The Human Capital Legacy of a Trade Embargo *

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November 17, 2018

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 large decline in reported live births shortly after it began. Families without older sons to bolster household income also experienced increased child miscarriage and infant mortality during the initial embargo months. Adult women survivors of in-utero exposure have higher wages than untreated cohorts, due in part to receiving larger parental educational investments during childhood to compensate for embargo exposure, and also marry into households with higher income.

Key Words: In-utero, trade embargo, long-term health
JEL Codes: I15, O11, O15

*We thank the editor Claudio Michelacci and three anonymous referees for very helpful comments and suggestions. We thank Shiva Raj Adhikari for help with the NLSS data, and Alexandra Marr and Richard Wright for excellent research assistance. We further thank Alan Barreca, Sonia Bhalotra, Justin Cook, Adeline Delavande, Lakshmi Iyer, Robert Jensen, Eliana La Ferrara, Giovanni Mastrobuoni, Peter Schott, and numerous seminar and conference participants for their comments. All remaining errors are our own.

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1 Introduction

Scant rigorous evidence exists regarding the impact of trade embargoes[1] on health and economic outcomes of individuals in embargoed countries. 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 frequently used policy instrument today. 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 – such as the embargo of Sudan imposed in 1997. Importantly, embargoed countries in current times are nearly always poor and import-dependent and thus vulnerable to trade shocks. While estimates of the impact of embargoes have been reported across academic disciplines[3], these often do not disentangle the effects of the embargoes from those of other circumstances, such as conflict during the embargo period.[4] In this paper we examine 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. 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 the 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 begin by showing that the embargo reduced the number of reported live births by as much as 27 percent a month after it began, indicating that the shock was severe enough to cause significant early-life child mortality. In light of this stark finding, our subsequent analyses focus both on exploring the causes of this decline, as well as investigating its long-run implications. With respect to the short run, we find that having men in the household at the time of the embargo was key. We show that in families *without* a son at the time of the embargo miscarriages increased by 1-2 percentage points due to the shock, while male infant mortality in these households rose by about 5 percentage points, a finding that is also consistent with the higher health fragility of newborn boys (e.g., see Waldron, 1983; Naeye et al., 1971). In contrast, households with at least one older son in the family experienced no such increases in child mortality. Taken together, these findings suggest that sons' earnings

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

[2] Historical 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.

[3] For 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.

[4] For example, the U.N. Security Council imposed a complete embargo on Iraq in 1990 that lasted until 2003, and 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.

were critically important to insure households against the embargo's adverse impact.

Shifting to the long-run effects, we find that adult women who survived in-utero exposure obtained 0.7-0.9 additional years of schooling compared to unexposed cohorts. We show that this effect is driven by exposed women's increased entry into education by a margin of 7-10 percentage points, pointing to increased parental human capital investment in embargo-affected children.[5] We also find that the exposed cohort of surviving adult women had 14.8-17.2 percent higher adult income compared to unexposed cohorts, and on average married into households with significantly higher income. We take these results further by exploiting heterogeneity in households' exposure to the shock, which we argue is a function of their effective proximity to international markets. We begin by outlining a theoretical framework (developed formally in Appendix B) in which a region's relative "remoteness" from global markets has an ambiguous effect on the relative severity of embargo effects. On the one hand, due to Nepal's poor infrastructure and mountainous terrain more remote regions may be effectively insulated from international trade shocks; that is, high transport costs within the country may mitigate the effects of external shocks. On the other hand, more remote regions are much poorer, in part as a consequence of the domestic trade frictions that they face. As a result, they may be relatively vulnerable to a trade shock,[6] particularly when key subsistence goods such as fuel, salt and pharmaceuticals are imported, as they were in Nepal in 1989. Taking this discussion to the data, we find that women that were exposed in-utero to the embargo and survived, and were located in districts relatively far from trade routes are 1.6-2.5 centimetres taller than unexposed cohorts, whereas the heights of women survivors in districts close to trade are unaffected. This is consistent with positive selection of survivors and increased early-life death of less healthy children in poor, remote regions, indicating that these regions experienced the most adverse impact of the embargo (Bozzoli et al., 2009; Almond, 2006).[7] Similarly, we find that the increased wage income and marital household income of exposed women survivors is concentrated in districts close to trade routes, which are wealthier. We argue that this is consistent with evidence that the returns to education are significantly higher in these wealthier areas compared to more remote districts. Finally, men in poorer remote districts who survive exposure have lower wage income by 8.7 percentage points compared to unexposed cohorts, consistent with their higher fragility in childhood.

We argue that the context of the Nepalese economy and society is key to interpreting our findings. In particular, we highlight two features of the Nepalese economy and society at this time that likely quickened, and heightened, the impact of the embargo: low savings and

[5] We find no discernible effects on the education of male survivors, but the sample of surveyed men is limited as they are not surveyed in every DHS wave, and only some are interviewed when they are.

[6] This 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.

[7] The heights of surveyed men are not measured, preventing us from analysing the impact on height for both genders.

stocks of goods due to widespread poverty, and a patriarchal economy that relied on male labour for earned wage income outside the home. On the first point, it is unlikely that most Nepalese households had savings or stocks of essential goods to smooth the adverse impact of the embargo – i.e., most households were likely living at subsistence income levels. This suggests that the consequences of the embargo were felt quickly and harshly by most Nepalese households, which is consistent with news reports at the time that reported shortages of essential goods just days into the embargo.[8] This is also evidenced by the high child labour rate in the country at the time. Over 40% of children aged 5-14 are estimated to have participated in the labour force in 1996 according to a large national study conducted by the International Labour Organisation (Suwal et al., 1997). Both the empirical and theoretical literatures indicate that the decision to send children into the labour force, rather than to school, is driven by the inability to meet subsistence levels of consumption without the child's income (e.g. see Basu and Van, 1998; Baland and Robinson, 2000; Ravallion and Wodon, 2000). These pressures were likely heightened due to the embargo. On the second point, like other countries in South Asia, Nepal is a patriarchal society (e.g. see Furuta and Salway, 2006; Suwal et al., 1997; Acharya and Bennett, 1983). Men are likelier to participate in the wider market economy to earn wages and the overwhelming majority of women engage in home production or are self-employed, mostly due to social norms that limit their movements outside the home from childhood until after marriage. Son preference and discrimination against girls in health investments from a young age are highly prevalent (Guilmoto, 2009; Leone et al., 2003), such that the child sex ratio has historically been male-biased. Even among children, boys are much likelier to be earning wages by working in the market economy than girls (Suwal et al., 1997). Taken together, these two features of the Nepalese economy suggest that having at least one son in the family, even if relatively young, was crucial to insure household income against the embargo's adverse effects. Indeed, we find the existence of a previous son to be key to mitigating the impact of the embargo.

An oft-cited concern is that lost pregnancies and infant deaths are underreported. We find that this is likely true in our data as well; specifically, the large decrease in reported live births that we find following the start of the embargo is not matched by an equivalent increase in reported child deaths, stillbirths, or miscarriages in the DHS data.[9] In general, this phenomenon has been attributed to the fact that over half of all births in developing countries take place at home rather than at a medical facility, and thus may not be reported, as well as the fact that there is a social stigma for mothers who lose late-term pregnancies

[8]For example, the LA Times (April 10, 1989) reported three weeks into the embargo that "Women and children [...] said they hadn't eaten a cooked meal in a week". Six weeks into the embargo the NY Times reported that traffic across the main border crossing in Birgunj "is a trickle compared with the level before March 23".

[9]Lawn 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.

(Darmstadt et al., 2009; Stanton et al., 2006; Heazell et al., 2016). Reassuringly, our long-run estimates provide additional confirmation of the broad finding of large effects. Since the long-run data are collected from more recent survey respondents who were in the womb in or close to March 1989, the respondents are of course different from the women who provided the information on pregnancy outcomes around March 1989, and the information on education and income they provide is not subject to the stigma or reporting challenges associated with pregnancy. Finally, the data on adult women's education and adult income come from independently gathered datasets (the DHS and NLSS, respectively), and our results show that in-utero exposure to the embargo affected these outcomes in both samples, increasing confidence in their robustness.

Our work relates to existing studies that examine the health effects of in-utero exposure to economic shocks. For example, van den Berg, Lindeboom, and Portrait (2006) find that children born during economic downturns in the Netherlands have shorter lifespans. In contrast, Cutler, Miller, and Norton (2007) find no effects on long term health of children born during the American dust bowl era. One advantage of our study is that the embargo that we examine constituted a large economic shock with a clearly defined start and end date, allowing us to accurately identify cohorts that were exposed in-utero, and to compute sharply identified quasi-experimental estimates of exposure on live births and human capital using data at the level of individual pregnancies.[10] 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 identify causal impacts. In contrast, our paper examines the impact of a policy instrument, one that affects the welfare of significant numbers of people who are typically economically vulnerable.

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 presents the empirical findings. Section 6 concludes with a discussion of the implications of our findings.

2 Background on 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.[11]

[10] Adhvaryu et al. (2018) uses individual-level data in Ghana to show that in-utero exposure to adverse cocoa price shocks increases the risk of mental health problems in later life, using a difference-in-differences strategy and regional variation in cocoa production.

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

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

Being a landlocked nation 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 (2006) 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 neighbours 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).

Nominally the breakdown in the relationship between India and Nepal had been caused by the inability of the two countries 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 (NY 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 LA Times (April 10, 1989) reported three weeks into the embargo that "Women and children [...] said they hadn't eaten a cooked meal in a week". 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 (shown as solid line). Since trade between Nepal and the rest of the world almost exclusively travelled via India, we also show Nepalese imports from all countries as well (dashed line). 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 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.

The effects of sudden and extreme shocks to market access have been explored in several papers, primarily in a historical context in which the focus is typically on the short-run consequences. Juhász (2018) explores the effect of the Napoleonic Blockade on long-term economic development. Similarly, Feyrer (2009) exploits the closing of the Suez Canal in 1967 and its reopening in 1975 to estimate the impact of trade cost shocks on income. 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 related study, 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, exploring the role that regional differences in market access play in their outcomes.

3 Data

We use the 1996, 2001, 2006, 2011, and 2016 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.[12] 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 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. We also present estimated embargo impacts on the probability of infant mortality (death aged one year or less) and miscarriage, and test for impact heterogeneity by child gender and also by whether the family already has a son who potentially mitigates the embargo's adverse effect on household income. In the sample of completed pregnancies in a 24-month bandwidth around the cut-off, approximately 53% take place in households with at least one previously born son.

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 infant mortality rate (death aged one year or less) is 9.3 percent. The probability of a stillborn birth is however lower at 1.8 percent.[13] The other statistics show that mothers are 24.80

[12]Miscarriages 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.

[13]The 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

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 examine the effect of in-utero exposure to the embargo on educational attainment in adulthood for surviving women survey respondents, both at the extensive and the intensive margin, to see how parents' adjusted their educational investments in children exposed to the embargo. We further investigate the impact of in-utero exposure to the embargo on adult women's height to measure the extent of the health insult from the shock in the womb.[14]

For the analysis of long-run economic outcomes we exploit 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.[15] 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 monthly income of 3,523 Rupees, the equivalent of about 35 US dollars in 2017. We primarily exploit detailed information on respondents' sources of income from the Wave III survey – separating these sources into wage and non-wage components – since those exposed in-utero to the 1989 embargo (which lasted through 1990) will be 20-22 years of age, and will therefore (mostly) have finished school and will have some work history.[16] We define non-wage income as total household income less the respondent's own wage income.

