Do Rural Roads Create Pathways out of Poverty? Evidence from India *

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Abstract

This paper studies the impact of road provision on investments in physical and human capital in rural areas. The context is a large scale road construction program in rural India. Using data from household surveys and agricultural markets, the paper provides 2 main pieces of reduced form evidence. First, beneficiary farmers were more likely to adopt new technologies, such as chemical fertilizer and hybrid seeds. Second, teenaged children were more likely to drop out of school and join the labor force. I argue that these changes stemmed from altered relative prices, as there is evidence of reduced price dispersion in areas that got more roads. There is also evidence of changes to the household consumption mix.

JEL Classification: O18, J24, Q16, R42, F14

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1 Introduction

Markets in developing economies are often characterized by spatial fragmentation due to poor transportation infrastructure. This inhibits households' and firms' ability to access goods and labor markets, technological innovations, and government services (World Bank, 2007; 2009). Policy-makers have increasingly attempted to address this problem by directing large sums of money towards the provision of roads and railroads.[1] However, the causal impact of these investments is not well-understood as placement tends to be driven by endogenous factors such as demand, political economy, and social objectives. This precludes us from drawing rigorous conclusions about the first-order relationship between infrastructure and market integration, and its subsequent bearing upon economic and social welfare.

This paper exploits a rule-based public program that led to plausibly exogenous provision of roads in rural India, to provide four distinct pieces of evidence on the relationship between roads and economic outcomes. Following Donaldson (2013), I start by establishing that road construction indeed reduced transportation costs and led to greater market integration, as dispersion of food prices declined in districts with greater road construction. I then provide reduced form evidence on the impact of this relative price change on farms' and households' incentives to invest in technology adoption and human capital. Specifically, I show two things: first, farmers in districts which received more roads increased their use of fertilizer and hybrid seeds; and second, teenaged children dropped out of school and started working as their access to labor market opportunities improved. Finally, I provide reduced form evidence that households responded to these supply changes by adjusting consumption on the intensive as well as the extensive margins.

The program in question - the Prime Minister's rural road scheme (hereafter, PMGSY) - is unprecedented in its scale and scope. Under a federal mandate to bring all villages with a

[1]For instance, the World Bank has spent more than $20 billion on transportation infrastructure projects annually since 2006 (Private Participation in Infrastructure projects database, The World Bank).

population of at least 500 within reach of the nearest market via an all-weather road, PMGSY provided paved roads to more than 110 million people between 2001 and 2010, about 14.5 percent of the entire rural population, or 47 percent of the unconnected rural population[2] of India as of the 2001 census.[3]

This rule-based allocation also allows me to make an empirical contribution. I exploit program roll-out across different districts over a 10 year period to pin down the causal impact of road connectivity. Identification is based on each district's annual exposure to new roads, which is a function of the distribution of village sizes in the district In the existing literature on infrastructure effects, identification has largely stemmed either from instruments based on historical routes,[4] or from variations in the straight line distance between peripheral regions and the (rail)road.[5] However, these approaches might have potential threats to validity as infrastructure has been shown to create long-term path dependencies (Bleakley and Lin, 2012; Berger and Enflo, 2013; Jedwab et al., 2013). Similarly, there may be endogeneity in the spatial layout of the road network, and is well-documented in the political economy literature. For instance, Nguyen et al. (2012) and Burgess et al. (2013) provide evidence of mistargeted construction projects in Vietnam and Kenya on account of nepotism and ethnic favoritism. Alesina et al. (1999) and Banerjee et al. (2005) show that areas with greater ethnic fragmentation have lower public good provision.[6] Rasul and Rogger (2013) highlight the relationship between bureaucratic practices and the quality and quantity of public goods in the context of the Nigerian civil service. Khemani (2004) and Rogger (2013) find evidence from India and Nigeria, showing that public good provision improves when there is a higher degree of political competition. Knight (2004) provides evidence from US Congressional

[2]While very large, these numbers are representative of the connectivity status of rural populations globally. According to the World Bank's Rural Access Index, over 1 billion rural inhabitants (or 31 percent of the world's rural population) do not have adequate access to transportation. 98 percent of these individuals live in developing countries. See http://www.worldbank.org/transport/transportresults/headline/rural-access.html

[3]The program is still underway as of this writing.

[4]See, for instance, Duranton and Turner (2012), Garcia-López et al. (2013), Volpe Martincus et al. (2013)

[5]See, for instance, Atack et al. (2010), Datta (2012), Jedwab and Moradi (2012), Ghani et al., (2013)

[6]This is in line with predictions from the median-voter theorem.

votes showing that legislative support for public spending in different congressional districts is correlated with the influence wielded by their representative in the House, leading to an overall misallocation of public goods.

The research agenda is further complicated by the fact that road construction is very investment intensive.[7] This makes a randomized control trial of road provision unlikely.[8] Since my identification strategy is underpinned by an exogenously determined rule, I am able to provide a clean estimate of the causal impact of roads even in a non-randomized setting.

There has been a great surge in recent research on understanding infrastructure effects. However, much of this work has focused on railroads and highways, and our understanding of the effects of rural roads remains limited. This is an important distinction as differences in the placement and reach of transportation infrastructure are likely to generate different qualitative and quantitative impacts.[9] Moreover, many of these papers are in the fields of urban economics and spatial industrial organization.[10] This is one of the first papers to study the development impact of road connectivity in rural areas.

The remainder of this paper proceeds as follows. The next section reviews the relevant literature, and highlights my contribution in the context of the existing body of knowledge. Section 3 describes the PMGSY scheme in greater detail. Sections 4 and 5 describe the

[7]Estimates suggest that roads constructed under PMGSY cost $23,000 per kilometer per lane

[8]The aforementioned susceptibility to political capture also stems partly from the high costs involved in road construction.

[9]For instance, Atack et al. (2010) find that the railroad was an important factor in the urbanization of the American Midwest. On the other hand, Baum-Snow (2007) finds that the US Interstate system caused people to move out to the suburbs, and suggests that aggregate city population would have grown by 8 percent in the absence of the highways. In addition, Chi (2012) shows that even for similar infrastructure, effects can vary depending on the type of area being connected. He finds that in Wisconsin, highway improvements promoted population growth in rural areas, facilitated population flows in the suburbs, and had no statistically significant effect on population changes in urban areas.

[10] Baum-Snow and Turner (2012), Duranton and Turner (2012), Baum-Snow et al. (2013), Faber (2013), Garcia-López et al. (2013), Gutberlet (2013), Mayer and Trevien (2013), Rothenberg (2012)

data and empirical strategy. Section 6 presents the estimation results. Sections 7 and 8 present robustness checks, and consider alternative hypotheses. Section 9 briefly discusses the implications of some of the results, and concludes.

2 Literature Review

The primary channel through which we expect roads to affect economic outcomes is via a reduction in transport costs. A rich literature in international trade has established a negative relationship between transport costs and trade flows (Bougheas et al., 1999; Baier and Bergstrand, 2001; Limão and Venables, 2001; Clark et al., 2004; Hummels and Skiba, 2004; Feyrer, 2011; Storeygard, 2012). Therefore, a question of first order interest is whether enhancements to transportation infrastructure lead to increased trade flows. A relatively recent empirical literature has investigated this question, and has found large effects. Donaldson (2013) finds evidence from colonial India consistent with large increases in trade volumes in response to the British government's railroad expansion program. Volpe Marticus and Blyde (2013) flip the infrastructure-provision experiment on its head, and utilize the variation in damages to the road network caused by the 2010 Chilean earthquake. They find a large drop in trade volume associated with these damages. Duranton et al. (2013) use an instrumental variable strategy based on historic routes to show that the weight of exports is highly responsive to the construction of highways in the US. Datta (2012) finds that firms located in cities closer to newly improved highways carry smaller inventories, suggesting a decrease in the fixed cost of getting a shipment. In a study set in Sierra Leone, Casaburi et al. (2013) find evidence that improved rural feeder roads facilitated easier market access for farmers, leading to a decrease in the observed market price of both rice and cassava. They attribute this drop to a reduction in both transport costs as well as search costs.[11]

An older, non-empirical strand of the literature also has similar findings: Coulibaly and Fontagne (2006) estimate that paving all unpaved interstate roads in West Africa would lead to a threefold increase in intraregional trade; Buys et al. (2006) estimate gains in overland trade of up to $250 billion over 15 years by upgrading the highway network in Sub-Saharan Africa; simulations by Shepherd and Wilson (2007) suggest that upgrading the road infrastructure between Europe and Central Asia can increase trade flows by 50 percent over baseline.

As trade costs go down, a direct implication is that the spatial price differential of traded goods should go down by the extent to which this differential was composed of transport costs. Accordingly, Donaldson (2013) finds large reductions in price differences between regions connected by the railroad. Keller and Shiue (2008) find similar evidence from 19th century Germany, showing that the adoption of steam trains led to a 14 percent decline in grain price dispersion across 68 markets. Utilizing a slightly different source of variation in transport costs, Keller and Shiue (2007) show that the price of rice in 18th century China displayed a greater degree of correlation between markets that were integrated with each other due to their locations along the Yangzi river and its tributaries. My study corroborates the results of this literature, albeit in a different setting, by showing that access to paved roads decreases the spatial dispersion of prices for almost all types of food items.

We might also expect increased trade flows to be mirrored in household consumption. To my knowledge, there is no evidence in the current literature on the relationship between trade and household consumption mix, and scant evidence on consumption levels. In a

[11] Jensen (2007) and Aker (2010) have studied the impact of communication infrastructure as a way of lowering search costs / information frictions in developing countries, and have found similar reductions in price dispersion. Aker also finds that the effect of mobile phones on lowering price dispersion is greater for market pairs that are connected by a road, suggesting that access to good modes of transportation and communication can substitute for each other in this context. On the other hand, Mitra et al. (2013) find that in the absence of direct access to wholesale markets, an information intervention did not significantly improve farmers' bargaining position with middlemen, suggesting complementarities between physical and virtual networks.

review article, Goldberg and Pavcnik (2007) argue that this is because good measures of consumption are extremely hard to come by, limiting researchers to use measures of income, rather than consumption. Nevertheless, Topalova (2010) provides evidence that Indian districts with greater exposure to trade liberalization witnessed smaller gains in consumption levels. However, the channels at work in her paper operate through differences in industrial composition, and are likely to be far less important in the context of remote areas in rural India.

Accordingly, my examination of consumption levels in the wake of road construction reveals no significant changes. However, if trade changes the access to goods, then analyzing consumption patterns, on both the intensive and the extensive margins, might still be informative. Of these, the extensive margin is much easier to measure as gains on the intensive margin are likely to get attenuated if households switch to better quality goods, or choose to consume a greater variety of goods (which is precisely the extensive margin effect). While there is no study that directly explores the relationship between transportation infrastructure and consumption variety, there is a large literature on the variety gains from trade; wherein trade increases the availability of different types of goods available from different trading partners (Feenstra, 1994; Romer, 1994; Klenow and Rodriguez-Clare, 1997; Broda and Weinstein, 2006). Alternatively, a complementary strand of the New Economic Geography literature has asserted that variety gains can arise, at least in part, from agglomeration economies (Handbury and Weinstein, 2011; Li, 2012). Since Krugman (1991), this literature has hypothesized low transportation costs as playing a central role for agglomeration economies to emerge. More recently, the emergent literature on transportation infrastructure in the field of spatial IO has verified this empirically (Duranton and Turner, 2012; Faber, 2013; Mayer and Trevien, 2013; Rothenberg, 2012). Therefore, variety gains in the wake of road construction could be viewed as either emerging directly as rural and urban areas start trading more intensively, or as arising as a consequence of economies of scale in production.

In a framework with CES utility, this increase in variety directly enters the utility function in the form of new goods, and is welfare enhancing by itself. Moreover, even in the absence of assumptions on the exact form of the utility function, the gains in diversity in food consumption can be viewed as providing much needed micronutrients to combat malnutrition and increase productivity, especially in developing countries (Marshall et al., 2001; Tontisirin et al., 2002; Kennedy et al., 2007; Arlappa et al., 2010; FAO, 2011).

In this study, I provide evidence that in response to the program, there are significant changes along the variety dimension in households' consumption basket. Further, the impacts are heterogeneous and varied by type of good: newly connected households decrease the types of non-perishables, and increase the types of perishables and non-locally produced goods in their consumption basket. To my knowledge, this is the first paper to use survey data on household consumption to measure variety gains,[12] the first to estimate variety gains from infrastructure provision, and also the first to show that there may be heterogeneity by good-type in how households adjust their consumption when they move out of relative autarky.

Independent of trade, roads can influence key economic variables by lowering the transport, time and information costs of accessing a host of different markets. Consider the example of technology adoption in agriculture. Suri (2011) shows that farmers with high gross returns to inputs like hybrid seeds may still choose not to adopt them if there are high costs to acquiring these due to poor infrastructure. In a very similar vein, Ali (2011) finds that road improvements in Bangladesh led farmers to take up hybrid varieties of rice at a faster rate. She proposes a different mechanism for her results, suggesting instead that as transportation costs go down, it becomes possible for farmers to intensify production. Other

[12]Much of the existing trade literature uses countries' import composition to measure variety gains. See, for instance, Klenow and Rodriguez-Clare (1997),and Arkolakis et al. (2008). Broda and Weinstein (2006), Handbury and Weinstein (2011), and Li (2012) use supermarket scanner data, which provides an alternative measure of household consumption but does not allow the researcher to control for household charactersitics. Hillberry and Hummels (2008) analyze this from the firms' perspective and show that trade frictions reduce aggregate trade volumes primarily by reducing the number of goods shipped and the number of establishments shipping.

potential explanations for greater technology take-up also come to mind. For instance, Croppenstedt et al. (2003), Devoto et al. (2012), and Tarozzi et al. (2013) have found evidence that credit constraints hamper the adoption of technology. Roads could potentially alleviate some of these constraints by increasing output prices (Khandker et al., 2009), or by increasing the collateral value of land (Gonzalez-Navarro and Quintana-Domeque, 2012a; Shreshtha, 2012; Donaldson and Hornbeck, 2013). Although data limitations preclude me from isolating the exact channels at play, the findings in this paper confirm the association between road construction and technology adoption, wherein I find that there was high take-up of fertilizer and hybrid seeds by farmers who were newly connected to markets via roads.

The affect of roads can also extend to human capital accumulation. There is a rich literature in development that finds large positive effects of school construction on children's school enrollment and attendance (Duflo, 2001; Handa, 2002; Aaronson and Mazumder, 2011; Burde and Linden, 2013; Kazianga et al., 2013). To the extent that the operative channel in these studies is greater proximity to the school, constructing a road might have similar positive effects by reducing the effective distance (in terms of travel time) and the cost of traveling to school. In a recent paper, Muralidharan and Prakash (2013) analyze precisely the effect of reducing the effective distance to school without constructing any new schools. They use a public program from the Indian state of Bihar that provided bicycles to girls continuing to secondary school, and find a 30 percent gain in enrollment.

