester version of the specification. The sample consists of individuals classified as the children of household head in the PSLM data, where we pool waves from 2004–2019. In Columns (1)-(4), the exposure indicator equals one if the individual's father was exposed in utero to Ramadan during any gestational month in odd-numbered columns and during the first, second, or third trimester in even-numbered columns. Columns (5)-(8) report the parallel estimates in which exposure is defined analogously using the mother's in-utero exposure. The outcome variable is the log income of children. To align this sample with the administrative data, we restrict attention to the same 66 birth cohorts (1924–1989). Parents exposed to Ramadan in months 0–3 of their pregnancy are included in the First Trimester group, in months 4–6 in the Second Trimester group, and in months 7–9 in the Third Trimester group. Please see Figure I on how we define the gestation month of exposure. Certainly unexposed parents, who were conceived two months after Ramadan (month $\tau = -2$), are the omitted category. Partially-exposed Parents conceived in months $\tau = -1$ are excluded. The last row reports the p-value from an F-test of the null hypothesis that the exposure coefficients are jointly zero. Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE G.II: **ASSORTATIVE MATING**

In-Utero Ramadan	Outcome: $\mathbb{1}$(Husband is Exposed)					
Exposure of Wife in	(1)	(2)	(3)	(4)	(5)	(6)
Dummy Specification:						
Exposed	0.011		0.008		0.006	
	(0.009)		(0.009)		(0.009)	
Trimester Specification:						
First Trimester		0.013		0.013		0.013
		(0.011)		(0.011)		(0.010)
Second Trimester		0.022*		0.021*		0.021*
		(0.012)		(0.012)		(0.012)
Third Trimester		0.000		0.004		0.004
		(0.009)		(0.010)		(0.010)
Observations	44,637	44,637	44,637	44,637	44,637	44,637
Joint test, coefficients on trimesters 1-3 equal 0:						
p-value	0.183	0.017	0.345	0.093	0.489	0.115
Fixed Effects:						
District of Birth	-	-	-	-	✓	✓
Month of Birth	-	-	✓	✓	✓	✓
Year of Birth	✓	✓	✓	✓	✓	✓

Notes: The table explores assortative mating in the marriage market. We report estimates from equation (2). The odd-numbered columns report results from the dummy version of the specification, whereas the even-numbered columns from the trimester version of the specification. The sample consists of individuals classified as the head of household in the PSLM data, where we pool waves from 2004–2019. In odd-numbered columns, the exposure indicator equals one if the individual's wife was exposed in utero to Ramadan during any gestational month and in odd-numbered columns during the first, second, or third trimester. The outcome variables are dummies indicating that the husband was exposed to Ramadan in any gestation month. To align this sample with the administrative data, we restrict attention to the same 66 birth cohorts (1924–1989). Wives exposed to Ramadan in months 0–3 of their pregnancy are included in the First Trimester group, in months 4–6 in the Second Trimester group, and in months 7–9 in the Third Trimester group. Please see Figure I on how we define the gestation month of exposure. Certainly unexposed wives, who were conceived two months after Ramadan (month $\tau = -2$), are the omitted category; partially-exposed wives conceived in months $\tau = -1$ are dropped. The last row reports the p-value from an F-test of the null hypothesis that the exposure coefficients are jointly zero. Standard errors are in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

H Additional Results on TS2SLS Estimates

This section presents additional results on our TS2SLS estimates. Some of these results are described and referenced in the main text.

H.1 Household Survey: Protocol and Design

We designed the household survey to estimate the first stage of our empirical framework. The primary objective of this survey was to elicit detailed retrospective data on prenatal fasting habits during Ramadan—variables that are not observed in administrative data. Following Stantcheva (2023), we designed the instrument as an integral part of the identification strategy, allowing us to recover treatment-on-the-treated parameters from our reduced-form estimates.

H.1.1 Sampling Frame and Target Population

The survey was conducted in Lahore, the second-largest city of Pakistan. We limited the sampling frame to tax filers who submitted at least one return between 2015 and 2020 and whose registered address was in Lahore. From this administratively defined population, we drew a simple random sample of households and interviewed ever-pregnant women residing in the sampled households. The probability sampling method we used ensured that every eligible household had a known and equal chance of selection. Table H.III confirms the efficacy of this design. Comparing the survey sample against the full administrative frame, we find no statistically significant differences across key observables, including age, gender, and tax-reported earnings. A Hotelling's T^2 test fails to reject the null hypothesis of joint equality ($p = 0.68$), indicating that our sample is statistically indistinguishable from the population. This balance was crucial for producing unbiased estimates and for justifying the generalizability of our findings to the target population of Lahore-based tax filers. The sampling choice also linked the survey to our administrative earnings data and minimized coverage error relative to convenience panels. In line with best practices, we pre-tested the instrument, soft-launched the full protocol to diagnose technical issues, and monitored field progress in real time (drop-off points, completeness, and quota balance) before scaling up (Stantcheva (2023)).

H.1.2 Respondent Recruitment and Unit of Analysis

Within each randomly selected household, the unit of analysis was the ever-pregnant women. These respondents were interviewed to collect detailed retrospective data on their complete pregnancy histories. To limit non-response and to maximize the effective sample size, a systematic protocol was established for households where the originally selected unit was non-responsive. In such cases, the survey team was instructed to survey a next-door neighbor. To prevent the introduction of a systematic directional bias—for instance, if the team always surveyed the neighbor to the left, who might share specific, unobserved characteristics—the protocol mandated randomized replacement selection. The team alternated between the immediate neighbor to the left and the immediate neighbor to the right for each subsequent instance of non-response. This procedural randomization introduced a stochastic element that helped preserve the representativeness of the final sample against potential unobserved correlations at the neighborhood level.

H.1.3 Survey Instrument and Data Elicitation

The survey instrument was designed to minimize measurement error and respondent fatigue, adhering to best practices in survey question design. The landing script emphasized survey length, anonymity, contact details, and data use, but deliberately provided only high-level study information to avoid priming or discouraging participation based on perceived stance; visual design was mobile-friendly and concise. The instrument began with light verification. We kept such checks sparse to avoid attrition, and we flagged improbable response patterns ex post for review. The core of the questionnaire involved collecting detailed information from respondents regarding their fasting habits during Ramadan for each pregnancy. The questions were crafted to be precise and clear, avoiding vague quantifiers and instead asking about specific, countable behaviors. The questionnaire was structured retrospectively to guide respondents chronologically through their pregnancy history, a technique that aids memory recall and improves data accuracy.

Prior to full-scale deployment, the survey underwent extensive soft launches and cognitive interviews. This pre-testing phase was invaluable for identifying ambiguous questions, technical issues with the survey flow, and for assessing the feasibility of reliably recalling the required retrospective data. All consent materials and recruitment artifacts were archived; IRB requirements on confidentiality and data protection were followed.

H.2 Social Desirability Bias

Social desirability bias occurs when respondents misreport their behaviors or attitudes to align with perceived social norms. This bias can lead to over-reporting of socially approved behaviors and under-reporting of stigmatized ones (Bertrand & Mullainathan (2001)). We used neutral, non-judgmental wording for our questions and took two further steps to mitigate the bias in our survey. First, because the bias arises from the pressure to conform to social norms, we ensured that enumerators did not observe respondents' answers to sensitive questions. Our survey protocol required that respondents completed these questions on their own without the enumerator either listening or seeing their answers. Furthermore, because people are more likely to give truthful answers if they feel their identity is protected, our protocol assured respondents of the anonymity and confidentiality of their answers. Second, our survey embedded a list experiment to estimate the prevalence of the bias among our respondents. List experiments, also called item-count technique, are a common technique to assess and mitigate the bias by hiding the sensitive item among a list of innocuous items (Blair & Imai (2012)).