In our long-run analyses we also exploit information on the district of birth of an individual, which allows us to exploit heterogeneity in the relative remoteness of households. Specifically, we calculate the distance from each district headquarters to Nepal's main border crossing in Birgunj, referred to as the "Gateway to Nepal".[17] The vast bulk of trade with

on these.

[14]Data for men's education is very limited as they are not surveyed in every round, and most are not interviewed even when they are. Men's heights are not recorded in any of the rounds.

[15]The 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.

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

[17]In the midst of the embargo, the NY Times in May 1989 stated that "The bridge to India [in Birgunj] is an economic lifeline for this impoverished nation of 18 million people because it has long been the main

India goes via Birgunj, as Birgunj hosts the only rail-connected multi-modal clearance depot (i.e., transportation hub) in Nepal. Particularly in the years prior to the embargo when air shipments were less common, most international trade would have travelled via Birgunj due to the fact that Nepalese trade with the rest of the world primarily went via the Port of Kolkata in India, after which it was put on rail or truck to Birgunj. Thus, the distance of a household from Birgunj in 1989-90 we feel is a reasonable proxy for the magnitude of domestic transport costs faced by households – i.e., their relative remoteness from international markets. We also note that rates of inter-district migration were reported at 8.6% and 9.6% in the 1981 (pre-embargo) and 1991 (immediately post-embargo) Censuses of Nepal respectively, increasing confidence that our results based on distance to international trade networks are not driven by embargo-induced migration.

4 Research Design

4.1 Estimation Methods

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 $t \geq t_0$, while 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.[18]

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 cutoff 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 $X_1, X_2, ..., X_J$ of width b covering all the calendar months t in the sample. Define the normalized cell-size for the jth 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 cutoff t_0, which is estimated as:

entry point by road and rail for most of Nepal's consumer and industrial goods."

[18] When we investigate specific pregnancy outcomes (miscarriage and infant death), adult educational attainment, as well as the height of women survey respondents, we use individual-level data on pregnancies i and mothers j, rather than monthly aggregates (as we do for live births).

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

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 using an automated procedure after fitting a fourth-order global polynomial separately on each side of the cutoff 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 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, $\mathbf{r}_p(u) = (1, u, u^2, ..., u^p)' \in \mathbb{R}^{p+1}$, $K_h(u) = \frac{K(u/h)}{h}$, $K(.)$ 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:

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

where $\hat{\sigma}^2_{1,p}(t_0)$ and $\hat{\sigma}^2_{0,p}(t_0)$ 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 cutoff 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 cutoff month, t_0). We use the optimal bandwidth algorithm described in Calonico et al. (2014) to determine the bandwidth h of observations on either side of the cutoff t_0. We report results from setting $g(t)$ to be linear or quadratic in t, but we do not attempt to use polynomials of higher order as this may bias the results (Gelman and Imbens, 2018). We follow Kolesár and Rothe (2018) and report robust standard errors to provide the most accurate confidence intervals for our estimates in the presence of a discrete running variable.

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 cutoff t_0 and also report results from specifications using half the optimal bandwidth, or $0.5\,h^*$. This reduces the precision of the estimates, but also increases confidence considerably in the accuracy of the embargo impact estimate. 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.

We also use (3) to estimate embargo impacts on adult wage and non-wage income in the NLSS data, by respondents' gender and distance to the main border crossing with India. In this case we use year of birth to define in-utero exposure as month of birth is not reported. We define cohorts as treated if they are born in 1989 or 1990, as nearly all respondents born in these years are exposed to the embargo for at least one trimester in-utero, barring those born before March in 1989.

4.2 Robustness of Results

The main threats to identification with this research design are: 1) the potential for household manipulation of exposure to the embargo during pregnancy, and 2) adverse shocks besides the embargo that occur around March 1989 and that also affect natal outcomes. With respect to 1), since 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. To ensure that our results on live births are not driven by non-random reporting close to March 1989, we report estimates from "donut" regression discontinuity designs that re-estimate specification (3) after dropping observations close to the cutoff, as recommended in Barreca et al. (2011). With respect to 2), since in-utero rainfall shocks have been shown to affect birth outcomes, we plot deviations from 20-year average monthly rainfall by calendar month in Figure A.1 to ensure that these are not driving our results. Reassuringly, there are no statistically significant deviations in monthly rainfall at the time of the embargo or in the months preceding.[19] We further report results from various robustness checks alongside our main results in the following section to rule out competing explanations such as seasonality effects and imperfect recall by survey respondents.

Throughout, we follow Hsu and Shen (2018) and rely on sub-sample analyses, rather than interaction terms, in order to examine impact heterogeneity, since using interaction terms of treatment with covariates can potentially bias the estimates and over-reject the null hypothesis of no effect. In addition, we perform a Benjamini-Hochberg correction on the p-values of the split sample to adjust them for potential false discovery of effects arising from multiple hypothesis testing, which would again lead to over-rejection of the null (Benjamini and Hochberg, 1995).

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

5 Results

The impact of the embargo on live births is the first critical finding that we seek to explain, and whose implications the paper explores. We do this primarily by estimating versions of specification (3) for alternative outcomes, as well as by progressively introducing additional heterogeneity via sub-sample analyses. More specifically, our approach proceeds as follows. First, we examine the characteristics of mothers who were pregnant during the embargo in order to determine whether there was embargo-induced selection on mother characteristics into pregnancy. Second, we explore the impact of the embargo on specific pregnancy outcomes (miscarriage and infant death), for which we also produce sub-sample estimates distinguished by whether the household had a previous son or not. Third, we explore the long-run impact of the embargo on adult educational attainment and income, as well as the adult height of women.[20] For these outcomes we introduce sub-sample heterogeneity in a household's relative remoteness from international markets during the embargo, an approach that we motivate by outlining a formal model that we relegate to an Appendix. Specifically, we examine impact heterogeneity by distance to Nepal's main international border crossing.[21] Finally, we investigate whether any of the effects are driven by households changing their fertility in response to the embargo, which we measure by examining changes in the probability that children exposed to the embargo have a younger sibling.

5.1 Live Births

Our first key result finds a sharp reduction in live births due to the embargo. Figure 2 plots 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 and the associated 95% confidence intervals using a triangular kernel and a 3-month data bandwidth on either side of the cutoff at March 1989. There is a large visible drop of just over 25% in the number of live births when the embargo begins. The same discontinuous decline is reflected in completed pregnancies in Figure A.2, approximately 85% of which are live births in any month, indicating that there is no corresponding 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, Figure A.2 suggests that these two months are broadly similar to the trend in monthly live births beginning two years before the embargo in 1987. Figure A.3 also shows no decrease in reported pregnancy intervals in these two months, which would be the case if mothers were erroneously reporting post-

[20] We 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.

[21] Note that we cannot perform this heterogeneity analysis by distance for the pregnancy outcomes, as district identifier information is not available for the 1996 wave of the Nepal DHS survey, which is the survey round that contributes the majority of our sample of pregnancy outcomes.

embargo births as taking place in these months. The figure instead indicates a decline in pregnancy intervals after the embargo, which is consistent with pregnancies ending sooner due to increased prenatal and perinatal death. Figure A.4 also shows that the decline in live births persists after dropping the data in the two months previous to the embargo. We still test the robustness of our results to potential non-random reporting of live births close to the cutoff using donut regression discontinuity specifications. The number of stillbirths reported in any calendar month is always less than 20, and while there is a small increase in reported miscarriages in March 1989 in Figure A.5, it is several orders of magnitude smaller than the decline in reported live births. 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 since, if they did, the number of total pregnancies would remain unchanged after March 1989 even if live births declined.

Table 3 shows the results from specification (1) in Panel A. We restrict the data bandwidth to lie within an 18-month window either side of March 1989. This yields an estimated bandwidth of 6.93 months in column (1), 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 27.0% in the month that the embargo began, an effect significant at the 1% level. In column (2) we present results from a donut regression discontinuity after dropping one month of data either side of the cutoff to test the robustness of the result in column (1). Despite the handicap that the reduced sample around the cutoff introduces, we still find a highly statistically significant decline of 21.0% in live births. Column (3) shows results from a donut regression discontinuity estimation to test for robustness, which drops two months of data before the cutoff, and one month of data after. Again, despite this handicap we still find a decline of 13.5%, statistically significant at the 10% level. The estimated bandwidths in columns (2) and (3) are 6.19 and 5.15 months, respectively. Columns (4)-(6) repeat the specifications in columns (1)-(3) for live male births, and columns (7)-(9) for live female births. Column (4) shows a highly statistically significant 27.4% decline in live male births in the month the embargo began, which falls to 20.5% in column (5) in the donut regression discontinuity specification, but remains significant at the 5% level. In column (6), the more severe donut regression discontinuity design reduces the estimated decline in male births further to a statistically insignificant 3.0%. In contrast, the estimated decline in female births in the month the embargo began is consistent and highly statistically significant across the standard McCrary test and the two donut regression discontinuity estimations at 28.5%, 23.2%, and 26.5% in columns (4)-(6), respectively. The difference in estimates by gender suggests that the "missing" live births induced by the embargo are disproportionately female, possibly due to high son preference and discrimination against daughters in health investments in Nepal that skews the child sex ratio at birth towards boys, despite male children having more fragile health in early life (Guilmoto, 2009; Leone et al., 2003).

Panel B of Table 3 shows the estimated CJM t-statistics (Cattaneo et al., 2017) described by equation (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 -3.63, which is statistically significant at the 1% level, and is consistent with column (1) in Panel A, indicating a large decline in the number of live births in the month the embargo begins. The donut regression discontinuity designs in columns (2) and (3) also yield t-statistics of 2.54 and 2.19 after dropping observations close to the cutoff, both statistically significant at the 5% level. Columns (4) and (5) show statistically significant declines in male births that mirror those in columns (1) and (2), with t-statistics of 2.71 and 2.02 respectively. Unlike the McCrary test in Panel A, the more demanding donut regression discontinuity estimation in column (6) shows a marginally significant decline in male births in Panel B, albeit with a much smaller t-statistic of 1.73. In columns (7)-(9) we find stronger evidence of declines in female births in the initial embargo month as in Panel A, with statistically significant t-statistics of 3.01, 2.27, and 1.98.

Panel C of Table 3 shows estimates from specification (3) using an 18-month bandwidth, as in Panel A. The results are qualitatively very similar 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 C 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)-(3) indicate that live births declined by 17.5%, 11.3%, and 9.1% respectively, with estimates declining as data is dropped around the cutoff, but nevertheless all statistically significant. Columns (4) and (5) show statistically significant estimated declines of 17.1% and 10.9% respectively in live male births, while in the most stringent donut regression discontinuity estimation in column (6) the estimated decline reduces to 7.0% and becomes statistically insignificant, as in Panel A. Columns (7)-(9) show consistent, statistical significant declines in female live births as in Panels A and B of 18.0%, 11.7%, and 11.5%, respectively.