On the other hand, greater access brought about by roads may open up greater labor market opportunities for children (say, in the nearest town or market center), raising the opportunity cost of schooling and causing some of them to drop out. Atkin (2012) provides evidence showing that the availability of jobs due to new factory openings led children to drop out sooner from high school. Similarly, Menon (2010) and Nelson (2011) find that improving self-employed households' access to credit leads their kids to drop out of school and start working in the family enterprise. Duryea and Arends-Kuenning (2003), Schady

(2004), Kruger (2007), and Shah and Steinberg (2013) find similar effects for very transient labor market shocks, showing that kids are more likely to be in school during when jobs are scarce (commodity price busts, droughts, and recessions), and more likely to be working when jobs are abundant.

However, even this relationship is far from clear as the final effect will depend on which of the two effects - income and substitution - dominates. Accordingly, another set of studies finds diametrically opposite effects, wherein children's school enrollment moves in the same direction as income (Jacoby and Skoufias, 1997; Edmonds and Pavcnik, 2005Edmonds et al, 2010).[13]

In addition, the schooling decision might be further complicated if the advent of roads increases access to the kind of jobs that have a skill premium or, if trade alters the skill premium of existing jobs, inducing kids to attend school. Michaels (2007) provides evidence that increased trade following the construction of the US Interstate Highway system caused regions to shift production in line with their comparative advantage, as predicted by Hecksher-Ohlin. This caused an increase in the demand for, and returns to skilled labor in skill-abundant counties and a decrease elsewhere, and *vice versa*.

While there are no papers that directly study the link between roads and human capital accumulation, a host of recent papers have showed that children's schooling decisions change when access to skill-intensive jobs improves (Foster and Rosenzweig, 1996; Munshi and Rosenzweig, 2006; Heath and Mobarak, 2011; Jensen, 2012; Shastry, 2013; Oster and Steinberg, 2013). This paper contributes to the literature by being the first to analyze how school enrollment changes in rural areas in the presence of roads.

This is an important contribution as education is one of the pre-eminent development priorities.[14] Moreover, providing market connectivity is also emerging as a key policy goal.

[13]See Ferreira and Schady (2008) for a review that includes many others.

[14]Universal primary education is one of the eight millennium development goals. Secondary education is also central to the policy agenda in most countries.

As such, that makes it critical to understand how these goals might interact to produce unintended consequences, so that appropriate policy measures can be designed in order to address them.

3 Context

The government of India announced PMGSY on December 25, 2000, with actual work beginning in 2001.[15] The goal of the program was to provide an all-weather road within 500 meters[16] of all sub-villages (the program refers to these as "habitations") with a population of at least 500 (250 in the case of tribal areas, or areas pre-defined as desert or mountainous). A habitation is a sub-village level entity, and is defined as "a cluster of population, whose location does not change over time".[17] For the purpose of this study, I use the terms sub-village, habitation, and village interchangeably. The population of each village was determined using the 2001 census. The scheme was federally funded,[18] but implemented by individual states.

At the outset of the scheme, states were asked to draw up a core network of roads, which was defined as the bare minimum number of roads required to provide access to all eligible villages. Only those roads that were a part of the core network could be constructed under this scheme. Within the core network, construction was to be prioritized using population

[15]The program website ishttp://pmgsy.nic.in/pmgsy.asp

[16]For mountainous areas, this was defined as 1.5 kilometers of path distance. As per an amendment made to the program rules in February, 2008, in mountainous regions located next to India's international borders, this distance could be up to 10 kilometers (Ministry of Rural Development, letter no. P-17023/38/2005-RC dated February 29, 2008).

[17]A village will have multiple habitations if it has 2 or more clearly delineated clusters. For instance, there might be two separate clusters of houses on either side of the village well. India has about 640,000 villages comprising of about 950,000 habitations.

[18]This scheme was funded by earmarking 1 Rupee per liter out of the tax on high speed diesel. The funds were disbursed to the states using a pre-determined formula known as "additional central assistance", which has the following weights: population - 0.6, per capita income - 0.25, tax efforts - 0.075, special problems - 0.075.

categories, wherein, villages with a population of 1000 or more were to be connected first, followed by those with a population of 500-1000, ultimately followed by those with a population of 250-500 (if eligible). The rules further stipulated that in each state, villages from lower population categories could start getting connected once all the villages in the immediately larger category were connected. Exceptions were allowed if a smaller (by population category) village lay on the straight path of a road that was being built to a larger village. In this case, the smaller village would get connected sooner. The program also allowed for multiple villages to come together as a group and be treated as a single entity, as long as these were located within 1.5 kilometers of each other.

Therefore, the program presents a potentially suitable setting to examine the causal impact of rural roads. Before we proceed with a causal analysis of outcomes in this context, we must ensure that the program guidelines were followed and that there were minimal deviations from the population rule. This is especially pertinent in the Indian setting as corruption is widespread. Accordingly, Table 1 looks at the determinants of road construction under the program over the period 2001-2010. We can see that by endline, villages with a population of 1000+ were 42 percent more likely, and those with population 500-1000 were 26 percent more likely to have received a road as compared to villages with less than 500 inhabitants. However, the coefficients on Panchayat headquarters and primary school raise some concerns about potential selection on observables. In my empirical analysis, I deal with this issue by using various different specifications, with and without controlling for observables. My findings stay robust to the inclusion of controls, suggesting that the results are not being driven by selection.

I analyze program compliance in a slightly different manner in Figure 1, where I show the likelihood of road construction for more finely defined bins. The discontinuous jump in the probability distribution of road construction is more apparent here. In looking at both Table 1 and Figure 1, it is clear that as stipulated by the program, the larger villages dominated the smaller ones in terms of construction priority.[19] However, the prioritization is not completely clean as smaller villages begin to get roads before the larger ones are completely done. This may be explained by two factors. One, the program did allow for out-of-order connectivity if the location of the villages on the path to the market necessitated so, or if a number of small villages located close to each other chose to be treated as a single village. Second, it is virtually impossible to completely eliminate all deviations from the rule in a program of this scale. However, I must admit at the outset that the possibility of a small degree of political manipulation cannot be completely ruled out, especially in light of the significant predictive power of Panchayat HQ on road construction.

In fact, corruption is a smaller concern here, than it is in other public programs, as it is not immediately obvious why political economy would dictate deviations from the rule. It would have been in the interest of state and district level politicians to follow the population-based rule of the program as a mechanism to garner votes. For instance, Cole (2009) shows that politicians in India use their influence to get banks to disburse more credit during election years. More generally, even in the absence of "vote buying", the median voter theorem would predict that in a majority rule political setting like India, public goods are allocated in a manner where they benefit the most number of people. In fact, Gonzalez-Navarro and Quintana-Domeque (2012b) show that politicians in Mexico realized a 20 percent gain in terms of vote share if an unpaved electoral section got fully paved during their term in office.

As it stands, a far graver corruption concern pertaining to this program would be that the roads were not built at all, and that the funds were appropriated by local politicians and bureaucrats. 2 different factors help me mitigate this concern: 1) The government of India was hugely invested in making this scheme transparent to the extent possible. As a result, the program was very closely monitored by many different stakeholders and all of the construction details are publicly available,[20] and 2) All of my specifications control for either

[19] Appendix A1 presents cumulative density functions of connectivity by population category.

[20] The program has a three-tier monitoring system at the district, state and federal level. For details, see

district or state level unobservables. Moreover, in case some areas did not get roads as per plan, then my estimates represent a lower bound on the causal impact of roads.

Nevertheless, my empirical analysis consists of a number of robustness checks. I am able to show that there were no pre-trends in outcomes as placebo specifications with roads built during the program period have no predictive power in explaining changes in outcomes over the pre-program period, 1993-1999. I also try to rule out selection into program by controlling for a number of different observable characteristics, and by absorbing unobservables at the district and state level into fixed effects.

4 Data

I use data from a number of different sources in my estimation.

4.1 Online Management and Monitoring System (OMMS)

Due to concerns of corruption of funds in large public programs, the Government of India has mandated that the ministry in charge of any such program make all program data available to the public through the program's website. As a result, habitation-level road construction data is available through OMMS. Thus, for the universe of rural habitations, I have data on their baseline level of connectivity, population (in order to determine eligibility), whether they got a road under the program, and if so, the year in which the road was approved and built. In all of my analysis, in order to get around issues of implementation and quality, I use the approval date as the date on which the road was built, and use the words "approved" and built" interchangeably.

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the program's operation manual, available at http://pmgsy.nic.in/op12.htm.

4.2 Population Census, 2001

I use the village directories included in the 2001 census of India. I merge these villages with those from the OMMS, and get a ~80 percent match. I then use these to study differences in baseline characteristics for connected and unconnected villages at the outset of the program.[21] These are presented in Appendix Table A1. Table 2 highlights the fact that at baseline, the average village with a road was significantly different from an average village without one, along all observable parameters. These statistics underscore the setting in which the inhabitants of the average unconnected village lived, and help us contextualize the findings of this paper. Further, they also highlight the stark distinction between the 2 types of villages, and therefore, caution us against using the connected villages as a control group.

4.3 National Sample Survey (NSS) Data

The NSS is a very rich, nation-wide, repeated cross-section survey of individuals and households, or a panel of the districts that they reside in. The surveys contain extremely granular household-level information on the quantity and value of more than 350 distinct items, and individual-level information on education and labor-market participation. Even though the unit of observation is the household in case of the consumption data, and the individual in case of the education and employment data, the smallest identifiable unit provided by the Government of India is the household or individual's district of residence. In order to examine the consumption and human capital outcomes, I use data from the rural schedules of rounds 57 (year 2001) to 66 (year 2010) of NSS. However, since some modules are not fielded every year, this translates to consumption data for years 2001-2008 and 2010,

[21] Once village-level data from the 2011 census is available, the empirical analysis in this paper can be further refined by using the discontinuities at the population cut-offs

and education and employment data for 2004-2006, 2008, and 2010. Since the smallest identifiable unit is the district, this necessitates that my unit of analysis be the district. I discuss this in greater detail in the next section.

4.4 Agricultural Inputs Survey

The Ministry of Agriculture conducts a 5-yearly survey on the usage of advanced inputs in agriculture, including the use of fertilizer, hybrid seeds, and pesticides. For this survey, all operational holdings from a randomly selected 7 percent sample of all villages in a sub-district are interviewed about their input use. These responses are then aggregated by crop and plot-size category (these categories are reported as: below 1 hectare (ha), 1-1.99 ha, 2-3.99 ha, 4-9.99 ha, and above 10 ha), and reported at a district level. The survey also reports the irrigation status (rain-fed or irrigated) of the holdings separately. Therefore, I have a district-crop-plot size-irrigation status-year panel of operation holdings in rural India, which I aggregate at the district-crop-year level. I use the 2001-02, and the 2006-07 rounds of the survey for this study. To my knowledge, this is the first instance of the use of this survey in the literature.

4.5 Agricultural Prices Data

I also use high frequency price data at a weekly level for highly disaggregated food varieties from 3,566 agricultural markets, or *mandis*. Every day, these markets report the modal price of every animal/crop variety sold therein to a Ministry of Agriculture initiative known as Agmarknet.[22] I manually downloaded this data for each market and each crop for one day every week (each Thursday). I use this to supplement my results on price dispersion from

[22] Website: http://agmarknet.nic.in/

the NSS consumption module. To my knowledge, this is the first instance that this data has been used for research.

5 Identification Strategy

The NSS does not have village-level identifiers, and everything is aggregated to the district. Therefore, I am unable to exploit the program rule of providing roads to villages based on their population category in a regression discontinuity design. Instead, I have to rely on a difference-in-differences strategy to estimate the differences between treatment and control over time. If I had individual level data on road connectivity status, my estimating equation would have been the following:

$$y_{idt} = \alpha + \gamma_t + \beta * D_{idt} + \eta Z_{idt} + \varepsilon_{idt} \tag{1}$$

where subscript i denotes individuals or households (depending on the outcome of interest), d denotes district, and t denotes survey year. δ is a set of district fixed effects,[23] γ is a set of year fixed effects and Z is a vector of individual / household control variables. D_{idt} is an indicator variable for whether individual i in district d at time t has been exposed to the program, which amounts to an indicator for whether or not a road has been built to his village under the program.[24] However, with district-level outcomes, I must aggregate equation (1) as the following, where N_{dt} is the number of individuals in district d at time t:

$$y_{idt} = \alpha + \gamma_t + \delta_d + \beta * (D_{idt}/N_{dt}) + \eta Z_{idt} + \varepsilon_{idt} \tag{2}$$

which amounts to using the variations in the percentage of population that received a road

[23] All estimating equations were also specified alternately to have state fixed effects, and yield similar results. The results from these specifications, where not presented in the paper, are available on request.

[24] As mentioned before, but as a reminder to readers: this is in fact an indicator for whether or not a road was approved to be built.

in each district in each year.

It is worth keeping in mind here that the variations in the percentage of population in each district are fundamentally a function of variations in the distribution of village sizes in each district. This is because the program rule was applied at the village level, wherein each village's likelihood of receiving a road was an increasing step function of its population, as shown in Figure 1. When aggregated up to the district, the implication of the rule is that the number of roads built in each district would be some increasing function of the number of villages in each population-size category in that district.

For some parts of my analysis, I only have access to, or make use of, just 2 rounds of data. In such cases, my estimating equation is given by:

$$y_{idt} = \alpha + \delta_d + T + \beta * Pr(D_{idt}) * T + \sigma Z_{idt} + \epsilon_{idt} \tag{3}$$

Here, T is an indicator for the post-treatment period.

In all specifications, the coefficient β is my estimate of the causal effect of road construction. All errors are clustered at the district level.

6 Estimation Results

6.1 Price Dispersion

Following Donaldson (2013), I argues that if roads indeed had the intended effects of reducing transportation costs and integrating markets, then we should observe a reduction in price dispersion across these markets.[25] Consequently, I seek to establish a "first-stage" effect of roads via price dispersion. I use 2 distinct data sources for my analysis of price dispersion. First, I back out prices based on household responses in the NSS: the survey does

[25]It is possible that there may be districts where a majority of the villages are inaccessible, and prices (including transport costs) are consequently, high in all of them. In such districts, building roads to some villages, while others stay inaccessible, may actually increase district-level price dispersion. However, it is reasonable to expect a negative coefficient on price dispersion for the average district.

not directly report price data, reporting instead the value of each good consumed. However, for food items, the survey reports both the value and the quantity consumed, which enables me to back out the unit values. It must be borne in mind that this strategy will yield price information for only those households that report consuming a positive amount of a particular food. Further, since the survey questions disregard the quality dimension,this approach to computing prices is likely to understate the reductions in prices brought about by roads if households switch to higher quality goods.

