

THE HUMAN CAPITAL LEGACY OF A TRADE EMBARGO ^{*}

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Abstract

We estimate the effects of in-utero exposure to a trade embargo on survival and human capital in an import-dependent developing country. Using a sharp regression discontinuity design, we show that a nearly comprehensive embargo imposed by India on Nepal in 1989 led to a close to 30 percent decrease in reported live births the month after it began. Adult survivors of exposure have more education and earn an additional 20 U.S. dollars per month compared to unexposed cohorts. The regional variation in post-embargo income is consistent with a model that combines internal and external trade costs with non-homothetic preferences.

Key Words: In-utero, trade embargo, long-term health

JEL Codes: I15, O11, O15

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1 Introduction

Scant rigorous evidence exists regarding the impact of trade embargoes¹ on embargoed citizens' health and economic outcomes. This is somewhat surprising, as trade embargoes have historically been employed often and widely as a tool of coercion,² and continue to be a popular policy instrument in modern times. The U.S. alone had approximately 40 formal embargoes in place as of the end of 2016, some covering specific items such as luxury goods and arms – for instance, the embargo of North Korea imposed in 2006 – and others that are nearly comprehensive – for instance, the embargo of Sudan imposed in 1997. Importantly, embargoed countries in current times are nearly always poor, import-dependent and thus vulnerable to trade shocks, and highly populated; for example, Iraq had a population of approximately 17.5 million when the U.N. Security Council imposed a complete embargo on the country that lasted until 2003. While estimates of the impact of embargoes have been reported across academic disciplines³, these do not disentangle the effects of the embargoes from those of other circumstances. For instance, the war in Iraq that followed soon after the imposition of the U.N. embargo had significant repercussions that are highly likely to have contributed to welfare declines during the embargo period. In this paper we overcome these typical obstacles to identification by examining an unanticipated, 15-month-long embargo of Nepal by India beginning in 1989 that placed Nepal and its 17.6 million citizens in a state of virtual autarky, an uncommon instance of such a transition. We use Nepal Demographic and Health Survey (DHS) data and implement a sharp regression discontinuity design (RDD) to provide quasi-experimental impact estimates of in-utero exposure to an embargo on survival, health, and adult educational attainment. We then augment this analysis by exploring whether in-utero exposure to the embargo affected income in adulthood using the Nepal Living Standards Measurement Survey (NLSS) dataset.

We further exploit the NLSS data to investigate how proximity to trade routes influences the relative magnitude of the impact of the embargo on adult income. To start, we develop a simple model that highlights a key role for differences in domestic trade costs faced by communities. The model highlights the fact that a region's relative "remoteness" from international markets has an ambiguous effect on the relative severity of the impact of the embargo. On the one hand, due to Nepal's poor infrastructure and mountainous terrain more remote regions may be effectively insulated from trade shocks. In other words, high transport costs within the country may mitigate the effects of external shocks, and more so for more remote regions. On the other hand, more remote regions are much poorer, in part as a consequence of the high transport costs and corresponding lack of access to international markets. The relative poverty of these regions may make them relatively vulnerable to a trade shock,⁴ particularly when key subsistence goods such as fuel, salt and pharmaceuticals are

¹We refer to legal trade restrictions rather than military blockades that rely on naval or armed forces.

²Historical examples include the U.S. Embargo Act of 1807 imposed on all naval trade during the Napoleonic wars, or the 1935 League of Nations embargo of Italy imposed after the latter's invasion of Abyssinia.

³For example, see Ali and Shah (2000) for estimates of embargo impacts on child mortality in Iraq, and Garfield and Santana (1997) on the effects of the tightening of the long-running U.S. embargo on Cuba in 1992.

⁴This also applies to exposure to different shocks. For example, in different circumstances, the earthquake in Nepal in April, 2015 had dire consequences in part because relatively poor, remote regions were shut off from both internal and international markets.

imported, as they were in Nepal in 1989. The model highlights these forces and, in addition, provides a novel theory-consistent measure of domestic transport costs, and hence regional remoteness, that we bring to the data on outcomes.

We find that the embargo reduced the number of reported live births by nearly 30 percent a month after it began. However, adult women who survived in-utero exposure obtain 0.76-1.05 additional years of schooling compared to unexposed cohorts, driven by exposed women's increased entry into education by a margin of 5.6-10.6 percentage points. The exposed cohort of surviving adults (both men and women) also had a higher adult income by approximately 2,400 Nepalese Rupees per month (a 30% increase) compared to unexposed cohorts. Unsurprisingly, these effects differed widely across Nepalese districts. The most remote regions of the country were virtually unaffected by the embargo while the least remote were hit relatively hard, consistent with an important role for internal transport costs as a buffer to international shocks. In contrast, relatively poor regions at a moderate distance from access points to international markets – i.e., moderately-remote regions – had similar or worse effects to regions with direct access to international markets, such as Kathmandu, which is suggestive of a role for income and the composition of the consumption bundle as an independent determinant of vulnerability to trade shocks.

Existing studies that examine the health effects of in-utero exposure to economic shocks often present inconsistent findings because the timing of such shocks is diffuse, making identification difficult. For example, van den Berg, Lindeboom, and Portrait (2006) find that children born during economic downturns in the Netherlands have shorter lifetimes than others. In contrast, Cutler, Miller, and Norton (2007) find no effects on long term health of children born during the American dust bowl era. The embargo that we examine was a significant economic shock with a clearly defined start and end date, allowing us to accurately identify cohorts that were exposed in-utero, and to deliver sharply identified quasi-experimental estimates of exposure on live births and human capital in Ghana using data at the level of individual pregnancies. Adhvaryu et al. (2014) has recently also used individual-level data to show that in-utero exposure to adverse cocoa price shocks increases the risk of mental health problems in later life, relying on difference-in-differences estimations and regional variation in cocoa production. Another prominent feature of the literature on the welfare effects of in-utero exposure to shocks is a reliance on natural phenomena such as rainfall shocks to empirically identify causal impacts (see Section 2.2 for examples of specific studies). Our paper makes a novel contribution to this literature by examining the impact of a policy instrument. Our findings therefore have direct policy relevance, as the political economy that determines the imposition of embargoes consequently also affects the welfare of significant numbers of people who are already economically vulnerable.

We find that the decrease in reported live births following the start of the embargo is not clearly matched by a corresponding increase in reported child deaths, stillbirths, or miscarriages in the DHS data. In fact, we find almost no statistically significant increases in any of these indices, though there

is some evidence of a rise in miscarriages.⁵ This raises an oft-cited concern that lost pregnancies and infant deaths are underreported; a phenomenon noted in other developing country studies, and attributed to over half of all births in these countries taking place at home rather than a medical facility, and significant social stigma for mothers who lose late-term pregnancies (Darmstadt et al., 2009; Stanton et al., 2006; Heazell et al., 2016). Notably, we find impacts of in-utero embargo exposure on adult women’s educational outcomes and adult income, information on both of which is collected from survey respondents who were in the womb in or close to March, 1989. This group of surveyed women is completely different from surveyed women who report on their pregnancy outcomes taking place in this same small window of months around March, 1989, as the latter women were actually pregnant during this time and are therefore older. Furthermore, the data on adult income and adult women’s education come from independently gathered datasets, and our results show that in-utero exposure to the embargo affected these outcomes in each of these separate samples. Hence, we are confident that what we capture in our results showing a decline in live births is lost pregnancies that are underreported, and not aberrations in the birth dates or the number of live births. Our findings suggest that using mothers’ reports of lost pregnancies and child deaths from household surveys to address underreporting in official birth registration data may not be a viable solution.

The remainder of the paper is organized as follows. Section 2 provides background on the embargo and discusses the literature on early-life health shocks and the effects of trade embargoes. Section 3 describes the data we use for the analysis. Section 4 presents the research design we use to analyze the impact of in-utero exposure to the embargo on pregnancy outcomes and adult educational attainment. Section 5 discusses the results from the pregnancy-level analysis. Section 6 presents the region-level analysis, with an economic model detailing the potential impacts of the embargo based on regional remoteness and supporting empirical results. Section 7 concludes with a discussion of the implications of our findings.

2 Background and Literature

2.1 The 1989 Embargo

On March 23, 1989 treaties on trade and transit between India and Nepal were allowed to expire, at which point India closed all but two entry points into landlocked Nepal.⁶ Historically, the treaties had been renewed as a matter of course, and the embargo clearly took the Nepalese authorities by surprise; for instance, as late as February 6 the Nepal Foreign Minister dismissed reports that India may not renew the treaties as “misleading propaganda”, stating that the relationship with India was “excellent” (Koirala, 1990). Given Nepal’s near-total reliance on India as a conduit to the world, it is unlikely the Nepalese government sought such a schism with India, and once the embargo had begun the Nepalese government continued to state that the breakdown had been unforeseen. At the time of

⁵Lawn et al. (2011) estimates the number of miscarriages and stillbirths in the developing world to be 2.08-3.79 million per year. 98% of the stillbirths in this range are estimated to take place in developing countries.

⁶The two transit points that remained open only allowed a few critical goods into Nepal, such as some medicines.

the border closure, Nepal had a per capita Gross National Income of \$180; fifth-lowest in the world tied with Malawi (World Development Report, 1990).

Nominally the breakdown in the relationship had been caused by the inability of India and Nepal to agree on whether trade and transit should be negotiated within a single agreement, as India desired, or whether the status quo should be maintained, in which the treaties were individually negotiated. However, the underlying impetus for the standoff was more complicated, and three factors are usually given as main causes. First, Nepal had recently agreed to arms purchases from China, India's rival. Second, in mid-1988 Nepal unilaterally imposed 55 percent tariffs on some Indian goods. Third, in 1987 Nepal began requiring work permits for non-Nepalese, a policy that was made more restrictive in 1989 (Koirala, 1990). At the time, approximately 150,000 Indians were living in Nepal and approximately 5 million Nepalese were living in India, and required no permit to work (Koirala, 1990). To make matters worse for Nepal, on March 31 an agreement between the countries, in which India agreed to transport coal and oil purchased from third countries into Nepal, expired. Next, on June 23 a contract for warehouse space in Calcutta port – Nepal's only reliable outlet to the sea – also expired. Finally, throughout the embargo India refused to supply rail cars for shipments across the remaining two transit points and remittances from Nepalese working in India were restricted (Garver, 1991).

The consequences for Nepal were predictably severe. Without sufficient oil and kerosene, forests were cut down for fuel and transportation within the country slowed dramatically (The N.Y. Times, May 10, 1989). The near cessation of vehicle traffic cut vital links to rural areas that were dependent on shipments of medicine, food and other necessities. From the beginning of the embargo the government instituted rationing, which increased the prices of commodities and most imported items. The L.A. Times reported three weeks into the embargo that “Women and children...said they hadn't eaten a cooked meal in a week” (LA Times, April 10, 1989). Industrial output took a large hit due to its dependence on imported intermediates while agriculture was less affected. Finally, in July of 1990, 15 months after the embargo began, India lifted the embargo and the countries began negotiations over new trade and transit treaties.

Figure 1(a) documents the impact of the embargo on imports from India. Since trade between Nepal and the rest of the world almost exclusively travelled via India, we include Nepalese imports from all other countries as well. Figure 1(b) then breaks down imports from India by sector, and Figure 1(c) charts the effect on exports, which may have had an indirect effect on outcomes. When interpreting the patterns one should keep in mind that the embargo formally began in March of 1989 and ended in July of 1990, such that in both years there were substantial periods in which trade relations were effectively normal.

The figures present a consistent pattern and point to a large decline in international trade due to the embargo. As expected, trade with India was disproportionately affected. In line with the anecdotal evidence, Figure 1(b) indicates that coal and petroleum imports were hit particularly hard. Indeed,

coal and petroleum imports from India fell 95 percent during 1989. Food imports from India fell 45 percent and manufactures fell 49 percent. Pharmaceutical imports from India fell 13 percent, a drop that was likely mitigated by supplies via the remaining open border crossings. Finally, Figure 1(c) indicates that exports also fell, particularly exports to India, as industrial production stumbled.

2.2 Literature

There is substantial evidence that exposure to economic shocks in early life potentially leads to lower height in adulthood (see Almond and Currie (2011) for a comprehensive survey of the literature). For example, children affected by the phylloxera infestation in wine-growing regions in France in the late 1800's were found to be 0.5-0.9 centimeters shorter at age 20 than unexposed cohorts (Banerjee et al., 2010). Similarly a 10 percent increase in district local rainfall led to increases in adult female height in Indonesia of up to 0.3 centimeters (Maccini and Yang, 2009). Adult height is a salient health outcome, as taller adults also have better cognitive, social, and economic outcomes (E.g. see Currie, 2009; Steckel, 2008). Potential explanations include that taller workers are stronger, healthier, and more productive (E.g. Strauss and Thomas, 1998). A related hypothesis argues that common inputs to height and cognitive ability mean taller workers are also smarter (Case and Paxson, 2008). Taller workers may also have better self-esteem (e.g. Young and French, 1996), be socially dominant (e.g. Hensley, 1993), and benefit from positive discrimination (e.g. Magnusson et al., 2006).

Regarding in-utero health shocks and adult height, studies find that the Chinese famine reduced height by as much as 3 centimeters among children born or conceived during the famine period (Chen and Zhou, 2007; Gorgens et al., 2012). The famine also had significant effects on fetal mortality, which these and other studies account for in various ways to deal with the resulting sample selection bias. Mortality selection bias in impact evaluations of in-utero health shocks is a particular concern in high mortality environments typical of many developing countries, including Nepal. Bozzoli et al. (2009) present a theoretical framework for both low-mortality and high-mortality countries where health measured as adult height is assumed normally distributed. This framework suggests that mortality selection can dominate scarring effects in high mortality environments, leading survivors of early-life shocks to be taller in adulthood. Almond (2006) similarly deals with mortality selection using a theoretical framework that assumes unobserved, one-dimensional health is sufficient for survival to adulthood if it is above some mortality threshold. Meng and Qian (2009) address the selection bias empirically by focusing on the top decile of heights, and showing that the stunting is most pronounced among these likely healthiest children as the unhealthiest children were culled by the famine. Valente (2015) builds on the frameworks presented in Almond (2006) and Bozzoli et al. (2009), and uses the same data as in our paper to examine how in-utero exposure to conflict during the Nepalese civil war affected fetal loss, the sex ratio at birth, and neonatal health. The study finds significant increases in fetal loss and the probability of a female birth with increased exposure to conflict. However it finds no significant effects on probability of neonatal survival or newborn health, indicating that scarring effects of exposure to conflict, which worsen health, are of the same magnitude as selection effects, which improve health conditional on prenatal survival.

An important feature of Nepal in our context is that it is landlocked, which has been found to be a major impediment to international trade. For instance, Anderson and van Wincoop (2004) find that the median landlocked country faces transport costs that are 55 percent higher than the median coastal country, and similar results are found throughout the literature. MacKellar et al. (2000) conclude that being landlocked reduced economic growth during 1960-1992 by an average of 1.5 percent per year. Elliott (2005) argues that United Nations sanctions on Iraq during the 1990s imposed an unprecedented cost on the economy because Iraq was nearly landlocked. In a paper that is particularly relevant to our study Faye et al. (2004) argue that it is dependence on neighbors that often leads to the relatively poor performance of landlocked countries. The case we focus on represents perhaps an extreme case, since not only was India the major trading partner of Nepal, but was also Nepal's sole source of access to international markets (very little trade went via China).

