JG|U
JOHANNES GUTENBERG UNIVERSITÄT MAINZ

Interdisciplinary Public Policy Mainz
IPP

Gutenberg School of Management and Economics
& Research Unit “Interdisciplinary Public Policy”
Discussion Paper Series

In Utero Ramadan Exposure and Children's Academic Performance

Douglas Almond, Bhashkar Mazumder and Reyn van Ewijk

September 2014

Discussion paper number 1410

Johannes Gutenberg University Mainz
Gutenberg School of Management and Economics
Jakob-Welder-Weg 9
55128 Mainz
Germany
wiwi.uni-mainz.de

Contact details

Reyn van Ewijk
IMBEI
University Medical Centre Mainz
Obere Zahlbacher Str. 69
55131 Mainz
Germany

vanewijk@imbei.uni-mainz.de

In Utero Ramadan Exposure and Children's Academic Performance*

Douglas Almond, Bhashkar Mazumder and Reyn van Ewijk

Abstract

A large literature has linked the *in utero* environment to health in adulthood. We consider how prenatal nutrition may shape human capital acquisition in childhood, utilizing the month-long Ramadan fast as a natural experiment. In student register data for Pakistani and Bangladeshi families in England, we examine whether Ramadan's overlap with pregnancy affects subsequent academic outcomes at age 7. We find that test scores are 0.05 to 0.08 standard deviations lower for students exposed to Ramadan in early pregnancy. Our results suggest that brief prenatal investments may be more cost-effective than traditional educational interventions in improving academic performance.

JEL: I12, O15, I21

Keywords: Ramadan, fasting, test scores, developmental origins

Pagehead: Ramadan in Utero & Academic Performance

---

*Corresponding author: Reyn van Ewijk, IMBEI, University Medical Centre Mainz, Obere Zahlbacher Str. 69, 55131 Mainz, Germany. Email: vanewijk@uni-mainz.de. We gratefully acknowledge comments from seminar participants at the Academic Medical Center Amsterdam, ESPE, Erasmus University Rotterdam, London School of Economics, the Nordic Summer Institute in Labor Economics, Northwestern University, the Swedish Institute for Social Research (SOFI), University of Amsterdam, University of Bristol, University of Chicago, University of Mannheim, and "Effects of Early Life Events Conference" at the University of Michigan. We thank Iftikhar Hussain, Francois Keslair, Richard Murphy, Sandra McNally and Felix Weinhardt for helpful comments and the Center for Economic Performance (CEP) at the London School of Economics for providing research facilities. Part of the research was carried out when Van Ewijk worked on the Netspar theme Health and Income, Work and Care Across the Life Cycle. Almond thanks the NSF CAREER program (award no. SES-0847329) for financial support. The views expressed here do not reflect the views of the Federal Reserve system.

Academic interventions typically target school-age children. An emerging literature in economics questions this ‘pound of cure’ approach and evaluates whether early investments may be an under-utilised means of improving academic achievement (Heckman and Masterov, 2007). Indeed, brief investments prior to birth may be more effective than even early childhood interventions and significantly less costly to undertake (Doyle *et al.*, 2009). Why could early investments have such outsized effects on achievement?

As emphasised by Heckman and others, learning is a dynamic process that begins well before school: ‘capabilities beget capabilities’ (Heckman, 2007). Furthermore, human capacity is inherently multidimensional, including health, cognitive, and non-cognitive components which are synergistic over the life course. For example, better health early in life may facilitate learning during school going years. The fetal origins literature has highlighted the staged, developmental nature of early human growth wherein specific pregnancy sub-periods are thought to imprint distinct physiologic functions. Furthermore, research in developmental neuroscience has demonstrated the greater plasticity of the brain early in life and that there are ‘sensitive periods’ during which particular aspects of cognitive development take place (Doyle *et al.*, 2009). These brief windows naturally lend themselves to targeted interventions that may be especially cost effective during the prenatal period. This may be contrasted with more conventional educational interventions which may be costly to implement. For example, Project STAR (Krueger, 1999; Krueger and Whitmore, 2001; Chetty *et al.*, 2011), which improved student outcomes through reduced class sizes for a two year period, (on average) cost over $10,000 per student in 2010 dollars. Finally, a signature feature of fetal-induced changes to health and educational outcomes is their persistence into adulthood. In contrast, the cognitive effects from more conventional education interventions may be subject to “fade out” (e.g. Heckman *et al.*, 2010, Rothstein, 2010, Cascio and Staiger, 2012).

Much of the literature on fetal origins has focused on the effects of prenatal nutritional conditions on long-term health outcomes such as diabetes and heart disease that are only manifested relatively late in life. Therefore it is very much an open question as to whether these same kinds of nutritional conditions influence the development of human capital earlier in the life cycle. We know very little, for example, about the extent to which prenatal nutritional shocks have an effect on academic achievement and how the size of these effects compare to other commonly studied factors in the education literature such as teacher quality or peer influences. Another challenge is to find aspects of the nutritional environment that are actually modifiable through some kind of intervention.

In this paper, we argue that observance of the fasting month of Ramadan by pregnant Muslims has significant effects on academic outcomes that are visible at age 7. Further, the size of the effects are comparable to commonly studied ‘contemporaneous’ educational interventions. A central feature of our identification strategy is that Ramadan follows a lunar calendar, and thereby falls on different dates (and seasons) in different years. We present evidence showing that the timing of pregnancy *vis à vis* Ramadan appears exogenous on observable characteristics. We can therefore compare persons born just before Ramadan, and thereby not exposed during pregnancy, to those exposed at different months of gestation using an 'intent to treat' approach. As Ramadan lasts one lunar month, the exposure period is necessarily brief, especially compared to conventional educational interventions that target later developmental ages.

Our approach departs in four ways from previous design-based observational studies of fetal origins effects. First, we consider an input, the timing of prenatal nutrition, that is relatively manipulable. Although pregnant women are not automatically exempted from fasting, they can request an exception which typically requires them to make up the days later. Many Muslim scholars argue that Ramadan observance is not obligatory for pregnant women, and, although observance is the norm, fasting rates during pregnancy do vary somewhat across societies. This suggests that there is scope for adaptation in practices.[1] In contrast, other studies have utilised natural experiments, such as famines or disease outbreaks, where variation in the environment is 'caused by conditions outside the control of the mother' (Currie, 2009). While extreme natural events provide credible sources for identification, they may not inform individual behavior or health recommendations. Second, since Ramadan observance primarily affects the diurnal timing of nutrition, it constitutes a far less extreme treatment than the famine episodes, pandemics, and natural disasters previously analyzed. Therefore, it is informative about whether milder shocks to the fetal environment also have long-term effects. In particular, meal skipping, 'morning sickness' and dieting during pregnancy (especially prior to pregnancy recognition) are fairly common in developed countries and likewise alter the diurnal timing of nutrition and thereby the intrauterine environment. Thus, the identified linkages to the prenatal period may generalise to other populations. Third, as most Muslims were *in utero* during a Ramadan, the population affected is substantially larger than that afflicted by historical famine episodes or disease outbreaks. Furthermore, Ramadan fasting during pregnancy (particularly early pregnancy) remains common today. Finally, most previous studies in the fetal origins literature have examined outcomes in adulthood (including Almond and Mazumder, 2011 and Van Ewijk, 2011). Instead, we focus on measures of human capital at age 7 to shed light on how fetal-induced effects manifest during childhood.

We analyze school register data from England containing the national 'Key Stage 1' assessments in math, reading and writing. Pakistani or Bangladeshi ancestry is used to identify Muslim students and to estimate the effects of Ramadan exposure on Muslims compared to non-Muslims during the *in utero* period. Our

---

[1] Although there is some variation between countries in fasting rates, in all countries where Ramadan observance among pregnant women has been measured, fasting seems to be the norm. Between 70 and 90% of pregnant Muslims fasted in countries as diverse as Iran and Singapore and regions in West-Africa (Prentice *et al.*, 1983; Arab and Nasrollahi, 2001; Joosoph *et al.*, 2004). The only survey studies available for England are relatively old, although they took place close to the birth dates of the oldest cohort in our data (1991). A majority Muslim women reported fasting during pregnancy that coincided with Ramadan at Sorrento Maternity Hospital in Birmingham. (Eaton and Wharton, 1982; Malhotra *et al.*, 1989).

main finding is that Muslim students exposed to Ramadan in the first trimester of their mothers' pregnancy have significantly lower achievement scores. For example, math scores of students who were exposed to Ramadan during the first trimester are reduced by 0.06 to 0.08 standard deviations. To the extent that not all pregnant Muslim women observe Ramadan, our intent to treat estimates understate the effect of Ramadan observance. Nevertheless, the magnitudes of the effects are comparable to many conventional educational interventions such as the effects of charter schools, Teach for America or Head Start (Dobbie and Fryer, 2011),which are also an order of magnitude longer in duration than Ramadan. An important caveat to our analysis is that we cannot separate the effects of fasting from other activities that take place during Ramadan so our results should be interpreted as capturing the overall effect of all facets of Ramadan observance. Further, our results are based on children of Bangladeshi or Pakistani origin living in England and future research may be needed to verify whether our results generalise to other Muslim populations. Nevertheless, our results suggest that a life course perspective may help identify inexpensive and untapped investment opportunities.

1 Literature Review

1.1 Economics

As suggested by Heckman, the different stages of childhood can be conceived of as distinct inputs into the production of subsequent 'capacity'. Let I_1 denote investments occurring during the prenatal period and I_2 investments during the postnatal period (i.e. the rest of childhood). Given that cognitive development early in life occurs in distinct stages, I_1 and I_2 are likely imperfect substitutes in the production of capacity. In the extreme case of a Leontieff technology, human capacity cannot exceed that determined by the minimum of investments during the prenatal period (Heckman, 2007).

Furthermore, if there are multiple dimensions to 'capacity' (e.g. cognitive, non-cognitive, health) then there can be synergies across these dimensions. For example, a child born in better health may have an advantage in creating cognitive and non-cognitive capacity. Finally, the production technology may incorporate 'dynamic complementarities' (Heckman, 2007) whereby investments in stage t of childhood are more productive when there is a high level of capability in stage $t-1$.

Economists have also sought to establish links between prenatal conditions and human capital outcomes empirically. Pioneering work by Currie and Hyson (1999) used the British National Child Development Survey and found that the pass rate for math and English O-level tests was roughly 25% lower for low birth weight children. More recent design-focused empirical studies have found that human capital outcomes respond to a range of prenatal shocks, particularly those experienced during first half of pregnancy. Field *et al.* (2009) evaluated the effect of prenatal iodine supplementation on subsequent educational attainment in Tanzania, finding that supplementation during the first trimester increased completed schooling by as much as half a year. These effects persisted in a siblings comparison, and were generally stronger among girls. Almond, Edlund and Palme (2009) studied prenatal exposure to radioactive fallout from the 1986 Chernobyl meltdown on middle school performance in Sweden. Exposure to ionizing radiation between weeks 8 and 25 of gestation reduced the likelihood of qualifying for high school by 3% and reduced math grades by 6% (the measure closest to IQ). Interestingly, no health effects of radiation exposure were detected in the prenatally exposed cohorts, suggesting the damage was 'subclinical'. Kelly (2011) considered the impact of prenatal exposure to the fall 1957 'avian flu' pandemic in Britain, using the serendipitous timing of the 1958 British cohort study (born March 1958). Kelly (2011) found negative impacts on test scores that interestingly appeared independent from the negative impact of the 1957 pandemic on birth weight.

While recent studies have successfully exploited natural experiments to demonstrate causal pathways, such prenatal experiences are relatively rare and depart from modifiable differences in the prenatal environment today, particularly within developed countries. As we describe in the next section, the biophysical changes induced by Ramadan fasting during pregnancy more closely resemble those occasioned by other determinants of nutrition timing in developed countries such as meal skipping, dieting, and nausea and vomiting ('morning sickness').