With these caveats in mind, I turn to the first part of my analysis of prices. In order to compute the effect on price dispersion of each broad category of foods, I create an index for each of these categories as the weighted average of the price dispersion of the individual food items included in the category. The weight for each item varies by district, and is given by the share of that item in the district's median household's budget share in the baseline year. The dispersion itself is the standard deviation of the price of each good reported by all households in each district. Any household that does not report consuming a good gets dropped from the calculation of the dispersion. Therefore, a downside to this approach is that as the number of households consuming a good expands, the dispersion will weakly increase as a mathematical construct. Further, since we have already seen that roads were associated with an expansion in variety, the results on price dispersion should be interpreted as a lower bound on the true program effect. The results from this analysis are presented in Panel A of Table 2. The results in this table are suggestive that the construction of roads lead to a reduction in the prices of all types of food items, other than lentils and processed food.

For the second part of this analysis, I use prices reported by agricultural markets. I calculated the district-wide dispersion in the modal price of each good, as reported by the markets. This analysis is presented in Panel B of Table 2. As in Panel A, I find evidence suggesting that there were huge reductions in the dispersion of prices in districts that were newly connected by roads.

6.2 Education & Employment

After establishing that road construction did in fact impact market access, I turn to analysis of human capital accumulation and market participation.I start by looking at the impact of road construction on school enrollment of 5-14 year old children. The results are presented in Panel A of Table 3. In my preferred difference-in-difference specification with district fixed effects (column 4), there is a 5 percentage point increase in enrollment. This finding is of immense importance for public policy. For instance, the UN's Millennium Development Goals website notes that as of 2010, enrollment in primary school stood at 90 percent. These results suggest that rural road construction alone could potentially bridge half of the gap toward achieving universal primary education in India. From an external validity standpoint, it would be useful to isolate the channels through which these gains arise. For instance, roads might alter the returns to education, increasing the household's incentives to send children to school. Alternatively, roads might be leading to increases in family income, or relaxing credit constraints, or improving physical access to the primary school. However, I am unable to do so with existing data sources.

In Panel B, I do identical analyses for 14-20 year old individuals. In this case, the effects are strongly negative, and robust to the inclusion of various covariates and fixed effects. The interpretation is straightforward: going from not having a road to having one, leads to about an 11 percentage point drop in school enrollment, which is an almost 25 percent decline over mean enrollment rates at baseline. An alternative way of interpreting these coefficients is in terms of network effects: since the program was implemented at the village-level, but my results track changes for the district, it is possible that some of the observed gains and losses from the program arose outside the beneficiary villages. At the district level, the average treatment effect needs to be rescaled by the average treatment size, which in this case is .05. Viewed in this manner, the program led to about a 0.006 percentage point drop in school enrollment for 14-20 year-olds, which translates to a .01 percent decline over mean.

There are a number of important points about Table 3. One, on decomposing by gender, I do not find any differences in the enrollment gains or losses between girls and boys. This is of great importance in a setting like India, where investment in girls tends to be disproportionately low due to cultural norms of son preference (Pande, 2003; Jayachandran and Kuziemko, 2011; Bharadwaj and Lakdawala, 2013). My results indicate that even though excludable private resources tend to overwhelmingly be concentrated on male children,[26] the benefits from public goods are enjoyed by both genders equally. Two, in both panels, columns 2 and 4 differ from 1 and 3 in that the former control for household-level observables. Specifically, I control for the the household's religion, social group (scheduled caste, scheduled tribe, backward caste, or none of these), household type (self-employed or not, agricultural or non-agricultural), size of land owned, and the size of the household. Note that the inclusion of these controls does not alter the coefficients. To the extent that household characteristics are closely correlated with village-level unobservables, this provides additional evidence against selection on observables in road construction. Three, while the first two columns control for fixed effects at the state level, the latter two control for these at the district level. The coefficients on school enrollment remain substantively unaltered across these two specifications. This is an important observation as it suggests that the effect of road construction did not vary significantly between the cross-section (different districts of a state getting connected in the same year) and the panel (villages of the same district getting connected over time). Not only does this provide further evidence for the robustness of my estimates, it also enables us to generalize these results to other road construction programs in different settings.

[26] This is also apparent in the great gender disparity in baseline mean enrollment rates, especially for older children.

While the age-groups of 5-14 and 14-20 were created due to contextual relevance,[27] it may still be informative to analyze the effects of roads on enrollment for each age year separately. Figure 2 presents the results from this decomposition - the Xs represent the baseline mean of enrollment for each age, and the dots represent the treatment effect. While the biggest changes lie at the tails, the distribution strongly supports the manner in which the ages have been pooled in my regression results.

Table 4 summarizes the next set of my results, pertaining to market employment of 14-20 year old children and of adults. Panel A suggests that the school drop-out instance of the 14-20 age group that we witnessed in Table 3, is matched almost one to one by increased market employment. As before, these effects do not vary by gender: both girls and boys witness about a 10 percent rise in market employment, which constitutes more than a 40 percent increase over baseline employment levels.[28] Further, this increase in market employment is not limited to children, as can be evidenced in panel B. On receiving a road, prime-aged women were also 9 percentage points more likely to start working, a 25 percent increase. On the other hand, there is no comparable change for men, which is to be expected, as their employment was nearly universal even at baseline.

I attempt to investigate the mechanisms behind this observed jump in market participation by looking at the occupations that the newly-employed are joining. The results are presented in Table 5. For girls, the most marked increase in employment comes from animal-rearing, followed by textile manufacturing and tailoring. They are less likely than before to be working in forestry, and there is no impact on any of the other occupations. For

[27] 14 marks the threshold between primary and secondary education in India. Further, the employment of children below 14 is considered child labor, and is a crime under the Constitution of India (The Child Labour (Prohibition and Regulation) Act, 1986 (http://indiacode.nic.in/fullact1.asp?tfnm=198661)

[28] A breakdown by age, similar to the one for school enrollment, is presented in Figure 3.

boys, on the other hand, the biggest increase comes from construction[29], followed by smaller increases in animal-rearing and tailoring. The increase in animal-rearing is in line with the reduced transportation cost explanation as roads might make it possible to transport dairy and meat to the nearest market in a timely fashion. The increase in tailoring and making textiles also comes up in the anecdotal evidence provided on the program website as "success stories"[30]: the presence of the road makes it easier for weavers, embroiders, and other similar artisans to sell their crafts in the nearby town. The increases in animal-rearing and tailoring may also explain some of the observed increases in school enrollment for younger children. For instance, Heath & Mobarak (2011) show that the advent of garment manufacturing in Bangladesh was associated with enrollment gains for young girl as tailoring jobs require a basic level of numeracy. In looking at occupations for women, I still find the biggest gains in animal rearing. There is also a small increase in textile manufacturing as an occupation. Taken together with the occupational choices of teenaged children, these results suggest that program villages saw the biggest increases in animal-rearing as an occupation, likely due to access to bigger markets. This increase in animal husbandry also constituted a positive supply shock for rural areas themselves, and led to increases in the kinds of dairy and meat products consumed by village inhabitants, which I will discuss later in the paper.

6.3 Technology Adoption

The results thus far provide evidence that road construction lead to a reduction in transport costs, and consequently, better access to goods and labor markets. As discussed before, the "reduction in transport costs" channel may also operate in input markets by making it cheaper to either buy the inputs themselves, or by easing credit constraints that hamper

[29]The occupation codes for this category correspond to working as casual labor on private construction sites, and not to working on construction of public works, including roads.

[30]See http://pmgsy.nic.in/pmgi112.asp#6

technology adoption in agriculture. I test this hypothesis by looking at the area under cultivation using advanced agricultural inputs. Specifically, I look at the adoption of chemical fertilizers and high-yielding varieties of seeds. Before we analyze the results, it would be useful to understand the underlying data.

The data that I use for this subsection comes from the input survey module of the 2001-02 and 2006-07 rounds of the agricultural census. The data from this survey are reported by the Ministry of Agriculture as district-level aggregates. So, for any district in the country, I have the aggregate acreage, as well as the acreage under modern inputs for all crops grown in that district. This implies that for this part of the analysis, all treatment effect coefficients would need to be rescaled by treatment intensity. I now turn to the results, which are presented in Table 6. From Column 1, the average crop-district had 22,000 hectares under cultivation at baseline, and would have seen an increase of a little over 10,000 hectares in the area under fertilizer use in going from 0 to 100 percent connected. Therefore, the average district, where about 7 percent of the population received new roads, this translates to a 700 hectare, or a 3 percent gain in the area under fertilizer per crop. Similarly, for hybrid seeds, there was a 2 percent increase in the area under cultivation per crop. When I break down the analysis by crop type, significant differences emerge: the gains in technology use are entirely concentrated in food crop cultivation, and absent for cash crops. A potential explanation for this might be that cash crops tend to be grown more by bigger farmers, who are less likely to be constrained by low availability of credit. Alternatively, using the district as the unit of analysis might be masking significant heterogeneity in the pattern of cultivation within the district. Specifically, it is possible that remote regions with low road connectivity do not grow cash crops due to limited market access. In that case, the road construction program is likely to have benefited only those farmers that cultivate food crops.

6.4 Consumption

Based on the analysis so far, treatment households witnessed supply-side changes in the goods available to them due to multiple reasons. The first-stage change arises from better access itself. In addition, occupational changes in the village, and the presumed expansion in agricultural production due to advanced inputs may have also led to a greater availability of goods. Therefore, it is a reasonable prediction that households are likely to start consuming a larger number of goods. Predictions are less clear for quantities consumed as households might choose to switch to higher quality goods as their prices decline.

6.4.1 Variety

I start by running a regression that looks at differences in outcomes at baseline and endline only, i.e. in 2001 and 2010 only, as mediated by road construction.[31] My outcome of interest is variety in the consumption basket, which I measure as number goods in a particular category (say, fruits or dairy) that are consumed by a household. Note that in this case, consumption of each variable is a binary variable that takes the value 1 for any positive reported amounts, and 0 otherwise, and so is the extensive margin effect.[32] Therefore, my specification is given by where all variables are as defined in case of equation 4, and T is the dummy for year 2010. Results are presented in Panel A of Table 7. The results suggest that among food items, a household that goes from not having a road to having one, consumes 0.6 fewer types of cereals and 0.4 fewer types of lentils. Additionally, there is a gain of 0.14 in the number of diary products being consumed by such a household. Other food groups

[31]The stated objective of the program was to provide all-weather roads, which could be achieved either by paving existing roads, or by constructing new ones. My analysis only considers new roads.

[32]My estimate could still, in some sense, be a lower bound on the consumption effect of roads if there are households that completely switch out of consuming a certain good, and substitute it with another, say, if the substituted good is inferior (for instance, a switch from coarse grain to fine grain). The estimated coefficient, in this case, would be 0, since the total number of goods consumed did not change, even though the household potentially moved to a higher indifference curve.

also have positive, albeit insignificant coefficients. For non-food items too, the estimates are large, positive, and significant. It stands out that for all types of non-food items, the coefficient on the interaction between roads and the time dummy is much larger (in some cases, by an order of magnitude) than the coefficient on the time dummy alone. Given that the Indian economy witnessed very rapid growth over this period,[33] these estimates provide remarkable testimony to the effectiveness of infrastructure provision in this regard.

Since Panel A is based on just 2 rounds of data (baseline and endline), the estimates contained therein are quite underpowered. I try to bolster these by utilizing the annual variation in outcomes available to me from successive rounds of the NSS, using the specification described in Equation (3). These estimates are presented in Panel B of table 7. By utilizing the entire panel, I find that not only do the coefficients from Panel A continue to be robust, variety gains in the consumption of fruit and processed food are also now significant. Many things stand out in looking at this table. One, for food items, we see a marked decrease in the consumption of non-perishables (cereal and lentils), and an increase for perishables and processed food. The increase in processed foods is consistent with the transport cost explanation as these foods tend to be produced in urban areas. For locally-produced foods, this upsurge is potentially explained by changes in production patterns. For instance, both Muto and Yamano (2009), and Goyal (2010) find supply responses by farmers to a reduction in search costs due to the introduction of mobile phones. In addition, in Muto and Yamano, this response is limited to perishable foods (bananas), while the non-perishable commodity (maize) stays unaffected.

Two, even though the estimated coefficient on "Meat" is insignificant, it should be borne in mind that this has been estimated off a sample with a large number of zeros due to the cultural prevalence of vegetarianism in Indian society. [34] This preference for vegetarianism

[33]According to the IMF's World Economic Outlook database, the average annual growth rate of per capita GDP (at constant prices) was 6.3 percent per annum for the period 2001-2010

[34]According to a 2006 survey by the Hindu and CNN-IBN, 40 percent of those surveyed reported being vegetarian (http://hindu.com/2006/08/14/stories/2006081403771200.htm). This number is likely higher in

would reflect itself as some households choosing to forgo consuming meat, even though it is easily available.

Three, while the growth in vehicle ownership and the use of hired means of surface transport (given by the column titled "road-fares") are outcomes of interest in their own right, they also serve as a robustness check for my results, especially when viewed along side the absence of effects on non-road means of transportation.[35]

6.4.2 Quantities Consumed

As mentioned before, the analysis of quantities is also complicated by the possibility of substitution of one good for another, and also of higher/lower quality variants of the same good for each other. For instance, if households substitute a smaller quantity of fine grain for a larger quantity of coarse grain, the survey will record it as a reduction in quantity consumed. Similarly, it is hard to conclude anything about the welfare gains or losses for a household which substitutes say, a liter of milk for 200 grams of yogurt. Nevertheless, I do such an analysis in the hope of being able to parse out some broad trends. It bears mentioning here that the survey reports quantities consumed only for food items, limiting my analysis to food consumption only. In order to facilitate comparisons, I create an index of the quantities consumed of each broad food group in the following manner: first, for each individual good (say, yogurt or ketchup) I create a z-score of the quantity consumed by each household, using the mean and standard deviation of the consumption of that good in each district in the baseline year. I then combine the individual z-scores to create consumption

rural areas due to stricter adherence to traditional norms.