Figure H.I shows the results from our list experiment. Respondents were randomly assigned to either a treatment or a control group and were presented with a list of innocuous, neutral statements (see Table H.I for a list of these statements). The only difference between the two groups was that the treatment group received an additional *sensitive* statement concerning whether pregnant women should observe the Ramadan fast. Respondents in both groups were asked to report the *number* of statements they agreed with, without disclosing their individual responses. The difference in mean counts between the two groups provides an estimate of agreement with the sensitive statement that is uncontaminated by the bias. The difference in mean counts, mean(B) − mean(A) = 0.96 (s.e. = 0.01; N = 2,134) closely matches the directly asked belief question (see Figure H.II which shows that the self-reported and bias-corrected responses are statistically indistinguishable from each other). This comparison suggests that social pressure to over-report fasting is likely minimal in our setting. Despite this finding, all survey results in this section have been corrected for the small social desirability bias suggested by our list experiment.

H.3 Misperceived Social Norms

Economic theory highlights the role of misperceived social norms in driving seemingly suboptimal decisions.[19] Islamic doctrine typically exempts pregnant women from fasting, yet in practice many continue to fast during Ramadan, possibly due to perceived social or religious pressure. One key element of our survey is to measure whether this pressure is a strong driver of the fasting behavior we observe.

To test this, our survey asks whether respondents agree with the statement: "Pregnant women should observe the Ramadan fast" (first-order belief). We then incentivize them to estimate the proportion of other respondents who would agree with the statement (second-order belief). Figure H.II displays the results. More than 90% of women privately agree with the statement; this proportion remains stable even when corrected for social desirability bias using a list experiment. Crucially, however, respondents systematically underestimate the strength of the social norm. On average, they predict that less than 50% of other women agree with the statement. Table H.II examines whether this wedge between private views and perceived norms correlates with household income. We regress standardized respondent answers on standardized household income. We find no significant correlation, suggesting that these beliefs are structurally ingrained rather than driven by socioeconomic status.

These findings have important policy implications. The data reveal a pattern of "reverse" pluralistic ignorance: women are not fasting to conform to a perceived oppressive norm; rather, they fast due to strong intrinsic convictions, even while believing society is more lenient than it actually is. This implies that fasting behavior is driven by individual preferences rather than societal pressure. Consequently, effective policy should prioritize information interventions that target private beliefs directly—such as clarifying medical guidelines and highlighting permissible religious exemptions—rather than campaigns focused on correcting misperceived social norms.

[19] Bernheim (1994), for example, develops a model of conformity in which individuals trade off intrinsic utility against social status, mimicking what they perceive to be socially accepted behavior. Similarly, Acemoglu & Jackson (2014) present a dynamic model where erroneous expectations cause norms to persist, whereas a credible shock to beliefs can trigger rapid unraveling. Empirical evidence of misperceived norms driving suboptimal behavior exists in contexts ranging from female labor force participation to health seeking behavior (Bursztyn *et al.* (2020)).

H.4 Heterogeneity

Figure VI presented our headline result on average exposure implied by the household survey. In this section, we explore heterogeneity in exposure across key household characteristics. It is important to emphasize that any heterogeneity in average exposure is driven entirely by the fasting rate, as the other element of the average exposure—probability of being in-utero—is the same across compared groups (see Section V.F for details). Figure H.III presents these results. We begin with household income (Panel A). Our survey did not ask households to report their income, but we can merge the survey data with the tax data and thus can observe the household income reported on the tax return. Splitting the sample at the median, we find no systematic difference in exposure; both high- and low-income households within our sample exhibit nearly identical likelihood of exposure to the shock.

Next, we explore heterogeneity by family size (Panel B). To hold fertility stable across comparison groups, we restrict the sample to households whose youngest child was born at least five years before the survey. We use the number of children as a proxy for the SES of the household, given the established inverse relationship between SES and fertility in developing countries.[20] Consistent with the income analysis, we find no discernible differences in exposure along this dimension.

Maternal age at the time of pregnancy is another potential determinant of fasting behavior. We do not directly observe maternal age at the time of pregnancy and proxy it instead using the child's birth order. Retaining the sample restriction that the youngest child was born at least five years before the survey, our analysis (Panel C) reveals no significant heterogeneity in exposure by birth order.

Finally, we examine the association between exposure and maternal religiosity. Our survey asks respondents to classify themselves into one of four categories: (i) very religious, (ii) religious, (iii) somewhat religious, and (iv) not religious. For this analysis, we combine the latter two categories into a single "somewhat religious" group (only 0.4% mothers report themselves as not religious; see Figure H.VIII). The results show substantial variation in exposure by religiosity, with mothers identifying as "very religious" exhibiting substantially higher exposure (fasting rate) than the other groups. This finding underscores the importance of religious obligation as a primary driver of Ramadan fasting practices. We also assess whether religiosity

[20]In developing countries, fertility in general declines with household wealth. In the DHS (2017-2018) data, for example, total fertility rate in Pakistan is 4.5 for the poorest quintile and 2.8 for the richest quintile.

correlates systematically with income. Dividing households into income deciles and employing the non-parametric procedure of Cattaneo *et al.* (2024), we find no clear relationship between household income and reported religiosity (see Figure H.IV); households in the ninth income decile, for example, display religiosity levels comparable to those in the first decile.

Overall, the key implication of this heterogeneity analysis is that maternal religiosity appears to be the principal determinant of fasting rates and intensity within our sample. It is important to emphasize that this strong first-stage effect of religiosity on exposure does not pose an identification concern in our setting but rather suggests that the estimated average treatment effect of the in-utero shock is likely to be more pronounced among the religious sub-population. Consistent with this interpretation, Figure H.V shows that the Hijra birth rate does not vary with either religiosity or income, ruling out selection into treatment along these dimensions.

H.5 First-Stage Mechanisms

Section II details the biological and economic mechanisms that link maternal fasting to lower human-capital outcomes of children. Here, we present evidence that survey respondents directly experienced the physiological precursors to these mechanisms. Specifically, respondents were asked to report symptoms including stress, sleep deprivation, exhaustion, weakness, and under-nourishment during prenatal fasting. In terms of biological pathways, the existence of these symptoms could suggest elevated maternal cortisol levels and significant nutritional deficits—physiological environments that can trigger epigenetic changes in fetal development. In terms of economic pathways, maternal exhaustion and weakness suggest diminished capacity for parental investment, especially for postnatal caregiving, breastfeeding, and cognitive stimulation of children.

Figure H.IX (Panel A) summarizes the results. Approximately 40% of respondents report feeling weak and exhausted, while roughly 20% report sleep deprivation, undernourishment, and stress. Panel B offers qualitative validation via a word cloud derived from open-ended comparisons of fasting during pregnancy versus baseline fasting. The high-frequency terms—weakness, dizziness, hunger, fatigue, and fainting—strongly corroborate the structured responses, confirming the physiological intensity of the treatment.

H.6 TS2SLS

We are interested in estimating the causal effects of in-utero shocks on long-run labor market outcomes. Following our discussion in section IV.A, we define actual exposure to the in-utero shocks x_i as

$$x_i = \frac{1}{30} \sum_{d \in D} p_{di}(B_i) \, \overline{f}_i, \tag{3}$$

where $\overline{f}_i$ is the fraction of Ramadan days i's mother fasted. We do not observe $\overline{f}_i$ in our data and instead only observe if the pregnancy overlapped with Ramadan—our instrument z_i. This means that we can estimate an intention to treat effect but not a LATE or ATT.