The effects of sudden and extreme shocks to market access have been explored in a handful of papers, primarily in a historical context in which the focus is typically on the short-run consequences. In a well-known paper, Berhofen and Brown (2005) evaluate the aggregate welfare gains from Japan's opening to global trade after 200 years of near autarky, finding a rise in welfare equal to eight or nine percent of Gross National Product. Frankel (1982) finds that the U.S. self-imposed embargo of 1807-1809 effectively put Britain in a state of autarky, leading to a large rise in relative prices in Britain. Irwin (2005) also explores this historical event but from the U.S. perspective, finding that the embargo cost the U.S. approximately five percent of its Gross National Product in 1808. In a study closely related to ours, Redding and Sturm (2008) exploit the division and reunification of Germany to estimate the impact of market access on the population growth of West German cities. Similar to our approach, they also explore the role that differences in market access due to internal transport costs play in their outcomes. We exploit a similar role for internal barriers to trade, but due to the vast differences in market access and incomes across Nepal we also consider a countervailing role for non-homothetic preferences, and explore a very different set of outcomes.

3 Data

We use the 1996, 2001, 2006, and 2011 waves of the Demographic and Health Survey (DHS) data from Nepal in our analysis of birth and health outcomes, which are recorded at a monthly level. The Nepal DHS survey collects detailed information from women aged 15-49 on their entire history of pregnancies and whether these culminated in a live birth, a stillbirth, or if the child was lost before coming to term.⁷ The survey also records whether children who are born alive continue to survive, and their age of death if they do not. Information on a variety of respondent characteristics such as education, health, and contraceptive use is also collected. We first examine the impact of in-utero exposure to the embargo on the number of reported live births. This is to investigate the extent of fetal loss due

⁷Miscarriages after 24 or 28 weeks of pregnancy are often technically defined as stillbirths. However we follow the survey methodology and define births where the child was born dead as stillbirths, and pregnancies lost before term as miscarriages.

to the embargo, and also to account for prenatal mortality selection effects that potentially dominate scarring effects on survivors. We also examine embargo impacts on reported fetal losses directly, but we rely on the indirect approach of measuring reported live births for the main analysis, as research using globally available data including the DHS surveys shows that final-trimester miscarriages are underreported by as much as 40% (Stanton et al., 2006). Our results most likely capture perinatal deaths, which include both stillbirths of fetuses past 28 weeks of gestation, and neonatal death of children within the first 7 days of birth. In our analysis we define neonatal death as death aged 30 days or less, as the age at death in days of newborn children who die soon after birth is often not reported with perfect recall in DHS surveys.

Table 1 shows descriptive statistics on mothers and their completed pregnancies during 1985-1995, which is broadly the period from which our estimation sample is drawn. This period yields a sample of 19,833 mothers who have 52,232 completed pregnancies. The data show that Nepal was a high mortality environment during this period. The probability of a pregnancy ending in a reported miscarriage in the sample is 6.0 percent, and the neonatal mortality rate (death aged one month or less) is 5.8 percent. The probability of a stillborn birth is however lower at 1.8 percent.⁸ The other statistics show that mothers are 24.80 years of age on average when they complete a pregnancy, have an average height of 151 centimeters, and have a 34 percent likelihood of belonging to one of the two dominant castes in Nepal (Brahman and Chhetri castes). Notably, mothers in this sample (and adult women respondents to the survey in general) have very low levels of education. Only 21.9 percent of mothers have any education, with the majority having none at all. The average years of schooling obtained by mothers is 1.21 years. We also examine how in-utero exposure to the embargo affected educational attainment in adulthood for surviving women survey respondents⁹, both at the extensive and the intensive margin.

For the analysis of economic outcomes at the national and regional level we use the 2010/2011 (Wave III) wave of the Nepal Living Standards Survey (NLSS). These data are recorded at the annual level and as a consequence the treatment period – the period of the embargo – is measured at that level and defined as the 1989/1990 period. The NLSS Wave III is a household survey of 7,020 Nepalese households and 28,689 individuals (both men and women) and covers a range of topics related to household welfare. The surveys cover 71 of Nepal’s 75 districts and were stratified geographically across 14 regions.¹⁰ Due to this stratification, this is the level of geography we focus on in our regional analysis. Table 2 provides some descriptive statistics for the NLSS sample, where we see that the majority is male, married and self-employed, with approximately 8 years of schooling. Incomes are of course quite low in Nepal, with a mean annual income of 3,523 Rupees, the equivalent of about 35 US dollars in 2017. We primarily exploit detailed information on respondents’ sources of income and household consumption from the Wave III survey, since those exposed in-utero to the 1989 embargo

⁸The reported monthly frequencies of both stillbirths and intentionally terminated pregnancies are very low and show little change after the imposition of the embargo, so we do not report the estimation results on these.

⁹There is no corresponding data for men.

¹⁰The exact strata are: mountains, urban areas of the Kathmandu valley, other urban areas in the hills, rural Eastern hills, rural Central hills, rural Western hills, rural mid-Western hills, rural far-Western hills, urban Terai, rural Eastern Terai, rural central Terai, rural Western Terai, rural mid-Western Terai, rural far-Western Terai.

(which lasted through 1990) will be 20-22 years of age, and will therefore (mostly) have finished school and will have some work history.¹¹

Finally, in Section 6.1.6 we exploit data from the 1995/1996 (Wave I) of the NLSS in our calculation of the relative remoteness of Nepalese regions. Here we also adopt a measure of road distance from Kathmandu to a district's headquarters obtained from the Department of Roads, Government of Nepal.

4 Research Design: DHS Survey Data

We implement a standard RDD estimation, including a suite of density break tests, to examine the impact of the embargo on pregnancies and human capital outcomes. Let calendar month be the running variable t , with treatment cutoff t_0 set at March, 1989. Hence, treated pregnancies are those that complete in $t \geq t_0$, with those in calendar month $t < t_0$ are untreated. The outcome variable we first focus on is the total number of pregnancies that result in live births in a calendar month, denoted by Y .

We first perform the density break test outlined in McCrary (2008), which builds an estimator of the density of Y as a function of the running variable t , and then tests for a discontinuity in the density estimator at the cut-off t_0 . To create the estimator for the density of Y , a histogram $g(t_i)$ of Y by calendar month t is built, where the total number of live births in the sample is denoted by n , and t_i indicates the calendar month of live birth i . The histogram is created by dividing the frequency table of Y into an equi-spaced grid X_1, X_2, \dots, X_J of width b covering all the calendar months t in the sample. Define the normalized cell-size for the j th bin as $Y_j = \frac{1}{nb} \sum_{i=1}^n \mathbf{1}(g(t_i) = X_j)$. The triangular kernel function $K(v) = \max\{0, 1 - |v|\}$ is then used to smooth this histogram as it is boundary optimal, yielding the density estimator of Y . Let the number of live births Y in calendar month r be denoted by Y_r , and the density estimator of Y in calendar month r be given by $\hat{f}(r)$. The parameter of interest θ is the log-difference in the height of the density at the cut-off t_0 , which is estimated as:

$$\begin{aligned} \hat{\theta} &= \ln \lim_{t \downarrow t_0} \hat{f}(t) - \ln \lim_{t \uparrow t_0} \hat{f}(t) \\ &= \ln \left\{ \sum_{X_j > t_0} K\left(\frac{X_j - t_0}{h}\right) \frac{S_{n,2}^+ - S_{n,1}^+(X_j - t_0)}{S_{n,2}^+ S_{n,0}^+ - (S_{n,1}^+)^2} Y_j \right\} \\ &\quad - \ln \left\{ \sum_{X_j < t_0} K\left(\frac{X_j - t_0}{h}\right) \frac{S_{n,2}^- - S_{n,1}^-(X_j - t_0)}{S_{n,2}^- S_{n,0}^- - (S_{n,1}^-)^2} Y_j \right\} \end{aligned} \quad (1)$$

where $S_{n,k}^+ = \sum_{X_j > t_0} K\left(\frac{X_j - t_0}{h}\right) (X_j - t_0)^k$ and $S_{n,k}^- = \sum_{X_j < t_0} K\left(\frac{X_j - t_0}{h}\right) (X_j - t_0)^k$. The bandwidth h determines the number of observations included in the local-linear regression, and is computed us-

¹¹We define in-utero exposure to the embargo based on respondents' birth year in the NLSS data, as their month of birth is not reported.

ing an automated procedure after fitting a fourth-order global polynomial separately on each side of the cut-off t_0 . Under standard non-parametric regularity conditions, $\hat{\theta}$ is consistent and asymptotically normal with approximate standard error $\hat{\sigma}_\theta = \sqrt{\frac{1}{nh} \frac{24}{5} \left(\frac{1}{\hat{f}^+} + \frac{1}{\hat{f}^-} \right)}$, where $\hat{f}^+ = \lim_{t \downarrow t_0} \hat{f}(t)$ and $\hat{f}^- = \lim_{t \uparrow t_0} \hat{f}(t)$.

We further perform another density break test described in Cattaneo et al. (2017), which has certain advantages over the above procedure. This second test does not require pre-binning of the data or the construction of a histogram, relying instead on local polynomial techniques to smooth the empirical distribution of Y . Hence it only requires the choice of bandwidth h to implement, and not of any other tuning parameters. Additionally, this method uses a data-driven bandwidth selection procedure that minimizes the mean-squared error, which also corrects for potential bias arising from first-order approximation of the density by employing higher-order polynomials in the estimation.

This method begins with the empirical cumulative density function (c.d.f) estimator for Y , given by $\tilde{F}(t) = \frac{1}{n} \sum_{j=1}^n \mathbf{1}(t_j \leq t)$, and then uses local polynomial smoothers to propose the c.d.f. estimator $\hat{\beta}_p(t) = \arg \min_{\mathbf{b} \in \mathbb{R}^{p+1}} \sum_{i=1}^n \left(\tilde{F}_i(t_i) - \mathbf{r}_p(t_i - t)' \mathbf{b} \right) K_h(t_i - t)$, where $\tilde{F}_i(t_i)$ is the leave-one-out empirical c.d.f. estimator, $\mathbf{r}_p(u) = (1, u, u^2, \dots, u^p)' \in \mathbb{R}^{p+1}$, $K_h(u) = \frac{K(u/h)}{h}$, $K(\cdot)$ is a kernel function, and h_n is a positive vanishing bandwidth sequence. The estimator for the density function then takes the form $\hat{f}_p(t) = \mathbf{e}'_1 \hat{\beta}_p(t)$, where \mathbf{e}_v is the conformable $(v + 1)$ -th unit vector.

To bring this framework to the density break test, we define the control group sample ($t_i < t_0$) as N_0 and the treated sample ($t_i \geq t_0$) as N_1 so that $N_0 + N_1 = n$ by construction. Let the group indicator $g \in \{0, 1\}$ take value 0 for the control group and value 1 for the treated group respectively. The density estimator for each group is computed separately at the threshold t_0 , yielding the estimators $\hat{f}_{g,p}(t_0) = \mathbf{e}'_1 \hat{\beta}_{g,p}(t_0)$ for $g \in \{0, 1\}$ and $p \geq 1$. The test statistic of interest is then:

$$T_p(h_n) = \frac{\frac{N_1-1}{n-1} \hat{f}_{1,p}(t_0) - \frac{N_0-1}{n-1} \hat{f}_{0,p}(t_0)}{\sqrt{\left(\frac{N_1-1}{n-1} \right)^2 \hat{\sigma}_{1,p}^2(t_0) + \left(\frac{N_0-1}{n-1} \right)^2 \hat{\sigma}_{0,p}^2(t_0)}} \quad (2)$$

where $\hat{\sigma}_{1,p}^2(t_0)$ and $\hat{\sigma}_{0,p}^2(t_0)$ are jackknife standard error estimators computed for the treated and control groups respectively. The test statistic, which we refer to as the CJM t-statistic, is asymptotically normally distributed under the null hypothesis of no density break at t_0 . The bandwidth h_n is chosen to be the appropriate special case of the robust bias-corrected bandwidth as outlined in Calonico et al. (2014). We report results from setting $p = 2$, but the results are also robust to setting $p = 3$ or higher.

Finally, we perform a standard RDD analysis to investigate if the monthly number of live births Y is affected by the embargo imposed in March, 1989. Let treatment indicator D take value 1 for calendar months $t \geq t_0$ and value 0 for calendar months $t < t_0$. We estimate the following parametric

specification:

$$\begin{aligned}
 Y &= \alpha + \beta D + \gamma D * g(t) + \delta g(t) + \epsilon \\
 \forall t &\in (t_0 - h, t_0 + h)
 \end{aligned}
 \tag{3}$$

where $g(t)$ is a polynomial in t , that is interacted with the treatment indicator D to allow for differential impacts of this time-trend polynomial before and after the treatment cut-off t_0 . α is a constant, and ϵ is an idiosyncratic error term. the parameter of interest is β , which captures any effects of being born in or after March, 1989 (our designated cut-off month t_0). We use the optimal bandwidth algorithm described in Calonico et al. (2014) to determine the bandwidth h of observations either side of the cut-off t_0 with which to perform the estimation. We report results from setting $g(t)$ to be linear or quadratic in t , but we do not attempt using polynomials of higher order as this may bias the results (Gelman and Imbens, 2014). We also cluster the standard errors to correct for the bias induced by the discreteness of the running variable t as recommended in Lee and Card (2008). We additionally estimate (3) with pregnancy outcomes, adult educational attainment and height of women survey respondents¹², and mother characteristics as outcome variables, which we do using individual-level data on pregnancies i and mothers j , rather than monthly aggregates as we do for live births. These estimations yield evidence of embargo effects on exposed children’s health and adult human capital, and reveal if mothers reporting completed pregnancies immediately after the embargo begins differ in their attributes from those reporting completed pregnancies shortly before. The pregnancy outcomes we examine are the probability of miscarriage and neonatal death, and the mother characteristics we examine are caste, education, height, and age at completion of pregnancy.

The main threats to identification in our empirical analysis are potential household manipulation of their exposure to the embargo during pregnancy, and adverse shocks besides the embargo that occur in March, 1989 or close to that time that also affect natal outcomes. As the embargo was a sudden and unanticipated shock that affected the entire country simultaneously, it is unlikely that households could alter the exposure of their unborn children to the embargo during the third trimester, whether by migrating to less affected areas with prior knowledge of the coming border closure, or by inducing the birth of their child before the embargo began. Our search of the literature and other available records revealed no other adverse shocks of similar magnitude that occurred alongside the embargo that might lead us to erroneously attribute any findings to the latter. As in-utero rainfall shocks have been shown to affect birth outcomes, we we plot deviations from 20-year average monthly rainfall by calendar month in Figure A.4 to ensure that these are not driving our results. There are no statistically significant deviations in monthly rainfall at the time of the embargo or in the months preceding, ameliorating this concern.¹³

¹²we only include women aged 18 years or older at the time of the survey when examining embargo impacts on adult women’s education and height. Similar information for men’s education is not available in the data.