We re-estimate the specifications in Panels A and C of Table 3 using a data bandwidth of 24 months instead of 18 months, and find very similar results reported in Panels A and B of Appendix Table A.1. We also estimate specification (3) using the optimal bandwidth and half the optimal bandwidth calculated using the algorithm in Calonico et al. (2014). The results are in columns (1) and (2) of Appendix Table A.2. Column (1) yields an estimated decline in live births of 8.6% with an optimal bandwidth of 33 months, and column (2) an estimated decline of 18.0% with a bandwidth of 16 months, both statistically significant at the 1% level. Columns (3) and (4) of Appendix Table A.2 show results from (3) using data bandwidths of 18 and 24 months, but with quadratic rather than linear splines. The results are very similar to those in columns (1) and (2) of Table 3, showing estimated declines in

live births of 24.8% and 21.1%, respectively.

To reduce concerns of maternal recall bias driving the results, we re-estimate specification (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 Panel B of Appendix Table A.2 show that the results are again very similar to the corresponding Panel C in Table 3 and (with the exception of column (4) in Panel B of Table A.2) statistically significant. The estimated declines in live births actually become much larger when we use smaller bandwidths of 6 and 12 months in columns (1) and (2). As a further falsification check, we also re-estimate specification (3) for the full sample of live births after redefining the cutoff month to be March in the three pre-embargo years of 1986-88, and the three post-embargo years 1990-92 to confirm that we are capturing embargo impacts beginning in March 1989 rather than a seasonal effect. The results of these six placebo tests are in Appendix Table A.3, estimated with bandwidths of 18 and 24 months in Panels A and B, respectively. We find statistically significant estimates in column (2) for 1991, but these have a magnitude of at most one-third of the estimated effect of the actual embargo. The other estimates are also much smaller than the estimated embargo effect and statistically insignificant, lending confidence that the effect we capture is not a seasonal phenomenon.

In this paper, we focus on the beginning of the embargo. In principle, it would be possible to conduct a parallel analysis for the end of the embargo, after the embargo was lifted on July 1, 1990. Such an analysis is challenging because the timing is less clear-cut: The New York Times reported in April 1990 that an agreement to end the trade embargo had already been widely anticipated in the Spring of 1990, but was then delayed due to disagreements in the negotation (NY Times, April 14, 1990). Furthermore, it is not clear how quickly trade flows were re-established once the embargo was lifted. For these reasons we focus our analysis on the start of the embargo. Nonetheless, to complement our analysis, Figure A.6 in the Appendix shows the change in completed pregnancies at the end of the embargo in July 1990. After the end of the embargo, there is a slight but insignificant increase in the number of completed pregnancies, and the level after the end of the embargo is similar to the numbers seen in the months shortly before the start of the embargo.

5.2 Short-Run Outcomes and Channels

The estimated fall in live births we find is large, but is consistent with the risky conditions at childbirth for the overwhelming majority of Nepalese mothers at the time. These conditions are in large part caused by Nepal's landlocked geography and high rate of poverty, and in this context the findings are consistent with 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 do not provide

this information for births during the embargo period, 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. In addition, the 1996 DHS data show 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. Over half of these deaths are caused by intrapartum asphyxia during labour and complications from pre-term birth, both of which are preventable with skilled medical supervision during childbirth, and have been the leading causes of perinatal mortality from the 1980s to the present day (approximately two-thirds of Nepalese mothers still deliver without skilled assistance; see Geetha et al., 1995; or Pradhan et al., 2012).[22]

As a result, the embargo may have increased perinatal mortality due to the significant economic impact of the embargo combined with the longstanding vulnerability of Nepalese mothers during childbirth. Maternal stress is also a likely causal channel. Evidence on the impact of shocks, such as bereavement, on 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. Importantly, these studies were undertaken in countries such as Sweden and Denmark, which are much wealthier nations than Nepal (László et al., 2013a; László et al., 2013b). It is also possible that the embargo reduced maternal nutrition, which then increased late-term miscarriage. 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)). In the next sub-section we show evidence suggesting that these potential channels of adverse impact were mitigated by insuring household income during the embargo, which was possible if there was already a son in the family.

5.2.1 Mother Characteristics

Figure 3 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 pregnan-

[22] Women'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.

cies 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.

Turning to mothers' characteristics, estimates from (3) in Panel A of Table 7 shows no statistically significant changes in the caste composition, height, or education of women completing pregnancies once the embargo begins in March 1989 compared to those completing pregnancies in the immediately preceding months. There is a small statistically significant difference in the age at birth of mothers of treated cohorts in column (8), but the magnitude of the age difference is only approximately 4 months, and can be explained by the fact that treated cohorts are born some months after untreated cohorts in our research design. The optimal bandwidth (h^*) calculated using the algorithm in Calonico et al. (2014) exceed 24 months on either side of the cutoff 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 cutoff 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 18 and 24 months either side of March 1989 in Panel B of Table 7. Estimates using even smaller bandwidths of 6 and 12 months are reported in Appendix Table A.4. We still find no statistically significant effects on mothers' caste composition in columns (1) and (2) of Panel B of Table 7, and also none in Table A.4. The estimated age difference in columns (7) and (8) of Panel B in Table 7 is again statistically significant and positive, and slightly larger in column (8) of Table A.4 with a 12-month bandwidth, but never exceeds 6 months. In columns (3) and (4) of Panel B of Table 7, We find statistically significant, negative differences of 0.160 and 0.139 in years of schooling respectively for mothers completing pregnancies just after the start of the embargo compared to mothers completing pregnancies just before. A maternal disadvantage in education of comparable magnitude is also found in Table A.4. While we find no differences in mothers' height in Panel B of Table 7, the smallest possible bandwidth of 6 months yields a large, statistically significant negative height difference for mothers of treated cohorts of 0.790 centimetres in column (5) of Table A.4. 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 also possibly poorer health. This is potentially explained by differential exposure of women to the embargo across regions such that poorer women in more remote regions were more adversely affected.[23]

[23] We investigate the latter mechanism in the next section.

5.2.2 Miscarriages, Infant Mortality, and Fertility

In Table 4 we present estimates from (3) on impacts of in-utero exposure to the embargo on the probability of miscarriage. 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 24 and 18 months. Along with standard errors in parentheses, we also present p-values corrected using the Benjamini-Hochberg method for false discovery of statistically significant effects arising from multiple hypothesis testing. These are in square brackets where appropriate.[24] In both Panels A and B we find no statistically significant impacts of the embargo on miscarriages in columns (1) and (2), and both estimates are close to zero. However in columns (3) and (4) of Panel A, we find 1.4 and 2.0 percentage point increases in miscarriages immediately following the embargo in the sub-sample of households without at least one previously born older son in the family. These effects are statistically significant at the 5% level, both before and after adjusting the p-values for multiple hypothesis testing. The effect is also present in column (3) in Panel B, showing a statistically significant estimated increase of 2.3 percentage points in miscarriages, but is statistically insignificant in column (4) of Panel B, albeit at a similar magnitude to column (3) in Panel A at 1.6 percentage points. In contrast, the sub-sample of households with an older son in the family experience no discernible increase in miscarriages in columns (5) and (6) in either Panel A or B. Altogether, this pattern of results points to increased vulnerability of pregnancies during the embargo when a son was not present in the family to bolster household income that otherwise likely declined. In poor households with few savings, evidenced by the heavy reliance on child labour in the country, this could have potentially stressed pregnancies enough to induce higher rates of miscarriage.[25]

Table 5 shows estimates from (3) on the effect of in-utero embargo exposure on the probability of infant death, separately for male and female children in Panels A and B respectively. In columns (1) and (2) of Panel A, we find statistically significant increases of 3.3 and 3.1 percentage points respectively in male infant mortality in the initial stages of the embargo. However, we see no increase in female infant mortality in the same columns or any of the other columns in Panel B. This is consistent with male children being more fragile in early life, particularly in developing countries. Columns (3) and (4) of Panel A show that the increased infant mortality among male children is again driven by households without a previous older son in the family, with statistically significant estimated mortality increases of 4.9 and 5.0 percentage points respectively. In contrast, columns (5) and (6) show no similar increase in male infant mortality in families where an older son is present. These results exactly mirror those in Table 4, indicating the importance of sons in bolstering

[24] We follow this convention in the following results tables as well.

[25] Table A.6, Panel A verifies that the effects for miscarriage are not driven by seasonality, as confirmed for live births in Table A.3.

family income during the embargo, and preventing adverse consequences to siblings in the womb or newly born.[26]

Table 6 shows results from specifications based on (3) in which the object of interest is the probability of children exposed to the embargo in-utero having a younger sibling. This may be important to the extent that household fertility responses[27] to the embargo potentially affect children's long run human capital and wage outcomes. However, the estimates are close to zero and statistically insignificant after correcting for multiple hypothesis testing, suggesting that these long-run outcomes were not altered by endogenous changes to sibling competition for household resources.

5.3 Long-Run Outcomes and Channels

In this section we explore the long-run impacts of the embargo, focusing on standard outcomes associated with health and human capital, namely adult height, education and income. Collectively, the pattern of estimated effects can provide insight into the mechanisms linking the observed fall in live births to the long-run well-being of the affected cohort. In addition, the more recent waves of the DHS and NLSS that we exploit in this section will allow us to differentiate between households according to their relative exposure to international markets at birth, a potentially important source of heterogeneity in the face of a trade embargo. Formally, we proxy for this exposure using the distance between a household's district headquarters and the main border crossing between Nepal and India in Birgunj, across which the vast bulk of traded goods crossed. This measure is described in more detail in section 3.[28]

The effects that different levels of pre-embargo exposure to international markets will have on households is not straightforward. In general, there may be several reasons that more remote households respond differently to the embargo relative to less remote households.[29] However, the diverse and sometimes extreme geography of Nepal implies that differences in the level of domestic transport costs faced by different parts of the country – which determine their relative exposure to international markets – likely played an important role in propagating or mitigating the embargo shock. For instance, in a recent and related paper Storeygard (2016) found that the magnitude of transport costs between sub-Saharan cities was an important factor in determining how external trade shocks affected those cities. More generally, the literature indicates that Nepalese households located far from international markets would have experienced a relatively mild income shock, since domestic transport

[26]Table A.6, Panel B again confirms that the effects for infant mortality are not caused by seasonality.

[27]In line with Becker's (1991) hypothesis of children as 'normal' goods, Black et al. (2013) find that in the U.S. fertility increases with men's income.

[28]We note that the information on district of birth is not available in earlier waves of the DHS and so we cannot perform this analysis for the short-run outcomes.

[29]For instance, the availability of fuel substitutes was quite different since remote forested areas could substitute toward firewood, which anecdotes suggest they did.

costs create a price-buffer between local and international markets.[30] On the other hand, remote households are significantly more likely to have been poor (see Figure 4), and so 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 fact, Figure 5 indicates that goods that are typically considered necessities, such as medicines and fuel, on average comprised a larger share of lower-income household consumption.[31] Taken together, these mechanisms suggest ambiguity in the role played by pre-embargo exposure to international markets – as reflected in domestic transport costs – in determining the welfare impact of the embargo. The greater a household's distance to markets, the more likely it is to be poor and economically vulnerable, and yet it will also be less exposed to external shocks overall.