[35]This growth also provides evidence that once roads had been constructed, there was spontaneous growth in the availability of public means of transport. This is contrary to the evidence provided by Goldberg et al. (2011), who show that motorized public transportation is not profitable in rural Malawi, due to which many villages, despite having serviceable roads, are without regular bus lines. This could potentially be driven by differences in population density between the two countries. As such, population density can be a key factor in determining what kind of last-mile connectivity could be socially profitable in rural areas.

indices for broad categories like cereal and dairy. This index is the weighted mean of all the z-scores in each food category, where the weights are given by the share of that good in the median household's budget in the baseline year.[36]

The results are presented in Table 8. Panel A presentes the analysis of quantity indices for just the baseline and endline years, and Panel B replicates it for the entire sample period. In Panel B, we find that there is a large increase in the quantity consumed of cereals and lentils. This is in contrast to our analysis of consumption diversity, and suggests that even though households are consuming fewer varieties of cereals and goods, they are consuming a lot more of them, Similarly, while there are no variety gains in meat and vegetables, the variety changes are substantial. On the other hand, for dairy and processed foods, households are consuming fewer quantities, but more varieties. This anslysis suggests that households substitute between width and depth in their consumption basket. However, the welfare implications from this analysis are unclear.

7 Robustness

The fundamental concern with any study in a diff-in-diff setup is that trends might not be parallel, invalidating the results. This concern is especially acute in this case, as districts that had a lot of roads pre-program might be on a very different trajectory compared to the ones that had few roads. In order to rule this out, I adopt the standard method from the literature, which is to run placebo regressions of roads built during the program on outcomes during a pre-program period. The results from this test are presented in Table 9 for human capital outcomes, and in Table 10 for consumption outcomes. In both these tables, the post period is a dummy variable for the year 1999, the baseline year is 1993, and the roads built

[36] This index is akin to the one introduced by Kling et al. (2007). An index like this is particularly helpful when there is a large number of outcome variables (in this case the prices of close to 150 different types of food items) as it eliminates the problem of multiple inference.

variable gives the percentage of population that received roads over the entire treatment period up to 2010. In all cases (except number of vegetables consumed), the point estimate is statistically insignificant.

In an alternative test, I regress future roads on current outcomes during the program. Here, my placebo variables are the percentage of population connected 1 and 2 years in the future from the present period. These results are presented in Table 11. The point estimates on the 2 placebo variables are consistently insignificant, and often alternate in sign. The results from Tables 9-11, taken together bolster our confidence in the hypothesis that my results are not picking up spurious effects.

In addition to these tests, I document in section 6.2 above that the results for human capital outcomes stay similar across a range of different specifications with and without covariates, and with and without fixed effects. This helps me rule out selection on observables in road construction. As a final robustness test, I look at consumption effects during the monsoon season. Since the program aimed at providing all-weather roads, its effects were likely to be most keenly felt during the Monsoon when the fair-weather roads to the town are most likely to be flooded or washed out. This is especially true for consumption outcomes, as households are unlikely to make seasonal adjustments to their enrollment or employment decisions. Moreover, any Monsoon-specific effects are unlikely to have come about due to other confounding factors. In order to do this, I combine the information provided by NSS on the date of the survey with consumption information for food, which has a 30 day recall period in the survey. Unfortunately, I am unable to replicate this exercise for non-food items as the survey asks households to report these for a 365 day recall window. Using the Indian Meteorological Department's Monsoon maps as a guide,[37] I create a "monsoon" dummy to indicate whether the household was interviewed during the rainy season, or outside of it. I then interact this dummy with the road construction variable to confirm the robustness of

[37] Available at http://www.imd.gov.in/

my results, which are presented in Appendix Table A3. The specification underlying this table checks for the variety in a household's consumption basket. If the results presented so far are indeed causal, then I should expect to see bigger changes during the monsoon season, and smaller changes outside of it. The pattern of coefficients confirms this hypothesis for perishables and processed food - the categories most likely to have been affected by the roads.

8 Alternative Hypotheses

One concern is that what are seemingly program effects might in reality be driven by other factors. One such potential explanation that comes to mind is employment in road construction: if the construction of roads themselves is generating local employment, then the observed outcomes might be short-lived. Further, the results might lose even their short-term generalizability in a setting where construction is managed without tapping the local labor market. I can test this using data on employment location: 2 of the survey rounds (rounds 61 and 66) query all employed individuals regarding the location of their workplace. The responses to this question enable me to ascertain whether an individual's primary place of work is rural or urban. If the mechanism behind the results so far is employment at the local road construction site, then I should not observe individuals commuting to an urban location for work. On the other hand, if the mechanism is increased access to urban areas, I should be able to observe this in individuals' employment location.[38] I present this analysis in Table 12. In program villages, there is an overall 13 percent increase in the number of people reporting their employment location as urban. For teenaged girls and prime-age men, the coefficients are very large (representing an almost 100 percent increase for men, and a 500 percent increase for girls) and significant. Teenaged boys also witnessed a nearly 100

[38] Any individuals in the survey are those that necessarily live in the rural household, and not emigrants as the survey collects information for only resident individuals.

percent increase in the proportion working in urban areas. Further, this increase is borderline significant. The findings for prime-age men suggest that even though we failed to detect any magnitude changes, being connected to the city brought about qualitative shifts in their employment. Additionally, the results from the analysis of occupations in table 5 also aid in ruling out this explanation. Table 5 shows that none of the gains in market participation are driven by increased employment at public construction sites.[39]

An alternative explanation is that my estimates could be picking up spurious effects from other contemporaneous welfare programs. This concern is especially acute in the case of NREGA, a large social insurance program that was contemporaneous with the latter half of PMGSY. Under NREGA, one member of every household was guaranteed 100 days of employment in local public works at a pre-determined wage.[40] Estimates from the Government of India suggest that NREGA generated 2.57 billion person-days of emplyment in 2010-11. Therefore, it is important to rule out that the purported PMGSY effects are not being driven by NREGA. In order to do so, I analyze changes in wage-employment. The NSS surveys query all employed individuals on whether they work for a wage. The indicator variable for "working for a wage" is my dependent variable. Results are presented in Table 13. The point estimate on roads built is statistically insignificant, and in fact, negative in case of women. This helps me establish that I am not attributing effects of NREGA to road construction. Further, the coefficients on road-fares, non-road-fares, and vehicles also support the hypothesis that the effects are due to treatment, and not because of other welfare programs.

Yet another potential explanation is that the observed outcomes might be driven by selective migration. However, the observed pattern of coefficients is unlikely to fit any sensible hypothesis about selective migration. For instance, for the observed results to conform with

[39]The occupation codes included in the category construction pertain to private construction sites. The bulk of this category corresponds to employment as casual labor at private individual homes.

[40]See http://nrega.nic.in/netnrega/home.aspx

greater out-migration, it would have to be true that the families that left were less likely to send their younger children to school, but more likely to send their older children to school. I try to further rule out selective migration by analyzing household size. If certain types of individuals or families are being induced by the program to leave the village, then we should be able to observe differential changes in household size in program districts. I present these results in Table 14 - there are no significant differences in household size in program districts.

9 Discussion and Conclusion

The results presented in this paper, especially the ones on consumption, technology adoption, price dispersion, and women's labor force participation underscore the great importance of investments in road construction. For instance, the technology adoption results alone have grave implications as governments in many developing countries provide large fertilizer subsidies to promote adoption. However, the increased probability of older children dropping out of school is both unexpected and unintended. Further, it has important policy implications. The labor literature documents significant returns to education. In this specific context, a Mincerian regression of wage on education pegs the return to education at 6.9 percent.[41] Therefore, dropping out of school at an earlier age might be reducing the lifetime earnings of these individuals.

On the other hand, it is debatable what the expexted returns to education are in rural India. Further, even if lifetime earnings were going down, there may not be any welfare losses for individuals with sufficiently high discount rates. Unfortunately, the available data does not allow me to isolate these parameters. However, we may still want to design public policy measures to mitigate this effect due to our normative preference for schooling. One

[41] Agrawal (2011) uses the 2004-05 India Human Development Survey and finds a similar Mincerian coefficient of 7.7 percent.

prescription might be to provide cash transfers conditional on school attendance.[42] Alternatively, there is potential for policy such that the expected premium to skill acquisition is greater than the short-run gains from market participation at a young age.[43]

Apart from the outcomes studied in this paper, roads can potentially impact many other economic variables. Access to credit markets, healthcare, service delivery, and changes to economic geography are some that come to mind. Research is needed on these before we fully understand the effects of infrastructure provision, especially the general equilibrium effects. Additionally, almost all of the current evidence is on short term impacts. The scant evidence on longer term impacts is provided by Banerjee et al. (2012), and Berger and Enflo (2013). However, this evidence on long-term impacts needs to be bolstered significantly as initial infrastructure placement can create a virtuous cycle of public and private capital investments. This makes causal effects hard to pin down. One alternative is to also attribute the subsequent developments to the initial shock, and argue that (rail)road placement moved the beneficiaries to a higher growth trajectory, in the same spirit as the literature on the long term effects of historic institutions (See, for instance, Acemoglu et al. (2001; 2002)). Viewed in this manner, the long-term consequences of infrastructure provision might be akin to those of inclusive institutions. However, more work is needed before anything conclusive can be said in this regard. Finally, another item that is open for further investigation in this research agenda pertains to the optimal level of investment in transportation infrastructure, as recent work from Shi (2013) suggests that the growth impact of infrastructure investments might follow an inverse U-shape.

[42] For instance, Progresa from Mexico has been very successful at promoting enrollment (Schultz, 2004)

[43] Policy-makers would also need to ensure that these gains are well-understood. For instance, Jensen (2010) provides evidence from the Dominican Republic showing that the perceived returns to education are much lower than actual.

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Tables and Figures

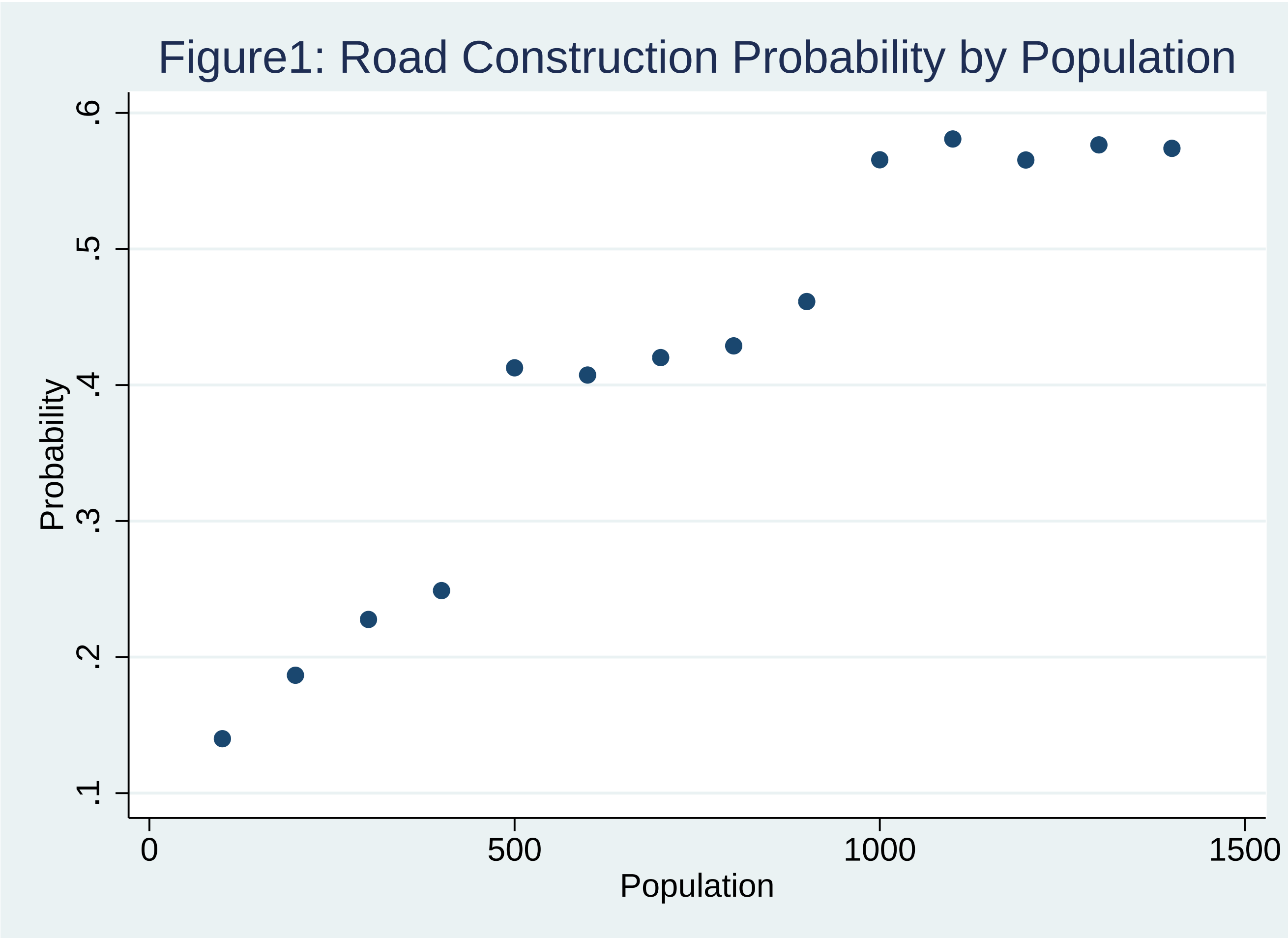
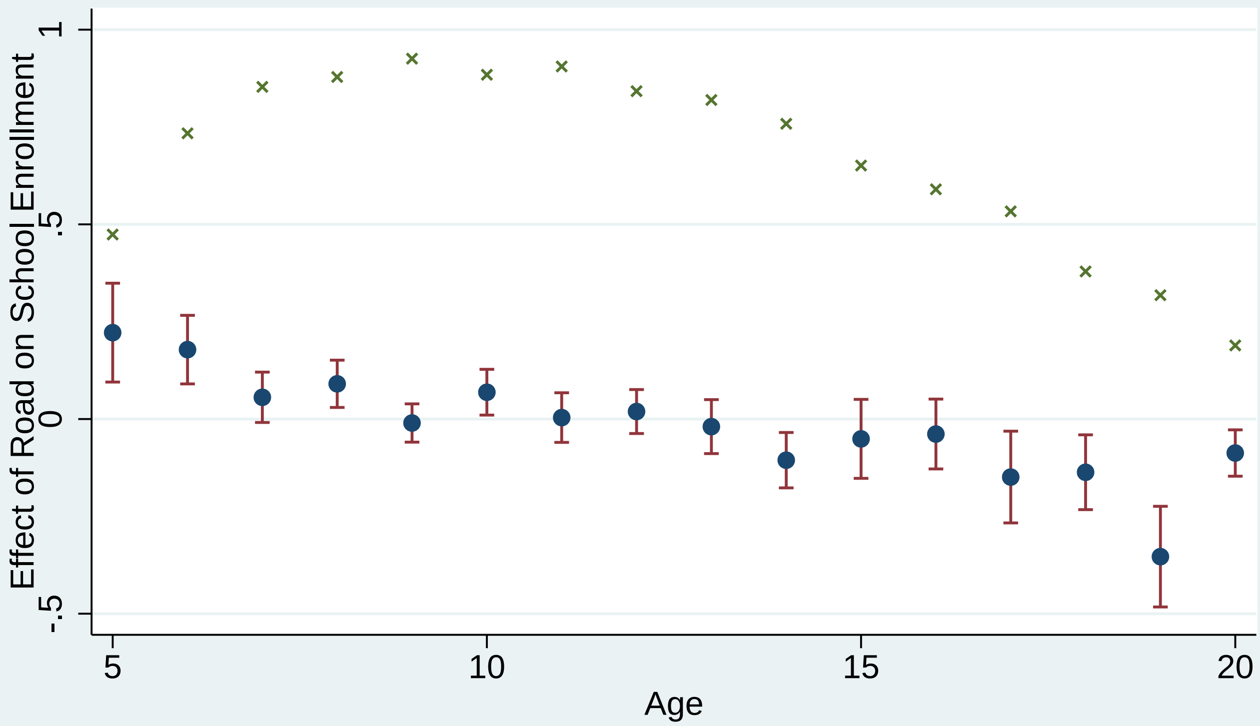


Figure 2: Effect of Road Construction on School Enrollment by Age



All estimates include district fixed effects.
Confidence intervals are at the 90% level.