¹³Nepal is an earthquake-prone country, but the closest earthquake chronologically to March, 1989 was in August, 1988.

We report estimates from (1) and (3) for a variety of bandwidths around t_0 . For the McCrary density break test in (1), we first specify a specific data bandwidth from which to then compute the automated bandwidth for the procedure as outlined in McCrary (2008). We set data bandwidths to be no larger than 24 months, as introducing observations from well before or after t_0 reduces comparability of treated and untreated observations, and classifies observations not immediately affected by the embargo as treated, biasing the estimated impact of the embargo. Typically this leads to a computed bandwidth of 6-7 months for the McCrary test in (1). For the RDD analysis in (3), we show results using the same bandwidths as the data bandwidths we set for (1), and also for the optimal bandwidth h^* as determined by the algorithm in Calonico et al. (2014). However h^* often exceeds 24 months by a large margin, so we undersmooth the polynomial fit on either side of the cut-off t_0 and also report results from estimations using half the optimal bandwidth, or $0.5h^*$. This reduces the precision of the estimates, but also increases confidence considerably in the accuracy of the embargo impact estimate. Further, we also report results from bandwidths set to 6 and 12 months, as these more closely match the automated bandwidths computed for the McCrary test in (1), facilitating a more informative comparison between the two sets of estimates. The density break test in (2) derives optimal bandwidths on either side of t_0 using the entire sample of pregnancies as part of the procedure.

5 Results: Health, Education, and Mother Characteristics

5.1 Graphical Analysis

Figure 2 plots a bar graph of the frequency of womens' reported live births by calendar month from the DHS data. It also includes a local-linear regression plot of these frequencies using a triangular kernel and a 3-month data bandwidth on either side of the cut-off at March, 1989. There is a large visible drop of just over 25% in the number of live births immediately when the embargo begins. The same discontinuous decline is reflected in completed pregnancies in Figure A.1, indicating that there is no compensating increase in womens' reports of miscarriages or stillbirths. There is a slight increase in live births in the two months preceding March, 1989 that could exaggerate the decline in live births once the embargo begins. However, removing these two months from the sample still yields a large, statistically significant decrease in live births, which is also visible in corresponding Figure A.2.¹⁴ The number of stillbirths reported in any calendar months is always less than 10, and while there is a small increase in reported miscarriages in March, 1989 in Figure A.3, it is several orders of magnitude smaller than the decline in reported live births, albeit sometimes statistically significant in the estimations. The fact that live births and total pregnancies both decrease by almost an identical margin suggests that women report very few lost late-term pregnancies in the survey, as if they did the number of total pregnancies would remain unchanged after March, 1989 even if live births declined.

In Figure 3 we plot women's years of schooling by their month of birth from the DHS data. We only include women who were at least 18 years of age at the time of the survey so as to more accurately capture their completed schooling in adulthood. We see a sharp increase in completed years

¹⁴The regression results are presented in Appendix Table A.2.

of education by approximately 1 year for women respondents who were born in March, 1989 or the month following compared to those born in the months just preceding, mirroring the decline in reported live births at the start of the embargo in Figure 2. This suggests that the decline in reported live births once the embargo begins is capturing lost pregnancies rather than just an aberration in reporting, as adult women survivors of in-utero exposure to the embargo appear positively selected in their human capital accumulation compared to unexposed cohorts. We are unable to determine with our data whether the increase in educational attainment among adult women survivors of exposure is due to positive selection on intrinsic endowments at birth, or a lack of competition in the education system as the embargo reduced live births in the same cohort. We do however investigate in our estimations whether the increase in educational attainment occurred on the intensive or extensive margin.

Figure 4 shows that there is no perceptible change in the caste composition or age in years of mothers reporting completed pregnancies in March, 1989. There are however visible declines in mothers' completed years of schooling and height, indicating that women completing pregnancies once the embargo began have lower human capital than those completing pregnancies just before. This is noteworthy given the significant decline in reported completed pregnancies at the same time, as it suggests that surviving births are born to less educated, less healthy (as proxied by their adult height) mothers rather than mothers positively selected on these margins. Although, mothers completing pregnancies after the embargo having lower human capital is consistent with the marginal concurrent increase in reported miscarriages.

5.2 Estimation Results

Table 3 shows the results from (1) in Panel A. In column (1) we restrict the data bandwidth to lie within a 24-month window either side of March, 1989. This yields an estimated bandwidth of 7.33 months using the algorithm that is part of the density break test procedure in McCrary (2008). The estimates show that the density of live births declined 28.9% in the month that the embargo began. Restricting the data bandwidth to 18 months either side of the cut-off in column (2) yields a smaller estimated bandwidth of 6.55 months, and a larger estimated decline of 29.6% in live births. Both estimates are highly statistically significant at the 1% level. Columns (3) and (4) repeat the estimations in columns (1) and (2) for live male births, and columns (5) and (6) do so for live female births. The results reveal no difference between the declines in male births and female births, indicating that pregnancies were similarly adversely affected by exposure to the embargo regardless of the gender of the unborn child. Columns (3) and (4) show estimated declines in live male births of 27.8% and 30.5% respectively, while columns (5) and (6) show estimated declines of 29.4% and 28.7% respectively for live female births. Again, the estimates are all highly statistically significant at the 1% level across columns (3)-(6).

Panel B of Table 3 shows the estimated CJM t-statistics (Cattaneo, Jansson, and Ma (2017)) from (2). The data bandwidths on the left side (h_l) and right side (h_r) are computed as part of the procedure using the entire sample. Column (1) shows a t-statistic of -3.384, which is statistically significant

at the 1% level, and is consistent with columns (1) and (2) in Panel A in indicating a large decline in the number of live births in the month the embargo begins. Similarly, column (2) shows a t-statistic of -2.628, and column (3) shows a t-statistic of -2.222, indicating statistically significant declines in the numbers of male and female live births respectively, as we find in Panel A.

Panel C of Table 3 shows estimates from (3) using the same data bandwidths as in Panel A. The results are qualitatively identical to those in Panel A, but somewhat smaller as the data bandwidth is also the actual bandwidth used in the parametric estimation, whereas in Panel A the analyses are carried out using the estimated bandwidths, which are significantly smaller than the data bandwidths. The results in Panel B therefore include more months from the post-embargo period in the regression sample, which might bias the estimates of the immediate impact of the embargo downwards. Columns (1) and (2) indicate that live births declined by 15.6% and 17.5% respectively, both highly statistically significant at the 1% level. Columns (3) and (4) show estimated declines of 17.1% and 17.8% respectively in live male births, and columns (5) and (6) show similar estimated declines of 14.1% and 17.1% respectively in live female births, all significant at the 1% level. We also estimate (3) on smaller data bandwidths to make the estimates comparable to those in Panel A. The results are in columns (1) and (2) of Appendix Table A.3. In column (1) restricting the data bandwidth to 12 months either side of the cut-off increases the magnitude of the estimated decline in live births to 22.5%. Further restricting the data bandwidth to 6 months in column (2) increases the magnitude of the estimated decline further to 29.6%, which is identical to the estimate in column (2) of Panel A in Table 3, which is derived from a nearly identical estimated bandwidth of 6.55 months. Columns (3) and (4) of Appendix Table A.3 again show results from (3) using data bandwidths of 24 and 18 months respectively, but with quadratic rather than linear splines. The results are qualitatively identical to those in columns (1) and (2) of Table 3, showing estimated declines in live births of 22.0% and 26.2% respectively.

To reduce concerns of maternal recall bias driving the results, we re-estimate (1)-(3) using only data from the 1996 wave of DHS data, which is the closest chronologically to the imposition of the embargo in 1989. The results in Appendix Table A.1 show that we lose precision for the CJM t-statistics in Panel B due to the much smaller sample size, but that the results in Panels A and C are identical to the corresponding Panels in Table 3. To check whether it is the possibly artificial increase in reported live births in the two months preceding March 1989 that is driving the results, we re-estimate the specifications in Table 3 after dropping the observations from these two months. The results in Table A.2 are smaller in magnitude and less precisely estimated, but show qualitatively identical findings to those in Table 3. As a further falsification check, we also re-estimate the specifications in Table 3 for the full sample of live births after redefining the cut-off month to be March, 1988 and March, 1990 to confirm that we are capturing embargo impacts rather than a seasonal effect. The results are in Appendix Table A.4. Aside from a statistically significant t-statistic in column (1) of Panel B (which indicates an increase in the number of live births rather than a decline), all the other estimates are close to zero and statistically insignificant.

In Appendix Table A.5 we present estimated impacts of in-utero exposure to the embargo on the probability of miscarriage and neonatal death, and on women’s adult height. These are again estimated using specification (3). Panel A shows the results from the optimal bandwidth calculated using the algorithm in Calonico et al. (2014) and from dividing it in half as we report in earlier results, and Panel B shows results from bandwidths of 6 and 12 months. In Panel A we find no statistically significant impacts on any of these outcomes. The optimal bandwidths for the miscarriage and neonatal death outcomes are however very large, so that even dividing them in half yields a data window of over 24 months either side of the cut-off in each case. In Panel B, both columns (1) and (2) show a marginally significant increase in miscarriages of 2.3 and 1.7 percentage points after reducing the bandwidth to 6 and 12 months respectively. However the impact estimates for neonatal death and adult women’s height remain statistically insignificant at these bandwidths. While the sample of adult women for whom heights are recorded is small and therefore may lead to imprecise estimates, the sample of live births for which neonatal survival outcomes are recorded is large and the coefficient estimates are close to zero.¹⁵ It is unclear whether this lack of impact on neonatal deaths is due to equivalent opposing scarring and selection effects of the embargo that cancel each other out, or simple underreporting of neonatal deaths. The latter is certainly possible, as there is not enough of a combined increase in stillbirths, miscarriages, and neonatal deaths to counterbalance the large decline in live births following the embargo, suggesting significant underreporting of births where the children don’t survive. The marginally statistical significant increase in miscarriages in Panel B potentially indicates positive selection on human capital endowments for surviving births that may partially explain the increased educational attainment of adult women exposed to the embargo in-utero we see in Figure 3.

The estimated fall in live births we find is large, but consistent with the risky conditions at child-birth for the overwhelming majority of Nepalese mothers at the time, Nepal’s vulnerability to the embargo due to its landlocked geography and high rate of poverty, and also the literature on the impact of adverse shocks in-utero on birth outcomes. The 1996 wave of the DHS data shows that only 8 percent of births in Nepal during 1994-96 were delivered in a health facility. The DHS data does not provide this specific information for births during the embargo, but an alternative National Fertility, Family Planning, and Health Survey (NFHS) carried out nationally in 1991 reports identical statistics, indicating that very few mothers delivered under trained medical supervision at the time of the embargo. Further, the 1996 DHS data shows that only 9 percent of births were delivered with the assistance of a doctor or a trained nurse (or midwife), up marginally from 8 percent in the NFHS 1991 data. 56 percent of births occurred at home with only mothers’ friends and relatives to provide assistance, and 11 percent of births occurred without any assistance at all (Pradhan et al., 1997). The lack of skilled medical assistance at birth has been a persistent factor in perinatal and neonatal mortality in Nepal, including during the embargo period. This is because over half these deaths are caused by intrapartum asphyxia during labour and complications from pre-term birth, both of which

¹⁵There are also no impacts of the embargo on infant mortality, defined as the death of a child aged 0-12 months. These results are available upon request.

are preventable with skilled medical supervision during childbirth, and have been the leading causes of perinatal mortality from the 1980s to the present day, as approximately two-thirds of Nepalese mothers still deliver without skilled assistance (Geetha et al., 1995; Pradhan et al., 2012).¹⁶ Hence, if the embargo increased complications during childbirth due to its significant economic impact, the vulnerable conditions at birth for most Nepalese mothers may have also caused a large increase in perinatal mortality. It is possible that the embargo reduced maternal nutrition that also increased late-term miscarriage, as episodes of widespread nutrition deprivation have been shown to increase pregnancy loss due to raised maternal progesterone levels (E.g. see Wynn and Wynn (1993)). Maternal stress is also a likely causal channel, as evidence on the impact of shocks such as bereavement to pregnant women from large samples of thousands of births show that such events can increase the risk of stillbirth by as much as 18 percent, and of preeclampsia during labour by over 50 percent in countries such as Sweden and Denmark, which are much wealthier nations than Nepal (László et al., 2013a; László et al., 2013b).

Turning to mothers' characteristics, estimates from (3) in Panel A of Table 4 shows no statistically significant changes in the caste composition, age at birth, height, or education of women completing pregnancies once the embargo begins in March, 1989 compared to those completing pregnancies in the immediately preceding months. However, the optimal bandwidth (h^*) calculated using the algorithm in Calonico et al. (2014) exceed 24 months on either side of the cut-off month by a large margin for each of these characteristics. In fact, undersmoothing the estimates by using observations from half the optimal bandwidth ($0.5 h^*$) still yields a data window of over 24 months either side of the cut-off in each case. This likely biases the estimated impact of the embargo downwards, as several untreated observations from the post-embargo period as well as control group observations that are less comparable to treated observations are then included in the estimation sample. We therefore present results using smaller bandwidths of 6 and 12 months either side of March, 1989 in Panel B of Table 4. We still find no statistically significant effects on mothers' caste composition in columns (1) and (2), or in mothers' age at birth in columns (7) and (8). In column (3), using a bandwidth of 6 months, we find a marginally significant negative difference of 0.264 years of schooling for mothers completing pregnancies just after the start of the embargo compared to mothers completing pregnancies just before. A larger bandwidth of 12 months in column (4) again renders this estimate statistically insignificant, but the estimated negative difference in education is not much smaller at 0.179 years. Similarly, the estimate using a 6-month bandwidth in column (5) shows that mothers completing pregnancies once the embargo begins are 0.842 centimeters shorter than those completing pregnancies just before; an effect highly statistically significant at the 1% level. Again, the coefficient becomes smaller in magnitude and statistically insignificant in column (6) after increasing the bandwidth to 12 months. Inasmuch as smaller bandwidths provide more accurate estimates of the embargo effects at the cost of lower precision, the results do suggest that mothers reporting completed pregnancies at the start of the embargo have less education and are less healthy. This is potentially explained by underreporting by healthier, more educated mothers of completed pregnancies that were lost, and also perhaps by

¹⁶Women's DHS survey responses on prenatal health investments and skilled assistance at delivery only cover pregnancies in 1993 and thereafter, preventing us from testing embargo effects on these health measures directly.

differential exposure of women to the embargo across regions such that poorer women in more remote regions were more insulated from its adverse effects.¹⁷

Table 5 shows results from (3) for the estimated impact of women’s in-utero exposure to the embargo on their educational attainment in adulthood. The optimal bandwidths calculated using the algorithm in Calonico et al. (2014) lie well within 24 months either side of the cut-off month. Panel A reports coefficients from estimations using a linear spline, and Panel B reports them from estimations with a quadratic spline. Column (1) in Panel A shows that adult women born immediately after the embargo have 0.761 years more schooling than those born just before; an effect statistically significant at the 5% level estimated using an optimal bandwidth of about 21 months. Re-estimating the specification with observations from half the optimal bandwidth increases the coefficient to 1.049 years, and the effect remains marginally statistically significant. Columns (3) and (4) report estimated embargo impacts on the binary outcome of whether adult women have any schooling at all. Column (3) shows no statistically significant effect, but reducing the sample to half the optimal bandwidth (8.23 months) in column (4) yields a statistically significant estimate of a 10.6 percentage point increase in entry into education after in-utero exposure to the embargo. In Panel B, using a quadratic spline yields a marginally statistically significant increase in adult women’s schooling of 1.074 years in column (1), but a smaller and statistically insignificant estimated impact of 0.657 years in column (2). Columns (3) and (4) however yield statistically significant estimates of 14.0 and 16.8 percentage point increases in women’s entry into education respectively. This extensive margin increase in entry into schooling is clearly an important factor in women having more education after being exposed to the embargo, as the majority of women in Nepal born during this time had no education at all (see Section 3). However it is ambiguous in our data whether the increase in education is due to positive selection on human capital endowments at birth for survivors of exposure, or because cohorts of survivors were smaller after the embargo, reducing competition in educational attainment.