In Appendix B we present an illustrative model that formally highlights this tension. In the model, more remote households have lower real incomes, all else equal. This is due to the fact that domestic transport costs create a wedge between local and international prices, such that the price that remote households get for their exports is relatively low and the price they pay for their imports is relatively high. We further assume that individuals have Stone-Geary preferences, which imply that household subsistence needs must be met prior to consuming other goods. Combining these features, we show that a sharp rise in external (international) barriers to trade – such as occurred due to the embargo – has an ambiguous *relative* impact on households,[32] such that it is not clear whether households facing higher domestic transport costs will be more or less harmed by the embargo relative to less remote households, since the answer depends on model parameters. For instance, we show that the welfare consequences of the embargo will be relatively worse for remote regions when the required expenditure on subsistence goods is large – an intuitive result.

With this framework in mind, we explore the health and human capital outcomes in aggregate, and then accounting for whether a household is above the median distance to the main border crossing (far) or below the median (close).

5.3.1 Height

We now present estimated impacts of the embargo on the adult height women exposed to the event in-utero. There is substantial evidence that exposure to economic shocks in early life potentially leads to lower height in adulthood (E.g. see Almond and Currie (2011), Banerjee et al., 2010; Chen and Zhou, 2007; Gorgens et al., 2012). 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

[30] See Redding and Sturm (2008) or Atkin and Donaldson (2015) for an overview of this literature.

[31] Note that the fitted lines for Pharmaceuticals and Kerosene & Liquid Propane Gas (LPG) have a negative and statistically significant slope

[32] Note that the effect of the embargo is *negative* for all households – i.e., the embargo reduces absolute welfare for all households.

taller adults also have better cognitive, social, and economic outcomes (e.g. see Currie, 2009; Steckel, 2008; Strauss and Thomas, 1998; Case and Paxson, 2008; Young and French, 1996; Hensley, 1993; Magnusson et al., 2006). Mortality selection bias arising from 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 where health measured as adult height is assumed normally distributed. This framework suggests that positive selection of survivors of in-utero shocks can dominate scarring effects in high mortality environments, leading survivors 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. However it finds no effects on 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.

Table 8 presents the results from specification (3) on the impacts of in-utero exposure to the embargo on women's height in adulthood. first in the whole sample, and then by distance to the main border crossing. Panel A shows results from the optimal bandwidth and half of this bandwidth, and Panel B shows results from bandwidths of 18 and 24 months around the cutoff. Columns (1) and (2) show no significant effects of exposure to the embargo on women's height in adulthood. However very different patterns emerge based on distance to trade. Columns (3) and (4) show negative effects of embargo exposure on adult height of women in districts closer than the median distance to the border crossing, albeit statistically insignificant, which are consistent with scarring. However, columns (5) and (6) show statistically significant *increases* in height for women exposed in-utero to the embargo in districts farther away from the border crossing. The estimates are large, ranging from approximately 1.6 to 2.5 centimetres, and consistent with selection effects that dominate scarring, indicating that households in these regions were more adversely affected by the embargo. This aligns with the fact that households in more remote regions are poorer than those residing closer to the main border crossing, and therefore were probably less likely to be able to insure their incomes and health investments during pregnancy from the trade shock. It is also consistent with our finding in Tables 7 and A.4 that mothers completing pregnancies immediately following the embargo have lower education and height. It is there-

fore likely that the decline in live births we observe in Table 3 took place disproportionately in more remote regions, though we cannot confirm this in our data.

5.3.2 Education

In Figure 6 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 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.[33] This suggests that parents invested more heavily in the education of embargo-affected children than parents of other cohorts, potentially as a means of compensating them for the adverse consequences of embargo exposure. We investigate further in our estimations whether the increase in educational attainment occurred on the intensive or extensive margin, and how returns to education in the labour market may help to explain the increase in earned income for affected cohorts.

Table 9 shows results from specification (3) for the estimated impact of women's in-utero exposure to the embargo on their educational attainment in adulthood. 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 0.702 years more schooling than those born just before; an effect statistically significant at the 1% level estimated using an optimal bandwidth of about 30 months. Re-estimating the specification with observations from half the optimal bandwidth increases the coefficient to 0.935 years, and the effect remains highly statistically significant. Columns (3) and (4) report estimated embargo impacts on the binary outcome of whether adult women have any schooling at all. Both columns show statistically significant estimated increases of 7.4 and 10.6 percentage points respectively 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 0.901 years in column (1), but a smaller and statistically insignificant estimated impact of 0.757 years in column (2). Columns (3) and (4) however yield statistically significant estimates of 10.1 and 14.2 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).[34] Increased entry of embargo-affected cohorts also

[33] The corresponding plot for the limited sample of men in Figure A.7 shows no discernible effect of the embargo on their education, and regression results available upon request confirm the same.

[34] We also investigated whether there were changes in government investment in education that may have

points to increased parental initiative to invest in treated children's education (or at least their daughters, given the lack of data for men).[35]

In Table 10 we examine whether the impact of in-utero exposure to the embargo had heterogeneous effects on women's adult education based on their remoteness from trade, as we do for height in Table 8. Columns (1) and (2) show the estimates from the first two columns of Table 9 again for the purpose of comparison. Columns (3) and (4) present the estimates for women residing in districts closer than the median distance from the border crossing in the sample, and columns (5) and (6) present those for women residing farther away than the median distance. The estimates in Panels A and B indicate that parental investment in the education of daughters exposed to the embargo increased significantly in both close and remote regions, with respect to distance from trading opportunities. The magnitudes of the estimated increases in years of schooling in close and remote regions are also very similar at 0.75-1.00 years depending on bandwidth choice. Parents therefore do not appear to have differed in the extent of their desire to provide their daughters exposed in-utero to the embargo with more education based on their remoteness from trade, possibly to compensate them for the adverse effects of exposure.

5.3.3 Income

Figure 7(a) plots the log of wage income by birth year cohort for NLSS respondents, and Figures 7(b) and 7(c) shows separate plots for female and male respondents respectively. There is a visible increase in wage income for treated cohorts, with women showing a larger increase than men. The results from (3) in Table 11 show the same pattern, with treated women earning a precisely estimated 17.2% and 14.8% higher wage income than untreated cohorts in columns (5) and (6) in Panel A respectively. This is consistent with the increase in parental educational investment we find in treated cohorts of women in Table 9, although the treated cohorts are not identically defined. We do not find a similar increase in adult wage income for treated male cohorts in columns (3) and (4) of Panel A. This gender difference is most likely driven by disproportionately adverse embargo impacts on men in childhood due to their higher fragility (as we find for infant mortality in Table 5), despite the fact that prevailing gender norms mean sons generally receive greater parental health and educational investments. Panel B shows that treated cohorts of women are also likelier to join households with more income on average than untreated women, ostensibly via higher quality marriage market matches that are possibly a result of their higher levels of education as well. However,

affected treated cohorts disproportionately relative to untreated cohorts. Using UNESCO data we see no change in pupil-teacher ratio or primary school enrollment for the exposed cohort relative to unexposed cohort (data available at https://data.worldbank.org/indicator/SE.PRM.ENRL.TC.ZS?locations=NP and https://data.worldbank.org/indicator/SE.PRM.ENRR?locations=NP).

[35] Table A.5 shows qualitatively very similar results for women's educational attainment estimated using smaller bandwidths.

these estimates do not remain significant after correcting for multiple hypothesis testing.

In Table 12, we re-estimate (3) by whether the respondent resides close (below median distance) or far (above median distance) from the main border crossing with India. For both men and women, we find that treated cohorts earn higher wages if they reside close to the border crossing compared to those residing farther away. Treated men earn an estimated 8.7% lower wage income than untreated men in districts farther away from the crossing in column (4); an effect significant at the 5% level. In contrast, treated men residing close to the border crossing show no significant difference in wages compared to untreated men also residing below the median distance to the crossing. Treated women however appear to earn higher wages compared to untreated cohorts in both close and far districts, with estimated wage differences of 18.3% and 6.5% in columns (5) and (6) respectively, although only the estimate in column (5) is significant at the 5% level after the Benjamini-Hochberg correction. The difference in wages between close and far districts for both treated men and women is partially explained by differing returns to education[36], which are greater in close districts as shown in Figures 8(a) and 8(b), and also by the harsher impact of the embargo in remote, poorer regions (as evidenced by our results on height in Table 8). The worse embargo impacts on wages for treated men compared to treated women on average are also consistent with disproportionately negative long-term consequences for men of in-utero exposure due to their higher fragility in early life. Column (5) of Panel B shows that treated women join households with 8.2% higher income after marriage compared to untreated women in close districts, while there is no such effect for treated women in farther away districts in column (6), consistent with the fact that households in close districts are wealthier on average.

6 Discussion and Concluding Remarks

Our findings provide a first set of rigorously estimated results on the human capital consequences of a trade embargo. Given the widespread use of embargoes as a policy tool, and the vast number of people affected, the lack of existing research on the welfare consequences of trade embargoes is notable. 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 a 15-month-long embargo. We find a substantial decline in reported live births shortly after the embargo began. We also find that adult women survivors of in-utero exposure to the embargo had nearly a year more education than untreated cohorts born shortly before March 1989, suggesting that parental investments in education increased to compensate embargo-affected cohorts for adverse effects of exposure. In households with no previously born son present, we also find a significant increase in reported miscarriages of 1-2 percentage points, and in

[36]See Akanda (2010) for a study of returns to education in Nepal.

male infant mortality of about 5 percentage points immediately following the imposition of the embargo. In contrast, we find no similar increases in child mortality in households with a previously born son, indicating that male wage earners bolstered household income against the embargo's adverse impacts. There appears to be substantial underreporting of the lost pregnancies captured 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). Adult women survivors of in-utero embargo exposure are 1.8-2.5 centimetres taller in remote, poorer districts located farther away from trade networks, indicating that embargo impacts were the most adverse in these regions, leading to positive selection of survivors on height.

We find that in-utero exposure to the embargo raised adult wage income for women survivors of exposure by 14.8-17.2 percentage points, and that treated women joined households with higher income after marriage as well. These gains are concentrated in districts close to trade networks, which are wealthier, have higher wage returns to education, and appear to have been less adversely affected by the embargo compared to remote, poorer districts. Adult male survivors of exposure have 8.7 percentage point lower wage income in adulthood in remote districts, consistent with harsher embargo effects in these areas, and higher male fragility in early life. Our results by distance to trade networks are a new contribution to the literature on within-country variation in access to trade and and its effects on poverty, which has focused largely on increased exposure to market competition and international price volatility, rather than remoteness from trade (e.g. see Topalova, 2010; McCaig, 2011; Kovak, 2013). 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 have been linked to 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.

The estimates we present likely represent upper-bound effects since 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. This suggests that our findings have wide-ranging policy relevance, especially so when we consider the additional set of countries that are heavily reliant on international trade, but not landlocked, and are currently embargoed or at risk of being embargoed in the future.