1.2 Biological Mechanisms

Almond and Mazumder (2011) and Van Ewijk (2011) provide overviews of the biomedical literature concerning the potential pathways between prenatal Ramadan exposure and long-term outcomes. We briefly review some of the mechanisms that may be particularly relevant for cognitive function. Prenatal Ramadan effects seem most likely to arise as a result of nutritional restriction, although stress, lack of sleep and glucose surges resulting from the consumption of sweet products in the evening might also exert effects.[2] One potential pathway arises through a set of biochemical changes known as 'accelerated starvation' that occurs in pregnant women who undergo an extended period of fasting. Pregnant women experience pronounced declines in blood glucose levels and sharp increases in ketones and free fatty acids as they begin to metabolise

---

[2] One could argue that breast feeding during Ramadan might have an effect of its own, which would potentially interfere with our estimates of Ramadan's in utero effects. However, our research design compares children whose gestational period had, versus had not overlapped with Ramadan. The latter, i.e. our control group, were born just before a Ramadan, whereas our exposed group was born longer before a Ramadan. Hence, if effects of Ramadan during lactation exist, this will arguably most strongly affect our control group, so that such effects would lead us to under estimate in utero effects of Ramadan.

their stores of fat. Such conditions can arise in as little as 12 hours and studies have documented these changes during the Ramadan fast in both developed and developing countries (Prentice *et al.* 1983, Malhotra *et al.* 1989). Animal studies have linked exposure to ketones early in pregnancy to neurological impairments (e.g. Hunter and Sadler 1987) and studies of humans have associated ketone exposure in diabetic mothers to diminished cognitive ability (Rizzo *et al.*, 1991).

The literature on the developmental origins of health and adult disease has emphasised how environmental exposures in pregnancy, such as nutritional disruptions, can lead to permanent alterations in the body's systems in order to improve the likelihood of survival to reproductive age in the perceived environment at birth (Gluckman and Hanson, 2005). These 'predictive adaptive responses' (PARs) make individuals more prone to poor health in adulthood. The most well-known examples relate to heart disease and diabetes, but there may be other manifestations of PARs as well. Although the literature has only begun to speculate at the precise mechanisms behind PARs, disruptions to the flow of glucose are thought to be one of the key signals of poor environmental condition during fetal development. Gluckman and Hanson note that 'the developing embryo will change the relative assignment of cells to the inner cell and outer cell mass according to whether it perceives a problem in glucose supply' (Gluckman and Hanson, 2005, p31-32).

One particular example of a PAR that has received significant attention in the literature is the notion that prenatal nutritional deprivation or maternal stress can lead to alterations in the neuro-endocrine system or 'HPA axis' which in turn, can lead to permanent health effects. PARs operating through the HPA axis are notable for our purposes for two reasons. First, several studies have linked maternal stress during pregnancy to behavioral and cognitive deficits in children (Kapoor *et al.*, 2006; LeWinn *et al.*, 2009; Aizer *et al.*, 2009), and it is hypothesised that this may be due to modifications to the HPA axis. Direct evidence linking the HPA axis to cognitive impairments has been found in animal studies. Second, a recent study documented elevated levels of the hormone cortisol, which occurs when there is heightened sensitivity in the HPA axis, among pregnant women who fasted during Ramadan (Dikensoy *et al.*, 2009). Another recent study co-authored by David Barker, one of the pioneering epidemiologists in the fetal origins field, linked Ramadan observance to alterations in placental growth due to fetal programming (Alwasel *et al.*, 2010).

Compared to other organs, the brain is thought to be especially susceptible to the fetal environment due to the complexity of its development. Gluckman and Hanson write: 'this complexity means that the fetal brain is very sensitive to environmental stimuli that might irreversibly damage it' and that 'the number of neurons is almost entirely determined in fetal life and is largely completed in mid-gestation' (Gluckman and Hanson, 2005, p46), suggesting that nutritional shocks in the first half of pregnancy may be especially harmful. They further point out that the fetal environment may play a contributing role in the development of certain psychiatric diseases.

1.3 Long-term Effects of Ramadan Exposure

We are aware of three studies that examine effects of prenatal exposure to Ramadan on childhood or adult outcomes.[3] Azizi *et al.* (2004) found no statistically significant effects of observance on the IQ scores of 191 children between the ages of 4 and 13 attending 15 primary schools in Iran. The study compared 98 treated children whose mothers fasted for at least 27 days during Ramadan, with 93 control children whose mothers did not fast at all during Ramadan. However, mean differences between the treatment and control groups were found in certain characteristics, such as breast feeding duration and socioeconomic status, that were either statistically significant or quantitatively meaningful.[4] In addition to having a relatively small sample, the study appeared to have selected cases based on potential outcomes which could have imparted some bias.[5]

Almond and Mazumder (2011) linked Ramadan exposure to adult outcomes in Uganda and Iraq using Census data. They find that full exposure to Ramadan in the first month of pregnancy increased the likelihood of a disability by about 20% with especially large effects on mental/learning disabilities. Van Ewijk (2011), also focusing on adult samples of Indonesians from the Indonesian Family Life Survey (IFLS), found effects of prenatal Ramadan exposure on a variety of measures of health including coronary heart problems and diabetes. Both Almond and Mazumder (2011) and Van Ewijk (2011) utilise a research design that compares Muslims whose *in utero* period overlapped with Ramadan to Muslims who were unexposed and show that pre-determined observable characteristics do not vary with exposure.[6]

2 Data and methodology

2.1 Data

In England, all students attending state schools are assessed at different points in their schooling career, or

---

[3] There are a number of studies in the biomedical literature that examine the effects of Ramadan observance on fetal and birth outcomes which are discussed in Almond and Mazumder (2011). As noted here, most previous studies rely on the strong assumption that non-observers are comparable to observers at a point in time and typically use samples that lack sufficient power to detect small but quantitatively meaningful effects. In contrast, using the universe of natality data on 18 birth cohorts from the U.S. state of Michigan, Almond and Mazumder (2011) find significant effects of fasting on lowering birth weight and the likelihood of a male birth.

[4] The duration of breastfeeding was about three months longer in the treated group (statistically significant at the 5% level). An index of socioeconomic status as well as income and home ownership were also all higher in the treated group, though not statistically significant. For example, 18% of the treated owned their own home compared to 13% of the control group.

[5] Of the 141 children who could have been included in the treated group the sample of 98 included *all* of those who fasted in the third trimester but only a sample of those who fasted earlier in pregnancy. The oversampling of those with late exposure is problematic since the neuro-development literature has emphasised the importance of early exposure. Further, they appear to have selected sample members such that mothers 'with any history of problems such as drug consumption, smoking, and thyroid dysfunction during pregnancy, dystocia, and other problems during different stages of development affecting children's IQ from the fetal stage to childhood were excluded'.

[6] Van Ewijk (2011) also finds that the results are robust to including mother fixed effects suggesting that any unobservable forms of selection would have to be sibling-specific.

‘Key Stages’, to measure their academic performance in different subject areas. We start with the population of students who were assessed at Key Stage 1 between 1998 and 2007 when they were approximately 7 years old. The Key Stage 1 score is based on a teacher assessment of the students’ proficiency in reading, writing and mathematics.[7] Teacher assessments are made following detailed guidelines based on ‘National curriculum levels’ that describe levels of proficiency in each subject area. The assessment is based on a combination of tests and tasks that take less than three hours to administer.[8] Students at Key Stage 1 should be at level 2. The teacher assessment can take on one of the following values: 1, 2C, 2B, 2A, 3 or 4.[9] Following Department of Education guidelines, we translate these assessments into numerical scores which we then transform in z-scores using the full sample.[10,11] We also use as an aggregate measure the first principal component from a principal components analysis (PCA) on math, reading and writing. This variable captures 83% of the total variance of the three constituent subjects.

We use a unique student identifier to link the Key Stage 1 scores to other student level data contained in the Pupil Level Annual School Census (PLASC). The PLASC is constructed based on electronic records provided by each school in England to the Department for Education and Skills (DfES) and covers all enrolled pupils as of January of each year. Starting with the 2002 data we link the Key Stage 1 scores to the PLASC for that year. However, prior to 2002, some of the key background characteristics such as ethnicity are unavailable in the contemporaneous PLASC. Instead we link these individuals through a 2-step process to the 2002 PLASC to obtain their characteristics as of 2002.[12]

2.2 Difference in Difference Strategy

Previous work on adult outcomes (Almond and Mazumder, 2011; Van Ewijk, 2011) has used an ‘intent to

---

[7] Prior to 2004, our data contained both teacher assessments *and* standardised tests (‘National Curriculum tests’) but starting in 2005 we only have the teacher assessment. Therefore in order to have a uniform measure across all years we use the teacher assessment measure. Since students at this level have only 1 teacher and since the teacher gives these assessments at the end of the school year, when he or she knows the child well, this may actually provide a more reliable measure than standardised test scores which contain considerable noise (Kane and Staiger, 2002). This is one reason why we chose to use Key Stage 1 scores rather than Key Stage 2 scores. In addition as we discuss later we have better cohort coverage which has the advantage of minimizing potential seasonal bias and also offers greater precision. A number of other studies in the economics literature have used Key Stage 1 scores as an outcome including Gregg *et al.* (2005) and Dustmann *et al.* (2010).

[8] Schools and local areas have some discretion as to how the teacher assessments are conducted. The following link provides a description of the assessment process: *http://www.direct.gov.uk/en/Parents/Schoolslearninganddevelopment/ExamsTestsAndTheCurriculum/DG_10013041*

[9] Levels 2A, 2B, and 2C do not exist before 2004 (only level 2 exists in these years). In addition, students can be assessed as: ‘W’ for a child who is working towards level 1; ‘A’ for a child not assessed due to absence, or a child who has had a long period of absence, or there is insufficient information to enable a teacher assessment result to be calculated; or ‘D’ for a child for whom teacher assessment has been ‘disapplied’.

[10] W = 3 points; level 1 = 9 points; level 2C = 13 points; level 2B = 15 points; level 2 = 15 points (where no breakdown of level 2 reported); level 2A = 17 points; level 3 = 21 points; level 4 = 27 points, see www.education.gov.uk/rsgateway/DB/SFR/s000867/sfr21-2009.pdf. Appendix Table 1 shows the distributions of the scores.

[11] Standardizing the scores per cohort instead of over the full sample gives virtually the same results as those presented below.

[12] We first must link these students to the 2005 PLASC where we are able to retrieve an identifier that allows us to link them back to 2002. This imposes a requirement that the students who take the Key Stage 1 prior to 2002 must have remained in the English school system through 2005. We do not think that this selection rule is much of a concern since the students who took the test as early as 1998 would still only be 14 years old as of 2005 and therefore highly likely to have remained in school. We will however, remove students who either left England or left the state school system by 2005.

treat' (ITT) design that compares the outcomes of Muslims who were *in utero* during Ramadan to those who weren't. One important issue in this identification strategy is separating seasonal factors from true fasting effects since it is well established that season of birth (or conception) has long-run effects (Doblhammer and Vaupel, 2001; Crawford *et al.*, 2007). This is particularly important in the context of studying educational outcomes because the age cutoffs that determine school entry lead to a sharp discontinuity in school performance by timing of birth. This is apparent in England as can be seen in Figure 1 where those born on or after September 1 have significantly higher Key Stage 1 math scores.[13] It is also clear that in addition to this discontinuity, there is a notable trend reflecting the age at which one is assessed.

The prior studies took advantage of the gradual movement over time of Ramadan throughout the Gregorian calendar year. Since Ramadan is based on the lunar calendar, it begins about 11 days earlier each year. Therefore, with data covering three decades of birth cohorts, one can employ standard seasonal controls (e.g. month dummies) and readily remove confounding effects since Ramadan would have made a complete cycle over the year. In these prior studies the effects are identified by comparing Muslims whose prenatal period overlapped with Ramadan to those who were never *in utero* during Ramadan, and the same estimation approach can be applied separately to non-Muslims as a falsification check.

In our case, however, we have only ten birth cohorts whose potential prenatal Ramadan exposure only could have occurred during one of five winter and spring months (December to April) and so it is not obvious that seasonal controls are sufficient.[14] Given the limited number of cohorts and the strong seasonal effects for educational outcomes, we instead utilise a 'difference in difference' strategy where we take the effect on Muslims and further subtract any effects for non-Muslims that may arise due to possible seasonal effects. As we show later, it turns out that the differencing is not critical.[15]

Ideally, for our treatment group we would like to identify Muslim students who were *in utero* during Ramadan. Since the PLASC does not identify the religion of the student, we instead assign Muslim status to students who report their ethnicity as Pakistani/Bangladeshi. According to the 2001 Census, 92% of Pakistanis and Bangladeshis report that they are Muslims. To reduce the scope for measurement error, we use only those Pakistani/Bangladeshis who are living in a region (local authority) where at least 90%

---

[13] For example, those born on August 31, 1999 would have received the Key Stage 1 assessment in 2006 whereas those born on September 1st 1999 would receive the assessment in 2007. Note that the September 1st cut-off is strictly observed, and that retention and grade skipping are very rare at this age: only 0.28% of all students are not in their expected cohort.