6 Adult Income: Theory, Regional Heterogeneity, and Mechanisms

The embargo likely affected different mothers very differently, thus impacting their birth outcomes very differently. For instance, a household whose principal earner worked in an export industry likely received a relatively severe income shock. At the same time, some households may have been critically reliant on imported necessities such as fuel or medicines, whereas others may have had access to locally available substitutes for these goods. More generally, poorer households may simply have been more vulnerable to any negative shock to the extent that their subsistence needs were only just being met prior to the embargo. In this section we present evidence on the relative importance of these channels, in part by noting that each channel is likely to be systematically related to the remoteness of the individual within the country. In other words, trade costs differ substantially within Nepal, mostly due to relatively poor transportation infrastructure and rugged terrain, and these differential domestic trade costs are highly correlated with – and are important determinants of – the level of household

¹⁷The latter mechanism is expanded in the next section.

income, the reliance on imported necessities, the availability of substitute goods, and the likelihood of working in an export industry, as we show below. As a result, households were undoubtedly affected by the embargo in direct relation to their relative remoteness within the country.

First, we graphically analyze whether adult income was affected by in-utero exposure to the embargo. Figure 5 plots average adult log income by year of birth from the NLSS data. There is a clearly visible, sharp increase in log income for adults born in 1989, the majority of whom would have been exposed in-utero to the embargo, compared to those born in 1988 who were not exposed. This is seemingly a very large effect, and we will see in the regional analysis below that the effect is driven mainly by richer areas with relatively high incomes. This income gap persists, but is somewhat smaller for adults who were born in 1990. This increased income in adulthood for survivors of in-utero exposure mirrors the increased educational attainment we find for adult women who survived exposure. We discuss the size and significance of this increase in income, and its relationship to regions' relative remoteness, in Section 6.2 below.

Standard international trade theory would suggest that more remote regions should be less affected by an external trade shock, as their initial exposure to trade is lower. To motivate our empirical approach, we first present a model of this type, but we add an additional factor in the form of non-homothetic preferences, which generate differences in agents' relative consumption of subsistence versus non-subsistence goods depending on their income level. Incorporating these preferences thus leads to large differences across regions in the share of subsistence goods in the consumption basket. Thus, on the one hand a particular region may be remote, and therefore insulated from trade. But on the other hand, if subsistence goods are primarily imported, and if remote regions are poorer, then households in remote regions will be more vulnerable to negative trade shocks. Furthermore, in relatively poor regions there may be fewer locally available substitutes, which may heighten household vulnerability. We think this model fits the case of Nepal well, which as we show has a large variance in internal trade costs across regions, which vary widely in their reliance on certain imported necessities. Beyond this, the model may be a good description of many developing countries where some regions are quite remote, poor, and heavily reliant on critical imports.

In line with this discussion, Figure 6 plots income as a function of a household's distance from Kathmandu, an imperfect proxy for the remoteness of the household (discussed in greater detail in Section 6.1.6 below), while Figure 7 plots the consumption shares of various goods against log income across households. Intuitively, the average income of a household tends to be lower at greater distances from Kathmandu, the most important gateway to international markets. Furthermore, goods that are typically considered necessities, such as food and fuel, on average comprise a larger share of lower-income household consumption (note that the fitted lines for Food and Kerosene & Liquid Propane Gas (LPG) have a negative and statistically significant slope). These facts outline the fundamental tension between remoteness, which can mitigate external shocks, and poverty, which can heighten them. With this tension in mind we proceed to a description of a simple model that highlights the welfare impact

of the embargo on households, where we have a particular interest in the welfare consequences for pregnant women.

6.1 A Model

We consider a small, open economy whose supply side is similar to the structure outlined in Coşar and Fajgelbaum (2014) but whose demand side features non-homothetic preferences. Specifically, we focus on non-homothetic preferences that emphasize a role for necessary goods in household welfare, a relevant case for the developing country context we explore in the empirics. The economy has I regions, indexed by i , and two industries, $j \in \{C, H\}$. Only certain regions, which we will refer to as “ports”, have access to international markets – e.g., they may have border crossings or international airports. We allow the index i to be ordered so as to reflect the distance of a region from its nearest port, such that regions with ports are indexed $i = 0$.

International trade costs take the iceberg form and are industry-specific, such that for each unit of goods shipped from a port τ_0^j units arrive in the foreign port. Similarly, internal trade costs between regions within the country take the iceberg form *per unit of distance* and are given by τ_1^j , so that for each unit of goods shipped from some region i a total of $\tau_0^j \tau_1^j i$ units arrive in the foreign port.

There are two factors of production, labor and land. Labor is mobile across industries and locations while land is assumed to be industry- and location-specific. The specificity of land therefore generates a congestion force (via decreasing returns to scale) in each region. The quantity of land and labor in an industry and region are denoted $T_j(i)$ and $L_j(i)$, respectively. The total amount of labor in the economy is L .

6.1.1 Preferences

The preferences of households located in region i take the Stone-Geary form and depend on their consumption of a manufactured good $C(i)$ and their consumption of a household good $H(i)$.¹⁸ The indirect utility of the household is therefore given by:

$$v(m(i), P_H(i), P_C(i)) = \phi \left(\frac{M(i)}{P_H(i)} - \gamma \right) \left(\frac{P_H(i)}{P_C(i)} \right)^\alpha, \quad \gamma \geq 0 \quad (4)$$

where $\phi \equiv \alpha^\alpha (1 - \alpha)^{1-\alpha}$, $\alpha \in [0, 1]$; $\gamma > 0$ indicates positive subsistence consumption of household goods, H ; $M(i)$ is total income in region i ; and $P_C(i), P_H(i)$ are the prices of the manufactured and household goods in region i , respectively. Here we assume that land owners are immobile and do not work, such that total income in region i is

$$M(i) = w(i)L(i) + r(i)T(i) \quad (5)$$

¹⁸The utility function is $U = C(i)^\alpha (H(i) - \gamma)^{1-\alpha}$.

where $L(i)$ and $T(i)$ are labor and land in region i and $w(i), r(i)$ are the returns to labor and land, respectively. Given these preferences, the share of income spent on each good, $\chi_C(i), \chi_H(i)$, is

$$\chi_C(i) = \frac{\alpha(M(i) - \gamma P_H(i))}{M(i)} \quad (6)$$

$$\chi_H(i) = (1 - \alpha) + \frac{\gamma P_H(i)}{M(i)} \quad (7)$$

where we see that richer households are less reliant on (consume proportionately less of) the subsistence good, H , while consuming proportionately more of the manufactured good, C .

6.1.2 Production

The aggregate production function is Cobb-Douglas in labor and land, such that the production technology for manufactured or household goods in region i is

$$Y_j[L_j(i), T_j(i)] = A_j(i)T_j(i)^{\eta_j}L_j(i)^{1-\eta_j} \quad \text{for } j = C, H \quad (8)$$

where $A_j(i)$ is the technology level in industry j and region i . In what follows we will assume that $\eta_C = \eta_H = \eta$ for simplicity and in order to focus on our key predictions of interest.

While the level of technology may vary across regions it is constrained such that the *relative* level of technology is constant across regions – i.e., $\frac{A_C(i)}{A_H(i)} = a, \forall i \in [0, I]$. This simplifying assumption ensures that the country’s comparative advantage in the world economy is determined at the national, rather than regional, level. We further assume that a differs across countries, providing a Ricardian motive for international trade.

Since the country is small in world markets it takes international prices as given. Defining $p^A \equiv P_C^A/P_H^A$ as the relative autarky price in all regions, region i will be fully specialized in (and will export) good C when the relative price $p(i) \equiv P_C(i)/P_H(i) > p^A$ and will specialize in (and export) good H when $p(i) < p^A$. Only when $p(i) = p^A$ can a region be incompletely specialized, in which case the region is indifferent between autarky or trade.

An important implication of this economy is that there potentially exists some threshold region, \bar{i} , beyond which all regions are in autarky. To see this, first denote the boundary region within the country that is furthest from a port as i_b . We next note that when a country exports, for instance, good C then all trading regions must specialize in C . The delivered (foreign) relative price of C , which we denote $p^* \equiv P_C^*/P_H^*$, is then

$$p^* = p(i) \tau_0^C \tau_1^C \tau_0^H \tau_1^H i^2 \quad (9)$$

where we have used the fact that $P_C(i) = P_C^*/\tau_0^C \tau_1^C i$ and $P_H(i) = P_H^* \tau_0^H \tau_1^H i$. Condition (9) therefore defines the dispersion in prices across regions in this economy. Most critically, it implies that the local relative price of the export good, $p(i)$, is decreasing in the distance to a port, as more of each unit price gets absorbed in trade costs (recall p^* is fixed). Thus, as long as $\bar{i} < i_b$, regions beyond some threshold \bar{i} would prefer to specialize in and export H , as the price of C will be too low;¹⁹ since this is ruled out, those regions, and all those for which $i \geq \bar{i}$, must be in autarky and are consequently incompletely specialized.²⁰

6.1.3 Production Problem

From (8), profits in industry j within region i are

$$\pi_j(i) = \max_{L_j(i)} \{P_j(i)Y_j[T_j(i), L_j(i)] - w(i)L_j(i) - r(i)T_j(i)\} \quad (10)$$

the solution to which indicates that the demand for labor in region i and industry j is

$$L_j(i) = \frac{1-\alpha}{\alpha} \left(\frac{A_j(i)P_j(i)}{w(i)} \right)^{1/\eta} \quad \text{for } j = C, H \quad (11)$$

6.1.4 Equilibrium

The general equilibrium consists of a set of local equilibria in combination with the requirement that welfare is equalized across regions and the national labor market clears.

Definition 1 *General equilibrium in region i takes international prices $\{P_C^*, P_H^*\}$ as given and consists of local labor demand $\{L_j(i)\}_{j=C,H}$, local land use $\{T_j(i)\}_{j=C,H}$ and factor prices $\{w(i), r(i)\}$ such that*

1. *A local equilibrium holds for each region. Formally, taking prices $\{P_C(i), P_H(i)\}$ and welfare \bar{v} as given,*

A. *Workers maximize utility, given by (4), where*

$$v(i) \leq \bar{v}, \text{ for } L(i) > 0 \quad (12)$$

B. *firms (regions) maximize profits, as in (10);*

C. *trade is balanced across regions; and*

D. *land and labor markets clear locally:*

¹⁹Regions exactly at distance \bar{i} are indifferent.

²⁰Another implication of this economy is that a no arbitrage condition implies that there is no domestic trade. Specifically, for any regions i and i' separated by distance d we know that $p_j(i') \leq p_j(i) \tau_1^j d$, where the condition binds when i sells to i' . In other words, the ratio of the prices between any two regions will be exactly equal to the transport costs between them, such that there are no gains from domestic trade.

$$\sum_{j=C,H} T_j(i) = T(i); \quad \sum_{j=C,H} L_j(i) = L(i) \quad (13)$$

2. The labor market adjusts such that indirect utility is constant across regions,

$$\int_0^{i_b} L(i) di = L \quad (14)$$

We first note that the local wage is set by the combination of (11) and the local labor supply condition (12), which leads to the following local labor demand condition:

$$L(i) = \begin{cases} \psi \left(\frac{A_C(i)p(i)}{\frac{p(i)^\alpha}{\phi} v(i)+\gamma} \right)^{\frac{1}{\eta-1}} & \text{if } p(i) \geq p^A \\ \psi \left(\frac{A_H(i)}{\frac{p(i)^\alpha}{\phi} v(i)+\gamma} \right)^{\frac{1}{\eta-1}} & \text{if } p(i) < p^A \end{cases} \quad (15)$$

where $\psi \equiv \left(\frac{1-\alpha}{\alpha}\right)^{\frac{\eta}{\eta-1}}$. These two cases reflect specialization and export of either C or H , depending on the prevailing relative international price.

The general equilibrium then follows from combining (15) with the local and aggregate labor market clearing conditions (13) and (14) while setting regional welfare equal to an economy-wide constant, \bar{v} .²¹

6.1.5 Comparative Statics

We focus on the short-run regional welfare response to rising international trade costs. It is important to note that, though we are ultimately interested in a country's sudden shift from being relatively open to nearly completely closed, we do not compare a trading regime with an autarky regime; rather, we consider the effects of rising trade costs within the context of the trading regime. The reason is that a move to autarky in the model leads to incomplete specialization, such that both goods are produced in all regions. However, the suddenness and short-run nature of the episode we are interested in rules out a transition away from specialization. In other words, there simply was not enough time for the country to, for instance, ramp up its kerosene and pharmaceutical industries in order to forestall shortages of these goods. As a result, the relevant case is the short-run case in which specialization was maintained, but international trade costs rose to levels that effectively prohibited international

²¹It follows that general equilibrium regional welfare (\bar{v}) is given by the following implicit functions:

$$\begin{cases} \psi \int_0^{i_b} \left(\frac{A_C(i)p(i)}{\frac{p(i)^\alpha}{\phi} \bar{v}+\gamma} \right)^{\frac{1}{\eta-1}} di - L = 0 & \text{if } p(i) \geq p^A \\ \psi \int_0^{i_b} \left(\frac{A_H(i)}{\frac{p(i)^\alpha}{\phi} \bar{v}+\gamma} \right)^{\frac{1}{\eta-1}} di - L = 0 & \text{if } p(i) < p^A \end{cases}$$

trade.