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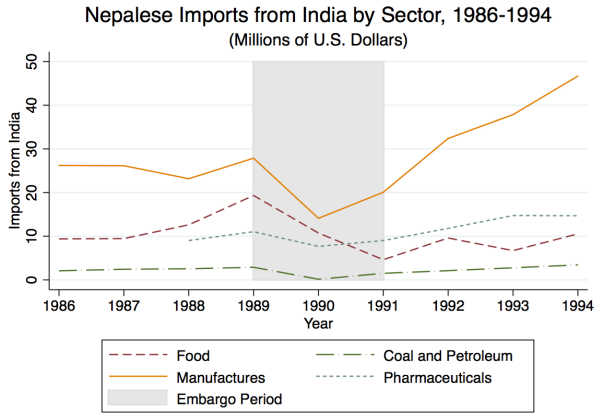
Figure 1: Nepalese Foreign Trade During the Embargo

(a) Nepalese Imports

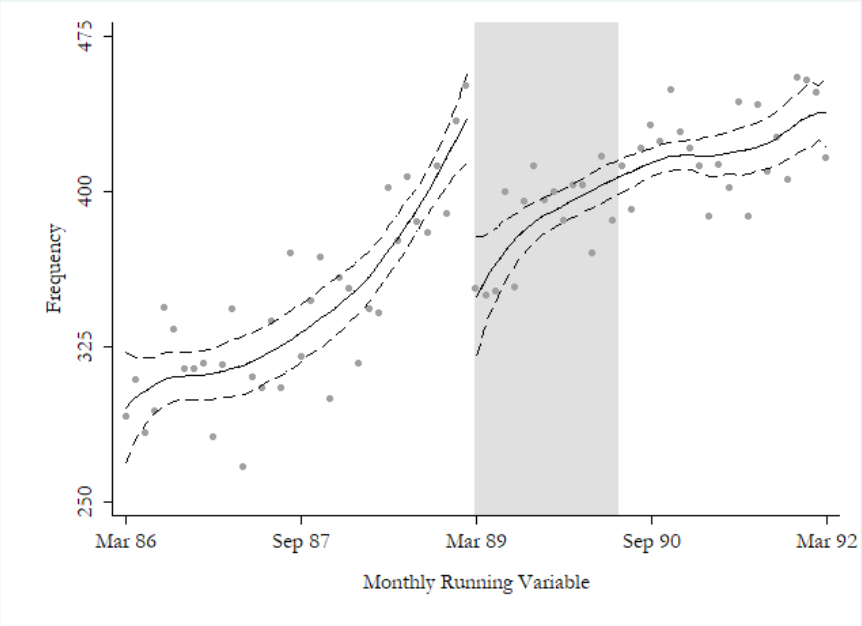
(b) Nepalese Imports by Sector



(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 from all countries. 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).

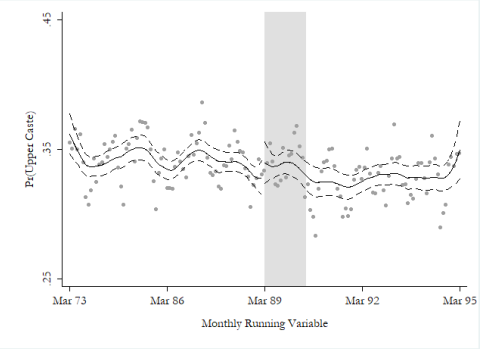
Figure 2: Live Births



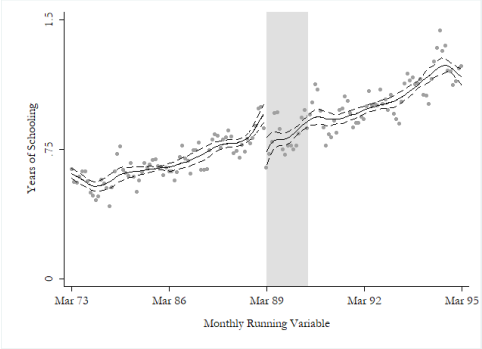
Notes: The graph shows the number of live births by month, and local linear regression plots with a triangular kernel. The dashed lines show the 95% confidence intervals around the local linear estimates.

Figure 3: Mother Characteristics

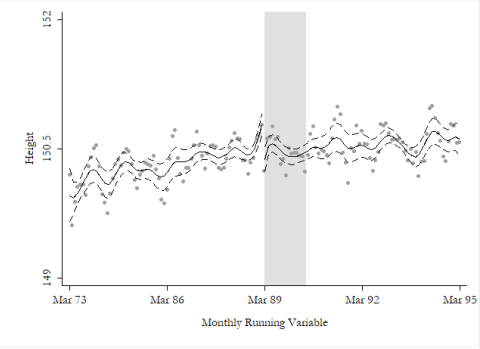
(a) Caste



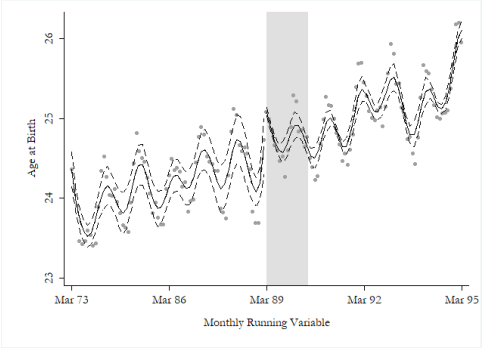
(b) Education



(c) Height

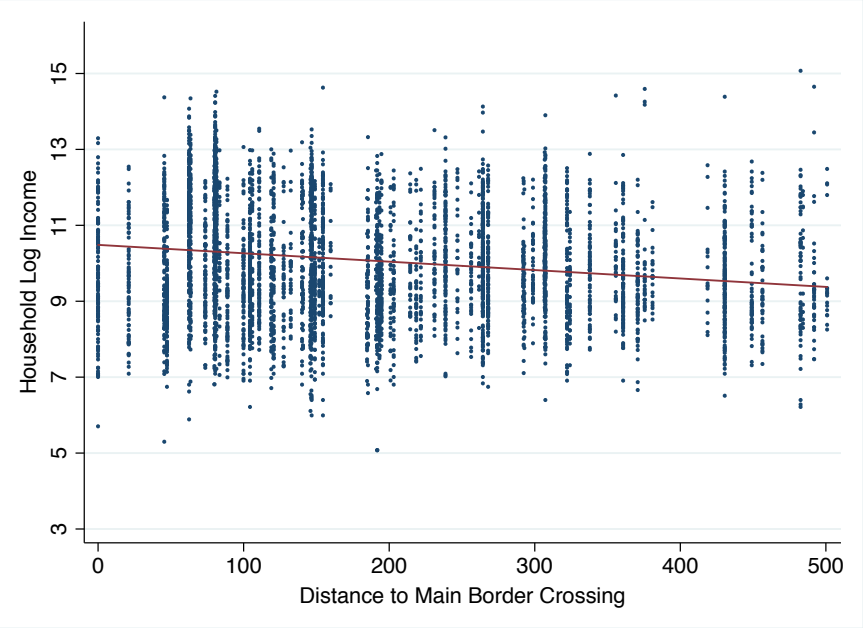


(d) Age at Birth



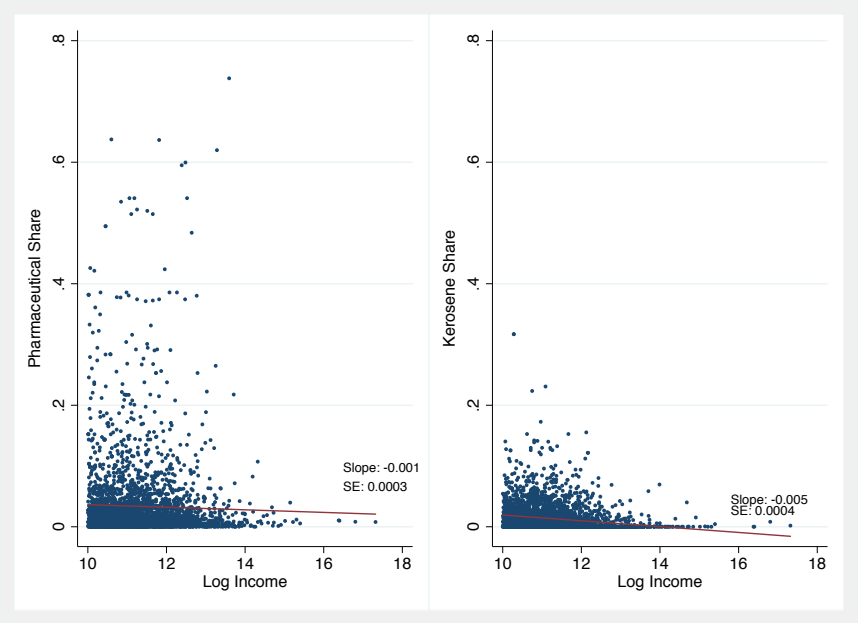
Notes: The graphs show 3-month smoothed averages of mother characteristics by child month of birth, and local linear regression plots with a triangular kernel. The gray shaded region indicates the embargo period. The dashed lines show the 95% confidence intervals around the local linear estimates.

Figure 4: Log Total Income and Distance to Main Border Crossing



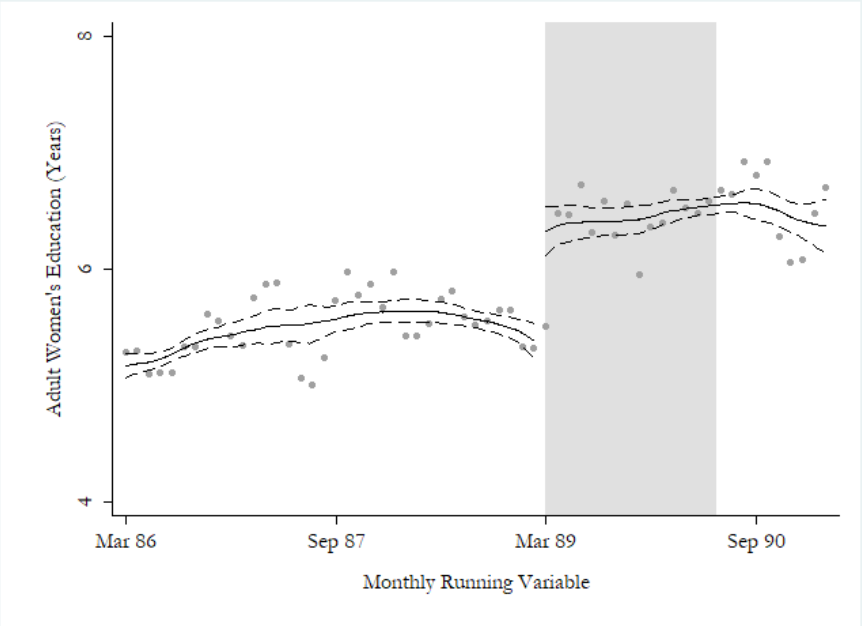
Notes: The graph plots log total individual income versus the distance to the main border crossing in Birgunj for all observations in the 2010/11 wave of the NLSS. Note that the R-squared is 0.04.

Figure 5: Log Total Income and Consumption Shares of Fuel and Medicines



Notes: The graph plots log total individual income versus the consumption share of fuel and pharmaceuticals, calculated from the 2010/11 wave of the NLSS.