[14] This is one of the reasons why we use the Key Stage 1 rather than Key Stage 2 scores. For Key Stage 2, we would be able to use only eight cohorts. We discuss the implications of using Key Stage 2 scores later.

[15] There are no systematic Ramadan effects for non-Muslims, echoing the 'placebo' results from the prior studies. Our effects remain even if we only use Muslims who were not exposed to Ramadan prenatally as the control group as in the previous studies. Additionally, we conduct a new placebo test in which we again run our difference in difference regressions, but this time compare our two main non-Muslim groups. We show that no effects appear in this specification, implying that our results are unlikely to be driven by model misspecifications that led residual seasonal effects to be correlated with Ramadan timing.

of Pakistani/Bangladeshis with a reported ethnicity are Muslims according to the Census.[16] We suspect that there is still some residual measurement error since the reported ethnicity of students is not always constant across years. Figure 2 shows the distribution of Pakistani and Bangladeshi in England. Areas with high concentrations of Muslims include London and the areas around Birmingham in the West and around Manchester in the North-West.

For our control group we use Caribbean students, who have similar levels of school performance and nearly identical rates of free school meal status –a proxy for socioeconomic status. In Table 1 we show that the average scores of Caribbeans are 0.20 standard deviations below the national average compared to -0.36 for our designated Muslim students. About 35% of both groups of students receive free school meals. This compares to an average rate of free school meals of about 16% for white British students. Although Indians are culturally more similar to Pakistanis and Bangladeshis, a sizable minority are Muslim and hence they would be a 'contaminated' control group. Further, Table 1 shows Indian students outperform the national average and are less likely to receive free school meals.

An issue that arises in classifying Caribbeans is that the ethnicity codes were expanded in 2003 to create a separate category for mixed-race Caribbeans ('white and black Caribbean') in addition to the traditional category of 'Caribbean'. For our main analysis we have combined both groups in order to maximise our sample. This leads to a large increase in the number of Caribbeans starting in 2003. As a robustness check we have also excluded mixed race Caribbeans and find similar, though less precise, results. As we discuss later, we have also run all of our models using white British students as an alternative control group and find similar, and much more precisely-estimated effects.

2.3 Ramadan Measures

In order to identify whether Ramadan overlapped with the *in utero* period, we use one's exact birth date and assume a normal gestation length of 266 days (since conception) for each individual. We then create a set of indicator variables to identify when during gestation Ramadan began. For each of the nine months of pregnancy we generate a separate variable (e.g. Month 1, Month 2, ..., Month 9) to indicate whether Ramadan began during that month of pregnancy. In addition, we create a 'Month 0' variable to capture conception during Ramadan and early gestation exposure to fasting. Those whose pregnancies, by this calculation, do not appear to overlap with Ramadan are further subdivided into two categories. We classify individuals as 'probably not exposed' if they were conceived within 14 days after Ramadan had ended and 'certainly not exposed' if they were conceived more than 14 days after Ramadan. Figure 3 shows an

---

[16] This removes only about 1.2% of all Pakistani/Bangladeshis. We also drop any students who report a mixed ethnicity of White and Bangladeshi or White and Pakistani.

example of how various pregnancies would be classified based on the exact date of birth and the timing of Ramadan. Our reliance on the normal gestation length creates some potential measurement error for most of these indicators of Ramadan exposure since some pregnancies will be preterm or longer than full term.[17]

Our coefficients capture the full effect of Ramadan observance if *all* Muslims who were pregnant during a Ramadan chose to fast. Since observance rates typically depart from unity, our ITT approach *underestimates* the treatment effect of observance and can be viewed as a lower bound. Our estimates can be rescaled by multiplying by the inverse of the fasting rate (not observed in our data) in order to approximate the treatment effect of Ramadan observance.

2.4 Specification

We regress Key Stage 1 assessments in math, reading and writing, as well as their first principal component, on the Ramadan exposure measures. Since the three subject tests are expressed as z-scores, coefficients can be interpreted as the effect sizes in standard deviation units. However, the standard deviation of the first principal component is 1.58, so the estimated effects for this outcome should be scaled down by this factor in order to be comparable to the other coefficients. The excluded group is those classified as 'certainly not exposed', so all effects are relative to this group. Additional controls include month of birth dummies, a dummy for female, a dummy for Pakistanis and Bangladeshis (henceforth 'Muslims'), a dummy for free school meal eligibility and a set of geographic dummies for each 'Census output area'.[18] To further address concerns about time trends, we also include a cubic in the number of days between the date of birth and January 1 1960. Standard errors are clustered at the school level. We fully interact each regressor (except for the geographic fixed effects) with a dummy for 'Muslim', so that we estimate:

$$y_{ig} = E_{ig}\beta^{Car} + Muslim_{ig} \times E_{ig}\beta^{Mus} + X_{ig}\gamma^{Car} + Muslim_{ig} \times X_{ig}\gamma^{Mus} + Muslim_{ig}\vartheta + \xi_g + \varepsilon_{ig} \quad (1)$$

in which y denotes a test score of student i in census output area g; E is a vector of Ramadan exposure measures; $Muslim$ is a dummy variable indicating ethnicity; X is a vector of control variables (month of birth, sex, FSM, days since Jan. 1, 1960) and ξ_g are geographic fixed effects. Interacting the regressors with a Muslim dummy allows for example, for separate time trends and seasonal patterns for Muslims and Caribbeans, (albeit with possibly imperfect seasonal control). The coefficients on the Muslim interaction

---

[17] Our Month 9 variable captures individuals who were born during Ramadan. To the extent that the exact date of birth is measured accurately there should be no misclassification. Similarly, those identified as 'certainly not exposed' would only be misclassified if the term of gestation exceeded 280 days which is rare (Kieler *et al.*, 1995). In that case conception would overlap with the end of Ramadan. We note that as long as the date of birth is not incorrect, premature births will never be misclassified as 'not exposed' if they actually were exposed but it is possible that they could be misclassified as 'exposed' even if they weren't. More generally, the fact that we do not know the date of conception may lead us to misclassify the exact exposure *month* but should not alter our conclusions regarding which *trimesters* have the largest effects. We also note that this issue is not unique to our study and is a concern in any study where the exact date of conception is not known.

[18] Our sample includes 51,187 census output areas. Output Areas are 'the base unit for the release of Census data' and are based on common demographic, household and economic characteristics.

terms for the Ramadan measures, β^{Mus} are the main objects of interest.

We also estimate the same specification using free school meal eligibility as the dependent variable to show that there is no selective timing of pregnancies related to socioeconomic status. As an additional test on selective timing of pregnancies, we examine whether birth patterns over the year differ between ethnic groups. Finally, as a 'placebo treatment', we estimate any prenatal Ramadan 'effects' for Caribbeans using white British as a control group, where we should not expect to see any effects. This helps ensure that our results are not driven by any other misspecification of the model such as any residual seasonal effects that might be correlated with the timing of Ramadan.

3 Results

3.1 Difference-in-differences vs. Caribbeans

In Table 2 we present the coefficients on the Ramadan measures interacted with an indicator for being Muslim. These show the effects on Muslims of Ramadan starting in each month of pregnancy compared to Muslims with no *in utero* exposure, relative to Caribbeans. The results show consistently significant negative effects of exposure in the first three months of pregnancy. For example, column (1) shows that Muslim students exposed to start of Ramadan in the first month of pregnancy have Key Stage 1 assessments in math that are 0.068 standard deviations lower. Similarly sized effects are found in the month of conception and the second and third months of pregnancy and for the same four periods for reading assessments (column 2) and writing assessments (column 3). The implied effect sizes for the first principal component of these subjects from principal components analysis (PCA) shown in column (4), when converted into standard deviation units, is also very similar. The largest effects appear to be in the third month of pregnancy when the effects on math and on the first principal component are about 0.08 standard deviations. This accounts for about 20% of the overall test score gap between Muslims and the national average in these subjects. As we discussed earlier, the fact that not all pregnant Muslim women observe the fast implies that our estimates should be viewed as a lower bound on observance's effect. Figure 4 plots the coefficients shown in columns 1 through 4 of Table and illustrates a similarity in the time pattern of effects across outcomes: Ramadans experienced in early pregnancy are most harmful. 2.[19]

Column (5) shows the results on math scores using a specification in which the geographic fixed effects have been omitted. Comparing columns (1) and (5), we find the results are actually a bit stronger once we

---

[19] We also find similar results if we use the original categorical coding of the assessment levels using ordered probit models or if we run linear probability models with an indicator for attainment of level 2 as the outcome, see Appendix Tables 2 and 3.

control for location fixed effects, suggesting that our results aren't driven by geographic differences.[20] In column (6) we only use Muslim sub-sample and no longer estimate a difference in difference model. In this specification our identification is based on only the effects of Ramadan exposure relative to other Muslims whose *in utero* period did not overlap with Ramadan. We again find that months 1 through 3 all show negative effects that are significant at the 5% level and that those conceived during Ramadan have scores that are significantly lower at the 10% level. Regressions analogous to those in columns (5) and (6) with reading, writing and PCA as the dependent variables yield similar patterns.

Overall, we find that effects are largest in the first three gestation months. While this is consistent with the hypothesised predictions of much of the developmental origins literature, it is possible that some of the gradient in the effect size may be due to differential rates of fasting during the course of pregnancy, for which we unfortunately have no data. Presumably observance would be highest shortly after conception when many women do not yet know they are pregnant. Therefore, our ITT estimates of first month exposure are probably closest to the effect of observance. Interestingly, however, our effects appear to rise monotonically over the course of the first trimester. Indeed, the largest effects appear to be in the third month of gestation when we speculate that Ramadan would not be universally observed, suggesting that the effects on cognitive development may be particularly large at this stage. Future research that can combine information on Ramadan observance over the course of pregnancy with a credible research design may be able to better sharpen our understanding of these patterns of effects.

As a falsification exercise, Table 3 shows the coefficients on Caribbean students, our control group. Importantly, we find no instances of negative effects of Ramadan exposure on our various outcomes that are quantitatively or statistically meaningful.[21] This is reassuring since it suggests that the negative effects on Muslims are not driven by other factors such as residual seasonality that may confound Ramadan exposure.

3.2 Evidence on Selective Timing of Pregnancies

Our identifying assumption is that there is no systematic selection with respect to the unobserved characteristics of Muslims who conceive relative to the timing of Ramadan. For example, if there were some reason that Muslims of lower socioeconomic status were more likely to conceive in the three months prior to Ramadan, then this might provide an alternate explanation for our findings of strong effects in the first

---

[20] However, we find that when we control for school fixed effects that the difference in difference effects are a bit weaker and are generally no longer statistically significant (see Appendix Table 4). However, those conceived during Ramadan still have statistically significantly lower scores. Furthermore, if we just use a sample of Muslims with school fixed effects, the effects remain statistically significant across the first trimester.

[21] We do find that 3 out of the 44 coefficients in columns 1 to 4 of Table 3 show effects that are statistically significant at the 5% level which is roughly what one might expect to find purely by chance. However, all of these are positive effects and none occur in months 1 through 3 where we find our largest effects on Muslims. For Muslims, 13 of the 44 coefficients in Table 2 are negative and significant at the 5% level and another 7 are negative and significant at the 10% level. Importantly every coefficient for Muslims in the first trimester (months 0 to 3) is negative and statistically significant at either the 5 or 10% level.

trimester. A detailed analysis of selection on observables by Almond and Mazumder (2011) using Michigan natality data found no evidence of selection bias in terms of the timing of pregnancies relative to Ramadan using variables such as parental education, maternal smoking behavior or a Medicaid receipt (a proxy for income). Van Ewijk (2011) finds no differences in parental health, income, and assets between Indonesian children by their *in utero* exposure during a Ramadan. Van Ewijk (2011) further shows that estimates of health effects of prenatal Ramadan exposure on children's health are robust to the inclusion of mother fixed effects, suggesting that any forms of selective timing of pregnancy must have been specific to each child.