We are interested in the differential regional welfare response to a trade shock, and so we apply our comparative statics to the local equilibrium, (15), since in the general equilibrium welfare is equalized across regions. In doing so, we implicitly assume that workers are unable (do not have time) to migrate in response to the shock. In this case, a shock to international trade costs affects regional welfare through its impact on the region-specific prices of the export and import goods. Taking the case in which C is the export good, we can solve (15) for $v(i)$ and calculate the effect on regional welfare of a change in international trade costs, given by $dv(i)/d\tau_0^C$ and $dv(i)/d\tau_0^H$:

$$\begin{aligned} dv(i)/d\tau_0^C &= \frac{\phi}{\tau_0^C} \left[-\theta A_C(i) L(i)^{1-\eta} p(i)^{1-\alpha} - \gamma \alpha p(i)^{-\alpha} \right] < 0 \\ dv(i)/d\tau_0^H &= \frac{\phi}{\tau_0^H} \left[-\theta A_C(i) L(i)^{1-\eta} p(i)^{1-\alpha} - \gamma \alpha p(i)^{-\alpha} \right] < 0 \end{aligned} \tag{16}$$

where $\theta \equiv \frac{(1-\alpha)\eta+1/\eta}{\alpha^{1/\eta}}$. The only difference in the impact on welfare between a change in τ_0^C versus τ_0^H is that their magnitudes are each inversely proportional to the initial size of the trade barrier in the sector. In both cases, the overall effect on welfare is unambiguously negative for $i < \bar{i}$ (and zero beyond this point). There are three primary channels through which welfare is impacted by rising international trade costs. The first term in brackets nests two effects: first, a reduction in the local price of the export good (due to the rise in τ_0^C) reduces the demand for labor in region i , which reduces household income – a negative “export effect”. Second, a rise in the local price of the *import* good directly reduces local consumption of that good – a negative “consumption effect” – while a fall in the local price of the *export* good directly increases its local consumption – a positive “consumption effect”. The second term in brackets then reflects the fact that the rise in τ_0^C and τ_0^H forces households to increase expenditure on the subsistence good, H , at the expense of the manufactured good, C – a negative “subsistence effect”. In fact, individuals will cease consuming the manufactured good C entirely beyond some threshold price of the subsistence (import) good – i.e., the budget share of the subsistence good may go to one. Beyond this price the desired expenditure on subsistence goods will exceed the household budget constraint, such that households must do without some amount of subsistence consumption. It is above this point that the most deleterious effects of the embargo will be incurred, as households do without subsistence levels of fuel or other necessary imports.

The move to autarky therefore reduces household incomes via a deterioration in the price of the country’s export good, while also hitting households on the consumption side as imported goods become more expensive. This then forces households to spend an increasing share of their dwindling incomes on subsistence goods in lieu of other goods. Importantly, the magnitude of these effects differs systematically across regions within the country, in the manner captured in the following proposition:

Proposition 1 *The welfare impact due to rising international trade costs, while always negative for $i < \bar{i}$, becomes less negative with the distance from a port as long as the subsistence parameter γ is sufficiently small. Formally, conditions (16) are increasing in i as long as*

$$\gamma < \chi A_C(i) L(i)^{1-\eta} \frac{p(i)i}{\tau_0^C} \left[\frac{A'_C(i)}{A_C(i)} + (1-\eta) \frac{L'(i)}{L(i)} - 2 \frac{(1-\alpha)}{i} \right]$$

where $\chi \equiv \alpha^{\alpha-1/\eta} (1-\alpha)^{\eta-\alpha+1/\eta+1}$.

See Appendix for proof. ■

Stated in the opposite way, when the necessary household expenditure on the subsistence good (γ) is *high* (enough), the overall welfare loss for remote regions will *exceed* the loss for less-remote regions. This is due to the relative poverty of these regions, such that subsistence imports are a relatively large share of total expenditure, as well as the fact that a given rise in external (international) trade costs raises import prices relatively more for more distant regions. This proposition represents our main result: on the one hand, when the condition in Proposition 1 holds, a region that is distant from international gateways is insulated from trade shocks due to the fact that local income is relatively unaffected by those shocks. On the other hand, if the region is relatively reliant on imported necessities then remoteness will be detrimental, an effect operating via heterogeneity in income as well as heterogeneity in the domestic transport costs faced by different regions.²²

Finally, we note again that at some threshold distance from a port, \bar{i} , internal transport costs become too large, and regions beyond this distance are fully isolated from international markets.

6.1.6 A Model-Based Measure of Remoteness

A convenient by-product of the model is that it provides a straightforward, and novel, framework for ordering Nepal's regions by their relative remoteness – i.e., by the magnitude of internal trade costs that they face in order to reach international markets. Given the difficulty of directly measuring domestic trade costs, this ordering will be useful in the empirics. For instance, Atkin and Donaldson (2015) estimate internal trade costs in Ethiopia and Nigeria, but their approach is reliant on difficult-to-obtain, location-specific price data as well as information on the location of consumption versus production. Here we exploit information on expenditure shares and income, which are commonly recorded in household surveys.

Specifically, we use the NLSS data on income and consumption shares across regions to back out the measure of relative domestic trade costs.²³ To do this, we focus on a good for which there is

²²To be clear about the mechanism we have in mind, in the empirics we assume that fetal (F) health in period t , h_t^F , is some function of the mother's (M) welfare in t – i.e., $h_t^F = G(v_t^M(i))$. Since fetal health is increasing in the mother's welfare – i.e., $G'(\cdot) > 0$ – the probability of miscarriage will be declining in her welfare. In this way the comparative statics with respect to welfare described above are linked to fetal health and the probability of miscarriage.

²³Though we are interested in these values for 1989 we do not have data for this year. We therefore perform these

no domestic production such that all consumption comes from imports, consistent with our model in which countries are completely specialized. Then, given prices, (9), and consumption shares, (6) and (7), and noting that $M'(i) < 0$, it is straightforward to show that $\chi'_H(i) > 0$ and $\chi'_C(i) < 0$ – i.e., relative expenditure on H is increasing in the household’s relative remoteness while the expenditure share of C is falling. These effects are driven by two channels of response. First, with respect to the imported household good, its price rises with the distance to a port as domestic transport costs accrue, and since it is a necessary good this *increases* the share of expenditure on the good. Second, income is lower further from a port – due to the fact that the price of the export good C is falling in i , and therefore so is the local wage – which, again, increases the share of expenditure on the household good H . A similar logic implies that $\chi'_C(i) < 0$. This implies the following approach to estimating domestic trade costs: first, we regress the consumption share of kerosene and LPG (a purely imported subsistence good) in a region on average income in that region as well as the share in consumption of firewood, the major substitute for kerosene and LPG. According to the model, the relative magnitude of the residuals from this regression then correspond to the relative internal trade costs faced by each region. In other words, conditional on income and the consumption of substitute goods, variation in the consumption share of a purely imported good must reflect variation in domestic trade costs.²⁴

As a simple check of the reasonableness of this exercise, Figure 8 plots this measure of domestic trade costs (the residuals from the regression) against the simple road distance from Kathmandu to the main city in each of the 75 districts, a somewhat crude measure of remoteness but one that offers the benefit of having a cardinal interpretation.²⁵ There is a clear positive relationship, indicating that regions that are a greater road distance from the capital city have higher shares of kerosene and LPG fuel in total consumption, conditional on income and the availability of substitutes.²⁶ This suggests, first, that this good is likely a necessity due to the fact that its consumption share rises with price; and, second, that this good is more costly in more remote districts due to variation in domestic transport costs.

Figure A.5 illustrates how the remoteness measure based on the kerosene and LPG expenditure share (which we use in the regression analysis below) varies across Nepal’s 75 districts in 1995/1996. As one would expect, districts “nearest” to international markets were, firstly, those encompassing the

calculations for 1995/96, the earliest period we have.

²⁴We clearly abstract from differences in preferences across regions, which we assume do not systematically bias the estimates. The model is also silent on the role of substitute goods, though we control for them here. An alternative modeling approach would be to construct the household good as a composite of many imperfectly substitutable goods, for instance in a Constant Elasticity of Substitution bundle. The production side could then feature monopolistic competition, perhaps with each region producing a unique variety. The results would ultimately be qualitatively similar, and so we abstract from these complexities here.

²⁵In part road distance will be a crude measure of internal trade costs (remoteness) due to the geography of Nepal, which going South to North rises from flatlands to the peaks of the Himalayas. As a result, there are some regions that are quite close to Kathmandu as the crow flies, and yet are very difficult to access due to the terrain and quality of the roads. Conversely, there are regions at some distance from Kathmandu but that lie directly East or West and are therefore fairly connected to the capital. In addition, the simple road distance clearly fails to capture accessibility via other transport modes and various inefficiencies with respect to domestic logistics. Perhaps most importantly, there are regions that are distant from Kathmandu that contain urban areas that are well connected to international markets. In principle, all of these aspects should be captured in the theory-based measure.

²⁶Consistent with the model the unconditional relationship is also positive.

major cities, in particular Kathmandu but also Pokhara and Patan, which makes sense in light of the fact that nearly all goods that crossed the border into Nepal were immediately placed on rail transport to the cities. Also near international markets were districts on the border with India that possessed transit points, in particular the regions in which the cities Biratnagar and Birganj are located, two main transit points. Beyond this, a combination of infrastructure, logistics networks, historical settlement and other idiosyncrasies likely determined a region’s relative remoteness.

6.2 Regional Differences in Impact

In terms of potential mechanisms, the impact of the embargo on live births might be, broadly speaking, due to either a sudden dearth of critical imports or else due to the income effects arising from stalled export industries, as the model highlights. We explore these specific channels in the next section. First, however, we note that the impact due to these channels would have varied systematically across regions, as indicated by Proposition 1. In light of this, we document regional differences in the impact of the embargo on long-run outcomes for the exposed cohorts, which will help motivate a more focused exploration of the channels.

Specifically, we estimate and plot an individual treatment effect for each of the 14 Nepalese regions, exploiting the geographic stratification of the survey as described in Section 3. We estimate the following specification, which extends the specification in (3):²⁷

$$y_{mh} = \sum_{h=1}^{14} \delta_h \phi_h + \sum_{h=1}^{14} (\delta_h \times D_m) \beta_h + g(t) \gamma_0 + \epsilon_{mh} \tag{17}$$

where the dependent variable is log income; m denotes an individual born in region h ; δ_h is a region fixed effect; and ϕ_h and β_h are coefficients to be estimated. The treatment indicator for in-utero embargo exposure D_m takes value 1 if individual m was born in 1989 or 1990, and value 0 otherwise.²⁸ In particular, we focus on the estimates of β_h which capture the deviations in log income for the affected cohort in individual regions relative to unaffected cohorts. This specification therefore allows for a fully flexible relationship between the relative remoteness of a region and the relative impact of the embargo.

Figure 9 plots the coefficients, where the size of each circle reflects the relative average share of kerosene in consumption for households in a region. In the Figure we see that the impact of the embargo was quite similar across the urban areas. Beyond the urban areas the Central rural areas were affected similarly to or to an even greater extent than the urban ones, a result that goes counter to a story in which the distance to international markets is the only determinant of the impact of the

²⁷See Redding and Sturm (2008) for an example of a similar approach.

²⁸Some “untreated” adults born in the first three months of 1989 before the embargo was in force will be part of the treated sample, but as we do not have month of birth information in the NLSS data this is the most accurate definition of treatment we can employ.

embargo. For these regions the selection-induced rise in average income for the affected cohort was on the order of 40 percent. Given the relatively large share of imported kerosene in expenditure in the Central regions, the pattern of estimates seems largely supportive of a channel of impact operating through a relative reliance on imported necessities. At the same time, the Mountain region (the most remote region) was relatively unaffected by the embargo, as were other relatively remote regions, which is in line with the model’s prediction that beyond some threshold distance to international markets the embargo would have little impact.

Overall, it is perhaps unsurprising that individuals in regions that were most exposed to international markets – i.e., those living in urban areas – experienced greater positive selection due to the embargo. On the other hand, the urban areas were relatively well-off and therefore consumed a relatively small share of most items in their consumption bundle. In addition, they had access to a range of goods that to some extent could substitute for various now-scarce items. Rural areas not only were more reliant on key consumption goods, but had access to fewer potential substitutes, and Figure 9 is consistent with these trade-offs.

6.3 Mechanisms

In this subsection we continue to focus on the long-run impact on adult incomes for the cohorts that were exposed to the embargo, but we now separately interact the treatment indicator with one of two measures intended to capture the extent to which the import or export channel impacted households. First, we estimate the impact of in-utero embargo exposure on country-level average adult income using the optimal bandwidth either side of the 1989-1990 period, calculated as described in Calonico et al. (2014). Second, we interact the treatment indicator with the average share of kerosene & LPG in consumption in an individual’s district-of-birth, representing a potential import channel of impact.²⁹ And finally, we interact the treatment indicator with the share of the population in a district that works in an export industry, noting that the informal sector is very large in Nepal so that in many districts this value is effectively zero. Since both the expenditure share of kerosene & LPG and the export intensity are potentially correlated with many other district features, we try to control for as many of these features as possible, though ultimately the estimates should be interpreted as suggestive rather than definitive. Most importantly, we control for district export intensity in the kerosene & LPG regression, and kerosene & LPG consumption in the export intensity regression. Furthermore, since both variables may be simple proxies for the average income in a district we control for this as well. We then add controls for the average age and percent male within a district and in the kerosene & LPG regression we control for the share of firewood in consumption, since it is the major alternative fuel source.³⁰

²⁹This measure is calculated using data from 1995/96 since that is the earliest data we have.

³⁰Many media outlets reported that the most outwardly visible effects of the embargo were the cessation of traffic throughout the country along with the widespread cutting down of forests for firewood. To the extent that cooking fuel shortages were an important driver of the observed cohort selection due to the embargo, we would expect that districts with greater reliance on firewood would be impacted less.

Table 6 plots the results from these specifications. Column (1) shows that being born in the embargo period raised average adult income for the exposed cohort by 30 percent, which is clearly a large effect. One should keep in mind the discussion from the previous section noting that the effect is nearly exclusively driven by the richer Nepalese regions.³¹ More generally, this is in line with our findings on adult women’s educational attainment in Section 5, which also increases for the cohort exposed in-utero to the embargo. As noted in Section 2, fuel imports fell by over 95 percent during the embargo period, and for much of the population kerosene or LPG was the primary fuel used for cooking. Column (2) reports the coefficient on the interaction of the embargo treatment with the average share of kerosene & LPG in district expenditure (conditional on the controls described above) and we see that it is positive and significant at the five percent level. We take this as suggestive evidence that the near-cessation of critical imported necessities played a role in the observed positive selection of the treated cohort. In column (3) we report the interaction of the embargo indicator with the share of the district population that works in an export industry (conditional on the controls described above). We find a strong, positive and highly significant effect, suggesting that the income shock due to the embargo may have been important. However, this measure may at the same time proxy for a district’s overall exposure to trade, rather than narrowly capturing the export channel, and the estimates should be interpreted with this in mind.

7 Concluding Remarks

This paper begins to fill the gaps in the literature on the impact of trade embargoes by providing estimates of the short-run (miscarriage rates and live births) and long-run (educational attainment and income) consequences of an 15-month-long embargo. The estimates we present likely represent upper-bound effects as the embargoed country we study, Nepal, is landlocked and poor. On the other hand, there are 32 landlocked developing countries recognized as such by the U.N., with an aggregate population of over 450 million citizens. 16 of these countries are in Africa, and 10 are in Asia, continents which together account for a significant majority of the world’s 1.2 billion poorest citizens, suggesting that our findings have wide-ranging policy relevance. This context becomes even wider when we consider the additional set of countries that are heavily reliant on international trade, though are not landlocked, and are currently embargoed or at risk of being embargoed in the future.