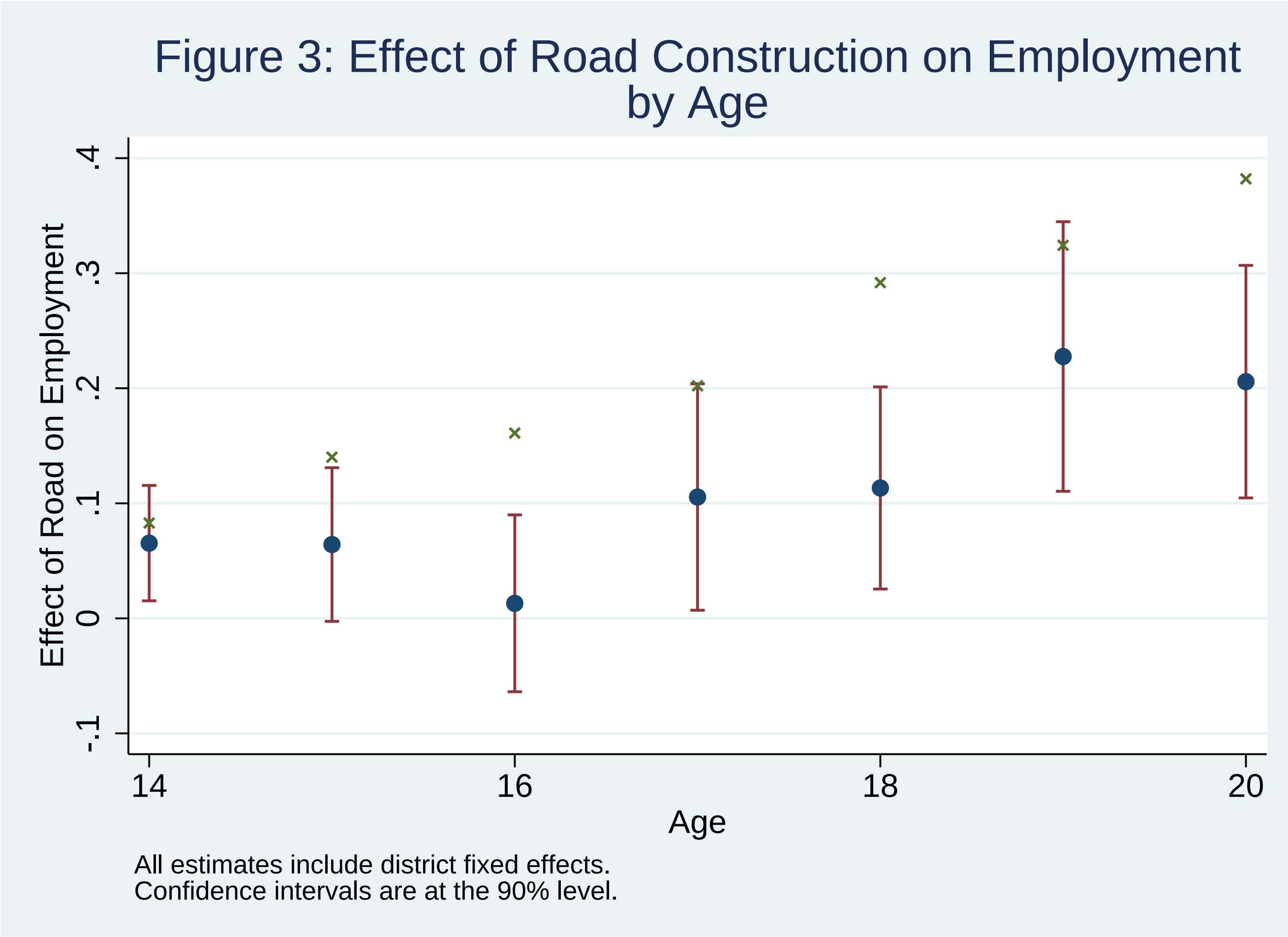


Figure 3: Effect of Road Construction on Employment by Age
Effect of Road on Employment
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Age
All estimates include district fixed effects.
Confidence intervals are at the 90% level.

Table 1: Likelihood of Road Construction by Endline

	Coefficient	Std. Error	Baseline Mean
500 > p > 1000	0.261***	(0.03)	
p > 1000	0.415***	(0.06)	
Population	0.595	(0.47)	625.70
SC Population	0.004	(0.01)	0.37
Distance from Town	-0.158	(0.17)	25.29
Panchayat HQ	0.080***	(0.02)	0.08
Primary School	0.036**	(0.01)	0.79
High School	-0.016	(0.01)	0.03
Adult Literacy Center	0.007	(0.01)	0.08
Primary Health Center	-0.013	(0.01)	0.03
Commercial Bank	-0.017	(0.01)	0.05
Post Office	-0.005	(0.01)	0.23
Telephone	-0.004	(0.01)	0.26
Power Supply	0.002	(0.01)	0.71
R-squared	0.216		

Standard errors in parentheses, clustered by state

***, **, * indicate significance at 1, 5 and 10%

Sample of 272,412 villages. Includes state fixed effects

Table 2. Impact of Road Construction on Price Dispersion

Panel A: Prices Reported by Households	By Item Type						
	Cereal	Lentils	Dairy	Meat	Vegetables	Fruit	Processed Food
Roads Built	-0.84*	0.18	-3.26	-2.17*	-0.73*	-6.05	17.12
	(0.43)	(0.39)	(2.18)	(1.28)	(0.42)	(10.16)	(23.74)
Observations	1,023,709	1,041,479	308,622	542,666	2,671,503	1,031,366	790,881
R-Squared	0.07	0.17	0.05	0.06	0.01	0.07	0.00
Mean of Dep. Var.	1.90	2.90	8.71	7.22	2.10	2.49	10.37
Std Dev of Dep. Var.	3.96	3.63	32.23	13.06	2.19	5.32	24.97

Panel B: Prices Reported by Agricultural Markets	By Item Type					
	Cereal	Lentils	Oilseeds	Animals	Vegetables	Fruit
Roads Built	66.29	-218.54	-1303.44	-1880.61	-67.05**	-179.03**
	(70.54)	(287.00)	(912.12)	(1563.98)	(33.21)	(84.36)
Observations	454,886	261,304	229,292	5,220	611,327	146,715
R-Squared	0.00	0.00	0.01	0.12	0.06	0.02
Mean of Dep. Var.	119.61	191.84	495.13	1399.27	104.94	379.75
Std Dev of Dep. Var.	4692.68	9460.33	14410.22	1553.33	241.79	3394.63

Standard errors in parentheses, clustered at the market level.

***, **, * indicate significance at 1, 5 and 10%

For Panel A, the dependent variable is the district-wide dispersion in the median price of each good in each category, as

For Panel B,the dependent variable is the district-wide dispersion in the mean price of each variety in each category, as reported by different markets every month

In Panel A, all specifications have time and district fixed effects, and household-level controls

In Panel B, all specifications have week, month, year, and state fixed effects

Mean of Roads Built for the period under consideration is 0.081 for panel A, and 0.056 for panel B

Table 3. Impact of Road Construction on School Enrollment

Panel A: Impact on 5-14 year-olds	Overall				Impacts by Gender					
					Girls			Boys		
	1	2	3	4	5	6	7	8	9	10
Roads Built	0.04	0.04	0.05*	0.05*	0.03	0.03	0.06*	0.05*	0.05*	0.05*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.031)	(0.031)	(0.031)	(0.027)	(0.027)	(0.029)
Pre-program Roads	0.06**	0.06**			0.05*	0.05*		0.06***	0.06***	
	(0.02)	(0.02)			(0.029)	(0.029)		(0.022)	(0.021)	
Observations	322,625	322,625	322,625	322,625	151,687	151,687	151,687	170,938	170,938	170,938
R-Squared	0.039	0.049	0.007	0.017	0.051	0.063	0.021	0.032	0.041	0.013
Baseline mean	0.8	0.8	0.8	0.8	0.77	0.77	0.77	0.83	0.83	0.83
Panel A: Impact on 14-20 year-olds										
Roads Built	-0.09**	-0.08*	-0.12***	-0.11**	-0.09*	-0.09*	-0.09*	-0.08*	-0.08*	-0.11**
	(0.04)	(0.04)	(0.04)	(0.04)	(0.046)	(0.044)	(0.048)	(0.045)	(0.044)	(0.044)
Pre-program Roads	0.00	0.00			0.01	0.00		0.00	0.00	
	(0.03)	(0.03)			(0.034)	(0.034)		(0.031)	(0.03)	
Observations	242,761	242,761	242,761	242,761	112,890	112,890	112,890	129,871	129,871	129,871
R-Squared	0.056	0.085	0.011	0.046	0.077	0.105	0.042	0.051	0.085	0.046
Baseline mean	0.46	0.53	0.46	0.46	0.37	0.37	0.37	0.53	0.53	0.53
Indiv Controls	N	Y	N	Y	N	Y	Y	N	Y	Y
District FE	N	N	Y	Y	N	N	Y	N	N	Y
State FE	Y	Y	N	N	Y	Y	N	Y	Y	N

Standard errors in parentheses, clustered at the district level

***, **, * indicate significance at 1, 5 and 10%

All specifications have time fixed effects

Mean of Roads Built: 0.052

The dependent variable is an indicator for whether the respondent reported that the child's primary occupation was going to school

Table 4. Impact of Road Construction on Employment

Panel A: Impact on 14-20 year-olds	Overall				Impacts by Gender					
					Girls			Boys		
	1	2	3	4	5	6	7	8	9	10
Roads Built	0.10***	0.09***	0.11***	0.10***	0.11***	0.10***	0.09***	0.10**	0.09**	0.12***
	(0.04)	(0.03)	(0.03)	(0.06)	(0.036)	(0.035)	(0.033)	(0.045)	(0.038)	(0.04)
Pre-program Roads	-0.01	0.13**			-0.02	-0.02		0.00	-0.02	
	(0.02)	(0.06)			(0.021)	(0.021)		(0.03)	(0.027)	
Constant					0.43***	-0.29**	-0.55***	0.48***	-0.06	-0.19
					(0.029)	(0.115)	(0.113)	(0.035)	(0.135)	(0.135)
Indiv Controls	N	Y	N	Y	N	Y	Y	N	Y	Y
District FE	N	N	Y	Y	N	N	Y	N	N	Y
State FE	Y	Y	N	N	Y	Y	N	Y	Y	N
Observations	216,225	216,225	216,225	216,225	103,993	103,993	103,993	112,232	112,232	112,232
R-Squared	0.057	0.22	0.006	0.172	0.092	0.16	0.078	0.058	0.244	0.206
Baseline mean	0.24	0.24	0.24	0.24	0.15	0.15	0.15	0.31	0.31	0.31

Panel B: Impact on Prime-Age Individuals	Impacts by Gender							
	Women				Men			
	1	2	3	4	5	6	7	8
Roads Built	0.12**	0.11**	0.09*	0.09	0.01	0.01	0.02	0.01
	(0.054)	(0.054)	(0.06)	(0.057)	(0.01)	(0.009)	(0.01)	(0.011)
Pre-program Roads	-0.12***	-0.12***			-0.02***	-0.02***		
	(0.038)	(0.037)			(0.006)	(0.006)		
Indiv Controls	N	Y	N	Y	N	Y	N	Y
District FE	N	N	Y	Y	N	N	Y	Y
State FE	Y	Y	N	N	Y	Y	N	N
Observations	187,597	187,597	187,597	187,597	186,412	186,412	186,412	186,412
R-Squared	0.137	0.222	0.004	0.103	0.014	0.049	0.001	0.036
Baseline mean	0.41	0.41	0.41	0.41	0.96	0.96	0.95	0.96

Standard errors in parentheses, clustered at the district level

***, **, * indicate significance at 1, 5 and 10%

All specifications have time fixed effects

Mean of Roads Built: 0.052

Table 5. Impact of Road Construction on Occupation Choice

Panel A: Teenaged Boys	Agriculture	Animal Rearing	Forestry	Textile Manufacturing	Tailoring	Manufacturing	Construction	Retail	Domestic Help
Roads Built	0.00	0.01*	0.00	0.01	0.01**	0.01	0.05**	0.01	0.00
	(0.035)	(0.008)	(0.002)	(0.006)	(0.003)	(0.009)	(0.021)	(0.011)	(0.010)
Observations	129,809	129,809	129,809	129,809	129,809	129,809	129,809	129,809	129,809
R-Squared	0.14	0.01	0.00	0.00	0.00	0.01	0.05	0.02	0.00
Mean of Dep. Var.	0.32	0.02	0.00	0.01	0.00	0.02	0.03	0.03	0.00
Panel B: Teenaged Girls									
Roads Built	0.01	0.08***	-0.01*	0.01**	0.01***	0.00	0.00	0.00	0.00
	(0.034)	(0.020)	(0.004)	(0.007)	(0.004)	(0.006)	(0.006)	(0.004)	(0.004)
Observations	112,858	112,858	112,858	112,858	112,858	112,858	112,858	112,858	112,858
R-Squared	0.09	0.01	0.00	0.01	0.00	0.01	0.01	0.00	0.00
Mean of Dep. Var.	0.21	0.04	0.00	0.01	0.01	0.01	0.01	0.00	0.00
Panel C: Prime-Aged Women									
Roads Built	-0.03	0.10***	-0.01*	0.01**	0.00	-0.01	0.00	0.00	0.00
	(0.048)	(0.031)	(0.003)	(0.007)	(0.004)	(0.012)	(0.019)	(0.007)	(0.003)
Observations	218,584	218,584	218,584	218,584	218,584	218,584	218,584	218,584	218,584
R-Squared	0.07	0.01	0.00	0.00	0.01	0.01	0.01	0.01	0.00
Mean of Dep. Var.	0.44	0.08	0.00	0.01	0.01	0.01	0.01	0.01	0.00