We re-examine the possibility of selective timing of pregnancy with our British educational data by running our statistical model using Free School Meal status (FSM) as a dependent variable. Free School Meal eligibility is a commonly used proxy for low socioeconomic status (SES) since it is means tested. The results of this exercise are shown in column (7) of Table 2. We find that Muslims who were *in utero* during the first three months of pregnancy when Ramadan began –the period where we find consistent effects on achievement– were no more likely to be eligible for FSM. Further looking at months 1 through 9, five of the months actually have negative coefficients suggesting that Ramadan was associated with lower rates of FSM, or higher SES.

We do note, however, that Muslims exposed to Ramadan in the seventh month of pregnancy and those who were conceived during Ramadan are slightly more likely to receive FSM. While it is possible that the latter result could reflect some type of actual behavioral difference during Ramadan between low and high socioeconomic status Muslims (perhaps because of differences in levels of observance or differences in sexual practices) we are somewhat doubtful of this. One reason for our skepticism is that those who we label as 'probably not exposed', many of whom were conceived well after Ramadan ended, have even *higher* rates of FSM. It would be surprising if lower socioeconomic status Muslims were more likely to conceive both during Ramadan and after Ramadan ended, if this was due to a behavioral difference associated with Ramadan. Finally, even if it were the case that more low income Muslim women systematically chose to conceive during Ramadan, this would not explain the pattern of results we find of negative effects for those women for whom Ramadan began during one of the first three months of pregnancy, and whose conception therefore, preceded Ramadan.[22][23]

As Figure 1 showed, there are strong seasonal patterns in test scores. Much of this is likely to result from relative age effects, with August-born students being almost a year younger than their September-

---

[22] We also ran our model with idaci (Income Deprivation Affecting Children Index) as the dependent variable instead of FSM. Idaci measures per super output area the share of children living in income deprived families. We found no associations between prenatal Ramadan exposure and idaci in area of residence.

[23] We also tried estimating our regressions separately for children who did vs. did not receive FSM. We found that point estimates for the latter were a bit larger in an absolute sense. This could mean that fasting rates among high-SES women are higher, or that the effect of fasting among high-SES individuals is stronger.

born peers at the moment of the test. If, however, this pattern also partially results from selective fertility and if, furthermore, seasonal patterns in test scores differ between ethnicities due to differences in selective fertility over the Gregorian year, this might pose a problem for our difference-in-difference strategy.[24] If fertility patterns differ between ethnicities, this would likely show up in the numbers of children being born at different times of the year.

Figure 5 shows the regression-adjusted distribution of births over the year for Muslims and Caribbeans. If births were uniformly distributed, 0.274% of births would occur on each day (reflected by the horizontal line). Overally, the birth patterns of both groups are highly comparable, speaking against differential seasonality.[25]

Our concerns regarding seasonality are a major reason why we chose to use Key Stage 1 scores where we cover 10 birth cohorts rather than Key Stage 2 scores where we only cover 8 cohorts. We also prefer Key Stage 1 scores since they provide us with larger samples, are measured early in the life cycle (at age 7 rather than age 11), and may be advantageous since they utilise the assessments of teachers taken at the end of the school year after having observed students over a long period of time. Nevertheless, we have also run our results using Key Stage 2 math and language scores as an outcome. Overall, we find somewhat weaker results when we use Key Stage 2 scores. We still find statistically significant effects for month 0 exposure that are consistent across all outcomes and are robust to whether we use a simple difference or a difference in difference. This is consistent with the results on adult outcomes in Almond and Mazumder (2011) who also find large effects from very early exposure in pregnancy. However, we do not find statistically significant negative effects of Ramadan exposure during the entirety of the first trimester as we did when using Key Stage 1 scores.[26] Therefore, we think it would be useful for future researchers to continue to examine how in utero Ramadan exposure affects academic performance and how these effects may differ at different ages.

3.3 Robustness Check Using White British

To probe whether our difference-in-difference estimates are an artifact of the Caribbean control group, we have also run the same specification using British students as controls. Although this comes at the expense of using an arguably less comparable control group, it increases the sample size to around 4.6

---

[24] Given the timing of Ramadan during our sample period (December to April), and the significantly estimated effects from first trimester exposure, we might have been concerned if there were very different fertility patterns between the two ethnic groups with respect to births occurring between June and November compared to births occurring between December to May. We find no evidence suggesting that the seasonal patterns differ in this way.

[25] Our regression controls for the Ramadan exposure measures and was weighed by cohort size. We took the five day moving averages of the residuals. Adjusting for Ramadan exposure addresses the fact that prenatal Ramadan exposure may lead to miscarriages and stillbirths in various periods of gestation (Almond and Mazumder, 2011; Van Ewijk, 2011). Appendix Figure 1 shows the same results when we do not control for Ramadan exposure.

[26] In many cases the coefficients are still negative but the sizes of the coefficients are smaller. We also find some small but negative coefficients for Caribbeans which may influence the difference in difference estimates. In this case the simple differences among Muslims only, are a bit more in line with our results with Key Stage 1 scores. Similarly, our difference in difference estimates with White British are also more consistent with our KS 1 results.

million observations and thereby provides much greater precision. The results for the three subject tests as well as for the principal component are shown in Table 4 and plotted in Figure 6. We once again find that early exposure is associated with lower assessments in all subjects. All estimates lie within the confidence intervals of the estimates for Caribbeans, and, in part due to the increased precision, now also some estimates for exposure during later months of pregnancy become significant. Table 5 shows the coefficients on our control group of white British students. As was the case for Caribbeans, we find no pattern of quantitatively meaningful coefficients; the coefficients are estimated much more precisely and tend to lie even closer to zero.

3.4 Robustness Check: Placebo Treatment

As an additional check on our identification strategy, we conducted a 'placebo' test by estimating the same difference in difference regression using Caribbeans as the treatment group and white British as the control group. We would be concerned if we found 'effects' on the Caribbean group who we know are not observing Ramadan. Any such placebo effects might suggest that our specification is not adequately dealing with seasonality. The results are shown in Figure 7. We find that only 1 out of the 44 coefficients is statistically significant at the 5% level. Further, there does not appear to be any systematic pattern in the results that would suggest that our first trimester effects on Muslims are in any way an artifact of seasonality.

4 Conclusion

Ramadan lasts just one lunar month, yet our results suggest that fleeting alterations to the prenatal environment potentially have ramifications on lifelong human capital. Most commonly studied educational interventions reflect investments that occur over much longer periods of time, may be subject to 'fade out', and are much more costly to undertake. Nevertheless, the magnitude of the effects of prenatal exposure to Ramadan are very similar to the size of the treatment effects of 'later-life' interventions. Dobbie and Fryer (2011) summarise the effects of successful educational interventions and the magnitude of their effects on student performance. Charter schools in New York were found to increase test scores by 0.09 standard deviations, Teach for America raised math and reading scores by 0.15 and 0.03 standard deviations (respectively), and Head Start increased scores on applied problems by 0.15 standard deviations. Our lower bound estimates suggest that Ramadan observance in the first trimester of pregnancy reduces academic scores by between 0.05 and 0.08 standard deviations. This suggests relatively low cost investments in the prenatal environment may yield high returns.

The only previous study to consider the effects of prenatal Ramadan fasting on *school age* outcomes found no effect (Azizi *et al.*, 2004).[27] If as we suspect, the human capital effects we find are unknown to Muslim parents, postponing the Ramadan fast until after pregnancy may offer a low cost route to improved outcomes. The fact that Ramadan fasting alters the biochemical characteristics of the intra-uterine environment in a way similar to other restrictions on the timing of prenatal nutrition, suggests that these effects may also generalise to non-Muslims. Future research should seek design-based approaches to assess the effects of dieting, meal-skipping (reported by 24% of pregnant mothers in US, see Siega-Riz *et al.* (2001)), and nausea and vomiting during early pregnancy, which can all affect the timing of nutritional intake. In the case of dieting and meal skipping, these potentially harmful behaviors tend to be more common early in pregnancy, especially prior to the pregnancy being recognised, see e.g. Ebrahim *et al.* (2000). An important caveat to our analysis is that we cannot distinguish the effects of fasting from other activities that take place during Ramadan. Furthermore, our results pertain to a specific Muslim population of children from South Asia living in England. Future research targeting other Muslim populations would be useful for validating our findings. In addition, subsequent studies might consider the effect of technologies and interventions that enable pregnancies to be recognised earlier in gestation, and thereby enable behavioral change.

Columbia University, NBER
Federal Reserve Bank of Chicago
University Medical Centre Mainz - IMBEI, University of Mainz, VU University Amsterdam, Netspar

Submitted: 29 May 2012
Accepted: 04 April 2014

---

[27] See our discussion of this paper in section 1.3

References

Aizer, A., Stroud, L. and Buka, S. (2009). 'Maternal stress and child well-being: Evidence from siblings', Manuscript, Brown University.

Almond, D., Edlund, L. and Palme, M. (2009). 'Chernobyl's subclinical legacy: Prenatal exposure to radioactive fallout and school outcomes in Sweden', *Quarterly Journal of Economics*, vol. 124(4), pp. 1729–1772.

Almond, D. and Mazumder, B. (2011). 'Health capital and the prenatal environment: The effect of ramadan observance during pregnancy', *American Economic Journal: Applied Economics*, vol. 3(4).

Alwasel, S., Abotalib, Z., Aljarallah, J., Osmond, C., Alkharaz, S., Alhazza, I., Badr, G. and Barker, D. (2010). 'Changes in placental size during ramadan', *Placenta*, vol. 31(7), pp. 607 – 610.

Arab, M. and Nasrollahi, S. (2001). 'Interrelation of ramadan fasting and birth weight', *Medical Journal of the Islamic Academy of Sciences*, vol. 14(3), pp. 91–95.

Azizi, F., Sadeghipour, H., Siahkolah, B. and Rezaei-Ghaleh, N. (2004). 'Intellectual development of children born to mothers who fasted during pregnancy', *International Journal of Vitamin and Nutrition Research*, vol. 74(5), pp. 374–380.

Cascio, E. and Staiger, D. (2012). 'Knowledge, tests, and fadeout in educational interventions', National Bureau of Economic Research.

Chetty, R., Friedman, J.N., Hilger, N., Saez, E., Schanzenbach, D.W. and Yagan, D. (2011). 'How does your kindergarten classroom affect your earnings? evidence from project star', *Quarterly Journal of Economics*, vol. 126(4), pp. 1593–1660.

Crawford, C., Dearden, L. and Meghir, C. (2007). 'When you are born matters: The impact of date of birth on child cognitive outcomes in england', Institute for Fiscal Studies Research Paper.

Currie, J. (2009). 'Healthy, wealthy, and wise: Is there a causal relationship between child health and human capital development?', *Journal of Economic Literature*, vol. XLVII(1), pp. 87–122.

Currie, J. and Hyson, R. (1999). 'Is the impact of shocks cusioned by socioeconomic status? the case of low birth weight', *American Economic Review*, vol. 89(2), pp. 245–250.

Dikensoy, E., Balat, O., Cebesoy, B., Ozkur, A., Cicek, H. and Can, G. (2009). 'The effect of Ramadan fasting on maternal serum lipids, cortisol levels and fetal development.', *Archives of Gynecology and Obstetrics*, vol. 279(2), p. 119.

Dobbie, W. and Fryer, R.G.J. (2011). 'Are high quality schools enough to increase achievement among the poor? evidence from the harlem children's zone', *American Economic Journal: Applied Economics*, vol. 3(3), pp. 158–187.

Doblhammer, G. and Vaupel, J.W. (2001). 'Lifespan depends on month of birth', *Proceedings of the National Academy of Sciences*, vol. 98(5), pp. 2934–2939.

Doyle, O., Harmon, C.P., Heckman, J.J. and Tremblay, R.E. (2009). 'Investing in Early Human Development: Timing and Economic Efficiency', *Economics and Human Biology*, vol. 7(1), pp. 1–6.

Dustmann, C., Machin, S. and Schönberg, U. (2010). 'Ethnicity and educational achievement in compulsory schooling', *The Economic Journal*, vol. 120(546), pp. F272–F297.