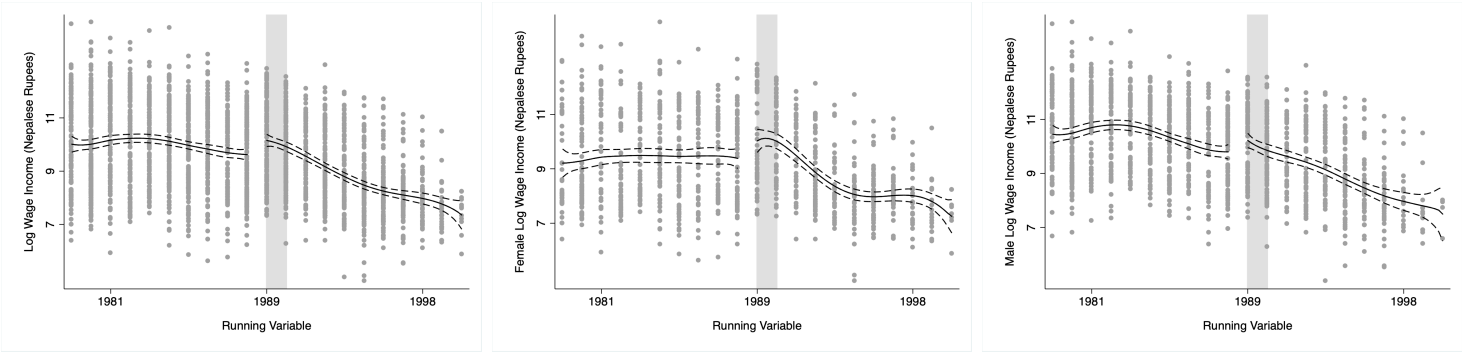
Figure 6: Long-Run Years of Schooling



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. The shaded region indicates the embargo months. The dashed lines show the 95% confidence intervals around the local linear estimates. The sample includes only women who were aged at least 18 years at the time of the survey.

Figure 7: Adult Wage Income by Gender, NLSS Data

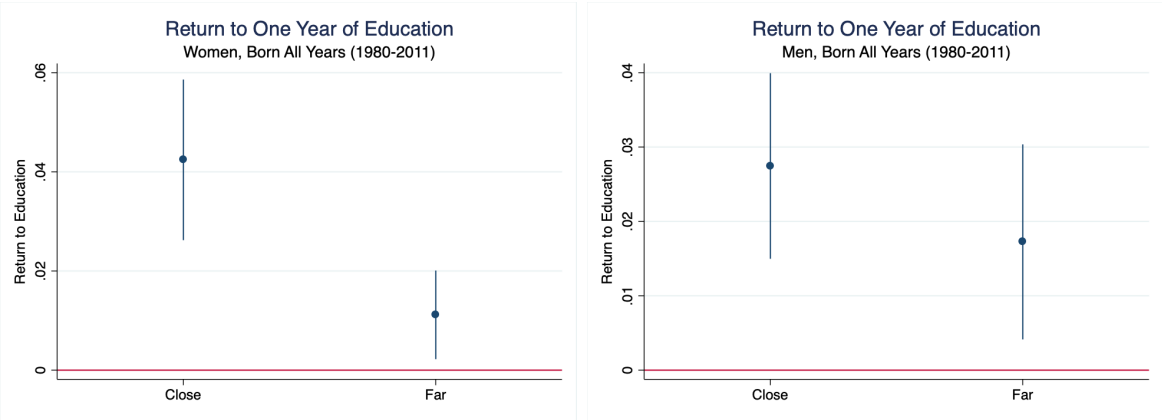
(a) All (b) Men (c) Women



Notes: The graphs show scatter plots and local linear regression plots with a triangular kernel of the log of wage income by year of birth. The dashed line indicate the 95% confidence intervals around the local linear estimates.

Figure 8: Wage Returns to Education, NLSS Data

(a) All (b) Men



Notes: The graphs show the estimated wage returns to a year of education from an OLS regression, conditional on district fixed effects and linear and quadratic terms in age. The spikes indicate the 95% confidence intervals around the estimates.

Table 1: DHS Summary Statistics

Variables	(1)	(2)	(3)	(4)	(5)
	Mean	S.D.	Min	Max	Observations
Miscarriage	0.061	-	0	1	57,473
Stillborn	0.019	-	0	1	57,473
Infant Death	0.093	-	0	1	57,473
Mother Age at Birth	24.478	5.612	10	48	57,473
Mother Years of Education	1.246	2.815	0	14	22,020
Mother Height	150.60	5.463	105.90	192.60	16,320
Mother Upper Caste	0.345	-	0	1	22,022
Mother Has Some Education	0.224	-	0	1	22,022

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	3,895
Married	0.555	-	0	1	3,895
Self Employed	0.626	-	0	1	3895
Age	21.248	2.349	17	25	3,895
Monthly Income	3522.928	12814.78	0	240,000	3,895
Highest Grade Completed	7.529	3.352	1	18	1,760

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		
Month Trim	*(0,0)*	*(-1,-1)*	*(-2,-1)*	*(0,0)*	*(-1,-1)*	*(-2,-1)*	*(0,0)*	*(-1,-1)*	*(-2,-1)*
Panel A	*McCRARY TEST*								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log-Diff	-0.270***	-0.210***	-0.135*	-0.274***	-0.205**	-0.030	-0.285***	-0.232**	-0.265**
	(0.060)	(0.064)	(0.071)	(0.085)	(0.090)	(0.087)	(0.086)	(0.103)	(0.110)
Observations	20	20	20	20	20	20	20	20	20
Estimated BW	6.93	6.19	5.15	6.20	5.55	6.41	7.38	5.40	4.77
Data BW	18	18	18	18	18	18	18	18	18
Panel B	*CJM TEST*								
T-statistic	-3.626***	-2.544**	-2.185**	-2.709***	-2.017**	-1.732*	-3.005***	-2.274**	-1.980**
Observations	137,103	136,299	135,865	70,202	69,780	69,535	66,901	66,519	66,330
h_l, h_r	27.30, 25.85	26.80, 26.15	26.50, 26.49	34.58, 31.75	33.62, 32.20	32.98, 32.82	30.52, 30.52	30.80, 30.39	30.92, 30.27
Panel C	*PARAMETRIC TEST*								
Log-Diff	-0.175***	-0.113***	-0.091**	-0.171***	-0.109**	-0.070	-0.180***	-0.117**	-0.115**
	(0.034)	(0.037)	(0.035)	(0.047)	(0.051)	(0.041)	(0.049)	(0.054)	(0.054)
Data BW	18	18	18	18	18	18	18	18	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 cutoff 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. (a, b) indicates a months trimmed before, and b months trimmed after the cutoff. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 4: Miscarriage

	Miscarriage					
	All Children		No Previous Son		Previous Son	
	$h*$	$0.5\,h*$	$h*$	$0.5\,h*$	$h*$	$0.5\,h*$
Panel A: Optimal Bandwidth	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.005	0.006	0.014	0.020	0.001	-0.004
	(0.004)	(0.006)	(0.006)**	(0.008)***	(0.005)	(0.008)
			[0.032]**	[0.020]**	[0.850]	(0.633)
Observations	52,258	26,494	27,410	13,841	30,223	15,002
Bandwidth	64.94	32.47	73.96	36.98	69.64	34.82
Panel B: Other Bandwidths	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.010	0.003	0.023	0.016	-0.002	-0.008
	(0.007)	(0.007)	(0.010)**	(0.011)	(0.009)	(0.010)
			[0.038]**	[0.298]	[0.817]	[0.438]
Observations	20,074	15,287	9,352	15,287	10,722	8,168
Bandwidth	24	18	24	18	24	18

Notes: The table shows results from a parametric OLS regression with a linear spline. Panel A reports results from optimal bandwidths, and Panel B reports results from smaller bandwidths. Robust Standard errors are in parentheses, and Benjamini-Hochberg corrected p-values are reported in brackets. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 5: Infant Death

	Infant Death					
	All		No Previous Son		Previous Son	
Panel A: Male Children	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.033	0.031	0.049	0.050	0.020	0.017
	(0.013)**	(0.015)**	(0.020)**	(0.023)**	(0.018)	(0.020)
	[0.024]**	[0.072]*	[0.052]*	[0.116]	[0.522]	[0.788]
Observations	9,563	7,336	4,444	3,403	5,119	3,933
Bandwidth	24	18	24	18	24	18
Panel B: Female Children	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.004	-0.002	0.003	0.006	-0.011	-0.010
	(0.011)	(0.013)	(0.016)	(0.018)	(0.016)	(0.018)
	[0.719]	[0.856]	[0.830]	[0.655]	[0.727]	[0.760]
Observations	10,511	7,951	4,908	3,716	5,603	4,235
Bandwidth	24	18	24	18	24	18

Notes: The table shows results from a parametric OLS regression with a linear spline. Panel A reports results for male children, and Panel B reports results for female children. Robust standard errors are in parentheses, and Benjamini-Hochberg corrected p-values are reported in brackets. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 6: Has a Younger Sibling

	Has a Younger Sibling					
	All		No Previous Son		Previous Son	
Panel A: Male Children	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.003	-0.006	0.028	0.027	-0.003	-0.023
	(0.014)	(0.016)	(0.013)**	(0.016)*	(0.023)	(0.026)
	[0.818]	[0.701]	[0.156]	[0.316]	[0.893]	[0.390]
Observations	9,563	7,336	4,444	3,403	5,119	3,933
Bandwidth	24	18	24	18	24	18
Panel B: Female Children	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.007	0.018	-0.010	-0.004	0.030	0.039
	(0.012)	(0.014)	(0.010)	(0.012)	(0.020)	(0.023)*
	[1.072]	[0.414]	[0.415]	[0.965]	[0.284]	[0.198]
Observations	10,511	7,951	4,908	3,716	5,603	4,235
Bandwidth	24	18	24	18	24	18

Notes: The table shows results from a parametric OLS regression with a linear spline. Panel A reports results for male children, and Panel B reports results for female children. Robust standard errors are in parentheses, and Benjamini-Hochberg corrected p-values are reported in brackets. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 7: Mother Characteristics

	Mother Characteristics							
	Caste		Education		Height		Age at Birth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Optimal Bandwidth	h^*	$0.5\,h^*$	h^*	$0.5\,h^*$	h^*	$0.5\,h^*$	h^*	$0.5\,h^*$
Treated	0.000	0.012	-0.061	-0.107*	-0.071	-0.072	0.137	0.373**
	(0.009)	(0.013)	(0.043)	(0.060)	(0.107)	(0.148)	(0.103)	(0.146)
Observations	42,576	21,672	46,688	23,221	42,999	21,620	45,122	22,454
Bandwidth	52.62	26.31	57.35	28.62	70.28	35.14	55.79	27.88
Panel B: Other Bandwidths	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	0.007	0.017	-0.160**	-0.139**	-0.171	-0.143	0.367**	0.345**
	(0.015)	(0.013)	(0.075)	(0.065)	(0.205)	(0.179)	(0.178)	(0.155)
Observations	15,287	20,074	15,284	20,071	11,373	14,981	15,287	20,074
Bandwidth	18	24	18	24	18	24	18	24

Notes: The table shows results from a parametric OLS regression with a linear spline. Robust standard errors are in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 8: Adult Women's Height by Distance to Main Border Crossing