Standard errors in parentheses, clustered at the district level

***, **, * indicate significance at 1, 5 and 10%

All specifications have time fixed effects

Mean of Roads Built: 0.052

Table 6. Impact of Road Construction on Technology Adoption in Agriculture, 2001-2007

	All Crops	Cash Crops	Food Crops
Area under Fertilizer			
Post-period Dummy	-43.17	1,583.61***	-678.47
	(391.82)	(605.78)	(555.69)
Post * Roads Built	10,266.18***	-2,162.47	17,944.83***
	(2524.90)	(2467.75)	(3990.16)
Baseline Mean	22,281.36	7,901.07	13,936.63
Baseline Std. Dev	76,771.20	32,036.28	44,764.57
Area under Hybrid Seeds			
Post-period Dummy	692.33	1,572.23***	282.12
	(463.56)	(581.45)	(675.55)
Post * Roads Built	6,056.85**	-2,709.28	13,067.63***
	(2372.22)	(2266.51)	(3740.42)
Baseline Mean	20,187.12	6,670.74	12,905.33
Baseline Std. Dev	76,794.03	27,471.54	46,340.40
N	19,087	6,666	12,421

Clustered standard errors in parentheses. ***, **, * indicate significance at 1, 5 and 10%
Includes district fixed effects and district-level covariates
Mean of roads built over the analysis period is 0.068

Table 7. Impact of Road Construction on Consumption Basket

Panel A: Baseline & Endline Only	Impacts by Item Type											
	Food							Non-Food				
	Cereal	Lentils	Dairy	Meat	Vegetables	Fruit	Processed Food	Contraceptives	Minor Manufactures	Road Fares	Non-road Fares	Vehicles
Post Dummy	0.60***	0.52***	0.05***	0.06***	0.75***	0.06*	0.44***	0.02***	0.30***	0.17***	0.01***	0.06***
	(0.04)	(0.04)	(0.01)	(0.02)	(0.11)	(0.03)	(0.05)	(0.01)	(0.03)	(0.02)	(0.00)	(0.01)
Post * Roads Built	-0.58***	-0.36**	0.14**	0.10	0.03	0.27	0.26	0.18***	0.43***	0.28***	0.01	0.20***
	(0.17)	(0.18)	(0.06)	(0.11)	(0.45)	(0.17)	(0.26)	(0.05)	(0.14)	(0.10)	(0.02)	(0.05)
Observations	93,266	93,266	93,266	93,266	93,266	93,266	93,266	93,266	93,266	93,266	93,266	93,266
R-Squared	0.09	0.08	0.04	0.04	0.07	0.05	0.07	0.02	0.06	0.04	0.00	0.05
Mean of Dep. Var.	2.82	2.77	0.89	1.47	10.01	1.66	2.07	0.07	1.30	0.81	0.04	0.36
Std Dev of Dep. Var.	1.33	1.59	0.71	1.29	3.48	1.31	1.55	0.26	0.91	0.64	0.19	0.48
Panel B: Entire Sample Period												
Roads Built	-0.36***	-0.35**	0.10**	0.04	-0.22	0.40***	0.37*	0.17***	0.36***	0.29***	0.02	0.18***
	(0.13)	(0.15)	(0.05)	(0.08)	(0.34)	(0.14)	(0.22)	(0.04)	(0.13)	(0.08)	(0.02)	(0.05)
Observations	269,572	269,572	269,572	269,572	269,572	269,572	269,572	269,572	269,572	269,572	269,572	269,572
R-Squared	0.07	0.08	0.04	0.04	0.08	0.07	0.11	0.02	0.05	0.03	0.00	0.05
Mean of Dep. Var.	2.82	2.77	0.89	1.47	10.01	1.66	2.07	0.07	1.30	0.81	0.04	0.36
Std Dev of Dep. Var.	1.33	1.59	0.71	1.29	3.48	1.31	1.55	0.26	0.91	0.64	0.19	0.48

Notes:

Standard errors in parentheses, clustered at the district level.

***, **, * indicate significance at 1, 5 and 10%

All specifications have time and district fixed effects, and household-level controls

Mean of % Connected is 0.154 for Panel A, and 0.081 for Panel B

The dependent variable is the number of surveyed goods in each category that are consumed by the household

Table 8. Impact of Road Construction on Quantities Consumed

Panel A: Baseline & Endline Only	Impacts by Item Type						
	Food						
	Cereal	Lentils	Dairy	Meat	Vegetables	Fruit	Processed Food
Post Dummy	14.24***	15.88***	11.35***	10.10***	14.91***	7.04***	11.02***
	(0.30)	(0.49)	(0.32)	(0.43)	(0.28)	(0.41)	(0.57)
Post * Roads Built	4.56***	5.66***	-3.99***	5.47***	-4.69***	4.07**	0.08
	(1.39)	(2.18)	(1.46)	(2.03)	(1.13)	(1.79)	(2.30)
Observations	94,551	94,551	94,551	94,551	94,551	94,551	94,551
R-Squared	0.10	0.22	0.18	0.10	0.50	0.04	0.01
Mean of Dep. Var.	0.30	0.56	0.31	0.58	0.40	0.90	0.68
Std Dev of Dep. Var.	0.96	1.07	1.02	1.14	0.70	1.37	1.25
Panel B: Entire Sample Period							
Roads Built	3.81***	2.92	-20.17	3.65**	-3.67***	0.90	-3.35
	(1.12)	(1.96)	(17.07)	(1.69)	(1.30)	(5.92)	(18.68)
Observations	269,572	269,572	269,572	269,572	269,572	269,572	269,572
R-Squared	0.16	0.10	0.00	0.05	0.06	0.01	0.00
Mean of Dep. Var.	0.30	0.56	0.31	0.58	0.40	0.90	0.68
Std Dev of Dep. Var.	0.96	1.07	1.02	1.14	0.70	1.37	1.25

Notes:

Standard errors in parentheses, clustered at the district level.

***, **, * indicate significance at 1, 5 and 10%

All specifications have time and district fixed effects, and household-level controls

Mean of % Connected is 0.154 for Panel A, and 0.081 for Panel B

The dependent variable is the weighted mean of the z-score of quantity consumed

The weights are given by the share of each commodity in the median household's budget in each district in the baseline year

Table 9: Placebo Test - Program Roads on 1993-1999 Outcomes

	Enrollment		Employment		
	5-14	14-20	14-20	Adult Men	Adult Women
Post Dummy	0.03***	0.03**	-0.05***	-0.01	-0.02
	(0.01)	(0.01)	(0.01)	(0.00)	(0.02)
Post * Roads Built	-0.04	-0.07	0.06	0	0.08
	(0.05)	(0.06)	(0.07)	(0.02)	(0.07)
Observations	145,440	88,325	46,213	74,607	75,373
R-Squared	0.01	0.00	0.01	0.01	0.02
Baseline mean	0.69	0.36	0.39	0.95	0.47

Notes:

Standard errors in parentheses, clustered at the district level.

***, **, * indicate significance at 1, 5 and 10%

All specifications have time and district fixed effects, and household-level controls

Table 10: Placebo Test - Program Roads on 1993-1999 Outcomes

	Impacts by Item Type											
	Food							Non-Food				
	Cereal	Lentils	Dairy	Meat	Vegetables	Fruit	Processed Food	Contraception	Minor Manufactures	Road Fares	Non-road Fares	Vehicles
Post Dummy	0.47***	0.23***	0.05***	-0.59***	1.15***	0.30***	0.26***	0.00	0.04	-0.01	0.01**	0.04***
	(0.04)	(0.04)	(0.01)	(0.03)	(0.10)	(0.03)	(0.03)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)
Post * Roads Built	-0.12	-0.07	0.05	0.14	1.40***	0.10	-0.17	-0.04	-0.21	0.02	-0.02	-0.02
	(0.18)	(0.16)	(0.05)	(0.15)	(0.43)	(0.14)	(0.12)	(0.04)	(0.15)	(0.08)	(0.01)	(0.04)
Observations	95,134	95,134	95,134	95,134	95,134	95,134	95,134	95,134	95,134	95,134	95,134	95,134
R-Squared	0.04	0.02	0.04	0.06	0.07	0.04	0.03	0.00	0.01	0.01	0.00	0.00
Mean of Dep. Var.	2.43	2.51	0.81	1.95	8.45	1.28	0.65	0.08	1.28	0.78	0.03	0.34

Notes:

Standard errors in parentheses, clustered at the district level.

***, **, * indicate significance at 1, 5 and 10%

All specifications have time and district fixed effects, and household-level controls

The dependent variable is the number of surveyed goods in each category that are consumed by the household

Table 11: Placebo Test - Effect of Roads Built in Future on Current Outcomes

	Enrollment		Employment		
	5-14	14-20	14-20	Adult Men	Adult Women
Roads Built by:					
t	-0.04	-0.17	0.29***	-0.02	0.21
	(0.12)	(0.14)	(0.11)	(0.04)	(0.21)
t+1	0.07	0.01	-0.06	-0.01	-0.06
	(0.14)	(0.12)	(0.09)	(0.05)	(0.15)
t+2	-0.11	-0.02	-0.15	0.05	0.1
	(0.10)	(0.12)	(0.10)	(0.05)	(0.15)
Observations	130,713	100,420	45,838	75,796	72,120
R-Squared	0.013	0.032	0.335	0.053	0.076
Baseline mean	0.8	0.46	0.24	0.95	0.41
Joint p-value	0.55	0.99	0.24	0.53	0.78

Notes:

Standard errors in parentheses, clustered at the district level.

***, **, * indicate significance at 1, 5 and 10%

All specifications have time and district fixed effects, and household-level controls

Table 12: Impact of Road Construction on Employment Location (location dummy = 1 for urban, 0 for rural)

	Overall	Impacts by Group			
		14-20 Boys	14-20 Girls	Prime-Age Men	Prime-Age Women
Post Dummy	0.02***	0.03*	0.00	0.03***	0.02*
	(0.007)	(0.014)	(0.021)	(0.008)	(0.008)
Post * Roads Built	0.13***	0.12	0.36**	0.14***	0.01
	(0.038)	(0.080)	(0.179)	(0.045)	(0.062)
Observations	134,860	9,787	3,106	50,853	13,271
R-Squared	0.01	0.01	0.02	0.01	0.00
Mean of Dep. Var.	0.13	0.16	0.07	0.16	0.06

Standard errors in parentheses, clustered at the district level.

***, **, * indicate significance at 1, 5 and 10%

All specifications have time and district fixed effects, and household-level controls

Table 13. Impact of Road Construction on Wage Employment

	Impacts by Gender							
	Women				Men			
	1	2	3	4	5	6	7	8
Roads Built	-0.08	-0.07	-0.03	-0.04	0.01	0.00	0.01	0.00
	(0.09)	(0.08)	(0.07)	(0.06)	(0.01)	(0.03)	(0.03)	(0.03)
Pre-program Roads	0.12**	0.13**			0.07***	0.06***		
	(0.05)	(0.06)			(0.02)	(0.02)		
Indiv Controls	N	Y	N	Y	N	Y	N	Y
District FE	N	N	Y	Y	N	N	Y	Y
State FE	Y	Y	N	N	Y	Y	N	N
Observations	162,840	162,840	162,956	162,840	434,642	434,642	435,045	434,642
R-Squared	0.18	0.24	0.00	0.04	0.14	0.17	0.01	0.13
Baseline mean	0.50	0.50	0.51	0.51	0.50	0.50	0.45	0.45

Standard errors in parentheses, clustered at the district level

***, **, * indicate significance at 1, 5 and 10%

All specifications have time fixed effects

Mean of Roads Built: 0.052

Table 14. Impact of Program Intensity on Household Size

	Size of Household
Roads Built	-0.08
	(0.115)
Observations	269,572
R-Squared	0.01
Mean of Dep. Var.	4.83
Std Dev of Dep. Var.	2.48

Standard errors in parentheses, clustered at the district level.

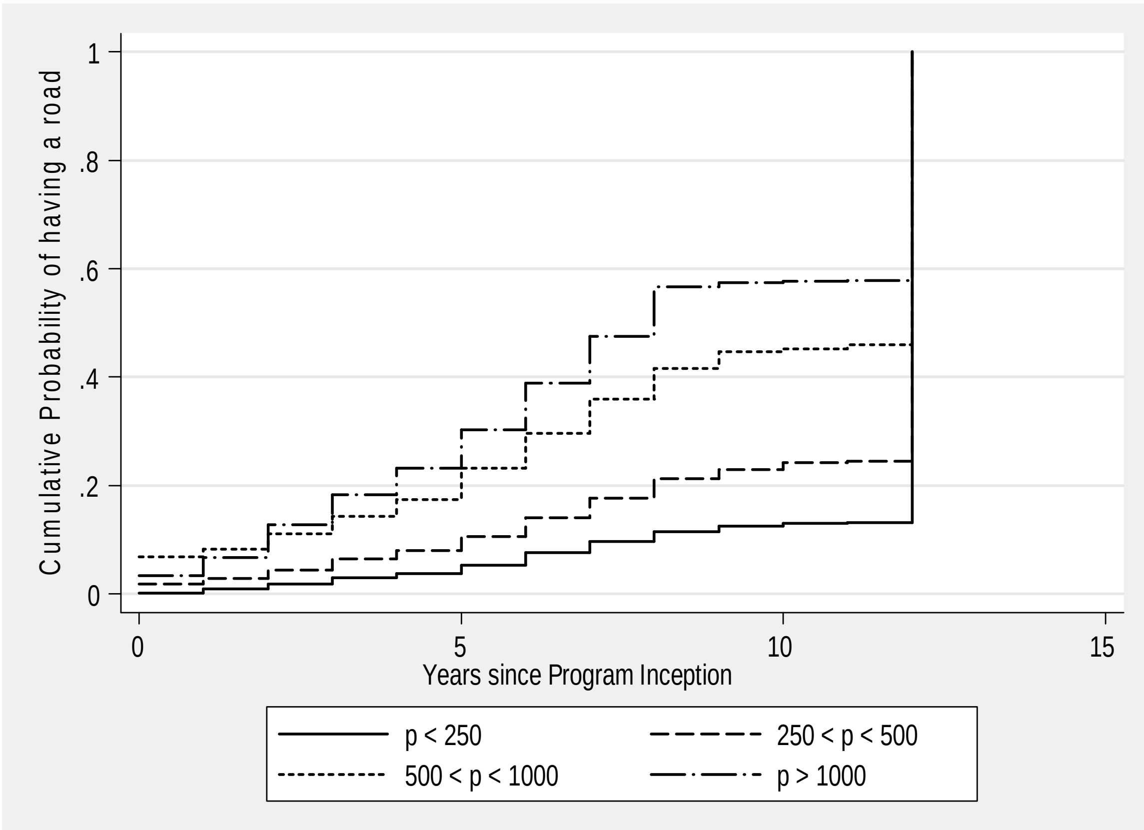
***, **, * indicate significance at 1, 5 and 10%

All specifications have time and district fixed effects, and household-level controls

Mean of Roads Built: 0.081

Appendix

A.1 CDF of Connectivity



- 12 years since program inception corresponds to being unconnected at the time this data was collected.
- This graph is based on all-India data, and does not account for state-wise differences in program implementation.