Eaton, P. and Wharton, P. (1982). *Applied Nutrition*, chap. Sorrento Study of Nutrition in Pregnancy, London: John Libbey, pp. 89–91.

Ebrahim, S.H., Floyd, R.L., Merritt II, R.K., Decoufle, P. and Holtzman, D. (2000). 'Trends in Pregnancy-Related Smoking Rates in the United States, 1987-1996', *JAMA*, vol. 283(3), pp. 361–366.

Field, E., Robles, O. and Torero, M. (2009). ‘Iodine deficiency and schooling attainment in tanzania’, *American Economic Journal: Applied Economics*, vol. 1(4), pp. 140–169.

Gluckman, P. and Hanson, M. (2005). *The Fetal Matrix: Evolution, Development and Disease*, Cambridge: Cambridge University Press.

Gregg, P., Washbrook, E., Propper, C. and Burgess, S. (2005). ‘The effects of a mother’s return to work decision on child development in the uk’, *The Economic Journal*, vol. 115(501), pp. F48–F80.

Heckman, J., Malofeeva, L., Pinto, R. and Savelyev, P. (2010). ‘Understanding the mechanisms through which an influential early childhood program boosted adult outcomes’, Manuscript, University of Chicago.

Heckman, J.J. (2007). ‘The economics, technology, and neuroscience of human capability formation’, *PNAS*, vol. 104(33), pp. 13250–13255.

Heckman, J.J. and Masterov, D.V. (2007). ‘The productivity argument for investing in young children’, *Review of Agricultural Economics*, vol. 29(3), pp. 446–493.

Hunter, E. and Sadler, T. (1987). ‘D-(-)-beta hydroxybutyrate-induced effects on mouse embryos in vitro’, *Teratology*, vol. 36, pp. 259–64.

Joosoph, J., Abu, J., Yu, S. *et al.* (2004). ‘A survey of fasting during pregnancy’, *Singapore Med J*, vol. 45(12), pp. 583–586.

Kane, T. and Staiger, D. (2002). ‘The promise and pitfalls of using imprecise school accountability measures’, *The Journal of Economic Perspectives*, vol. 16(4), pp. 91–114.

Kapoor, A., Dunn, E., Kostaki, A., Andrews, M.H. and Matthews, S.G. (2006). ‘Fetal programming of hypothalamo-pituitary-adrenal function: Prenatal stress and glucocorticoids’, *Journal of Physiology*, vol. 572(1), pp. 31–44.

Kelly, E. (2011). ‘The scourge of asian flu: in utero exposure to pandemic influenza and the development of a cohort of british children’, *The Journal of Human Resources*, vol. 46(4), pp. 669–694.

Kieler, H., Axelsson, O., Nilsson, S. and Waldenstroem, U. (1995). ‘The length of human pregnancy as calculated by ultrasonographic measurement of the fetal biparietal diameter’, *Ultrasound in Obstetrics and Gynecology*, vol. 16(5), pp. 353–357.

Krueger, A.B. (1999). ‘Experimental estimates of education production functions’, *The Quarterly Journal of Economics*, vol. 114(2), pp. 497–532.

Krueger, A.B. and Whitmore, D.M. (2001). ‘The effect of attending a small class in the early grades on college-test taking and middle school test results: Evidence from project star’, *The Economic Journal*, vol. 111(468), pp. 1–28.

LeWinn, K.Z., Stroud, L.R., Molnar, B.E., Ware, J.H., Koenen, K.C. and Buka, S.L. (2009). ‘Elevated maternal cortisol levels during pregnancy are associated with reduced childhood iq’, *International Journal of Epidemiology*, vol. 38(6), pp. 1700–1710.

Malhotra, A., Scott, P., Scott, J., Gee, H. and Wharton, B. (1989). ‘Metabolic changes in asian muslim pregnant mothers observing the ramadan fast in britian’, *British Journal of Nutrition*, vol. 61(3), pp. 663–672.

Prentice, A., Prentice, A., Lamb, W., Lunn, P. and Austin, S. (1983). ‘Metabolic consequences of fasting during ramadan in pregnant and lactating women’, *Human nutrition. Clinical nutrition*, vol. 37(4), pp. 283–94.

Rizzo, T., Metzger, B., Burns, W. and Burns, K. (1991). ‘Correlations between antepartum maternal metabolism and child intelligence’, *New England Journal of Medicine*, vol. 325, pp. 911–916.

Rothstein, J. (2010). ‘Teacher quality in educational production: Tracking, decay, and student achievement’, *The Quarterly Journal of Economics*, vol. 125(1), pp. 175–214.

Siega-Riz, A.M., Hermann, T., Savitz, D.A. and Thorp, J.M. (2001). ‘Frequency of eating during pregnancy and its effect on preterm delivery’, *American Journal of Epidemiology*, vol. 153(7), pp. 647–652.

Van Ewijk, R. (2011). ‘Long-term health effects on the next generation of ramadan fasting during pregnancy’, *Journal of Health Economics*, vol. 30, pp. 1045–1058.

Table 1: *Summary Statistics*

		Designated Muslims	Caribbeans	White British	Indians	Other
Math	Mean	-0.36	-0.20	0.04	0.05	-0.08
	(SD)	(1.04)	(0.99)	(0.99)	(0.97)	(1.03)
	N	221,873	106,543	4,426,857	106,543	770,412
Reading	Mean	-0.38	-0.14	0.04	0.05	-0.10
	(SD)	(1.01)	(1.00)	(0.99)	(0.92)	(1.05)
	N	221,855	106,538	4,426,772	116,612	770,350
Writing	Mean	-0.31	-0.13	0.03	0.09	-0.09
	(SD)	(1.06)	(1.02)	(0.99)	(0.94)	(1.05)
	N	221,856	106,540	4,426,696	116,612	770,333
First Principal Component	Mean	-0.60	-0.28	0.06	0.11	-0.15
	(SD)	(1.66)	(1.59)	(1.55)	(1.49)	(1.65)
	N	221,818	106,522	4,426,410	116,602	770,132
Free School Meal	Mean	0.35	0.34	0.16	0.11	0.26
	(SD)	(0.48)	(0.47)	(0.36)	(0.32)	(0.44)
	N	221,902	106,555	4,426,973	116,625	770,569

Notes. The first principal component is based on a principal components analysis of math, reading, and writing.

Table 2: *Effects of Prenatal Ramadan Exposure on KS 1 Scores of Designated Muslims*

*Coefficients on Muslim * Ramadan Exposure*

	Dependent Variable						
Month Ramadan Began	Math (1)	Reading (2)	Writing (3)	PCA (4)	Math (5)	Math (6)	FSM (7)
Probably	-0.003	-0.051 **	-0.031	-0.051	-0.004	0.005	0.027 **
Not Exposed	(0.026)	(0.024)	(0.026)	(0.040)	(0.022)	(0.015)	(0.012)
0	-0.054 **	-0.049 **	-0.051 **	-0.089 **	-0.039 **	-0.019	0.022 **
(conceived)	(0.023)	(0.022)	(0.023)	(0.036)	(0.02)	(0.012)	(0.011)
1	-0.068 **	-0.054 **	-0.052 *	-0.100 **	-0.045 *	-0.031 *	0.011
	(0.028)	(0.028)	(0.028)	(0.044)	(0.024)	(0.016)	(0.013)
2	-0.059 *	-0.067 **	-0.053 *	-0.103 **	-0.057 **	-0.046 **	0.006
	(0.032)	(0.032)	(0.032)	(0.050)	(0.028)	(0.018)	(0.015)
3	-0.081 **	-0.073 **	-0.055 *	-0.120 **	-0.063 **	-0.048 **	-0.004
	(0.033)	(0.032)	(0.032)	(0.051)	(0.028)	(0.019)	(0.015)
4	-0.046	-0.038	-0.036	-0.069	-0.047 *	-0.035 **	-0.002
	(0.029)	(0.029)	(0.030)	(0.047)	(0.026)	(0.017)	(0.014)
5	-0.023	-0.021	-0.023	-0.039	-0.032	-0.020	-0.003
	(0.028)	(0.027)	(0.028)	(0.044)	(0.024)	(0.015)	(0.013)
6	-0.022	-0.025	-0.011	-0.034	-0.016	-0.007	0.007
	(0.026)	(0.026)	(0.027)	(0.041)	(0.023)	(0.014)	(0.012)
7	-0.037	-0.033	-0.024	-0.054	-0.025	-0.012	0.026 **
	(0.025)	(0.025)	(0.025)	(0.040)	(0.022)	(0.014)	(0.012)
8	-0.027	-0.032	-0.016	-0.042	-0.025	-0.003	-0.001
	(0.023)	(0.023)	(0.023)	(0.036)	(0.02)	(0.012)	(0.011)
9	-0.024	-0.036 *	-0.040 *	-0.058 *	-0.015	-0.002	-0.007
(born)	(0.021)	(0.020)	(0.021)	(0.032)	(0.018)	(0.011)	(0.010)
Output Area FE's	Yes	Yes	Yes	Yes	No	Yes	Yes
Diff in Diff	Yes	Yes	Yes	Yes	Yes	No	Yes
N	326,549	326,526	326,529	326,549	326,549	220,844	326,592

Notes. Each column is a separate regression with dummies for month of birth, female and free school meal eligibility, and a cubic in the number of days between the date of birth and January 1 1960 as covariates. Standard errors are clustered at the school level. Columns 1 to 5 and 7 include Caribbeans and show the interaction of the exposure measure with a dummy for Muslim; all covariates in these regressions, except for the geographic fixed effects, are interacted with a dummy for 'Muslim'. The excluded Ramadan measure in all regressions is 'certainly not exposed by virtue of birth date'. The coefficients on Caribbeans are shown in Table 3. Column 6 only uses Muslims. PCA refers to the first principal component of math, reading and writing. FSM refers to free school meal status.

$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 3: *Prenatal Ramadan Exposure and KS 1 Scores of Caribbeans*

Coefficients on Ramadan Exposure for Caribbeans

	Dependent Variable						
Month Ramadan Began	Math (1)	Reading (2)	Writing (3)	PCA (4)	Math (5)	Math (6)	FSM (7)
Probably	0.007	0.042 **	0.016	0.040	0.018	–	-0.020 **
Not Exposed	(0.021)	(0.020)	(0.021)	(0.033)	(0.018)		(0.010)
0	0.038 *	0.043 **	0.036 *	0.067 **	0.025	–	-0.007
(conceived)	(0.020)	(0.019)	(0.019)	(0.031)	(0.016)		(0.009)
1	0.040 *	0.036	0.038	0.065 *	0.018	–	-0.005
	(0.023)	(0.023)	(0.023)	(0.037)	(0.02)		(0.011)
2	0.012	0.027	0.008	0.027	0.004	–	0.006
	(0.026)	(0.027)	(0.026)	(0.042)	(0.023)		(0.013)
3	0.036	0.040	0.007	0.047	0.014	–	0.014
	(0.027)	(0.027)	(0.027)	(0.043)	(0.023)		(0.013)
4	0.010	0.010	0.004	0.013	0.010	–	0.011
	(0.024)	(0.025)	(0.025)	(0.039)	(0.021)		(0.012)
5	0.005	-0.004	-0.011	-0.007	0.013	–	0.010
	(0.023)	(0.023)	(0.023)	(0.037)	(0.020)		(0.011)
6	0.013	0.007	-0.012	0.005	0.007	–	-0.000
	(0.022)	(0.022)	(0.022)	(0.034)	(0.019)		(0.010)
7	0.028	0.019	0.004	0.028	0.009	–	-0.016
	(0.021)	(0.021)	(0.022)	(0.033)	(0.018)		(0.010)
8	0.027	0.020	0.002	0.028	0.021	–	-0.003
	(0.020)	(0.019)	(0.020)	(0.031)	(0.017)		(0.009)
9	0.023	0.026	0.023	0.042	0.011	–	-0.000
(born)	(0.017)	(0.017)	(0.017)	(0.027)	(0.015)		(0.008)
Output Area FE's	Yes	Yes	Yes	Yes	No	Yes	Yes
Diff in diff	Yes	Yes	Yes	Yes	Yes	No	Yes
N	326,549	326,526	326,529	326,549	326,549	220,844	326,592

Notes. Each column is a separate regression and the columns correspond to those shown in Table 2. Entries show the coefficients on Ramadan exposure among Caribbeans. The excluded Ramadan measure in all regressions is 'certainly not exposed by virtue of birth date'. Covariates included in the regressions are dummies for month of birth, female and free school meal eligibility, and a cubic in the number of days between the date of birth and January 1 1960. Standard errors are clustered at the school level. All covariates in the diff in diff regressions, except for the geographic fixed effects, are interacted with a dummy for 'Muslim'. Column 6 has no entries because only Muslims are included in the regression. PCA refers to the first principal component of math, reading and writing. FSM refers to free school meal status.