We find a substantial decline in reported live births shortly after the embargo being imposed. We also find that adult women survivors of in-utero exposure to the embargo had nearly a year more of education than unexposed cohorts born immediately before March, 1989. It is unclear whether exposed women’s increased educational attainment is due to smaller cohort size that reduces competition in schooling, or because these women have stronger endowments at birth that aided them in surviving exposure to the embargo and then outperform unexposed cohorts. While there is a marginally significant increase in reported miscarriages of 1.7-2.3 percentage points immediately following the imposition of the embargo, there appears to be substantial underreporting of the lost pregnancies seemingly cap-

³¹Average income in these richer regions was around 15,000 Rupees.

tured in our results. This is a significant concern, as a currently estimated 2.08-3.79 million children are stillborn globally every year, and 98 percent of these children are in the developing world (Lawn et al., 2011). Taking into account the significant underreporting of miscarriages and stillbirths, the stillbirth rate is potentially much higher, indicating that we are capturing effects on a measure of child survival that may often be overlooked.

We find that in-utero exposure to the embargo raised adult income by approximately 2,400 Nepalese Rupees (approximately 20 U.S. Dollars) per month; a 30 percent rise for affected cohorts. We document a non-linear pattern of embargo effects on income based on costs of accessing international markets. The most remote regions of the country were virtually unaffected by the embargo while the least remote were hit relatively hard, consistent with an important role for internal transport costs as a buffer to international shocks. In contrast, relatively poor regions at a moderate distance from access points to international markets had similar or worse effects to regions with direct access to international markets, such as Kathmandu, which is suggestive of a role for income and the composition of the consumption bundle as an independent determinant of vulnerability to trade shocks. This is a new contribution to the literature on sub-national variation in access to trade and its effects on poverty, which has focused largely on increased exposure to market competition and international price volatility as causal channels rather than the composition of household consumption (e.g. see Topalova, 2010; McCaig, 2011; Kovak, 2011). Recent political instability in North Africa and the Middle-East has led to a renewed focus on the societal impacts of trade shocks to imported necessities, as these regions are highly dependent on food imports, and sharp increases in international prices of food following the start of the global recession in 2007 potentially set off the unrest that followed (e.g. see Bellemare, 2015; Ianchovichina et al., 2014). Our findings contribute to this literature on the intersection of economic forces and political phenomena.

After the outbreak of the Crimean conflict in 2014, Russia imposed an embargo on its own imports of nearly all food products from the U.S., Canada, Australia, the E.U., and several other European countries. This embargo is still in effect, barring a few minor relaxations, and has undoubtedly had a significant impact on the E.U. economy, which previously relied on Russian food purchases for over 10 percent of its total exports. Welfare effects of the self-imposed embargo in Russia are still largely un-investigated, but are also unlikely to be trivial. Northern Cyprus has been under U.N. embargo for over 30 years after declaring itself an independent state in 1983. All international travel and commercial business with the territory is routed through Turkey, which is the only country to recognize Northern Cyprus as a sovereign nation. The effects of this lengthy isolation from the global economy on welfare in Northern Cyprus again remain largely unexamined in the academic literature. The lack of existing research on how such embargoes impact the welfare of affected peoples is notable, as entire populations of countries are exposed in some cases, with potentially long-term, intergenerational consequences. Our findings provide the first set of rigorously estimated results on the possible human capital consequences of a trade embargo, and are clearly relevant given the widespread of embargoes as a policy tool globally.

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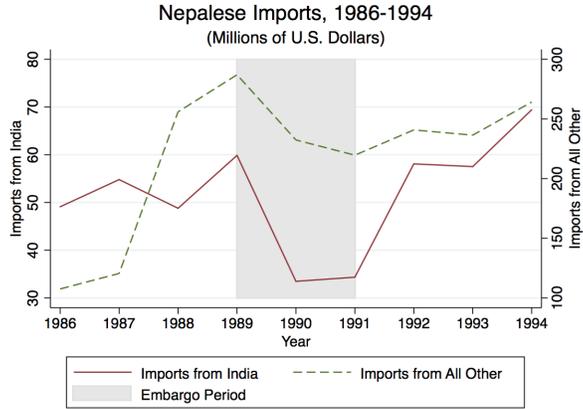
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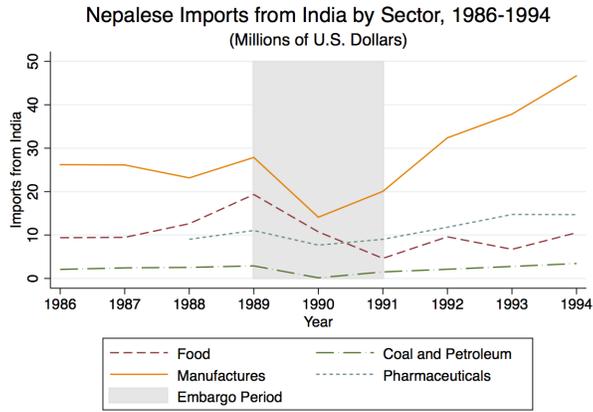
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Figure 1: Nepalese Foreign Trade During the Embargo

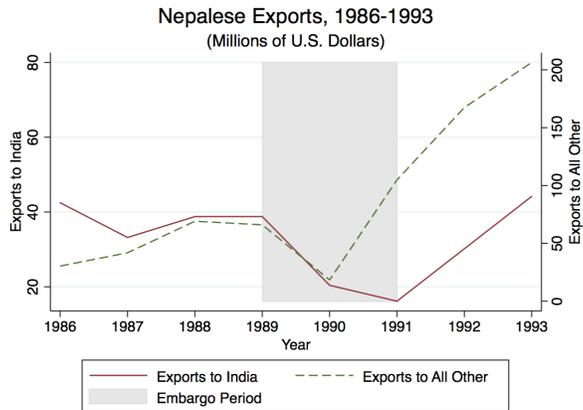
(a) Nepalese Imports



(b) Nepalese Imports by Sector

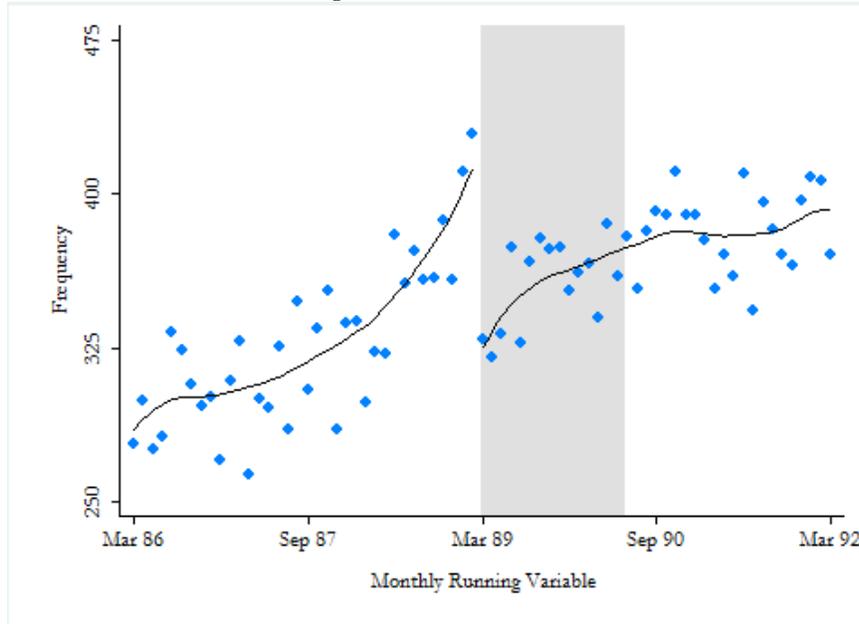


(c) Nepalese Exports



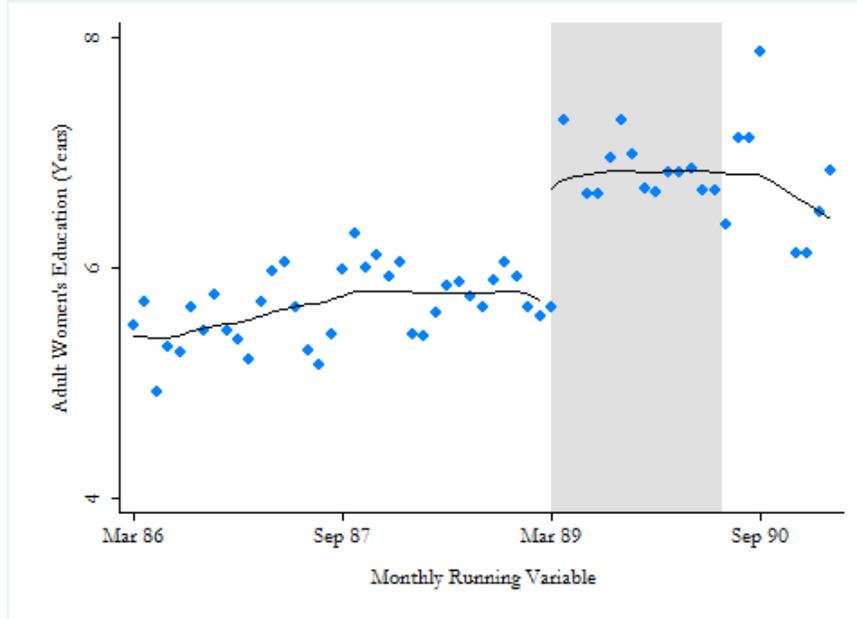
Notes: The figures show the value of annual Nepalese total imports, annual Nepalese imports by sector, and annual Nepalese exports in millions of US dollars from India and the rest of the world. The grey shaded area indicates the years the embargo was in effect, though it is important to note that the embargo was *not* in place for some portion of the first and final years (it began in March 1989 and ended in July 1990).

Figure 2: Live Births



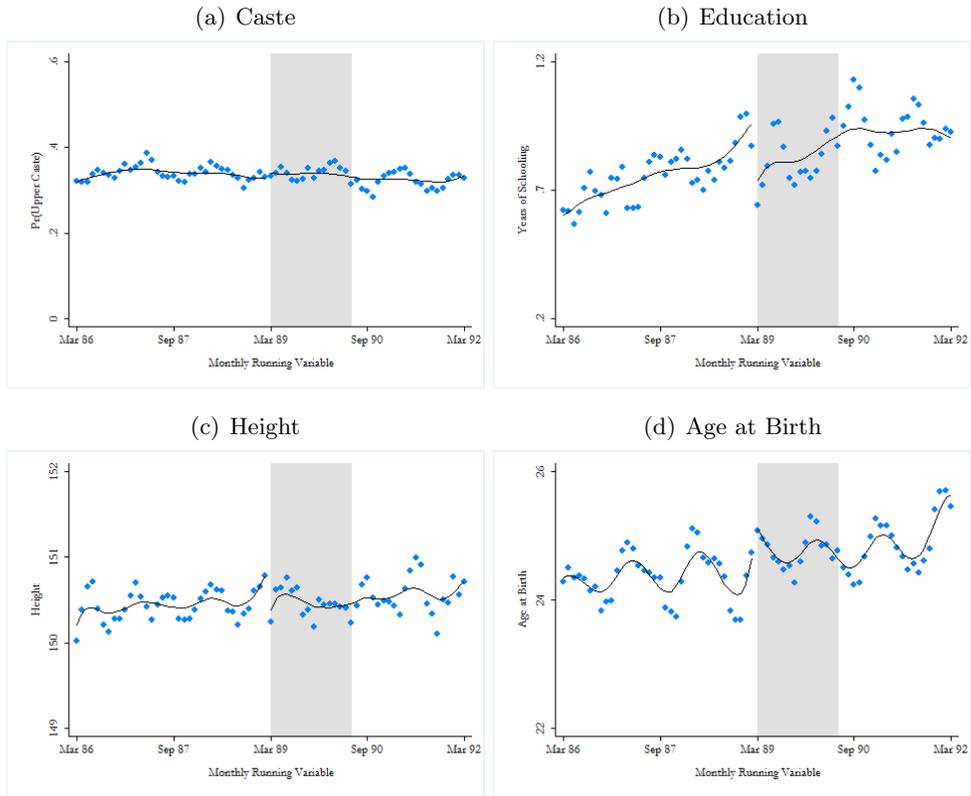
Notes: The graph shows the number of live births by month. The black curves are local linear regression plots of live births with a triangular kernel.

Figure 3: Long-Run Years of Schooling



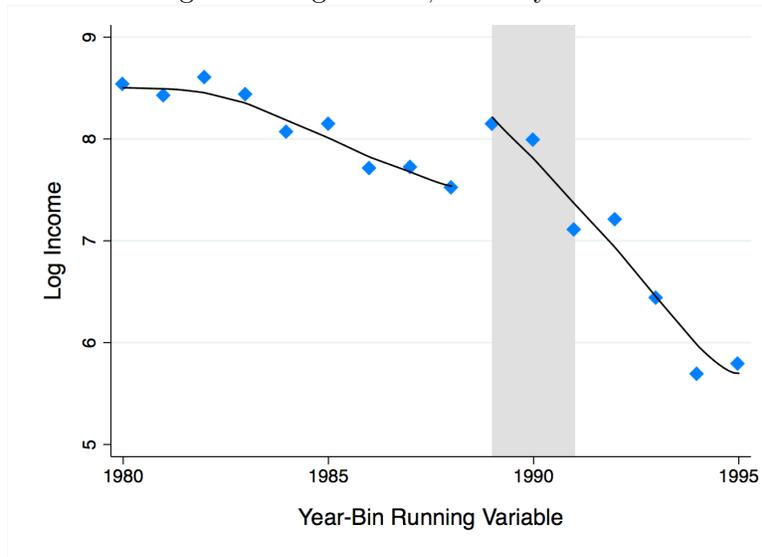
Notes: The graph shows 3-month smoothed averages of DHS survey respondents' years of completed schooling by month of birth, and local linear regression plots with a triangular kernel on either side of March 1989. The shaded region indicates the embargo months. The sample includes only women who were aged at least 18 years at the time of the survey.