A.2 Village-level Observables at Baseline

Table A1: Summary Statistics of Connected & Unconnected Villages

Observables	Means: Connected	Means: Unconnected	p value Connected = Unconnected
Total Population	929.20	625.70	0.01***
	(2345.04)	(864.47)	
SC Population	0.32	0.37	0.01***
	(3.69)	(0.81)	
Panchayat HQ	0.19	0.08	0.01***
	(0.39)	(0.27)	
Primary School	0.88	0.79	0.01***
	(0.32)	(0.41)	
High School	0.07	0.03	0.01***
	(0.26)	(0.17)	
Adult Literacy Center	0.16	0.08	0.01***
	(0.36)	(0.28)	
Primary Health Center	0.09	0.03	0.01***
	(0.29)	(0.18)	
Maternal & Child Welfare Center	0.11	0.06	0.01***
	(0.31)	(0.24)	
Commercial Bank	0.13	0.05	0.01***
	(0.33)	(0.22)	
Post Office	0.52	0.23	0.01***
	(0.71)	(0.49)	
Telegraph	0.05	0.01	0.01***
	(0.24)	(0.11)	
Telephone	0.53	0.26	0.01***
	(0.50)	(0.44)	
Power Supply	0.90	0.71	0.01***
	(0.30)	(0.46)	
Distance from Town	20.78	25.29	0.01***
	(21.45)	(27.31)	
Observations	477,917	280,210	

Standard deviations in parentheses.

A.3 First Stage Effects of Population Category on Percent Connected

Table A1: First Stage of Instrumenting Connectivity on Eligibility

	% Connected during program
Population	
250 - 500	0.26
	(0.161)
500 - 1000	0.31***
	(0.063)
> 1000	0.26***
	(0.054)
Pre-Program Connectivity	-0.04
	(0.06)
Observations	241,565
R-Squared	0.06
F-stat	11.56
P-Value	0

Standard errors in parentheses, clustered at state level
***, **, * indicate significance at 1, 5 and 10%
Includes state fixed effects.

A.4 IV Estimates

Table A2. Impact of Program Intensity on Consumption Basket (IV)

	Impacts by Item Type											
	Food							Non-Food				
	Cereal	Lentils	Dairy	Meat	Vegetables	Fruit	Processed Food	Awareness Goods	Minor Manufactures	Road Fares	Non-road Fares	Vehicles
% Connected by Program	-0.04	0.25	0.24*	0.24	0.31*	0.18*	0.33*	0.18	0.06	0.52***	0.03	0.78**
	(0.08)	(0.23)	(0.13)	(0.20)	(0.19)	(0.10)	(0.19)	(0.15)	(0.13)	(0.19)	(0.05)	(0.31)
Pre-Program Connectivity	-0.02	0.06	0.08***	0.04	0.07*	0.05***	0.07*	0.01	0.01	0.13***	-0.01	0.16**
	(0.02)	(0.05)	(0.03)	(0.04)	(0.04)	(0.02)	(0.04)	(0.03)	(0.03)	(0.03)	(0.01)	(0.06)
R-Squared	0.30	0.32	0.22	0.47	0.17	0.14	0.12	0.10	0.13	0.11	0.04	0.18
Mean of Dep. Var.	0.13	0.21	0.15	0.21	0.33	0.10	0.14	0.04	0.09	0.20	0.01	0.18
Std Dev of Dep. Var.	0.06	0.12	0.12	0.18	0.12	0.08	0.10	0.13	0.06	0.16	0.06	0.24

Standard errors in parentheses, clustered at the district level.

***, **, * indicate significance at 1, 5 and 10%

All specifications have time and state fixed effects, and district-level controls

Sample contains 265,382 households

Mean of % Connected Post-Program: 0.081

A.5 Consumption during Monsoon

Table A3. Impact on Consumption Basket during Monsoon

	Cereal	Lentils	Dairy	Meat	Vegetables	Fruit	Processed Food
Monsoon Dummy	0.00	0.00	0.00	-0.01***	-0.01***	-0.01***	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Roads Built * Monsoon Dummy	0.01	0.01	0.01*	0.04***	0.02*	0.04***	0.02*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Roads Built	-0.01	-0.03**	0.02*	-0.01	-0.02	0.02**	0.03*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Observations	232,772	232,772	232,772	232,772	232,772	232,772	232,772
R-Squared	0.07	0.07	0.04	0.04	0.07	0.07	0.10
Mean of Dep. Var.	0.14	0.22	0.15	0.20	0.34	0.10	0.14

Standard errors in parentheses, clustered at the district level.

***, **, * indicate significance at 1, 5 and 10%

All specifications have time and district fixed effects, and household-level controls

Mean of % Connected Post-Program: 0.081

The Bargaining Power of Missing Women: Evidence from a Sanitation Campaign in India

Yaniv Stopnitzky*

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Abstract

Female bargaining power in rural Haryana, as in much of northern India, is constrained by widespread discrimination against women. In recent years, however, women successfully demand private sanitation facilities from potential husbands as a precondition for marriage. I study this manifestation of bargaining power by modeling latrine adoption as an investment that males can make to improve their desirability on the marriage market, and I show that increasing proportions of females with strong sanitation preferences drive male investment in toilets. Moreover, I demonstrate women's ability to secure latrines increases when they are relatively scarce in a marriage market. I test these predictions empirically by studying a sanitation program in Haryana, India, known colloquially as "No Toilet, No Bride". Using a triple difference empirical strategy based on households with and without marriageable boys, in Haryana and comparison states, before and after program exposure, I provide evidence that male investment in sanitation increased by 15% due to the program. Further, the program effect is four times larger in marriage markets where women are scarce (26%) as compared to marriage markets where women are abundant (6%). These results suggest the relative scarcity of women in Haryana has, conditional on women surviving to marriageable age, improved the ability of the remaining women to secure valuable goods.

JEL Classification: D1, J12, O12, O13

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1 Introduction

Women in rural Haryana suffer from discrimination that is widespread and strong enough to generate the most skewed state-level sex ratio in all of India.[1] Due to parental preferences for sons over daughters, parents provide differential post-natal care to boys and girls (Das Gupta (1987)), invest preferentially in male fetuses (Bharadwaj & Nelson (2010)), and/or selectively abort female fetuses (Arnold *et al.* (2002), Qian (2008)). Each of these factors exacerbates the sex imbalance. Further, if women survive to adulthood, they face numerous gender-specific constraints on their ability to travel or work outside of the household (Eswaran *et al.* (2009)).

In this social context of discrimination, females in rural Haryana have in recent years demanded from men and obtained a particularly valuable good—toilets—as a precondition for marriage. Women value toilets to a greater extent than males because they suffer disproportionately from male staring and harassment when they defecate, urinate, or attend to menstrual hygiene in public places. For this reason, private latrines generate benefits that are disproportionately enjoyed by females. The change that has allowed women to successfully demand latrines in marriage negotiations is associated with an unusual sanitation campaign commonly known as "No Toilet, No Bride", which Haryana state authorities initiated in 2005. The campaign encouraged families of marriage-age girls to demand that potential suitors' families construct a latrine prior to marriage. Mass media messaging via billboards, posters, and radio advertisements emphasized phrases such as "no toilet, no bride" and "no loo, no I do". These messages were framed by women's concerns about privacy and dignity when they defecate in the open, a behavior that is routine among roughly 70% of rural households in Haryana in 2004. Although the rationale for public investment in sanitation programs is the reduction of fecal pollution and the morbidity associated with widespread open defecation, the emphasis of "No Toilet, No Bride", combined with the fact that private benefits accrue largely to women, provides a unique opportunity to study female bargaining power under widespread discimination.

The "No Toilet, No Bride" program serves as a source of exogenous variation that alters the distribution of female preferences for sanitation. Because women move into the house of their husbands or their husbands' families at the time of marriage, the program's focus on women's preferences indirectly targets male behavior. I study the male response to the program by modeling latrine adoption in a transferable utility model of the marriage market. In the model, men can choose to invest in sanitation in order to raise their returns from marriage, which also depend on marriage market conditions. The model generates two key

[1] Source: Indian Census, 2011. Note that two non-state union territories, Chandigarh and Delhi, both adjacent to Haryana, have slightly worse sex ratios.

empirical predictions. First, I show that increasing the proportion of women with strong preferences for sanitation will cause men to increase their investments in latrines. Second, I extend the model to focus on the role of sex ratios, and I demonstrate the marriage market return to male investment increases to a greater extent when females are relatively scarce as compared to when females are abundant. Finally, I analyze the potential role of dowries in altering these empirical predictions, and I establish that the model's predictions on male investment will remain unchanged because dowry is determined in equilibrium as one component of the marital surplus.

I test these predictions using two rounds of the District-Level Household and Facility Survey (2004, 2008/9), a nationally representative, household data set. I employ an empirical strategy based on the intuition that the "No Toilet, No Bride" campaign exerts disproportionate pressure to adopt a latrine on those households with boys active on the marriage market. If the program was successful in linking sanitation with the marriage market, then households with boys of marriageable age face exogenous pressure to build a latrine, and they should therefore have a higher probability of latrine ownership after exposure to the program. Because such households could differ from households without marriageable age boys in a variety of unobserved ways, my econometric specification controls explicitly for these unobserved characteristics. I implement a difference-in-difference-in-difference approach that compares latrine ownership in households with and without boys of marriageable age, in Haryana and comparison states from northern India, before the program started and three to four years after the program began.

I find an increase of 4.3 percentage points (a 15% increase from a base of 28%) in the latrine ownership differential between households with and without marriage-age boys in Haryana over the period 2004 to 2008 relative to the difference between latrine ownership households with and without marriageable boys in comparison states. In addition, I provide strong, complementary evidence that latrine adoption is driven by whether households have marriageable boys active in a highly competitive marriage market, i.e. one with an undersupply of women due to highly skewed sex ratios. Specifically, I find that Haryana's observed latrine adoption due to "No Toilet, No Bride" is driven largely by marriage markets with a scarcity of women; in these markets I estimate a program effect of 26% over baseline. In marriage markets without this scarcity, however, the "No Toilet, No Bride" treatment effect is one-fourth as large.

These results are robust to competing hypotheses that the program caused changes in male preferences or increased latrine ownership by changing female preferences outside of marriage market channels. Evidence against the former hypothesis comes from a test of program effects among households with boys slightly older than marriage age, and who were thus too old to be affected by the program when they were active on the marriage market. Similarly, I present evidence against the latter alternative hypothesis by analyzing the ability of strictly single marriage-age girls to demand and obtain latrines. Thus, the program appears to operate specifically through the channel of marriage market bargaining.

Finally, I seek evidence on shifts in the intrahousehold sharing rule due to the "No Toilet, No Bride" program. Using a range of household assets and marriage-related variables that women value, I find no evidence the program has caused women to substitute toilets for other goods they value. On the contrary, women's relative position appears to have improved both in terms of age at marriage and the educational quality of their male spouses, while males appear to have been compensated in the form of motorbikes, perhaps in compensation for their sanitation expenditures. I am unable to determine empirically the intrahousehold allocation due to unobservables, but these results provide suggestive evidence that although "No Toilet, No Bride" has altered marital negotiations in various ways, women's status has not worsened along multiple observable dimensions.

Consistent with theoretical predictions from a transferable utility marriage market model with endogenous investment in quality, these findings suggest that (i) the "No Toilet, No Bride" campaign has significantly increased latrine ownership by linking marriage matching to the acquisition of a good that females particularly value, and (ii) biased sex ratios have increased the relative bargaining power of women on the marriage market, thereby improving their ability to demand goods. Thus, in an area with one of the most severely skewed sex ratios on earth, a local scarcity of women appears to have increased women's bargaining power, allowing them to obtain additional goods that they value.

This paper is organized as follows. Section 2 provides a social and economic background to marriage markets in northern India and Haryana, specifically where the "No Toilet, No Bride" program operates. Section 3 presents a model of marriage matching with endogenous investments in quality; a fuller treatment can be found in the theoretical appendix. Section 4 discusses sanitation in rural India and important features of the "No Toilet, No Bride" program. Section 5 explains the empirical strategy, identification issues, and data. Section 6 contains the key empirical results. Robustness to competing hypotheses is discussed in Section 6.3, and Section 7 discusses additional evidence on female bargaining and the intrahousehold allocation. Section 8 concludes.

2 Marriage Markets in Northern India

Marriage markets in northern India are fundamentally shaped by social norms around patrilocality and caste endogamy. Moreover, the marriage negotiation process is structured by the phenomena of arranged marriage and dowry. Marriages are typically arranged by the parents of both families, often with the help of an intermediary matchmaker, who helps identify suitors according to the criteria established by the families. The most important dimensions along which potential spouses are valued include caste, religion, kinship, profession, education, and physical attractiveness; the attractiveness of women is a characteristic particularly important for men (Banerjee *et al.* (2009)). Together, these interlocking institutions play a primary role in shaping the opportunity sets faced by marriage-age individuals and in determining marital outcomes. In this section I provide an overview of these social practices and highlight those characteristics significant for the theory and empirics of this paper.