To overcome this limitation, we use data from the household survey we ran to provide the first stage of our empirical framework. Our survey sample is drawn randomly from the same population as the administrative tax data and enables us to estimate the TS2SLS. Specifically, let $s \in \{a, b\}$ index our two samples: the tax returns dataset (a) and the survey dataset (b). The structural relationships governing our variables of interest are

$$\begin{aligned} y_i^s &= g^s(x_i^s, u_i^s), \\ x_i^s &= f^s(z_i^s, v_i^s) \end{aligned} \tag{4}$$

Under the following standard assumption, pregnancy's overlap with Ramadan z_i is a valid instrument for exposure x_i

Assumption A1: *(IV Validity.) For $s \in \{a, b\}$,*

1. *Relevance: The treatment equation f^s is a non-constant function on the support of z^s*
2. *Independence: $z_i^s \perp\!\!\!\perp (u_i^s, v_i^s)$*
3. *Exclusion: The outcome equation g^s is not a direct function of z^s.*

We additionally invoke the standard monotonicity condition—overlap with Ramadan weakly increases realized fasting exposure—i.e., $x_i(1) \geq x_i(0);\ \forall i$. In our context this is plausible because overlap creates the opportunity for fasting exposure, whereas non-overlap mechanically implies zero exposure during Ramadan; any deviation would require systematically reducing fasting in overlapping pregnancies relative to non-overlapping counterfactuals.

We do not observe y_i, x_i, and z_i in the same dataset and hence use the TS2SLS to estimate the causal effects (Angrist & Krueger (1992); Inoue & Solon (2010); Zhao *et al.* (2019)). The validity of the TS2SLS requires the following additional assumption

Assumption A2: *(Structural Invariance.)* The structural relationships are common across samples: $g^a = g^b, f^a = f^b$.

Importantly, this assumption requires a common first stage, meaning that the relationship between potential exposure (z_i) and actual exposure (x_i) is the same in both samples. Our household survey sample is drawn from the population of tax filers and hence closely mimics it. The structural invariance assumption is thus plausible in our setting. One potential violation could arise because our household survey interviews women currently residing in the households of tax filers, whereas some mothers—especially those of older tax filers—may be deceased. Our specifications always include cohort fixed effects to mitigate this concern. Furthermore, we show that the fasting rate is stable across cohorts during our study window (see Figure H.VI). Individuals in our sample were born before 1987, a period over which fasting behavior shows little secular change.

Operationally, the first stage of our empirical framework is the following

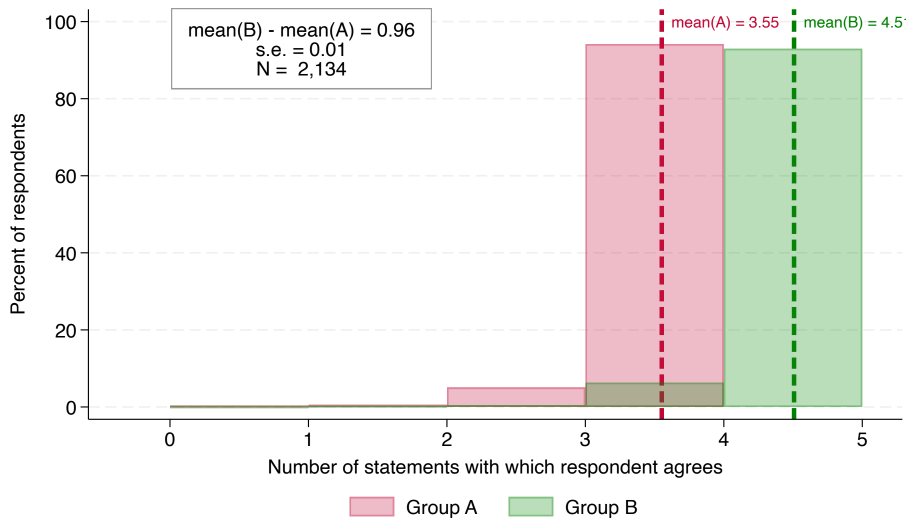
(5) $$x_i = \pi_0 + \pi_1 z_i + X_i'\Gamma + \varepsilon_i,$$

where X_i includes year of birth and month of birth fixed effects. We estimate this equation using our household survey data (sample b). The second stage of our setup is the following

(6) $$y_i = \beta_0 + \beta_1 \hat{x}_i + X_i'\Lambda + \xi_i,$$

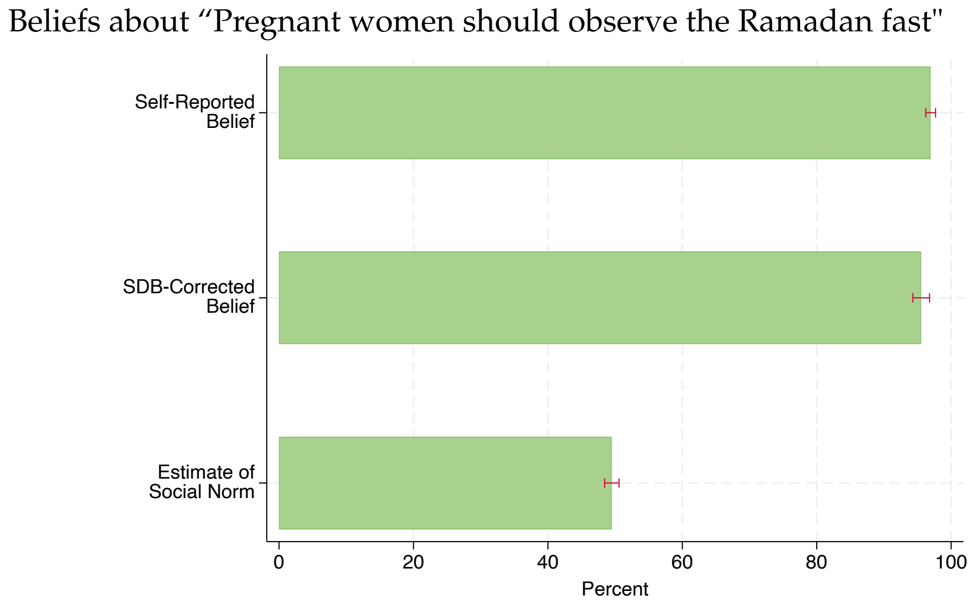
where $\hat{x}_i$ is the predicted exposure constructed using the coefficients estimated in the first stage. We use the standard error correction provided by Inoue & Solon (2010) generalized to the heteroskedastic case by Pacini & Windmeijer (2016).