Figure 4: Mother Characteristics



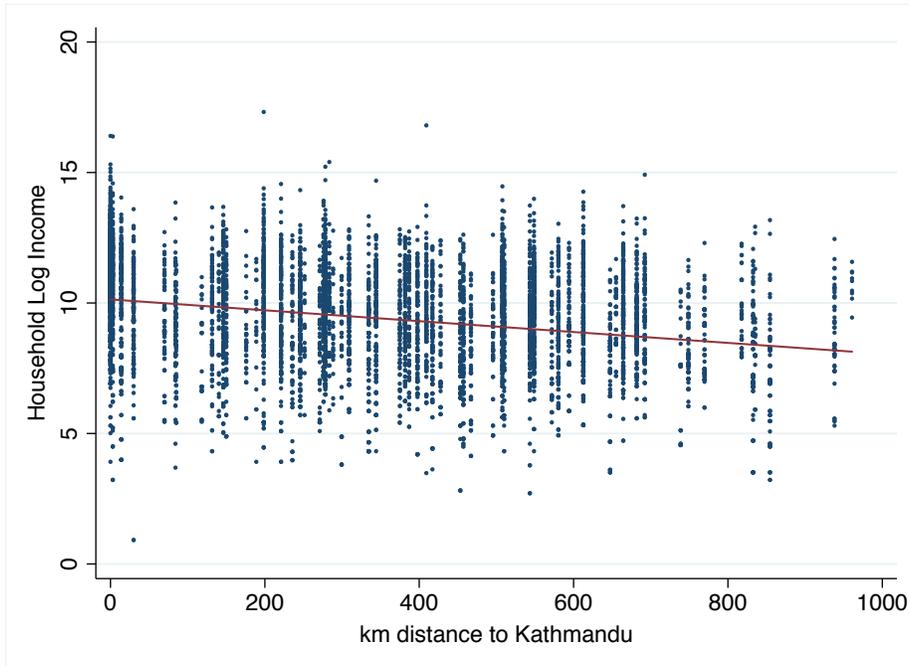
Notes: The graphs show 3-month smoothed averages of mother characteristics by child month of birth, and local linear regression plots with a triangular kernel on either side of March 1989. The gray shaded region indicates the embargo period.

Figure 5: Log Income, Country Level



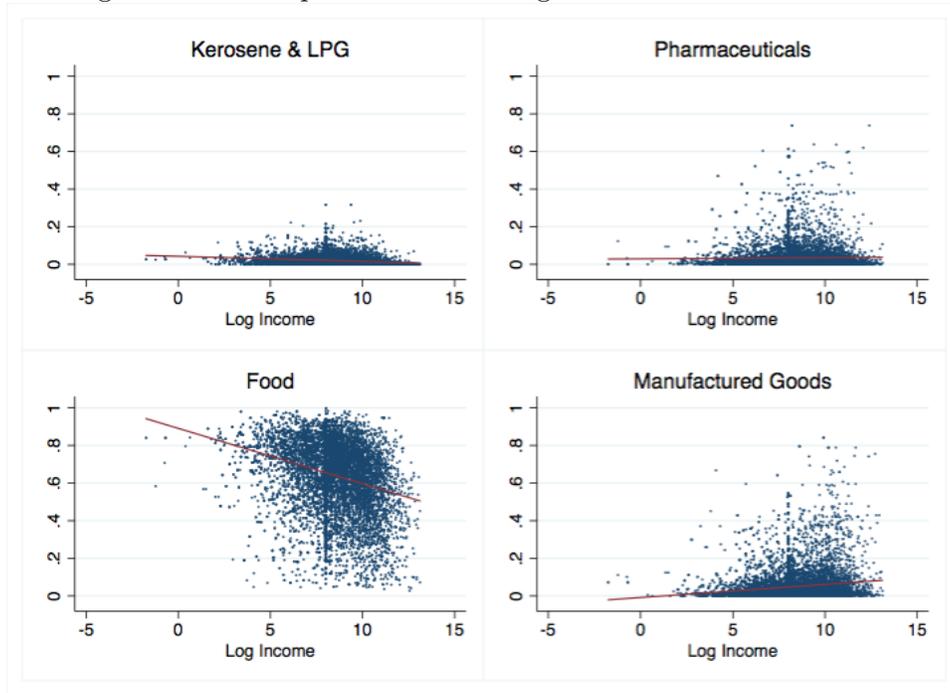
Notes: The figure plots the mean value of log total income by birth cohort. The gray shaded region indicates the embargo period. The curve fitted through the scatter plot is from a local linear regression with a triangular kernel.

Figure 6: Log Income Across Households vs Distance to Kathmandu



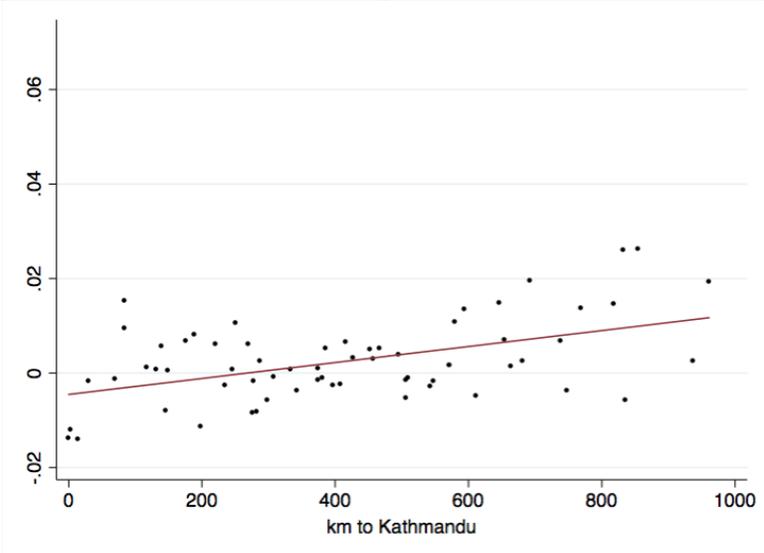
Notes: The Figures plot the log income across Nepalese households against the kilometer distance to Kathmandu.

Figure 7: Consumption Shares vs Log Income Across Households



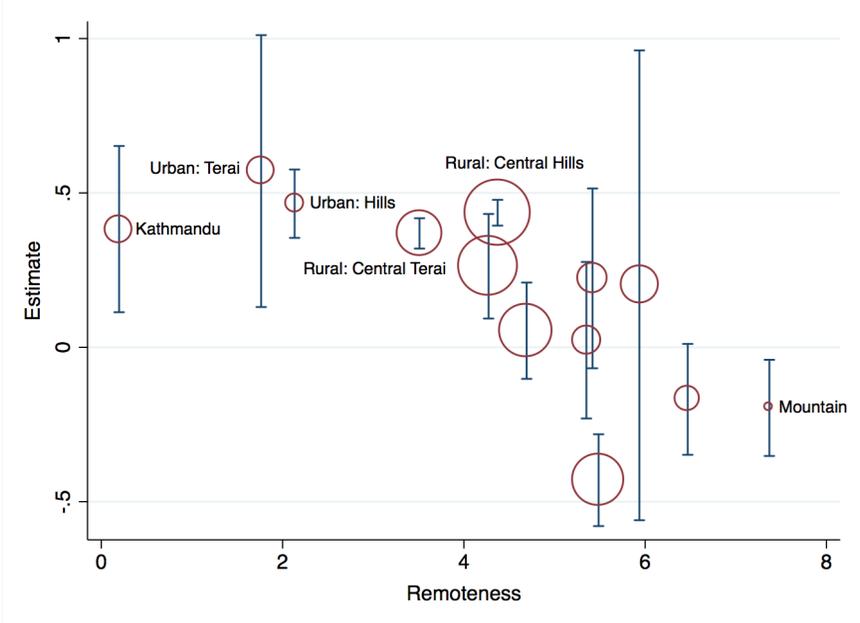
Notes: The Figures plot the consumption shares of each item versus log income across Nepalese households. The slope of the fitted line is negative and statistically significant for Kerosene & LPG; positive and not significant for Pharmaceuticals; negative and significant for Food; and positive and significant for Manufactured Goods.

Figure 8: Residual Fuel Consumption vs Distance to Kathmandu



Notes: The values on the Y-axis are the residuals of a regression of the average consumption share of kerosene and liquid propane gas in one of Nepal’s 75 districts on average income in the district as well as the average consumption share of other fuel types. The X-axis is the kilometer distance to Kathmandu. The fitted line has a positive and statistically significant slope (t-stat 4.42).

Figure 9: Estimates of Embargo Impact on Log Income



Notes: The figure plots the estimated impact of the embargo, including 95 percent confidence bands, on log income for each of Nepal’s 14 regions (the level of the survey stratification), obtained from a non-parametric regression, against the relative remoteness of the region from international markets. The remoteness measure is calculated as described in Section 6.1.6. The size of each circle reflects the relative average share of kerosene in each region’s household consumption.

Table 1: DHS Summary Statistics

Variables	(1)	(2)	(3)	(4)	(5)
	Mean	S.D.	Min	Max	Observations
Miscarriage	0.060	-	0	1	52,232
Stillborn	0.018	-	0	1	52,232
Neonatal Death	0.058	-	0	1	52,232
Mother Age at Birth	24.798	5.693	10	48	52,232
Mother Years of Education	1.214	2.787	0	14	19,831
Mother Height	150.560	5.458	105.9	192.6	15,246
Mother Upper Caste	0.343	-	0	1	19,833
Mother Has Some Education	0.219	-	0	1	19,833

Notes: The table shows summary statistics for children's health outcome variables and their mothers' characteristics from the pooled DHS data for children born during 1985-1995.

Table 2: NLSS Summary Statistics

Variables	(1)	(2)	(3)	(4)	(5)
	Mean	S.D.	Min	Max	Observations
Male	0.595	-	0	1	3895
Married	0.555	-	0	1	3895
Self Employed	0.626	-	0	1	3895
Age	21.248	2.349	17	25	3895
Income	3522.928	12814.78	0	240000	3895
Highest Grade Completed	7.529	3.352	1	18	1760

Notes: The table shows summary statistics for economic outcome variables and demographic characteristics from the NLSS Wave III (2010/11) for children born during 1985-1995.

Table 3: Live Births

Live Births						
	All Children		Male Children		Female Children	
<i>Panel A</i>						
<i>McCrary Test</i>						
	(1)	(2)	(3)	(4)	(5)	(6)
Log-Diff. in Height	-0.289*** (0.060)	-0.296*** (0.063)	-0.278*** (0.078)	-0.305*** (0.089)	-0.294*** (0.092)	-0.287*** (0.084)
Observations	26	20	26	20	26	20
Estimated Bandwidth	7.33	6.55	7.90	6.08	6.81	8.20
Data Bandwidth	24	18	24	18	24	18
<i>Panel B</i>						
<i>CJM Test</i>						
	(1)	(2)	(3)			
CJM T-statistic	-3.384***	-2.628***	-2.222**			
Observations	108,800	55,589	53,211			
Estimated Bandwidth (h_l, h_r)	(27.62, 25.22)	(33.40, 27.66)	(27.63, 25.56)			
<i>Panel C</i>						
<i>Parametric Test</i>						
Log-Diff. in Height	-0.156*** (0.035)	-0.175*** (0.039)	-0.171*** (0.047)	-0.178*** (0.055)	-0.141*** (0.051)	-0.171*** (0.058)
Data Bandwidth	24	18	24	18	24	18

Notes: Panel A shows results from McCrary tests implemented as described in McCrary (2008). Standard errors are in parentheses. The data bandwidths around the March 1989 cut-off in Panel A are restrictions on the sample placed by the authors before estimating the default bandwidth. Panel B shows estimates from a parametric OLS regression with linear splines, with standard errors clustered by the running variable in parentheses. Panel C shows results from density break tests as described in Cattaneo, Jansson, and Ma (2017). *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 4: Mother Characteristics

Mother Characteristics								
	Caste		Education		Height		Age at Birth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: Optimal Bandwidth</i>								
	h^*	$0.5 h^*$	h^*	$0.5 h^*$	h^*	$0.5 h^*$	h^*	$0.5 h^*$
Exposed In-Utero	-0.006 (0.010)	0.005 (0.012)	-0.072 (0.054)	-0.116 (0.080)	-0.102 (0.111)	-0.155 (0.172)	0.093 (0.171)	0.220 (0.247)
Observations	57,466	29,015	43,137	21,606	32,124	15,984	56,037	28,267
Bandwidth	77.86	38.93	57.85	28.92	55.52	27.76	75.27	37.63
<i>Panel B: Smaller Bandwidth</i>								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Exposed In-Utero	0.008 (0.016)	0.012 (0.015)	-0.264* (0.128)	-0.179 (0.126)	-0.842*** (0.264)	-0.264 (0.249)	0.002 (0.392)	0.537 (0.439)
Observations	5,093	9,674	5,092	9,673	3,896	7,399	5,093	9,674
Bandwidth	6	12	6	12	6	12	6	12

Notes: The table shows results from a parametric OLS regression with a linear spline. Standard errors clustered by the running variable are in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 5: Long-Run Education

	Education			
	Years of Schooling		Any Schooling	
	h^*	$0.5 h^*$	h^*	$0.5 h^*$
<i>Panel A: Linear Spline</i>	(1)	(2)	(3)	(4)
Exposed In-Utero	0.761** (0.365)	1.049* (0.597)	0.056 (0.041)	0.106** (0.050)
<i>Panel B: Quadratic Spline</i>	(1)	(2)	(3)	(4)
Exposed In-Utero	1.074* (0.636)	0.657 (0.786)	0.140** (0.054)	0.168** (0.074)
Observations	2,453	1,202	1,910	950
Bandwidth	21.47	10.73	16.47	8.23

Notes: The table shows results from OLS regressions with linear splines. The sample consists of survey respondents who were at least 18 years old at the time of the survey. Standard errors clustered by the running variable are in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 6: Impact on Adult Log Income

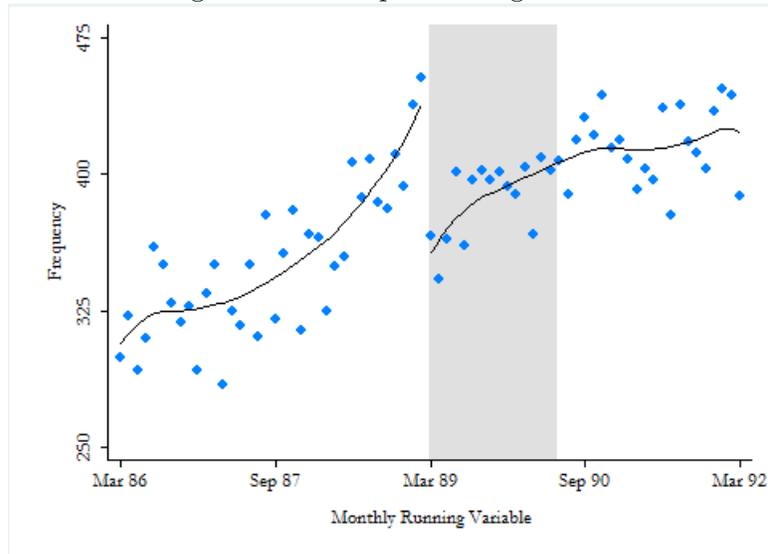
	(1)	(2)	(3)
Exposed In-Utero	0.30** (0.15)	-0.02 (0.02)	-0.38*** (0.11)
Exposed \times Kerosene & LPG		7.92** (3.60)	
Exposed \times Export Intensity			34.59*** (5.31)
Demographic Controls	N	Y	Y
Control for Regional Income	N	Y	Y
Observations	3,339	3,339	3,339
Bandwidth	5	5	5

Notes: Standard errors clustered by year of birth are in parentheses. Column (1) shows the impact on adult log income of having been born during the 1989-90 embargo. Column (2) interacts the embargo dummy with the average share of kerosene & LPG in total expenditure in a region. Column (3) interacts the embargo dummy with the share of formal employment in a region that is in export industries. In the final two specifications we also control for regional income as well as the age and sex of the individual. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