	Height					
	All		Close		Far	
Panel A: Optimal Bandwidths	h^*	$0.5\,h^*$	h^*	$0.5\,h^*$	h^*	$0.5\,h^*$
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.518	0.534	-0.429	-1.180	1.632	2.529
	(0.422)	(0.618)	(0.582)	(0.833)	(0.595)***	(0.853)***
			[0.460]	[0.157]	[0.012]**	[0.006]***
Observations	3,208	1,540	1,703	829	1,519	760
Bandwidth	29.23	14.63	35.67	17.84	30.08	15.04
Panel B: Other Bandwidths	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.788	0.528	-0.352	-0.748	1.826	1.797
	(0.470)*	(0.536)	(0.715)	(0.806)	(0.664)***	(0.771)**
			[0.622]	[0.354]	[0.012]**	[0.040]**
Observations	2,619	1,999	1,148	890	1,210	906
Bandwidth	24	18	24	18	24	18

Notes: The table shows results from a parametric OLS regression with a linear spline. The sample includes women who were at least 18 years old at the time of the survey. Robust standard errors are in parentheses, and Benjamini-Hochberg corrected p-values are reported in brackets. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 9: Adult Women's Education

	Education			
	Years of Schooling		Any Schooling	
	h^*	$0.5\,h^*$	h^*	$0.5\,h^*$
Panel A: Linear Spline	(1)	(2)	(3)	(4)
Treated	0.702***	0.935***	0.074**	0.100***
	(0.222)	(0.297)	(0.028)	(0.034)
Panel B: Quadratic Spline	(1)	(2)	(3)	(4)
Treated	0.901***	0.757	0.101**	0.142***
	(0.326)	(0.451)	(0.039)	(0.044)
Observations	5,608	2,857	3,404	1,753
Bandwidth	30.15	15.08	18.33	9.17

Notes: The table shows results from OLS regressions with linear splines. The sample includes women who were at least 18 years old at the time of the survey. Robust standard errors are in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 10: Adult Women's Education by Distance to Main Border Crossing

	Years of Schooling					
	All		Close		Far	
Panel A: Optimal Bandwidths	h^*	$0.5\,h^*$	h^*	$0.5\,h^*$	h^*	$0.5\,h^*$
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.702	0.935	0.742	1.118	0.772	1.184
	(0.227)***	(0.325)***	(0.296)**	(0.426)***	(0.376)**	(0.562)**
			[0.024]**	[0.018]**	[0.040]**	[0.035]**
Observations	5,608	2,857	3,362	1,655	1,992	1,013
Bandwidth	30.15	15.08	41.89	20.99	23.79	11.89
Panel B: Other Bandwidths	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.742	0.902	0.940	1.037	0.690	0.959
	(0.258)***	(0.296)***	(0.394)**	(0.446)**	(0.368)*	(0.429)**
			[0.034]**	[0.040]**	[0.061]*	[0.025]**
Observations	4,435	3,404	1,947	1,504	2,068	1,577
Bandwidth	24	18	24	18	24	18

Notes: The table shows results from a parametric OLS regression with a linear spline. The sample includes women who were at least 18 years old at the time of the survey. Robust standard errors are in parentheses, and Benjamini-Hochberg corrected p-values are reported in brackets. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 11: Long-Run Income

	All Workers		Male Workers		Female Workers	
Panel A	Log Wage Income					
	$h*$	$0.5\,h*$	$h*$	$0.5\,h*$	$h*$	$0.5\,h*$
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.238	0.201	0.010	0.013	0.172	0.148
	(0.123)*	(0.106)*	(0.052)	(0.017)	(0.061)***	(0.066)**
			[0.262]	[0.187]	[0.210]	[0.051]*
Observations	1,201	599	527	266	399	201
Bandwidth	4.23	2.11	4.01	2.00	3.79	1.89
Panel B	Log Non-Wage Income					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.115	0.102	0.196	0.153	0.104	0.101
	(0.056)*	(0.055)*	(0.133)	(0.110)	(0.049)*	(0.052)*
			[0.123]	[0.159]	[0.134]	[0.131]
Observations	641	322	265	132	345	172
Bandwidth	4.26	2.13	3.92	1.91	3.90	1.95

Notes: The table shows results from a parametric OLS regression with a linear spline. Robust Standard errors are in parentheses, and Benjamini-Hochberg corrected p-values are reported in brackets. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table 12: Long-Run Income by Distance to Main Border Crossing

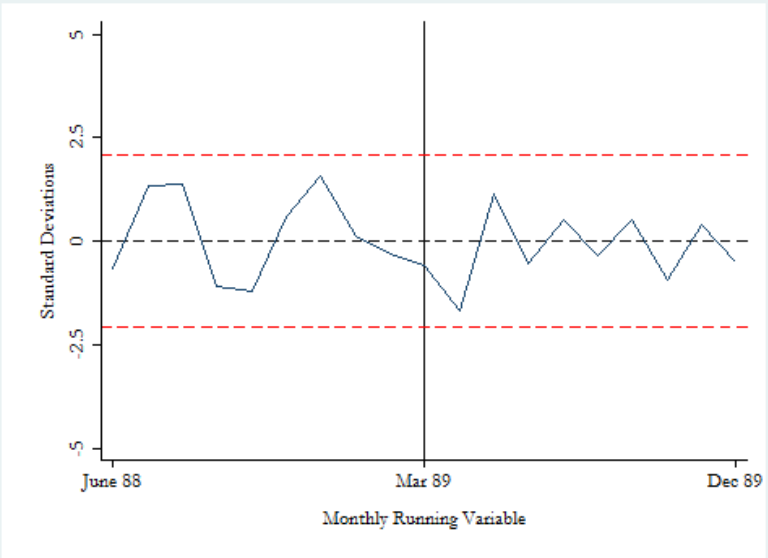
Panel A	Log Wage Income					
	All Workers		Male Workers		Female Workers	
	Close	*Far*	*Close*	*Far*	*Close*	*Far*
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.083	-0.026	0.018	-0.087	0.183	0.065
	(0.043)*	(0.052)	(0.050)	(0.020)***	(0.077)**	(0.037)*
			[0.204]	[0.030]**	[0.042]**	[0.152]
Observations	696	500	399	191	297	209
Bandwidth	4.88	2.44	4.60	2.30	4.31	2.16
Panel B	Log Non-Wage Income					
	Close	*Far*	*Close*	*Far*	*Close*	*Far*
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.121	0.105	0.116	0.041	0.082	0.132
	(0.062)*	(0.181)	(0.176)	(0.047)	(0.042)**	(0.196)
			[0.298]	[0.155]	[0.069]*	[0.219]
Observations	482	209	198	62	271	98
Bandwidth	4.29	2.15	4.71	2.36	4.66	2.33

Notes: The table shows results from a parametric OLS regression with a linear spline. Robust Standard errors are in parentheses, and Benjamini-Hochberg corrected p-values are reported in brackets. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

- For Online Publication -

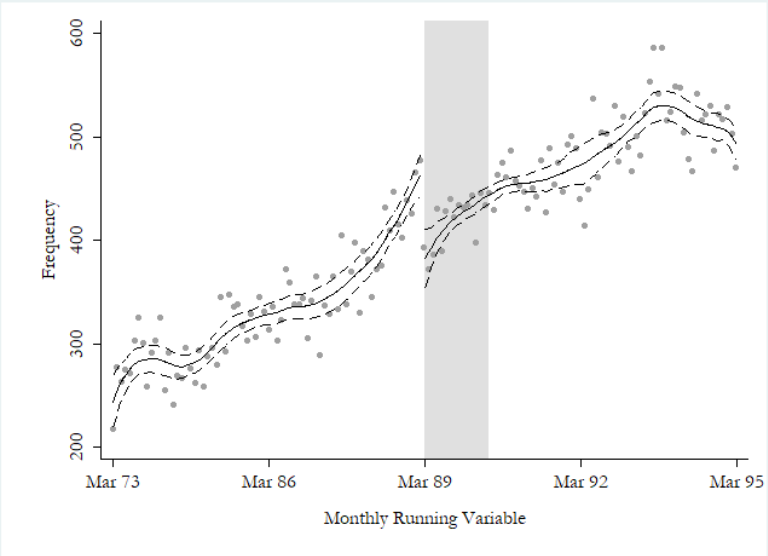
A Additional Figures and Tables

Figure A.1: Rainfall Shocks



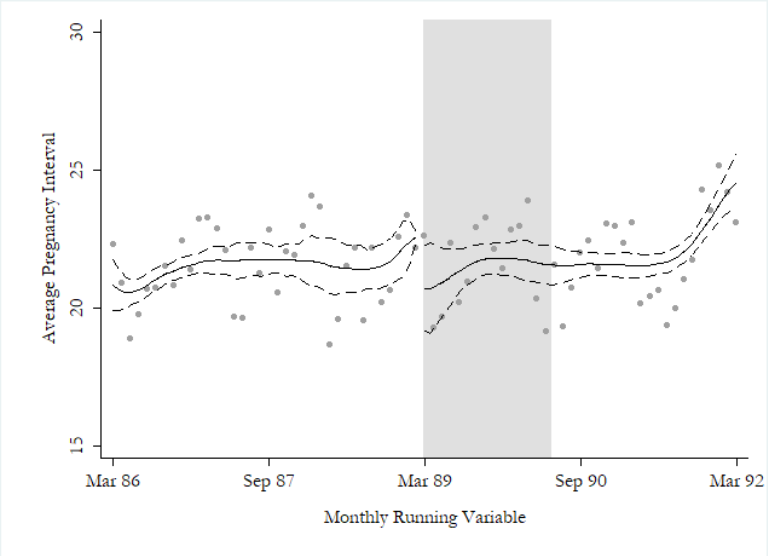
Notes: The graph shows monthly rainfall shocks, measured in standard deviations from the average rainfall in that month over the past 20 years. The red dashed lines show the 95% confidence interval.

Figure A.2: Completed Pregnancies



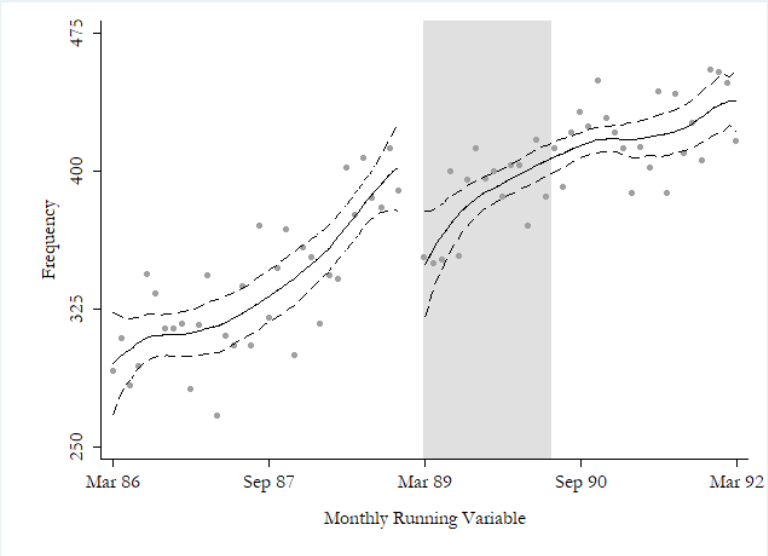
Notes: The graph shows the number of completed pregnancies by monthly bin, and local linear regression plots of completed pregnancies with a triangular kernel. The shaded region indicates the embargo months. The dashed lines show the 95% confidence intervals around the local linear estimates.