FIGURE H.I: **LIST EXPERIMENT**



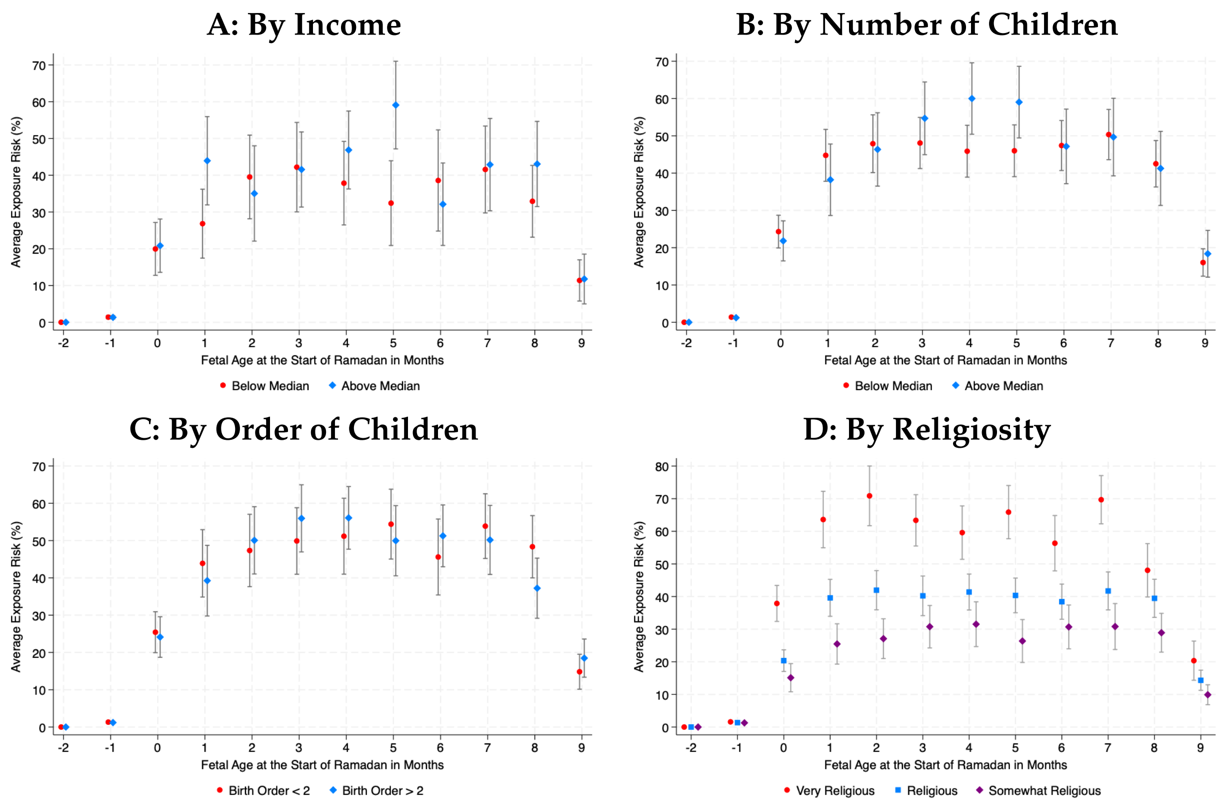
Notes: The figure reports results from our list experiment. Respondents were randomly assigned to Group A (control) or Group B (treatment). Both groups were shown four innocuous statements about pregnancy behaviors (light exercise, consulting a health professional, avoiding social events, avoiding strenuous activity) and asked to report only the number they agreed with. Group B received the same four items plus the sensitive item on fasting (see Table H.I), and likewise reported only a count. The difference in mean counts, mean(B) − mean(A) = 0.96 (s.e. = 0.01; N = 2,134), identifies the share agreeing with the sensitive item. This implied support rate closely matches the directly asked belief question, indicating minimal social desirability bias in our setting.

FIGURE H.II: **MISPERCEIVED SOCIAL NORMS**



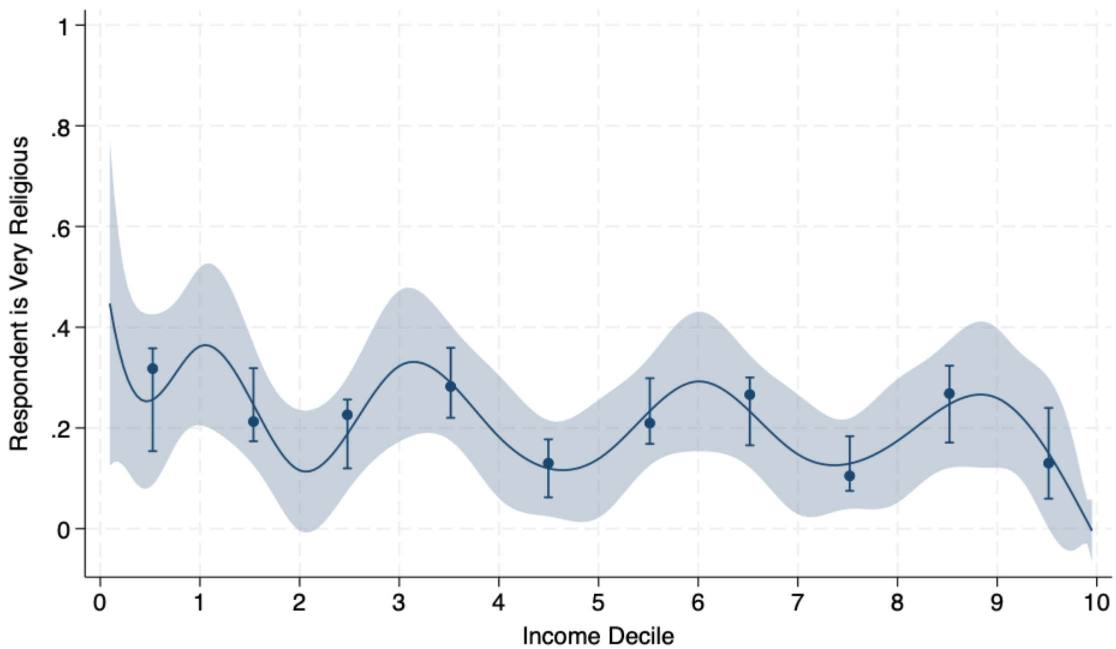
Notes: This figure compares three measures of support for the statement "Pregnant women should observe the Ramadan fast." The top bar reports First-Order Beliefs (self-reported agreement); the middle bar shows the Social Desirability Bias (SDB)-Corrected Belief obtained from the list experiment; and the bottom bar displays Second-Order Beliefs (the respondents' incentivized estimate of the shares of others who agree). The difference between self-reported and SDB-corrected beliefs is statistically indistinguishable, indicating minimal reporting bias. By contrast, the perceived social norm is significantly lower (≈50%), revealing a substantial wedge between private beliefs and expectations of societal consensus. Red ticks denote 95% confidence intervals.

FIGURE H.III: AVERAGE EXPOSURE RISK



Notes: This figure examines heterogeneity in realized in-utero exposure to Ramadan fasting in our household survey. The outcome is the average exposure rate (percent), constructed as potential exposure risk (Figure I) multiplied by the respondent's reported fasting rate (see Section V.F for details). Panel A splits households by below- vs above-median household income, where income is measured from matched administrative tax records. Panel B splits by number of children (restricting to households whose youngest child was born at least five years before the survey to hold fertility histories fixed at the time of measurement). Panel C splits by birth order (a proxy for maternal age at pregnancy, under the same restriction). Panel D splits by self-reported religiosity (very religious, religious, somewhat religious). Across Panels A–C, exposure profiles are similar across groups, indicating little systematic socioeconomic heterogeneity in fasting behaviour; by contrast, Panel D shows a clear gradient, with higher religiosity associated with higher exposure. Markers denote group means; vertical whiskers are 95% confidence intervals..