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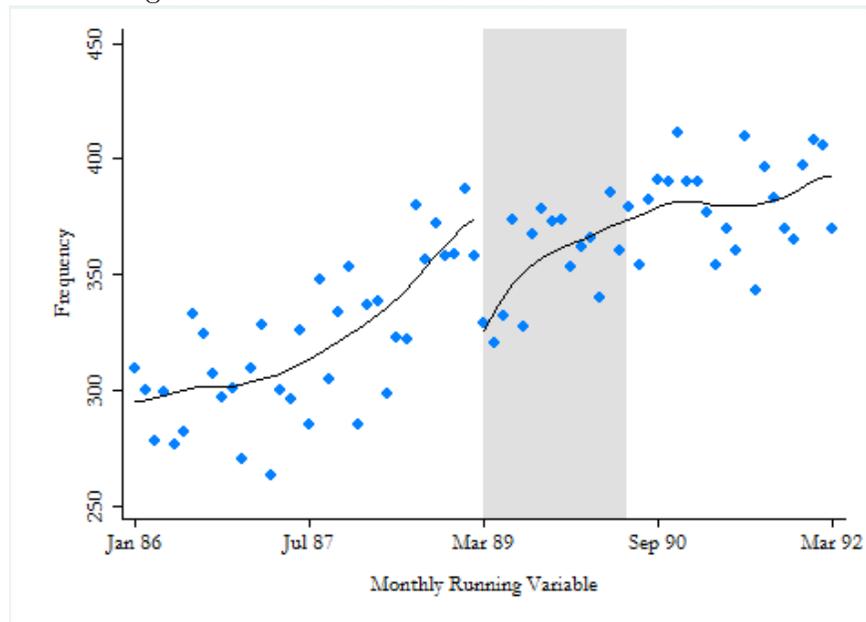
A Additional Figures and Tables

Figure A.1: Completed Pregnancies



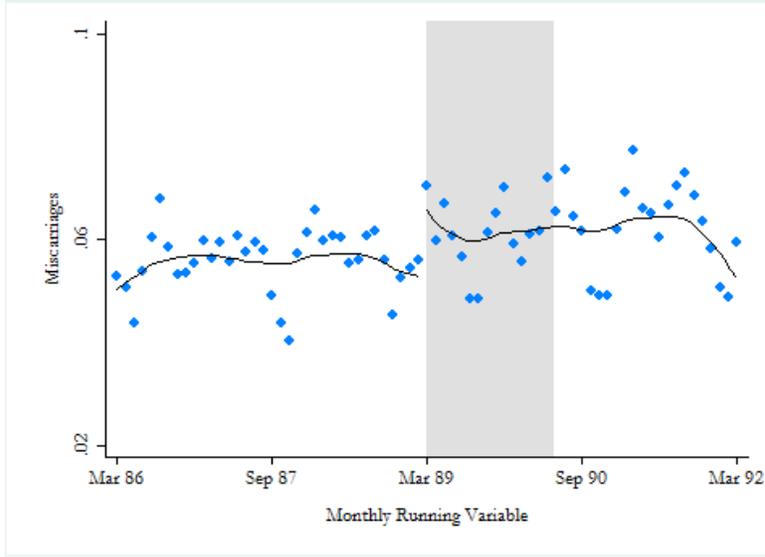
Notes: The bar graph shows the number of completed pregnancies by monthly bin. The red bars show the number of completed pregnancies during the embargo. . The black curves are local linear regression plots of completed pregnancies with a triangular kernel.

Figure A.2: Live Births without Jan and Feb 1989



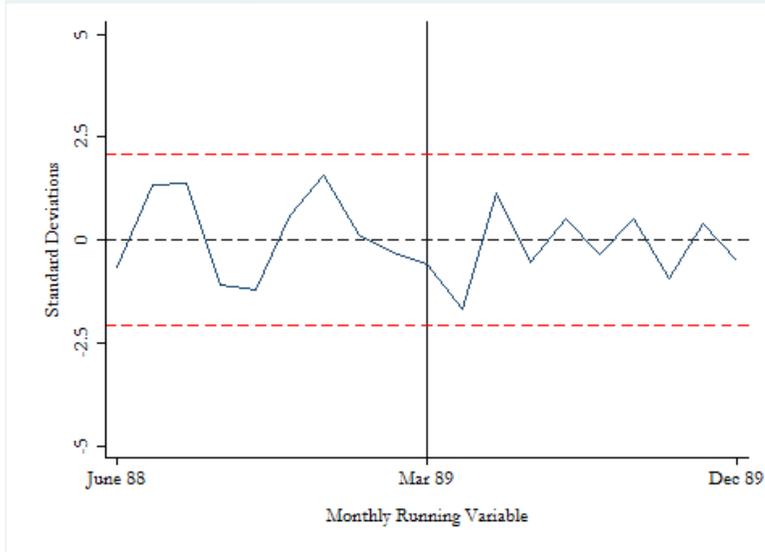
Notes: The graph shows the number of live births by month after dropping months January and February in 1989. The black curves are local linear regression plots of live births with a a triangular kernel.

Figure A.3: Miscarriages



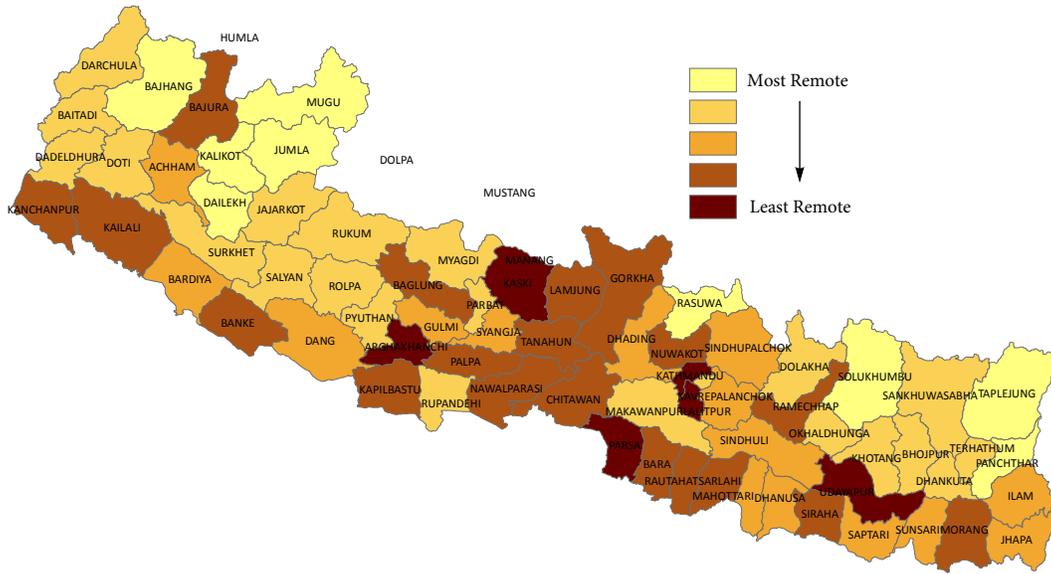
Notes: The scatter plot shows the 3-month moving average of the fraction of monthly completed births ending in miscarriage. The grey shaded area indicates the embargo period. The curve fitted through the scatter plot is from a local linear regression with a triangular kernel.

Figure A.4: Rainfall Shocks



Notes: The graph shows monthly rainfall shocks, measured in standard deviations from the average rainfall in that month over the past 20 years. The red dashed lines show the 95% confidence interval.

Figure A.5: Relative Remoteness of Nepalese Districts



Notes: The figure maps the relative remoteness of Nepal’s 75 districts, calculated as the residuals of a regression of the consumption share of an imported good (kerosene) across districts on average income across districts.

Table A.1: Live Births - 1996 DHS Wave Only

	Live Births					
	All Children		Male Children		Female Children	
<i>Panel A</i>	<i>McCrory Test</i>					
	(1)	(2)	(3)	(4)	(5)	(6)
Log-Diff. in Height	-0.253*** (0.090)	-0.321*** (0.115)	-0.226** (0.123)	-0.327** (0.165)	-0.265** (0.124)	-0.344** (0.146)
Observations	26	20	26	20	26	20
Estimated Bandwidth	10.08	5.94	10.66	5.76	10.86	7.73
Data Bandwidth	24	18	24	18	24	18
<i>Panel B</i>	<i>CJM Test</i>					
CJM T-statistic	-1.249		0.191		-1.042	
Observations	29,154		14,938		14,216	
Estimated Bandwidth (h_l, h_r)	(32.80, 29.83)		(82.11, 70.56)		(27.57, 24.76)	
<i>Panel C</i>	<i>Parametric Test</i>					
	(1)	(2)	(2)	(3)	(3)	(3)
Log-Diff. in Height	-0.156*** (0.035)	-0.175*** (0.039)	-0.171*** (0.047)	-0.178*** (0.055)	-0.141*** (0.051)	-0.171*** (0.058)
Data Bandwidth	24	18	24	18	24	18

Notes: Panel A shows results from McCrory tests implemented as described in McCrory (2008). Standard errors are in parentheses. The data bandwidths around the March 1989 cut-off in Panel A are restrictions on the sample placed by the authors before estimating the default bandwidth. Panel B shows results from density break tests as described in Cattaneo, Jansson, and Ma (2017). Panel C shows estimates from a parametric OLS regression with linear splines, with standard errors clustered by the running variable in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table A.2: Live Births - Without Jan and Feb 1989

	Live Births					
	All Children		Male Children		Female Children	
<i>Panel A</i>	<i>McCrary Test</i>					
	(1)	(2)	(3)	(4)	(5)	(6)
Log-Diff. in Height	-0.150*** (0.048)	-0.138* (0.074)	-0.093 (0.067)	-0.064 (0.094)	-0.230*** (0.075)	-0.205* (0.146)
Observations	26	20	26	20	26	20
Estimated Bandwidth	12.13	5.11	11.71	6.09	10.45	4.89
Data Bandwidth	24	18	24	18	24	18
<i>Panel B</i>	<i>CJM Test</i>					
CJM T-statistic	-2.323**		-1.612		-1.700*	
Observations	107,960		55,128		52,832	
Estimated Bandwidth (h_l, h_r)	(26.39, 26.12)		(31.27, 28.92)		(28.10, 25.12)	
<i>Panel C</i>	<i>Parametric Test</i>					
	(1)	(2)	(3)	(4)	(5)	(6)
Log-Diff. in Height	-0.083** (0.033)	-0.103*** (0.035)	-0.084** (0.039)	-0.088** (0.040)	-0.084 (0.053)	-0.121** (0.059)
Data Bandwidth	24	18	24	18	24	18

Notes: Panel A shows results from McCrary tests implemented as described in McCrary (2008). Standard errors are in parentheses. The data bandwidths around the March 1989 cut-off in Panel A are restrictions on the sample placed by the authors before estimating the default bandwidth. Panel B shows results from density break tests as described in Cattaneo, Jansson, and Ma (2017). Panel C shows estimates from a parametric OLS regression with linear splines, with standard errors clustered by the running variable in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table A.3: Live Births - Robustness Checks

	Live Births			
	(1)	(2)	(3)	(4)
	Smaller Bandwidths		Quadratic Spline	
Exposed In-Utero	-0.225*** (0.044)	-0.296*** (0.044)	-0.220*** (0.050)	-0.262*** (0.048)
Data Bandwidth	12	6	24	18

Notes: Columns (1) and (2) show results from regressions with a linear spline. Columns (3) and (4) show results from regressions with a quadratic spline. The sample includes all pregnancies completed in the specified data bandwidth around the cut-off. Standard errors clustered by the running variable are in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table A.4: Live Births - Seasonality Tests

Live Births				
	March 1988		March 1990	
<i>Panel A</i>				
<i>McCrary Test</i>				
	(1)	(2)	(3)	(4)
Log-Diff. in Height	-0.117 (0.066)	-0.107 (0.063)	-0.022 (0.062)	-0.036 (0.072)
Observations	26	20	26	20
Estimated Bandwidth	7.08	7.71	6.93	5.21
Data Bandwidth	24	18	24	18
<i>Panel B</i>				
<i>CJM Test</i>				
	(1)	(2)		
CJM T-statistic	2.398**	-1.020		
Observations	108,800		108,800	
Estimated Bandwidth (h_l, h_r)	(43.60, 55.61)		(22.14, 19.54)	
<i>Panel C</i>				
<i>Parametric Test</i>				
Log-Diff. in Height	0.057 (0.048)	0.030 (0.053)	-0.005 (0.032)	0.033 (0.030)
Data Bandwidth	24	18	24	18

Notes: Panel A shows results from McCrary tests implemented as described in McCrary (2008). Standard errors are in parentheses. The data bandwidths around the March 1989 cut-off in Panel A are restrictions on the sample placed by the authors before estimating the default bandwidth. Panel B shows estimates from a parametric OLS regression with linear splines, with standard errors clustered by the running variable in parentheses. Panel C shows results from density break tests as described in Cattaneo, Jansson, and Ma (2017). *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table A.5: Health Outcomes

	Miscarriage		Neonatal Mortality		Adult Female Height	
	h^*	$0.5 h^*$	h^*	$0.5 h^*$	h^*	$0.5 h^*$
<i>Panel A: Optimal Bandwidth</i>	(1)	(2)	(3)	(4)	(5)	(6)
Exposed In-Utero	0.007 (0.005)	0.006 (0.007)	-0.001 (0.004)	-0.001 (0.006)	-1.154 (1.175)	-2.283 (1.835)
Observations	42,668	21,357	51,196	26,121	372	183
Bandwidth	57.44	28.72	68.96	34.48	9.51	4.75
<i>Panel B: Other Bandwidth</i>	(1)	(2)	(3)	(4)	(5)	(6)
Exposed In-Utero	0.023* (0.012)	0.017* (0.010)	-0.005 (0.010)	0.004 (0.006)	-1.517 (1.530)	-0.144 (1.134)
Observations	5,093	9,674	5,093	9,674	243	490
Bandwidth	6	12	6	12	6	12

Notes: The table shows results from OLS regressions with linear splines. Standard errors clustered by the running variable are in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

B Proof of Proposition 1

Taking the first comparative static result from equation (16), we simply determine for the conditions under which $\frac{dv(i)}{d\tau_0^C di} < 0$. It is straightforward to show that:

$$\frac{dv(i)}{d\tau_0^C di} = \chi A_C(i) L(i)^{1-\eta} p(i)^{1-\alpha} \left[-\frac{A'_C(i)}{A_C(i)} - (1-\eta) \frac{L'(i)}{L(i)} + 2 \frac{(1-\alpha)}{i} \right] - \gamma p(i)^{-\alpha} \frac{2\alpha^2}{i} \quad (18)$$

where $\chi \equiv \alpha^{\alpha-1/\eta} (1-\alpha)^{\eta-\alpha+1/\eta+1}$. Setting $\frac{dv(i)}{d\tau_0^C di} < 0$ and rearranging produces the condition in Proposition 1. ■