$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 4: *Effects of Prenatal Ramadan Exposure on KS 1 Scores of Designated Muslims: Difference-in-difference vs White British*

*Coefficients on Muslim * Ramadan Exposure*

	Dependent Variable			
Month Ramadan Began	Math (1)	Reading (2)	Writing (3)	PCA (4)
Probably	0.013	0.000	-0.006	0.005
Not Exposed	(0.014)	(0.013)	(0.013)	(0.021)
0	-0.022 *	-0.013	-0.021 *	-0.032 *
(conceived)	(0.012)	(0.011)	(0.012)	(0.018)
1	-0.030 **	-0.027 *	-0.012	-0.041 *
	(0.015)	(0.014)	(0.015)	(0.023)
2	-0.057 ***	-0.052 ***	-0.041 **	-0.087 ***
	(0.017)	(0.017)	(0.017)	(0.027)
3	-0.057 ***	-0.047 ***	-0.045 **	-0.086 ***
	(0.018)	(0.017)	(0.017)	(0.028)
4	-0.043 ***	-0.035 **	-0.026	-0.061 **
	(0.016)	(0.015)	(0.016)	(0.025)
5	-0.027 *	-0.030 **	-0.028 *	-0.050 **
	(0.014)	(0.014)	(0.015)	(0.023)
6	-0.015	-0.023 *	-0.022	-0.035 *
	(0.014)	(0.013)	(0.014)	(0.021)
7	-0.018	-0.018	-0.023 *	-0.035 *
	(0.013)	(0.013)	(0.013)	(0.021)
8	-0.001	-0.009	-0.010	-0.012
	(0.012)	(0.012)	(0.012)	(0.019)
9	-0.005	-0.016	-0.018 *	-0.023
(born)	(0.011)	(0.010)	(0.011)	(0.017)
Output Area FE's	Yes	Yes	Yes	Yes
Diff in Diff	Yes	Yes	Yes	Yes
N	4,629,471	4,629,367	4,629,293	4,628,971

Notes. Each column is a separate regression. Regressions include white British and show the interaction of the exposure measure with a dummy for Muslim. The excluded Ramadan measure in all regressions is 'certainly not exposed by virtue of birth date'. The coefficients on white British are shown in Table 5. Covariates included in the regressions are dummies for month of birth, female and free school meal eligibility, and a cubic in the number of days between the date of birth and January 1 1960. All covariates, except for the geographic fixed effects, are interacted with a dummy for 'Muslim'. Standard errors are clustered at the school level. PCA refers to the first principal component of math, reading and writing.

$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Table 5: *Prenatal Ramadan Exposure and KS 1 Scores of White British*

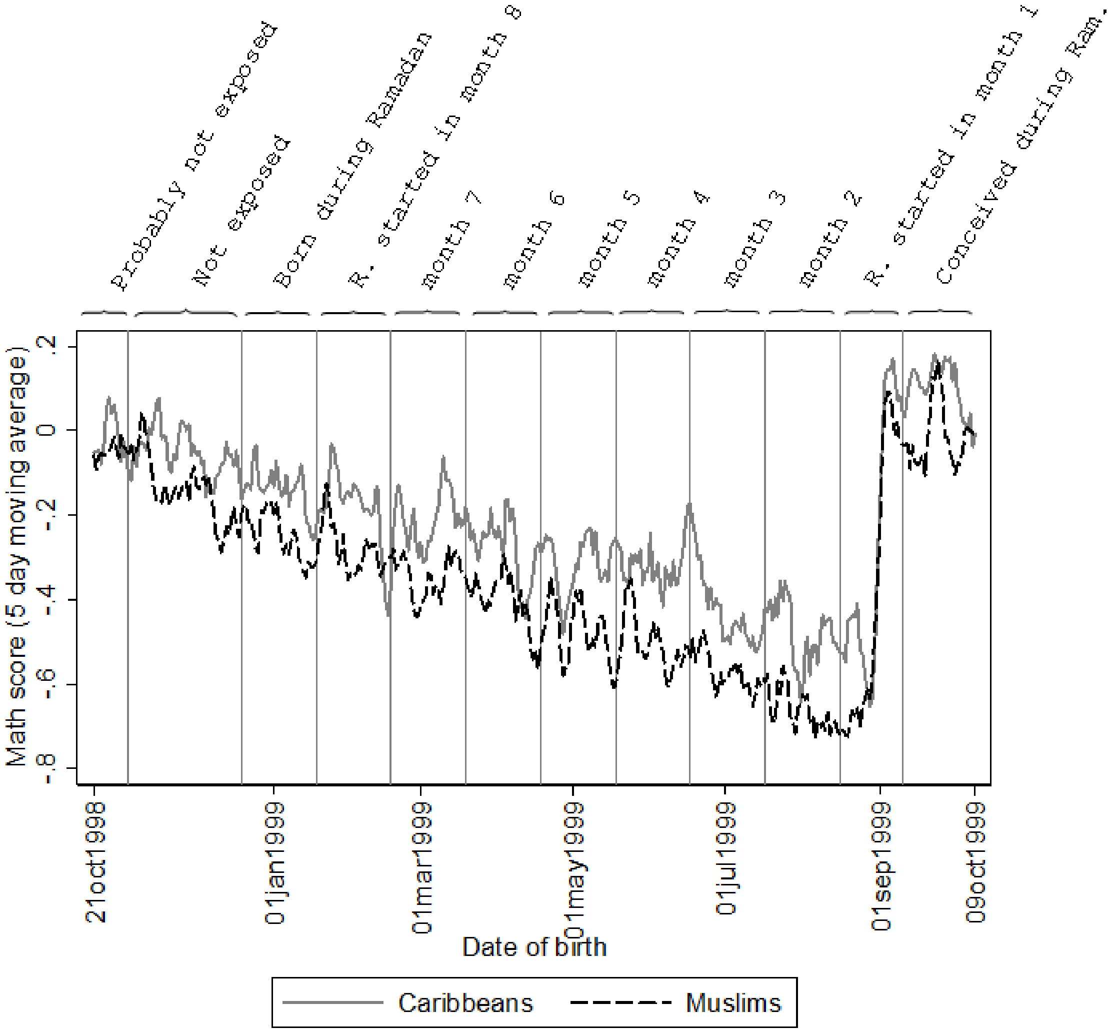
Coefficients on Ramadan Exposure for white British

Month Ramadan Began	Math (1)	Reading (2)	Writing (3)	PCA (4)
	Dependent Variable			
Probably	-0.004	-0.006 **	-0.003	-0.008 *
Not Exposed	(0.003)	(0.003)	(0.003)	(0.004)
0	0.002	0.000	0.001	0.002
(conceived)	(0.002)	(0.002)	(0.002)	(0.004)
1	-0.001	-0.000	-0.003	-0.002
	(0.003)	(0.003)	(0.003)	(0.005)
2	0.001	0.002	-0.009 ***	-0.004
	(0.003)	(0.003)	(0.003)	(0.005)
3	0.003	0.004	-0.005	0.001
	(0.004)	(0.003)	(0.003)	(0.005)
4	0.005	0.004	-0.004	0.002
	(0.003)	(0.003)	(0.003)	(0.005)
5	0.004	0.004	-0.002	0.004
	(0.003)	(0.003)	(0.003)	(0.004)
6	0.002	0.002	-0.002	0.001
	(0.003)	(0.003)	(0.003)	(0.004)
7	0.001	-0.001	-0.001	-0.000
	(0.003)	(0.003)	(0.003)	(0.004)
8	-0.002	-0.002	-0.002	-0.004
	(0.002)	(0.002)	(0.002)	(0.004)
9	-0.001	0.001	-0.004	-0.002
(born)	(0.002)	(0.002)	(0.002)	(0.003)
Output Area FE's	Yes	Yes	Yes	Yes
Diff in diff	Yes	Yes	Yes	Yes
N	4,629,471	4,629,367	4,629,293	4,628,971

Notes. Each column is a separate regression and the columns correspond to those shown in Table 4. Entries show the coefficients on Ramadan exposure among white British. The excluded Ramadan measure in all regressions is 'certainly not exposed by virtue of birth date'. Covariates included in the regressions are dummies for month of birth, female and free school meal eligibility, and a cubic in the number of days between the date of birth and January 1 1960. All covariates, except for the geographic fixed effects, are interacted with a dummy for 'Muslim'. Standard errors are clustered at the school level. PCA refers to the first principal component of math, reading and writing.

$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Figure 1: *Average Math Scores by Date of Birth and Timing of Ramadan Exposure for One Islamic Year*



Notes. Figure shows 5-day moving average Math scores for Muslims and Caribbeans born in one Islamic year (Oct. 21, 1998 to Oct. 9, 1999). The horizontal axis shows dates of birth. In the depicted year, Ramadan started on Dec. 19, and ended on Jan. 17. Vertical sections indicate periods of pregnancy during which exposure could have taken place. Months are calculated as 30-day periods. Note that the period ‘Ramadan started in month 1 of pregnancy’ is a few days shorter than the periods ‘Ramadan started in month 2-9 of pregnancy’. The reason is that an average pregnancy takes 266 days, which is somewhat less than nine (30-day) months. Calculating backwards, starting from the date of birth, the ‘month 1’-group consists of 26 days.

Figure 2: *Share of Pakistani/Bangladeshi Students by Local Authority, England 1998-2007*