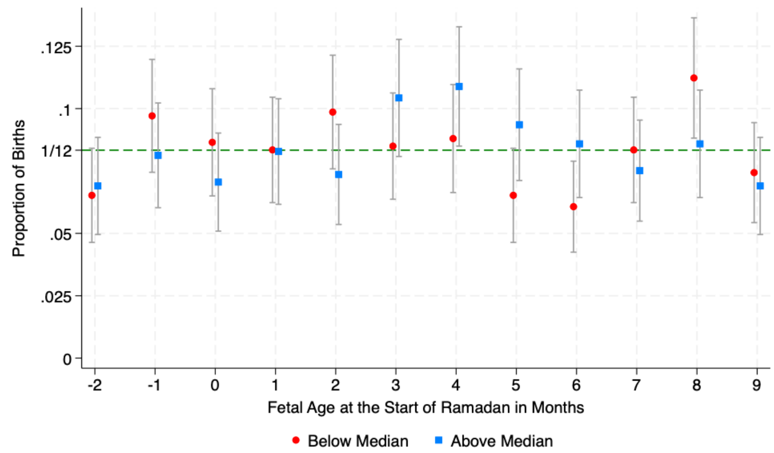
FIGURE H.IV: **RELIGIOSITY VS. INCOME**



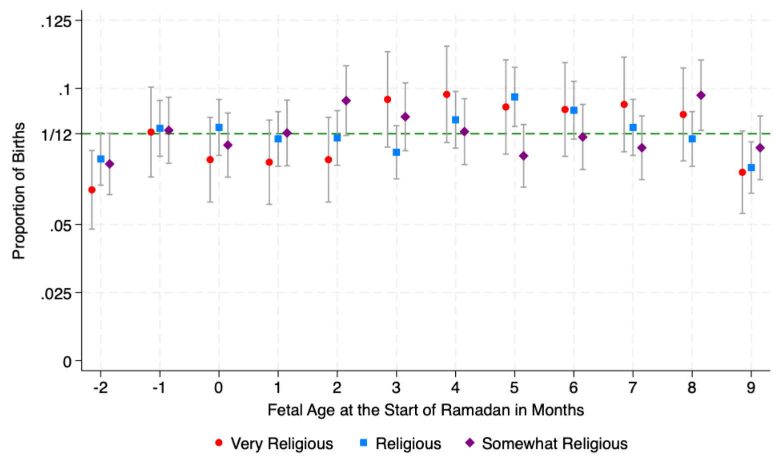
Notes: This figure assesses whether religiosity varies systematically with household income using the nonparametric binscatter procedure of Cattaneo *et al.* (2024). The horizontal axis partitions households into income deciles (constructed from linked administrative tax records). The plotted points show the mean share of respondents reporting being 'very religious' within each income decile; vertical whiskers are 95% confidence intervals around these binned means. The solid curve is the Cattaneo *et al.* (2024) binscatter fit, and the shaded region is its 95% confidence band. Overall, the relationship is flat and statistically indistinguishable from a monotone income gradient, indicating no clear correlation between income and self-reported high religiosity in our sample.

Figure H.V: **Hijra Birth Rate by Observables**

A: By Income

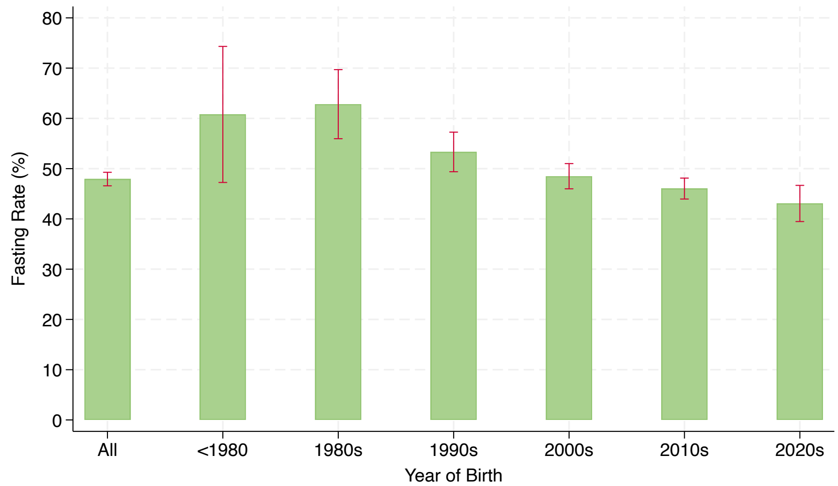


B: By Religiosity



Notes: This figure examines whether conception timing in the Hijra calendar varies systematically with key household observables. The horizontal axis reports the child's fetal age (in months) at the start of Ramadan, defined as in Figure I; the vertical axis reports the share of births falling in each exposure-month cell. Panel A splits the sample by household income (below vs. above the median, measured using linked administrative tax records). Panel B splits by self-reported religiosity from the household survey (very religious, religious, somewhat religious). Points plot the estimated birth shares within each subgroup, and whiskers denote 95% confidence intervals. The horizontal dashed line marks the no-seasonality benchmark of $1/12 \approx 0.083$. Across both panels, birth shares remain close to $1/12$ and statistically similar across subgroups, indicating no evidence that income or religiosity predicts Hijra conception timing and thus no observable sorting into prenatal Ramadan exposure along these dimensions.

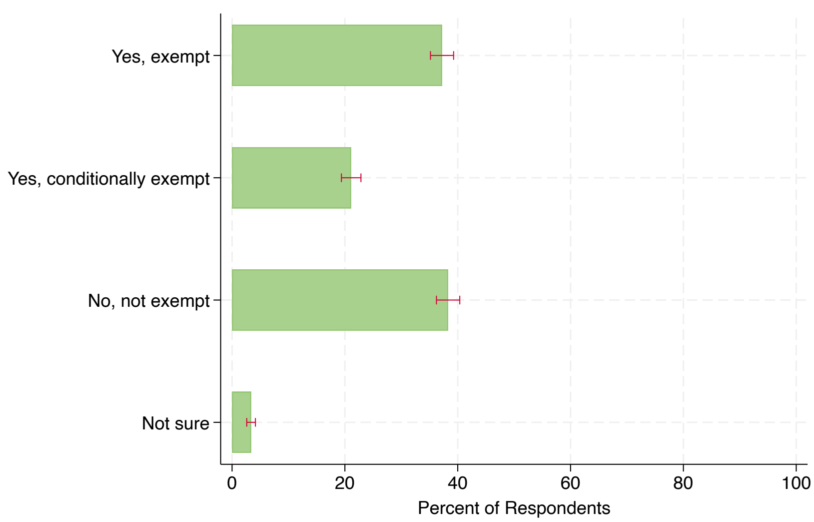
FIGURE H.VI: **FASTING RATE BY YEAR OF BIRTH**



Notes: The figure reports fasting rates during pregnancy from our household survey, disaggregated by the baby's year-of-birth cohort. Bars show the share of women who fasted at least one day while pregnant; red ticks denote 95% confidence intervals. The overall fasting rate is roughly 55%, with higher rates among older cohorts ($\geq$ 60% for women born before 1990) and modestly lower rates among more recent cohorts ($\approx$ 45-55% thereafter).

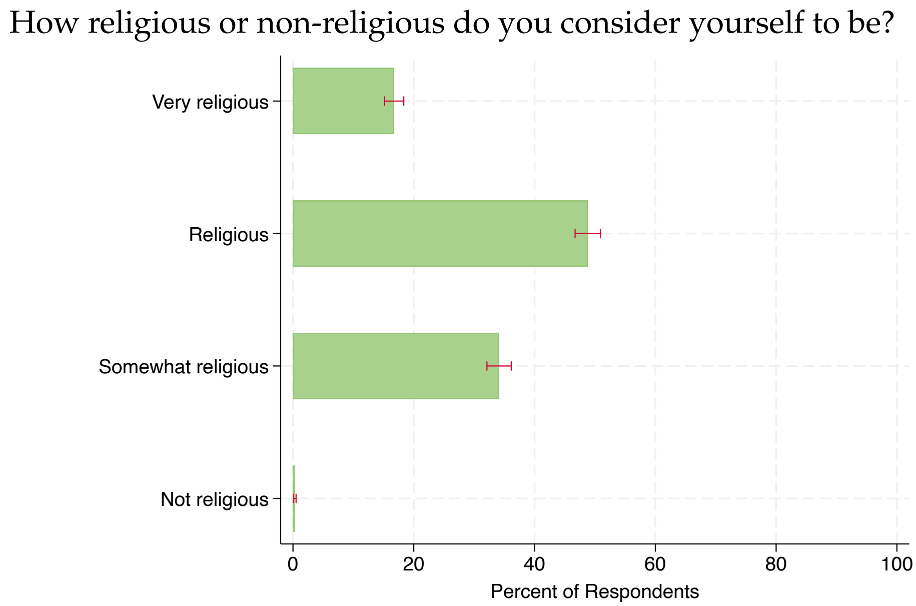
FIGURE H.VII: **BELIEFS ON CONDITIONAL FASTING EXEMPTION FOR DURING PREGNANCY**

Are pregnant women exempt from fasting during Ramadan?