Figure A.3: Pregnancy Intervals



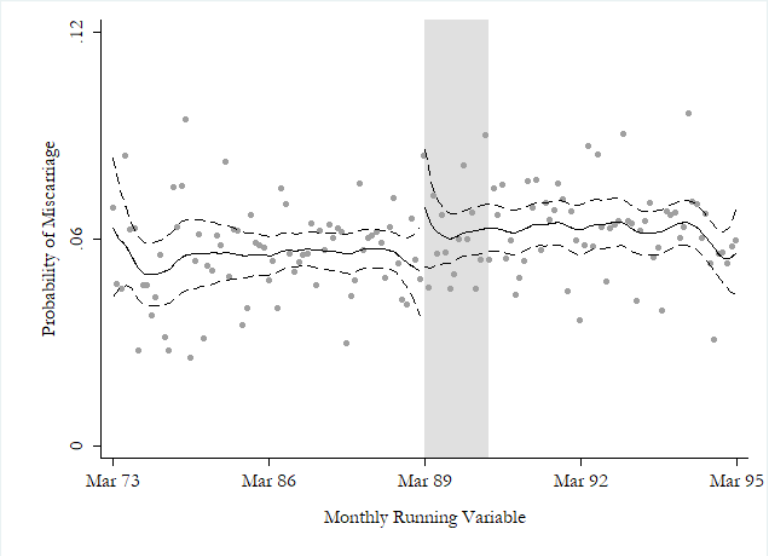
Notes: The graph shows the average interval in months between the last and the latest pregnancy by monthly bin, and local linear regression plots of the average interval with a triangular kernel. The shaded region indicates the embargo months. The dashed lines show the 95% confidence intervals around the local linear estimates.

Figure A.4: Live Births without Jan and Feb 1989



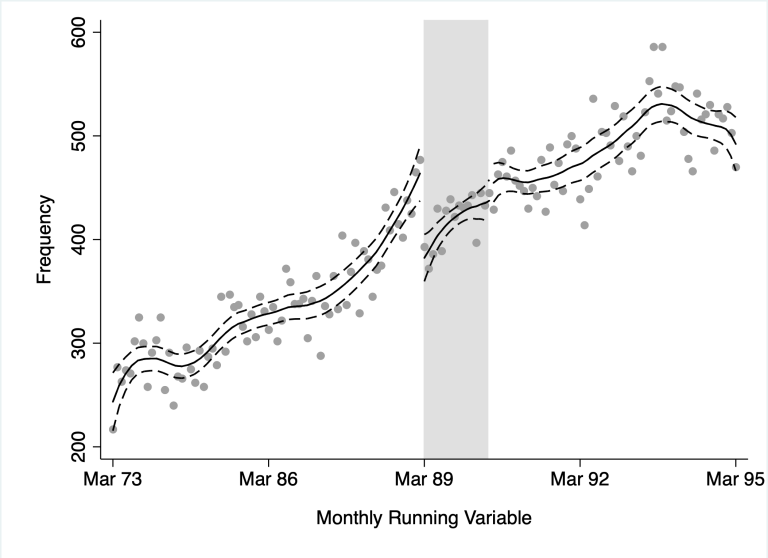
Notes: The graph shows the number of live births by month, and local linear regression plots with a triangular kernel. The dashed lines show the 95% confidence intervals around the local linear estimates.

Figure A.5: Miscarriages



Notes: The graph shows the 3-month moving average of the fraction of monthly completed births ending in miscarriage, and local linear regression plots with a triangular kernal. The grey shaded area indicates the embargo period. The dashed lines show the 95% confidence intervals around the local linear estimates.

Figure A.6: Completed Pregnancies: Comparing Start and End of Embargo



Notes: The graph shows the number of completed pregnancies by monthly bin, and local linear regression plots of completed pregnancies with a triangular kernel. The shaded region indicates the embargo months. The dashed lines show the 95% confidence intervals around the local linear estimates. The kernel bandwidth is set to 10 months, with a pilot bandwidth for the standard error calculation of 15 months.

Figure A.7: Long-Run Years of Schooling: Men

Notes: The graph shows 3-month smoothed averages of male DHS survey respondents' years of completed schooling by month of birth, and local linear regression plots with a triangular kernel. The shaded region indicates the embargo months. The dashed lines show the 95% confidence intervals around the local linear estimates. The sample includes only men who were aged at least 18 years at the time of the survey.

Table A.1: Live Births, 24-Month Bandwidth

	Live Births								
	All Children			Male Children			Female Children		
Month Trim	*(0,0)*	*(-1,-1)*	*(-2,-1)*	*(0,0)*	*(-1,-1)*	*(-2,-1)*	*(0,0)*	*(-1,-1)*	*(-2,-1)*
Panel A	*McCRARY TEST*								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Log-Diff	-0.270***	-0.175***	-0.133**	-0.246***	-0.168**	-0.076	-0.276***	-0.225**	-0.223**
	(0.060)	(0.047)	(0.046)	(0.075)	(0.076)	(0.060)	(0.092)	(0.086)	(0.073)
Observations	26	26	26	26	26	26	26	26	26
Estimated BW	6.86	11.20	11.89	8.025	7.88	13.48	6.37	7.71	10.06
Data BW	24	24	24	24	24	24	24	24	24
Panel B	*PARAMETRIC TEST*								
Log-Diff	-0.156***	-0.099***	-0.070**	-0.161***	-0.113**	-0.071*	-0.150***	-0.085*	-0.071
	(0.032)	(0.034)	(0.033)	(0.042)	(0.044)	(0.039)	(0.045)	(0.050)	(0.051)
Data BW	24	24	24	24	24	24	24	24	24

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 cutoff 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 robust standard errors in parentheses. (a, b) indicates a months trimmed before, and b months trimmed after the cutoff. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table A.2: Live Births, Alternate Specifications

	Live Births			
	(1)	(2)	(3)	(4)
Panel A	$h*$	$0.5h*$	*Quadratic Spline*	
Log-Diff	-0.086***	-0.180***	-0.248***	-0.211***
	(0.032)	(0.039)	(0.040)	(0.045)
Bandwidth	33	16	18	24
Panel B	*1996 DHS Wave*			
	(1)	(2)	(3)	(4)
Log-Diff	-0.346***	-0.183**	-0.135**	-0.088
	(0.057)	(0.075)	(0.065)	(0.058)
Bandwidth	6	12	18	24

Notes: All estimates are from a parametric OLS regression with linear splines, with robust standard errors in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table A.3: Live Births, Seasonality Tests

	Live Births					
	1992	*1991*	*1990*	*1988*	*1987*	*1986*
Panel A	(1)	(2)	(3)	(4)	(5)	(6)
Log-Diff	-0.040	-0.060**	0.027	0.038	-0.075	-0.060
	(0.043)	(0.028)	(0.030)	(0.053)	(0.047)	(0.037)
Bandwidth	18	18	18	18	18	18
Panel B	(1)	(2)	(3)	(4)	(5)	(6)
Log-Diff	-0.037	-0.070***	-0.011	0.064	-0.064	-0.058
	(0.037)	(0.026)	(0.030)	(0.047)	(0.045)	(0.036)
Bandwidth	24	24	24	24	24	24

Notes: All estimates are from a parametric OLS regression with linear splines, with robust standard errors in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table A.4: Mother Characteristics, Smaller Bandwidths

	Mother Characteristics							
	Caste		Education		Height		Age at Birth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	0.009	0.013	-0.257**	-0.159*	-0.790**	-0.162	-0.064	0.477**
	(0.026)	(0.019)	(0.122)	(0.089)	(0.351)	(0.251)	(0.307)	(0.218)
Observations	5,459	10,395	5,458	10,394	4,079	7,769	5,459	10,395
Bandwidth	6	12	6	12	6	12	6	12

Notes: The table shows results from a parametric OLS regression with a linear spline. Robust standard errors are in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table A.5: Adult Women's Education, Smaller Bandwidths

	Years of Schooling			
	Years of Schooling		Any Schooling	
	(1)	(2)	(3)	(4)
Treated	0.876*	0.766**	0.132***	0.079**
	(0.443)	(0.341)	(0.039)	(0.034)
Observations	1,128	2,296	1,128	2,296
Bandwidth	6	12	6	12

Notes: The table shows results from a parametric OLS regression with a linear spline. Robust standard errors are in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

Table A.6: Child Survival, Seasonality Tests

	1992	*1991*	*1990*	*1988*	*1987*	*1986*
Panel A	Miscarriage, No Previous Son					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.012	-0.012	0.002	-0.000	0.012	0.011
	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)
Observations	8,144	7,758	7,535	6,603	6,143	5,554
Panel B	Infant Death, Male Children					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.020	0.020	-0.030**	-0.020	0.011	0.002
	(0.014)	(0.013)	(0.014)	(0.016)	(0.017)	(0.018)
Observations	8,273	7,904	7,672	6,724	6,174	5,638
Bandwidth	18	18	18	18	18	18

Notes: All estimates are from a parametric OLS regression with linear splines. Robust standard errors are in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.10$.

B A Model

We consider a small, open economy whose supply side is similar to the structure outlined in Coşar and Fajgelbaum (2016) 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. We first develop the spatial equilibrium and then, in our comparative statics exercise, we consider a short-run adjustment to a trade shock in the absence of worker mobility across regions. Formally, 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, labour and land. labour 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 labour in an industry and region are denoted $T_j(i)$ and $L_j(i)$, respectively. The total amount of labour in the economy is L.

B.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 $H(i)$.[37] The indirect utility of the household is therefore given by:

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

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

$$M(i) = w(i)L(i) + r(i)T(i) \tag{5}$$

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

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

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

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

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.

B.2 Production

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

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

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 $p^A \equiv P_C^A \big/ P_H^A$ as the relative autarky price in all regions, region i will be fully specialized in (and will export) good C when the relative price $p(i) \equiv P_C(i) \big/ P_H(i) > p^A$ 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 $p^* \equiv P_C^* \big/ P_H^*$, is then

$$p^* = p(i)\, \tau_0^C \tau_1^C \tau_0^H \tau_1^H i^2 \tag{9}$$

where we have used the fact that $P_C(i) = P_C^* \big/ \tau_0^C \tau_1^C i$ 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;[38] 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.[39]

[38] Regions exactly at distance $\bar{i}$ are indifferent.

[39] Another 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.

B.3 Production Problem

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

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

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

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

B.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 labour market clears.

Definition 1 *General equilibrium in region i takes international prices $\{P_C^*, P_H^*\}$ as given and consists of local labour 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

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

B. firms (regions) maximize profits, as in (10);
C. trade is balanced across regions; and
D. land and labour markets clear locally:

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

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

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

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

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

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 labour market clearing conditions (13) and (14) while setting regional welfare equal to an economy-wide constant, $\bar{v}$.[40]

B.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 where specialization was maintained, but international trade costs rose to levels that effectively prohibited international 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 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

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

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

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

Proof: 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:

$$\frac{dv(i)}{d\tau_0^C di} = \chi A_C(i) L(i)^{1-\eta} p(i)^{1-\alpha} \left[-\frac{A_C^{'}(i)}{A_C(i)} - (1-\eta)\frac{L^{'}(i)}{L(i)} + 2\frac{(1-\alpha)}{i} \right] - \gamma p(i)^{-\alpha}\frac{2\alpha^2}{i} \quad (17)$$

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. ■

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.[41]

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.

[41] To 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., $h_t^F = G\big(v_t^M(i)\big)$. 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.