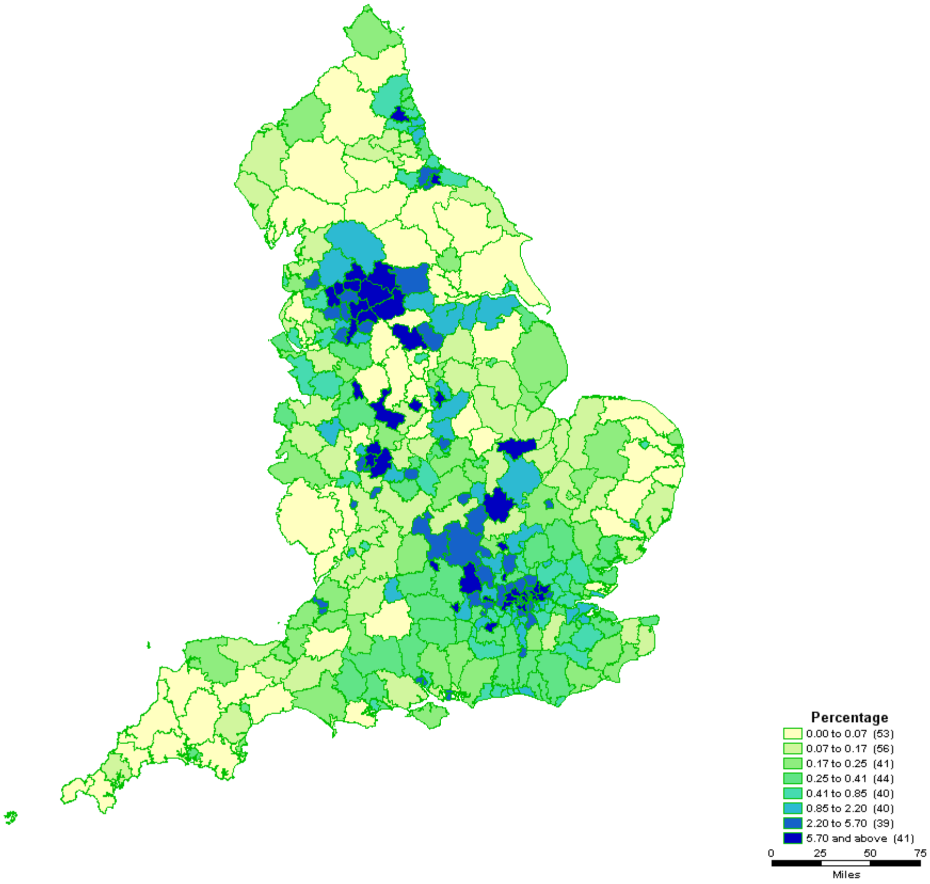
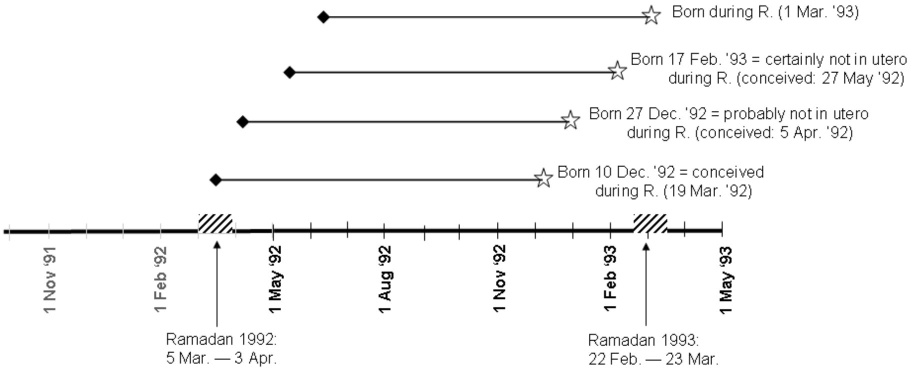
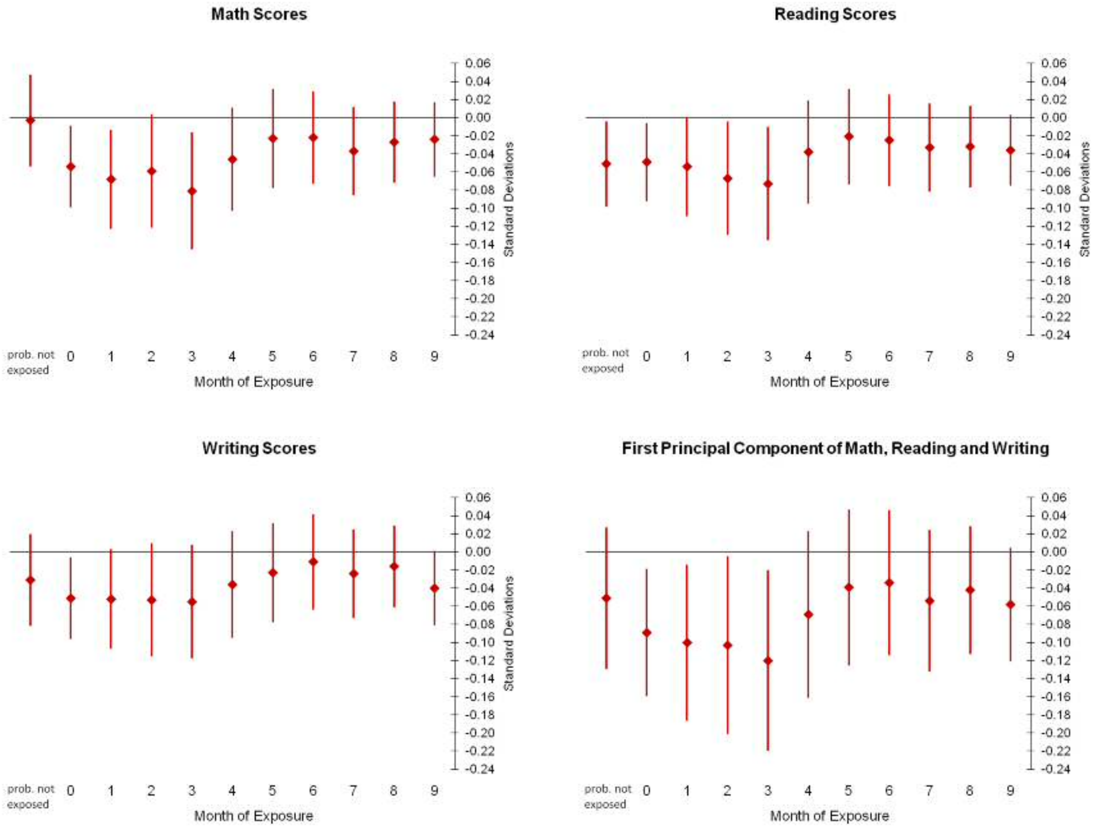


Figure 3: *Calculating whether a Person was in Utero during a Ramadan. Example: People Born in 1992/3*



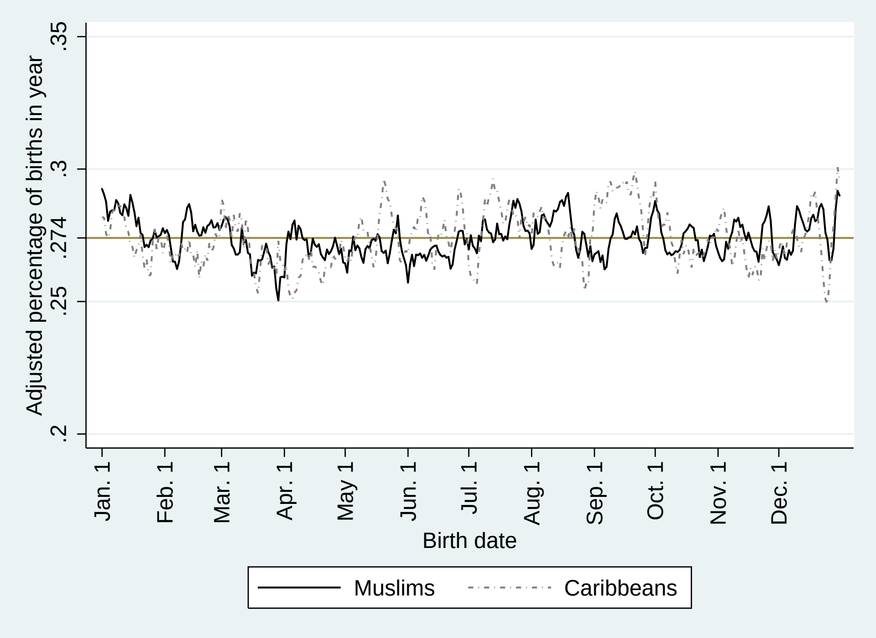
Notes. Figure shows people born between 1 Nov. 1991 and 1 May 1993. Stars indicate birthdates, diamonds the calculated day of conception. Each line is 266 days long (the average length of human gestation). The shaded areas indicate Ramadans.

Figure 4: *Prenatal Ramadan Exposure and Key Stage 1 Scores: Difference-in-differences – Designated Muslims vs Caribbeans*



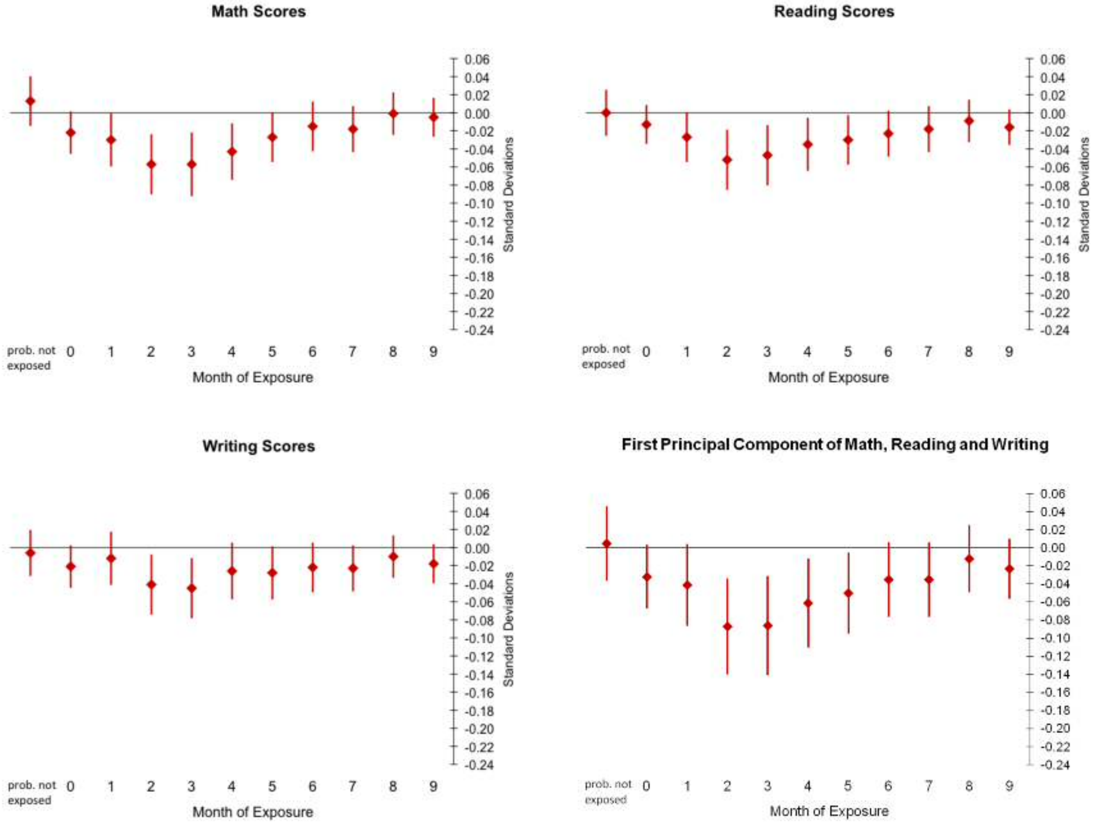
Notes. Figures show 95% confidence intervals for the interactions of Ramadan exposure with a dummy for Muslim, from difference-in-differences estimates for Muslims vs Caribbeans (see Table 2). Math, Reading and Writing are in units of z-scores. The first principal component of math, reading and writing has a standard deviation of 1.58.

Figure 5: *Five-day Moving Averages of Percentages of Births Occurring per Date - Adjusted for Ramadan Exposure*



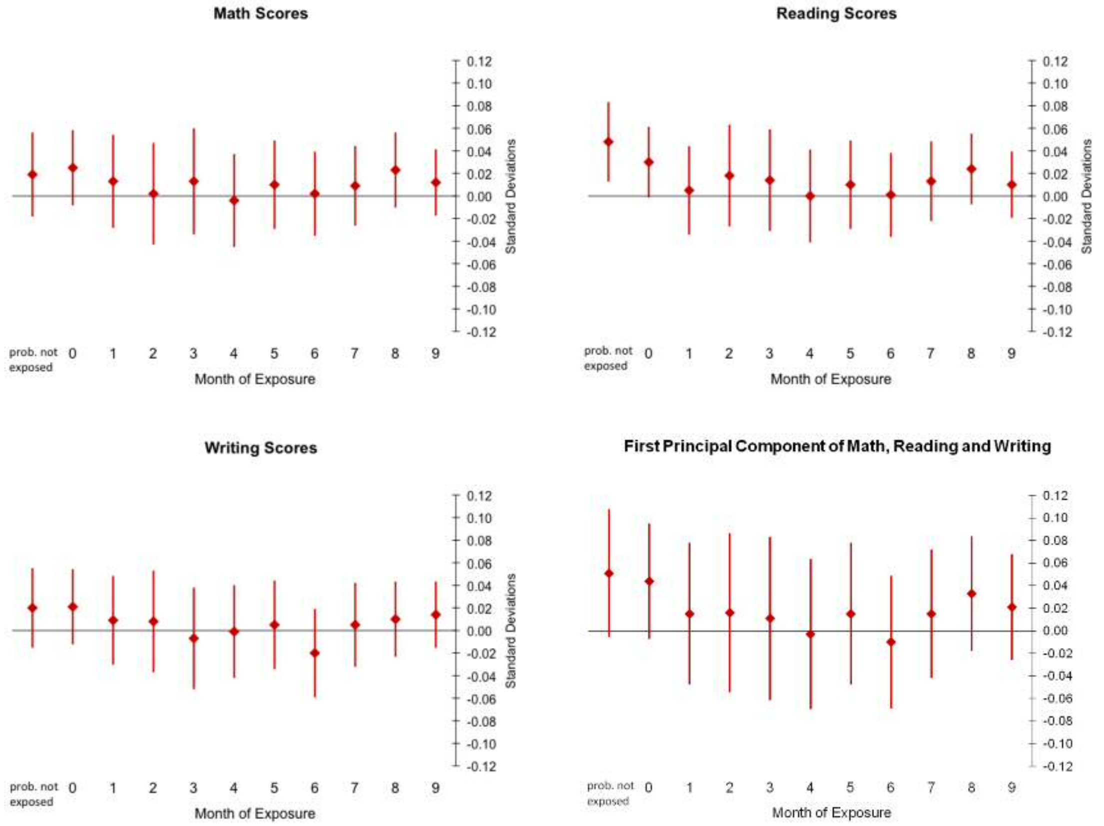
Notes. Per ethnic group, we calculated the share of births within a cohort that took place on each date and regressed these shares on our Ramadan exposure indicators. Regressions were weighted by cohort size. We took the residuals and added 0.274, to obtain a centering around the expected percentage of births per day (1/365) and took 5-day moving averages.