Notes: The figure reports responses from our household survey to the question: "Are pregnant women exempt from fasting during Ramadan?" Respondents chose among four options: Yes, exempt; Yes, conditionally exempt; No, not exempt; and Not sure. Approximately 40% answered No, not exempt, about 40% answered Yes, exempt, roughly 20% selected the conditional exemption, and a small residual were unsure. Thus, only about one in five respondents explicitly recognized the conditional nature of the exemption, while roughly two in five denied any exemption at all. Red markers denote 95% confidence intervals.

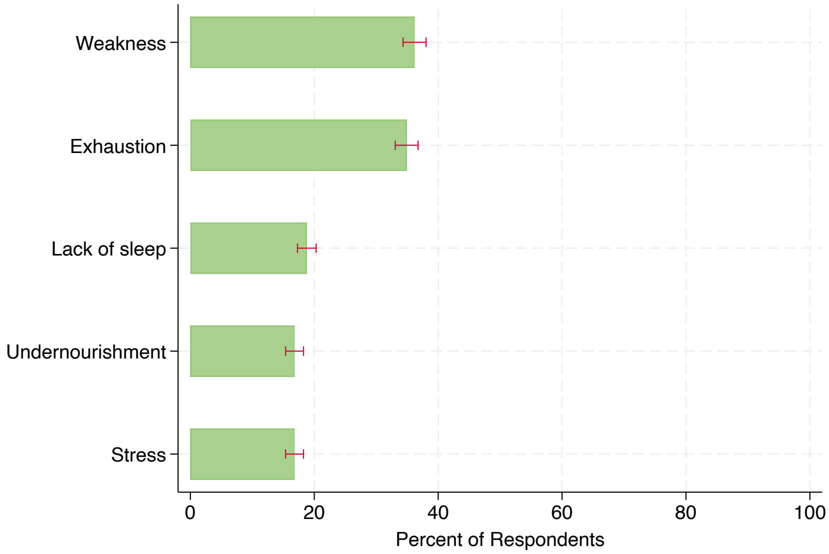
FIGURE H.VIII: **SELF-REPORTED RELIGIOSITY**



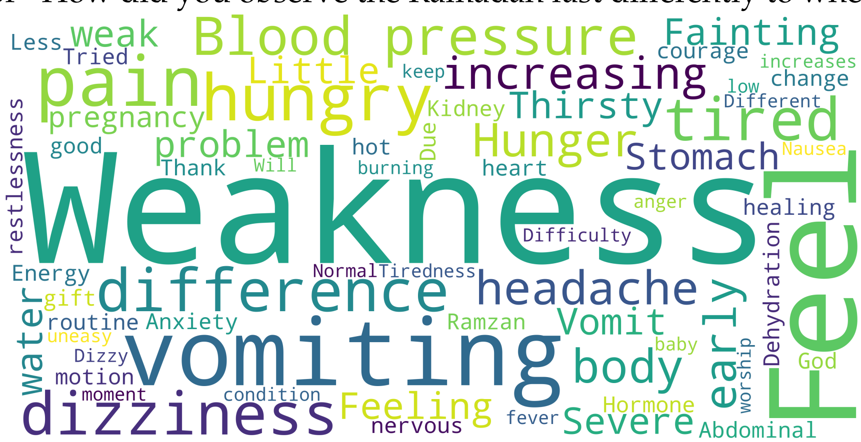
Notes: The figure summarizes self-reported religiosity from our household survey. Respondents classified themselves as Very religious, Religious, Somewhat religious, or Not religious. The modal response is Religious (≈ one-half of respondents), followed by Somewhat religious (≈ one-third) and Very religious (≈ one-fifth); Not religious is rare (≈ 0-1%). Red ticks indicate 95% confidence intervals.

FIGURE H.IX: **FIRST-STAGE MECHANISMS**

A: Did you experience any of the following when fasting during Ramadan while pregnant?



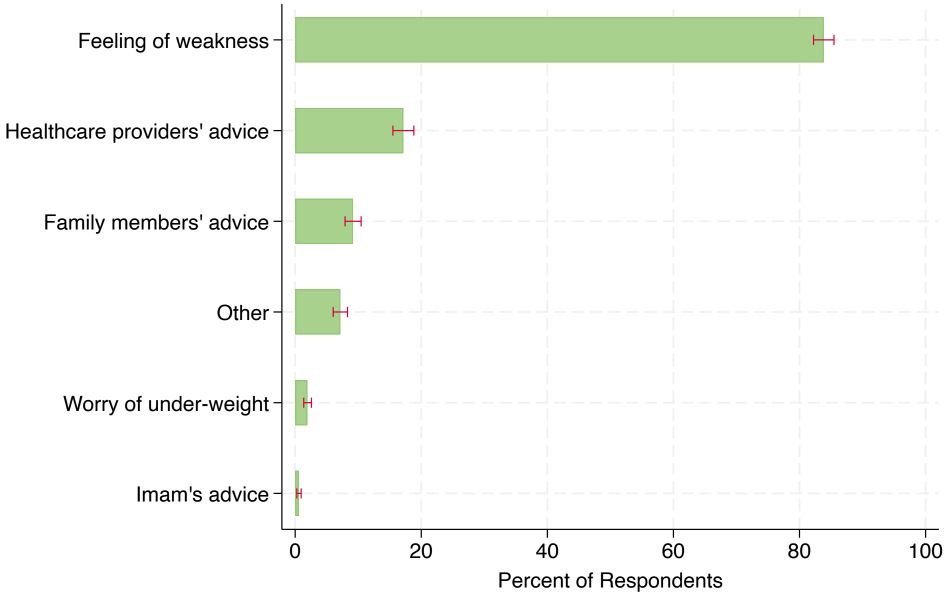
B: Word cloud for "How did you observe the Ramadan fast differently to when not pregnant?"



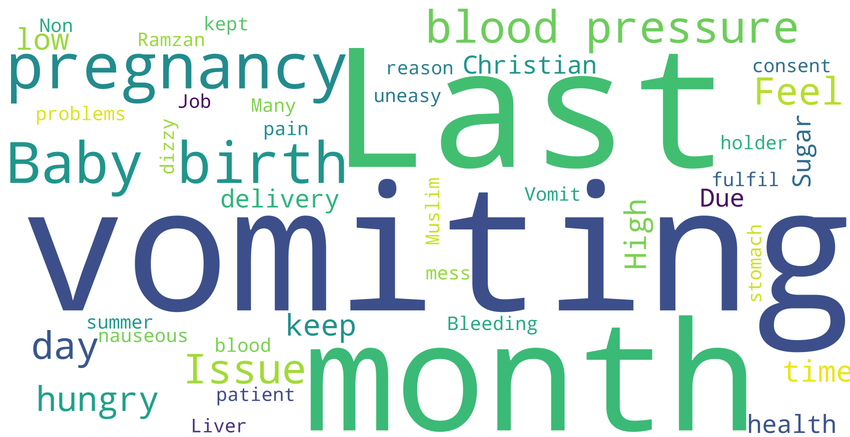
Notes: This figure summarizes self-reported mechanisms operating during Ramadan fasting in pregnancy. Panel A reports the share of respondents who experienced specific symptoms while fasting—weakness, exhaustion, lack of sleep, undernourishment, and stress—with red ticks denoting 95% confidence intervals; weakness and exhaustion are the most frequently cited. Panel B presents a word cloud from an open-ended question ("How did you observe the Ramadan fast differently when pregnant compared with not pregnant?"). Dominant terms—weakness, vomiting, hunger, dizziness, pain, and blood pressure—corroborate the symptom list and point to plausible channels such as caloric deficit, dehydration, nausea, and sleep disruption. Together, the panels provide micro-level evidence consistent with meaningful physiological strain during fasting in pregnancy.

FIGURE H.X: **REASONS FOR NOT FASTING**

A: Reason for not fasting



B: Word cloud for other reasons



Notes: This figure summarizes reasons reported by pregnant women for not fasting during Ramadan. Panel A presents the distribution of stated reasons with 95% confidence intervals (red ticks). The dominant motive is feeling of weakness, followed—at much lower frequencies—by health-care providers' advice, family members' advice, worry about low weight, and imam's advice. Panel B gives a word cloud of open-ended "other reasons," highlighting terms such as vomiting, last month, pregnancy, baby, and blood pressure, which point to late-gestation discomfort and medical concerns. Together, the panels indicate that non-fasting is driven primarily by physical debilitation and secondarily by medical or family guidance.

TABLE H.I: **DESIGN OF LIST EXPERIMENT**

Catalog of statements	
Group A	Group B
1. Pregnant women should engage in light exercise such as walking or stretching. 2. Pregnant women should regularly consult a health professional. 3. Pregnant women should not participate in social events. 4. Pregnant women should not engage in physically demanding activities such as climbing stairways.	1. Pregnant women should engage in light exercise such as walking or stretching. 2. Pregnant women should regularly consult a health professional. 3. Pregnant women should not participate in social events. 4. Pregnant women should not engage in physically demanding activities such as climbing stairways. **5. Pregnant women should observe the Ramadan fast.**

Notes*:* The table presents the list experiment eliciting beliefs about fasting behavior. Respondents were randomly assigned to either Group A or Group B. The prompt for the control group stated: "In this question, you will be presented with four statements. Please read all the statements carefully and indicate the number of statements with which you agree (from 0 to 4). Note that we are not interested in which statements you agree with but only how many of the statements you agree with." The list of statements for the treatment group additionally includes the sensitive item about fasting behavior, specifically if pregnant women should observe the Ramadan fast. The prompt for the treatment group thus asks about five statements.

TABLE H.II: **MISPERCEPTIONS AND HOUSEHOLD INCOME**

	Are pregnant women exempt from fasting during Ramadan?		
	Exempt Or Conditional Exempt	Not Exempt	Not Sure
	(1)	(2)	(3)
Household Income	-0.039	0.016	0.063
	(0.025)	(0.029)	(0.059)
Constant	0.097***	-0.066*	-0.087*
	(0.033)	(0.034)	(0.045)
Observations	1,236	1,236	1,236

Notes*:* Each outcomes is standardized to a standard normal variable. Household income is measured in terms of standard deviations of income. Robust standard errors in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

TABLE H.III: BALANCE BETWEEN SURVEY SAMPLE AND POPULATION OF LAHORE TAX FILERS

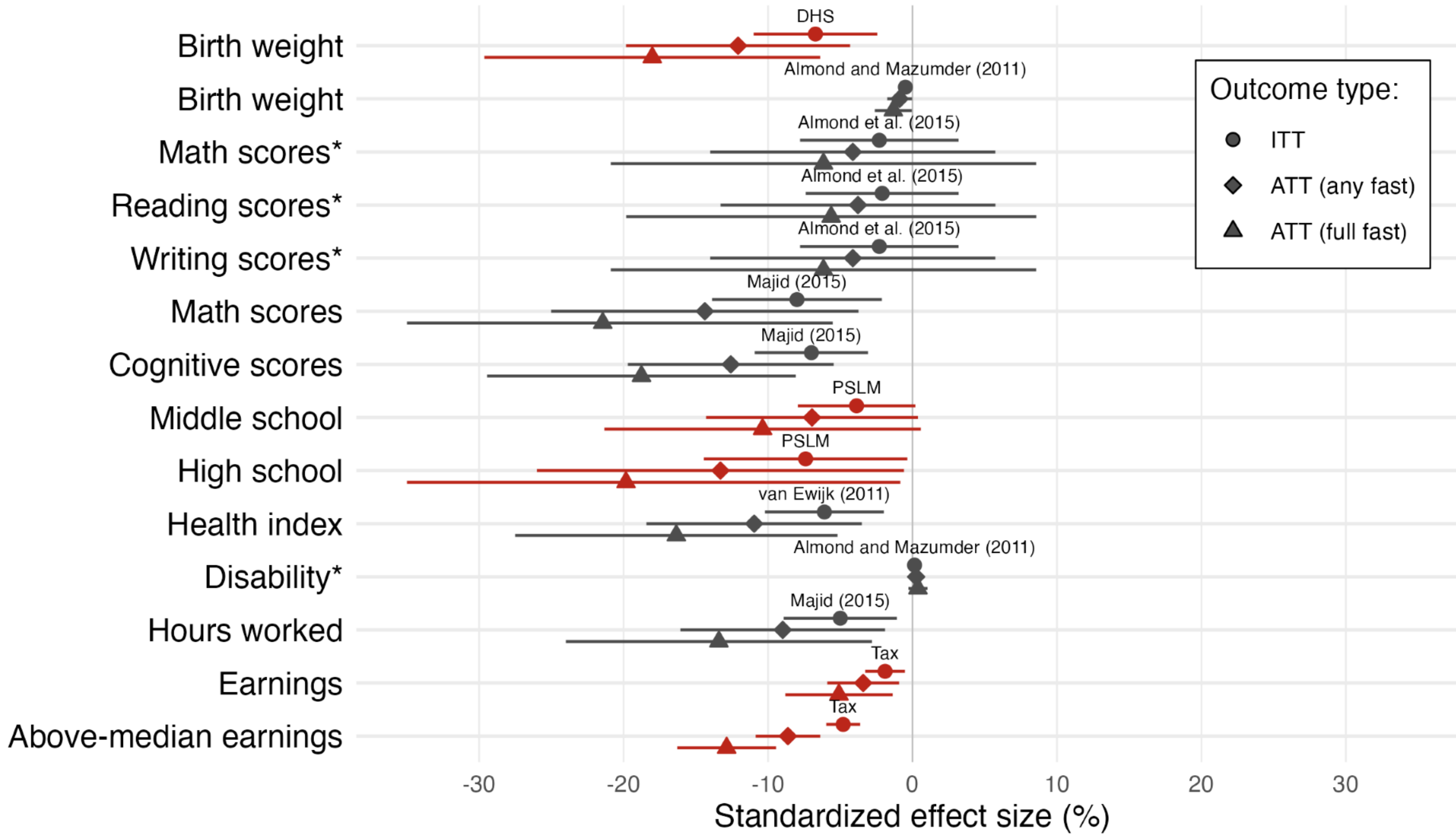
	Population Mean	Survey Mean	Diff.	Std. Diff.	P-Value	N_{pop}	N_{survey}
Age (Years)	29.060	29.362	-0.302	-0.023	0.368	146,423	1,684
Male (%)	0.820	0.825	-0.005	-0.012	0.591	174,109	2,000
Earnings (PKR '000s)	840.143	796.921	43.223	0.011	0.191	174,109	2,000
Strictly Positive Earnings (PKR '000s)	1109.096	1056.224	52.873	0.011	0.205	131,888	1,509
No Reported Earnings (%)	0.242	0.245	-0.003	-0.007	0.754	174,109	2,000
Joint test across rows							
Hotelling's T^2 p-value:				0.6762			