Figure 6: *Prenatal Ramadan Exposure and Key Stage 1 Scores: Difference-in-differences – Designated Muslims vs White British*



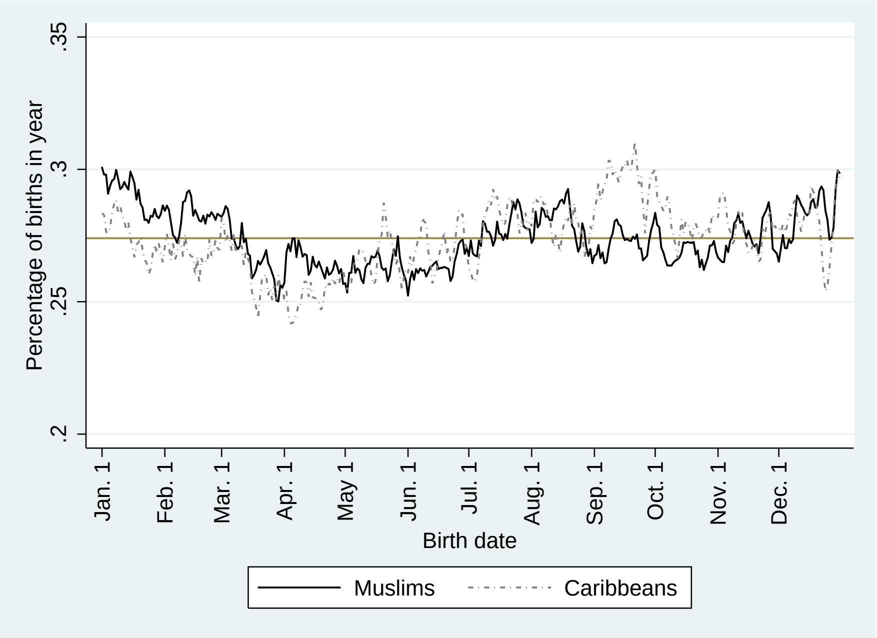
Notes. Figures show 95% confidence intervals for the interactions of Ramadan exposure with a dummy for Muslim, from difference-in-differences estimates for Muslims vs White British. Total sample size for each analysis is 4.6 million. Math, Reading and Writing are in units of z-scores. The first principal component of math, reading and writing has a standard deviation of 1.58.

Figure 7: *Prenatal Ramadan Exposure and Key Stage 1 scores: Difference-in-differences Caribbeans vs White British*



Notes. Figures show 95% confidence intervals for the interactions of Ramadan exposure with a dummy for Caribbean, from difference-in-differences estimates for Caribbeans vs white British which is used as a placebo test. Math, Reading and Writing are in units of z-scores. The first principal component of math, reading and writing has a standard deviation of 1.58.

Appendix Figure 1: *Five-day Moving Averages of Percentages of Births occurring per Date - Not Adjusted for Ramadan Exposure*



Appendix Table 1: *Distributions of KS1 Scores*

	Math	Reading	Writing
Working towards level 1 (3 points)	1.99%	4.66%	3.13%
Level 1 (9 points)	9.32%	13.59%	12.99%
Level 2C (13 points)	5.43%	6.88%	4.23%
Level 2/2B (15 points)	50.95%	55.28%	44.81%
Level 2A (17 points)	8.83%	6.55%	7.54%
Level 3 (21 points)	23.44%	13.03%	27.24%
Level 4 (27 points)	0.04%	0.02%	0.06%
Total sample size	5,644,923	5,644,655	5,644,745

Table shows the distributions of KS1 scores for the entire sample. Level 2A, 2B and 2C exist from 2004 on.

Before that year, level 2 was used instead.

Appendix Table 2: *Prenatal Ramadan Exposure and Designated Muslims' Probability of Attaining at Least Level 2: Estimates using Linear Probability Models*

*Coefficients on Muslim * Ramadan Exposure*

	Dependent Variable			
Month Ramadan Began	Math (1)	Reading (2)	Writing (3)	Math (4)
Probably	0.004	0.002	0.002	-0.003
Not Exposed	(0.009)	(0.010)	(0.010)	(0.005)
0	-0.017 **	-0.014	-0.020 **	-0.008 *
(conceived)	(0.008)	(0.009)	(0.010)	(0.005)
1	-0.020 **	-0.014	-0.022 *	-0.012 **
	(0.010)	(0.011)	(0.012)	(0.006)
2	-0.012	-0.013	-0.018	-0.012 *
	(0.012)	(0.013)	(0.013)	(0.007)
3	-0.024 *	-0.023 *	-0.027 *	-0.015 **
	(0.013)	(0.014)	(0.014)	(0.007)
4	-0.011	-0.010	-0.012	-0.012 *
	(0.012)	(0.012)	(0.013)	(0.007)
5	-0.006	-0.006	-0.010	-0.009
	(0.011)	(0.012)	(0.012)	(0.006)
6	-0.008	-0.009	-0.002	-0.008
	(0.010)	(0.011)	(0.011)	(0.006)
7	-0.016	-0.005	-0.004	-0.011 **
	(0.010)	(0.011)	(0.011)	(0.005)
8	-0.010	-0.015	-0.007	-0.007
	(0.009)	(0.009)	(0.010)	(0.005)
9	-0.006	-0.008	-0.009	-0.001
(born)	(0.008)	(0.008)	(0.009)	(0.004)
Output Area FE's	Yes	Yes	Yes	Yes
Diff in Diff	Yes	Yes	Yes	No
N	326,549	326,526	326,529	220,844

Notes. Each column is a separate regression. Regressions are linear probability models with an indicator for attainment of level 2 as the outcome and with dummies for month of birth, female and free school meal eligibility, and a cubic in the number of days between the date of birth and January 1 1960 as covariates. Standard errors are clustered at the school level. Columns 1 to 3 include Caribbeans and show the interaction of the exposure measure with a dummy for Muslim; all covariates in these regressions, except for the geographic fixed effects, are interacted with a dummy for 'Muslim'. The excluded Ramadan measure in all regressions is 'certainly not exposed by virtue of birth date'. Column 4 only uses Muslims.

$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Appendix Table 3: *Prenatal Ramadan Exposure and Designated Muslims' KS1 Scores: Ordered Probit using Unstandardised Test Scores*

*Coefficients on Muslim * Ramadan Exposure*

	Dependent Variable			
Month Ramadan Began	Math (1)	Reading (2)	Writing (3)	Math (4)
Probably	-0.007	-0.047 *	-0.037	0.017
Not Exposed	(0.025)	(0.025)	(0.025)	(0.014)
0	-0.041 *	-0.032	-0.037 *	-0.013
(conceived)	(0.022)	(0.022)	(0.022)	(0.012)
1	-0.047 *	-0.027	-0.017	-0.025
	(0.027)	(0.028)	(0.027)	(0.015)
2	-0.060 **	-0.069 **	-0.049	-0.056 ***
	(0.030)	(0.031)	(0.030)	(0.017)
3	-0.067 **	-0.054 *	-0.028	-0.047 ***
	(0.030)	(0.031)	(0.030)	(0.018)
4	-0.053 **	-0.044	-0.035	-0.037 **
	(0.027)	(0.028)	(0.028)	(0.016)
5	-0.040	-0.043	-0.034	-0.020
	(0.026)	(0.027)	(0.026)	(0.015)
6	-0.022	-0.029	-0.010	-0.009
	(0.025)	(0.025)	(0.025)	(0.014)
7	-0.027	-0.029	-0.030	-0.012
	(0.024)	(0.024)	(0.024)	(0.013)
8	-0.034	-0.044 **	-0.027	-0.005
	(0.022)	(0.022)	(0.022)	(0.012)
9	-0.019	-0.030	-0.030	-0.007
(born)	(0.020)	(0.020)	(0.020)	(0.011)
Diff in Diff	Yes	Yes	Yes	No
N	328,355	328,332	328,335	221,827

Notes. Each column is a separate ordered probit regression that uses raw (unstandardised) test scores and with dummies for month of birth, female and free school meal eligibility, and a cubic in the number of days between the date of birth and January 1 1960 as covariates. Standard errors are clustered at the school level. . Columns 1 to 3 include Caribbeans and show the interaction of the exposure measure with a dummy for Muslim; all covariates in these regressions, except for the geographic fixed effects, are interacted with a dummy for 'Muslim'. The excluded Ramadan measure in all regressions is 'certainly not exposed by virtue of birth date'. Column 4 only uses Muslims. Ordered probit models do not include fixed effects.

$*p < 0.10$, $**p < 0.05$, $***p < 0.01$

Appendix Table 4: *Prenatal Ramadan Exposure and Designated Muslims' KS1 Scores: Estimates using School Fixed Effects.*

*Coefficients on Muslim * Ramadan Exposure*

	Dependent Variable				
Month Ramadan Began	Math (1)	Reading (2)	Writing (3)	PCA (4)	Math (5)
Probably	-0.001	-0.036 *	-0.026	-0.038	0.010
Not Exposed	(0.022)	(0.022)	(0.023)	(0.035)	(0.013)
0	-0.049 **	-0.037 *	-0.048 **	-0.077 **	-0.024 **
(conceived)	(0.020)	(0.020)	(0.020)	(0.031)	(0.011)
1	-0.040	-0.024	-0.024	-0.050	-0.032 **
	(0.025)	(0.024)	(0.025)	(0.038)	(0.014)
2	-0.043	-0.043	-0.033	-0.069	-0.052 ***
	(0.028)	(0.028)	(0.028)	(0.044)	(0.016)
3	-0.034	-0.014	-0.007	-0.031	-0.047 ***
	(0.029)	(0.029)	(0.029)	(0.045)	(0.017)
4	-0.032	-0.020	-0.020	-0.041	-0.040 ***
	(0.026)	(0.026)	(0.027)	(0.041)	(0.015)
5	-0.029	-0.028	-0.024	-0.047	-0.018
	(0.024)	(0.025)	(0.025)	(0.039)	(0.014)
6	-0.024	-0.023	-0.011	-0.034	-0.007
	(0.023)	(0.023)	(0.024)	(0.037)	(0.013)
7	-0.029	-0.028	-0.032	-0.052	-0.015
	(0.023)	(0.022)	(0.023)	(0.035)	(0.013)
8	-0.029	-0.032	-0.033	-0.054 *	-0.005
	(0.021)	(0.020)	(0.021)	(0.032)	(0.011)
9	-0.021	-0.026	-0.033 *	-0.046	-0.006
(born)	(0.018)	(0.018)	(0.018)	(0.029)	(0.010)
School FE's	Yes	Yes	Yes	Yes	Yes
Diff in Diff	Yes	Yes	Yes	Yes	No
N	328,355	328,332	328,335	328,279	221,827

Notes. Each column is a separate regression with dummies for month of birth, female and free school meal eligibility, and a cubic in the number of days between the date of birth and January 1 1960 as covariates. Standard errors are clustered at the school level. Columns 1 to 3 include Caribbeans and show the interaction of the exposure measure with a dummy for Muslim; all covariates in these regressions, except for the geographic fixed effects, are interacted with a dummy for 'Muslim'. The excluded Ramadan measure in all regressions is 'certainly not exposed by virtue of birth date'. Column 4 only uses Muslims.

$*p < 0.10$, $**p < 0.05$, $***p < 0.01$