Notes: This table assesses the representativeness of the survey sample relative to the administrative sampling frame. Columns (1) and (2) report the means for the full population of Lahore tax filers and the survey sample, respectively. Columns (3)–(5) report the difference, standardized difference, and the p-value from a two-sided t-test of equality of means. The population comprises the complete administrative frame of tax filers in Lahore observed between 2017–2020. The row labeled 'Joint test across rows' reports the p-value from Hotelling's T^2 test of the null hypothesis that the sample and population means are jointly equal across all observables. The high p-value confirms that the survey sample is balanced and statistically indistinguishable from the population frame.

I Other Results

This section presents a figure comparing our estimates with the previous literature and a table listing fasting rate as documented by studies in a diverse group of countries. This table was referenced in section II of the paper.

FIGURE I.I: **COMPARISON WITH LITERATURE**



Notes: The figure benchmarks our estimated effects of prenatal Ramadan exposure against prior studies. We plot the estimated impact of in-utero Ramadan exposure across various outcomes, expressed as standardized effect sizes in percentage terms. Markers distinguish between Reduced Form/Intention-to-Treat (ITT) estimates (circles), structural Average Treatment Effects (ATT) based on any fasting (diamonds), and structural ATT based on full-month fasting (triangles). Red markers denote estimates from the current study using DHS, PSLM, and administrative tax data. Grey markers represent benchmark estimates from Almond & Mazumder (2011), van Ewijk (2011), Almond *et al.* (2014), and Majid (2015). Horizontal bars represent 95% confidence intervals. Outcomes marked with an asterisk (*) denote standardized test scores or indices. The comparison highlights that correcting for exposure intensity (ATT) yields systematically larger negative effects than reduced-form (ITT) estimates, particularly for long-run human capital outcomes.

TABLE I.I: **FASTING RATE**

Country	Study	Main Findings
(1)	(2)	(3)
Pakistan	Masood *et al.* (2018)	Around 82.8% of women reported fasting during Ramadan out of a sample of 279 pregnant women in a hospital in Karachi, Pakistan.; 11.6% reported fasting for more than 10 days.
Pakistan	Nusrat *et al.* (2017)	Nearly 53% of Muslim women reported fasting during pregnancy out of a sample of 150 women who underwent pregnancy during the 2016 Ramadan and attended a clinic in Karachi, Pakistan; 30% reported fasting for the whole month and 38% believed that fasting during pregnancy is essential.
Pakistan	Mubeen *et al.* (2012)	Around 87.5% of women reported fasting during pregnancy out of a sample of 353 women from Sindh and Punjab, Pakistan, who had experienced pregnancy during Ramadan at least once in their life. Nearly 42.5% reported fasting for the whole month, 23.8% on alternate days, and 10.5% on weekends/holidays only. About 88% believed that fasting during pregnancy (when in good health) is obligatory and 59% perceived no harm in doing so.
Iran	Firouzbakht *et al.* (2013)	About 31.8% of women reported fasting during Ramadan in 2011 out of a sample of 215 pregnant Muslim women who attended health centers in Amol, Iran; 16% reported fasting for more than 10 days.
Iran	Ziaee *et al.* (2010)	Nearly 65% of women reported fasting during pregnancy out of a sample of 189 women delivering in a hospital in Tehran, Iran during the 2004 Ramadan. Around 50% reported fasting for more than 10 days (mostly in first trimester), and 31.7% for more than 20 days.
Iran	Arab & Nasrollahi (2001)	Around 71% of women reported fasting 1-9 days of their pregnancy out of 4,343 women delivering in Hamadan, Iran in 1999. Nearly 40% of respondents reported fasting for more than 20 days. Fasting rates were 77% for the first, 72% for the second, and 65% for third trimester of gestation.

TABLE I.I: **FASTING RATE (CONTD.)**

Country	Study	Main Findings
(1)	(2)	(3)
Indonesia	van Bilsen *et al.* (2016)	Studied 187 Muslim women in a hospital in Jakarta, Indonesia, examining the determinants of the decision to fast. Odds of fasting fall by 4% each week of pregnancy.
Iraq	Bander (2005)	Around 50.7% reported fasting for the whole month of Ramadan out of a sample of 225 women in Iraq who were in 22nd-28th week of gestation.
Singapore	Joosoph *et al.* (2004)	Nearly 87% reported fasting for at least 1 day during pregnancy out of a sample of 182 Muslim women who had received antenatal care in a Singaporean hospital during Ramadan in 2001. Around 57% reported completing at least 20 days of fasting, 67% believed fasting is essential, and 79% perceived no harm in doing so.
Yemen	Makki (2002)	Almost 90% reported fasting for more than 20 days out of a sample of 2,242 women delivering in four hospitals in Sana'a City, Yemen, in 1995.
Malaysia	Salleh (1989)	Around 78.8% reported fasting out of a sample of 605 pregnant women attending a clinic in Muar, Malaysia, in 1985.
Gambia	Prentice *et al.* (1983)	Almost 90% of pregnant women (and all lactating women) from a village in Gambia fasted during Ramadan.
England	Petherick *et al.* (2014)	Nearly 43% of women reported fasting for at least one day and 14% for the full period of Ramadan out of a sample of 310 Muslim women of Asian or Asian British ethnicity giving birth in a hospital in Bradford, England, in 2010. Fasting occurred mostly in the 1st and 2nd trimester and was correlated with education and maternal age.

TABLE I.I: **FASTING RATE (CONTD.)**

Country	Study	Main Findings
(1)	(2)	(3)
England	Malhotra *et al.* (1989)	Almost 45% reported fasting out of 44 Pakistani and Bangladeshi Muslim mothers in a hospital in Birmingham, England.
England	Eaton & Wharton (1982)	Three quarters of mothers in a hospital in Birmingham, England fasted during Ramadan.
England	Fowler H. (1990)	Around 56% reported they would observe the fast while pregnant out of a sample of 78 Muslim women who attended a clinic in Birmingham, England in 1989.
Netherlands	Savitri *et al.* (2014)	Around 53.8% of Muslim women reported fasting to some extent out of a sample of 130 Muslim women from the Netherlands whose pregnancy overlapped with Ramadan in 2010. Nearly 37.7% fasted for more than half a month.
US	Lou & Hammoud (2016)	Around 30% (11) reported fasting during Ramadan in their most recent pregnancy out of a sample of 37 Muslim women who visited a clinic in metropolitan Detroit, Michigan, US during Ramadan in 2013.
US	Robinson & Raisler (2005)	28 out of 32 Muslim women from Michigan, US, reported fasting during at least one pregnancy, 16 for the whole month. Participants estimated a fasting rate of 60-90% for pregnant Muslim women in their communities, but only 30-50% for American-born Muslim women.

Notes*:* The table lists studies that estimate the fasting rate among pregnant Muslim women in different countries. The first column shows the country the study relates to, the second the study's citation and the third its main findings.

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