2.1 Patrilocal Exogamy

The first important aspect of marriage in northern India, of which Haryana is a part, is the practice of patrilocal/virilocal exogamy, i.e. the migration of newlywed brides out of their households and into the residence of their husbands' family located outside of the brides' home village (Gould (1961)). For example, data from the 1994 PGIRCS survey in the states of Uttar Pradesh and Karnataka suggest that 90% of imported brides originated from villages located within 67 kilometers of the sample villages (Bloch *et al.* (2004)). In their study of how village exogamy serves as a form of insurance against spatially correlated risks, Rosenzweig & Stark (1989) note that the average distance across which two rural Indian households linked through marriage was approximately 30 kilometers. These empirical findings are broadly consistent with other qualitative evidence such as Dutt *et al.* (1981), which details marriage-generated links for two Punjabi villages and finds that 80% of households had a marriage distance of 40 kilometers. Thus, although households practice strict village exogamy (and appear to seek villages whose incomes do not covary strongly with the home village), households are typically searching for partners within a geographically defined area. This descriptive fact is important for the purposes of this paper because later I adopt an empirical definition of marriage markets in reference to a household's home district.[2]

2.2 Caste Endogamy

A second, crucial feature of Indian marriage matching is caste endogamy, i.e. the practice of marrying a spouse from within one's own caste. For example, Banerjee *et al.* (2009) cite an opinion poll in which 74% of respondents from West Bengal define themselves as opposed to inter-caste marriage, and the authors note the practice of caste endogamy is so widespread that matrimonial classified advertisements, which are common in Indian newspapers, often group listings by caste. In addition, these authors present evidence that individuals are will-

[2]The full definition I use is the intersection of caste, religion, marriage-age cohort, and district.

ing to trade substantial benefits in terms of spousal beauty, education, and/or wealth in order to marry within-caste. Later in the empirical section of this paper, I use such widespread and strong preferences for caste endogamy, coupled with the pervasiveness of patrilocal exogamy described above, to justify my empirical definition of a marriage market.

2.3 Sex Ratios in Contemporary India

In India, particularly in the northern states, the phenomenon of "missing women", i.e. women absent from the population due to skewed sex ratios, has a long history. For example, under British rule in the 19th century, census officials documented low ratios of women to men in northern India and British officials suspected the Rajputs, a large northern clan, of female infanticide (Chakraborty & Kim (2008)). Whereas in the past much of the observed sex imbalance was explained by such infanticide and/or differential neglect of girls (Das Gupta (1987)), the spread of ultrasound, amniocentesis, and doctor-provided abortion technology in recent decades has driven sex ratios among younger cohorts.[3] Estimates using data from the National Family Health Survey (NFHS-2, 1998/9) indicate that more than 100,000 sex-selective abortions of female fetuses were being performed each year in India, many of them by private providers in contravention of (unenforced) government regulations (Arnold *et al.* (2002)).

The underlying driver of both differential neglect and selective abortion is a strong parental preference for sons. Parents prefer boys over girls for each birth order, but this effect increases dramatically for higher birth order children. In data from the District Level and Household Survey (DLHS 2008/9), 15% of married female respondents *without* children report wanting a boy but only 3% desire a girl, conditional on wanting another child. For birth orders higher than four, nearly 10 times more mothers state a preference for another son as compared to another girl (65% and 6%, respectively). Moreover, these patterns are dominated by preferences among households in northern Indian states.[4]

The consequence of these widespread preferences, coupled with abortion technology and differential neglect/care in the intrahousehold allocation, is a dearth of women relative to men as compared to sex ratios assumed to be natural in countries without discrimination. In this broad regional context, the Punjab region stands out as having the most imbalanced sex ratios. According to the newly released 2011 Census of India, the overall ratio in India is 940

[3]Induced abortion has been legal in India since the Medical Termination of Pregnancy Act (1971) but only under specific conditions that exclude preferences over the child's sex or overall family gender composition.

[4]The largest discrepancies between stated preferences for sons and daughters were found in Bihar, Chattisgarh, Gujarat, Haryana, Jharkhand, Madhya Pradesh, Orissa, Punjab, Rajasthan, Uttarakhand, and Uttar Pradesh. Among households in these states, the average ratio of son to daughter preference was 4.4, conditional on wanting another child. By contrast, among the southern states of Andhra Pradesh, Goa, Karnataka, Kerala, Maharashtra, and Tamil Nadu the same average was 1.6.

women for every 1000 men. But this aggregate figure masks substantial heterogeneity across Indian states. For example, the Indian state with the most favorable sex ratio for women is Kerala with a female-male ratio of 1084; Kerala is followed by Pondicherry and Tamil Nadu with ratios of 1038 and 995, respectively. The most sex-imbalanced state is Haryana with only 877 females for every 1000 males. Punjab, which has close historical, cultural, and economic ties with Haryana, has a sex ratio of 893 females for every 1000 males. These data are summarized in Figure I, which depicts state-level variation in sex ratios.

Marriage markets are, of course, shaped by the relative proportions of men and women, and the phenomenon of missing women thus increases competition between men for the remaining women. Using this empirical context as motivation, I develop in the next section a conceptual framework for understanding how sex ratios and other marriage market factors affect human capital investments that make people more desirable.

3 Marriage Matching With Endogenous Investment

The decision to marry is one of the most consequential economic decisions in an individual's life. Spouses not only bring qualities to the union that interact to shape household (re)production, with strong implications for intra- and inter-generational welfare, but spousal traits structure the matching process by which marriages form in the first instance. These facets of marriage have occupied a substantial fraction of economists' attention to the causes and consequences of marriages. Much of this research has examined how marriage market conditions and singles' outside options affect outcomes in marriage, with a common result emerging that the fiction of a unitary household inadequately characterizes the complexity of real-world intrahousehold behavior.[5]

Due to the importance of marriage, individuals surely anticipate their marriage prospects and, to the extent feasible, make decisions that maximize their gains from that (future) partnership. Yet only relatively recently has attention been paid to how marriage market conditions affect *pre*-marital behavior (Iyigun & Walsh (2007), Chiappori *et al.* (2009)). Given the widespread importance of traits such as income and education in people's marital deci-

[5]The theoretical literature on collective household and marriage models suggests that marriage market conditions, such as sex imbalance or divorce laws, should affect the intrahousehold allocation by shifting the resource sharing rule toward the scarcer sex (e.g. Becker (1973), Angrist (2002), Chiappori *et al.* (2002). In this sense, sex ratios are a prototypical distribution factor (Browning & Chiappori (1998)), i.e. a condition that alters the household sharing rule without changing the joint budget set or individual preferences. Evidence for this consistent theoretical result has been found in several developed country contexts. For example, Angrist (2002) studies immigrant populations in U.S. labor markets and finds that sex ratios characterized by many men to women results in lower female labor force participation among married women, and (Abramitzky *et al.* (2011)) find supportive evidence that sex imbalance affects the average gap in quality between men and women. Similarly, Chiappori *et al.* (2002) study changes in divorce legislation and find that favorable changes in the legal status of women diminish female labor supply.

sions, premarital behavior in anticipation of marriage will be critical in determining investments and human capital accumulation. The empirical evidence on these theoretical predictions is sparse, but Lafortune (2010) finds evidence that variation in sex ratios, and thus marriage market prospects, affect individuals' investment decisions in education. Similarly, Arunachalam & Naidu (2006) find that expectations over future fertility bargaining impacts dowry payments before marriage. By showing that men invest in latrines as a response to marriage market conditions, this paper contributes to this small literature.

This section outlines a simple, two-period model that describes how marriage market conditions affect premarital investments. I begin with the benchmark transferable utility model of the marriage market, incorporate useful simplifications drawn from a study by Chiappori *et al.* (2009) on educational attainment and marriage matching, and reinterpret the model to reflect preferences over sanitation. Moreover, I extend the model in two ways to adapt it to important features of the marriage market context in Haryana, as discussed in Section 2. First, I devote special attention to the interaction of sex ratios and the distribution of traits in the population. Second, I make the role of dowries explicit in the agents' marriage decisions and I show that investment decisions are unaffected by dowry amounts. In this section, I provide an intuitive discussion of the main assumptions and I focus on predictions that I later test empirically; the theoretical appendix contains additional details.

The conceptual framework is based on a frictionless, transferable utility model of marriage matching, which treats men and women as distinct decision makers with individual preferences, i.e. (potential) households are not assumed to be unitary. An equivalent interpretation is that the parents of men and women act as decision makers (and parental preferences are identical to the preferences of their children); this interpretation is arguably more apt in the context of arranged marriages. Prior to marriage, men choose to invest in their quality in order to maximize the utility they will receive over their two-period lifetimes. In the second period, men either marry or remain single. If they marry, the benefits from marriage are shared between spouses in a manner determined by marriage market constraints as well as the human capital investment decisions made prior to marriage.

The key characteristic of this model is that males and females are divided into *only* two classes, *high* and *low*, which correspond to gender-specific preferences for sanitation (Chiappori *et al.* (2009)). Individual utility for all agents is increasing in sanitation, but due to the gender-specific cost of entering the *high* sanitation class, a positive fraction of men and women have *low* sanitation class when time begins in the model. All payoffs for singles and married couples depend only on the sanitation class to which an individual belongs, i.e. utility is a function only of sanitation class. This implies that the shares of marital surplus, which are determined in equilibrium, also depend only on sanitation class. In particular, since all individuals have a perfect substitute, individuals of the same sanitation class must receive the same share of the marital surplus. In this way, the model assumptions generate specific bounds on the returns for males of being high or low sanitation class, and these bounds will be shown to vary with both the fraction of women with high sanitation preferences and with the sex ratio.

3.1 Basic Features of Transferable Utility

To formalize these arguments, begin by considering the benchmark transferable utility model in which individuals have exogenously determined index of quality. Let x reflect the quality of males and y the quality of females. The union of individuals x and y produces marital output denoted by $f(x, y)$, which is a function only of individual characteristics.[6] In this paper, as is typical in the literature, this marital production function is assumed to be super-modular, i.e. male and female traits are complements in production, which implies positive assortative matching in the marriage market (Becker (1973)).

If individuals do not marry, their utility is simply $f(x, 0)$ for males and $f(0, y)$ for females, with $f(\cdot, \cdot)$ assumed to be strictly increasing in both arguments. Given these individual utilities, we can define the *material surplus*, i.e. marital output minus singles' output, as:

$$z_{xy} = f(x, y) - f(x, 0) - f(0, y) \tag{1}$$

In addition, men and women have an exogenous, idiosyncratic gain from marriage θ_i, which is assumed to be distributed as $\theta_i \sim F(\theta)$ for $i = x, y$. A natural interpretation for the parameter θ is in terms of the underlying heterogeneity of the individual, emotional gains from marriage. Given this emotional gain from marriage, we can write total marital surplus as equal to $z_{xy} + \theta_x + \theta_y$.

The defining characteristic of the transferable utility framework is that marital surplus is divided and shared between spouses. In this way the TU model provides an attractive interpretation of dowries and brideprices. These payments at the time of marriage help to clear markets because individuals of lower quality have a well-established mechanism for providing their (potential) spouse with a larger share of the marital surplus.

3.2 Specific Model Assumptions

The general transferable utility framework can be simplified in an appealing manner in order to highlight the binary decisions most relevant to premarital investment in sanitation. These simplifications originate from a model presented in Chiappori *et al.* (2009), who focus on educational investments and marriage matching. The key assumption is that males and

[6] In the interest of exposition, I assume heterosexual partnerships for the remainder of this paper.

females can be divided into two classes, *high* and *low*, which in this context correspond to gender-specific preferences for sanitation. This assumes that singles' output and married people's output are functions *only* of sanitation class. Denote these classes by $x \in \{h, l\}$ for males and $y \in \{h, l\}$ for females (where h and l mean *high* and *low*). Finally, assume that the output functions are such that: (i) singles' utility is increasing in sanitation class, i.e. $f(h, 0) > f(l, 0)$ and $f(0, h) > f(0, l)$, and (ii) sanitation classes are complements, i.e. $f(x, y)$ is supermodular and $z_{hh} + z_{ll} > z_{hl} + z_{lh}$.

Thus, the three key assumptions that characterize this TU set-up are the following: (i) output/utility depend only on sanitation class, (ii) output is an increasing function of only sanitation class, and (iii) sanitation classes are complements in the production of marital output.

3.3 Grounding Model Assumptions in Empirical Context

Although these assumptions on spousal traits reflects a simplification of the decision-making process around marriage, they adhere closely to real-world conditions in general, as well as the particular context of this study. First, note that once an individual is of marriageable age and marriage inquiries begin, the investment options available to improve one's quality in the eyes of potential suitors are extremely constrained. The most important traits for marriage, as discussed in Section 2, are not chosen by individuals active on the marriage market, but are either (i) assigned to them by birth (caste, religion, kinship), (ii) represent the accumulation of multiple years of human capital investments (education, profession, beauty), or (iii) are constrained by external factors (profession and labor markets). By contrast, males' decisions to invest in latrines are endogenously chosen, relatively cheap, and available over even a very short time scales. In this way this model captures effectively the short-term investment decisions men can make over latrine construction in order to attract a bride.

Moreover, it is reasonable to assume that marital output is an increasing function of sanitation class, and that sanitation classes are complements in terms of marital output. Owning a toilet is likely to result in better health outcomes, e.g. reduced illness, and improved non-material welfare benefits, e.g. dignity and social status. This is true both for singles and for married couples. Similar arguments justify the assumption of complementarity in sanitation classes across spouses. Due to health-related externalities, for example, the health benefits an individual obtains from sanitation will enhance the benefits that individual's spouse obtains from sanitation. Further, dignity and social standing will increase to a greater extent not only if a given individual is known as being of high sanitation class but if their spouse is as well. Thus, there is good reason to believe that sanitation classes of men and women will be complements in the marital output function.

3.4 Marriage Decisions

Conditional on sanitation class, individuals in the marriage market choose the class of the potential spouse such that their share of the marital surplus is maximized. If the same conditions are met on the other side of the market, and if males' and females' respective shares are greater than zero, then the marriage forms. Formally,

$$u_x = \max\{U_x + \theta_x, 0\} \text{ where } U_x = \max_y \left[z_{xy} - V_y\right] \tag{2}$$

$$v_y = \max\{V_y + \theta_y, 0\} \text{ where } V_y = \max_x \left[z_{xy} - U_x\right] \tag{3}$$

Individuals choose the partner of a sanitation class that maximizes their share of marital surplus, which is given by U_x (for men) and V_y (for women). Due to the simplification regarding sanitation classes, there are only four possible pairing: high/high, high/low, low/high, low/low. Further, the set-up implies that all individuals in the same group have an identical substitute and therefore must receive the same share of marital surplus. In particular, observe that if U_h is the share of marital surplus obtained by men in the high sanitation class, and U_l by those in the low sanitation class, then the difference $(U_h - U_l)$ specifies the additional surplus a married man receives from being in the high as compared to low sanitation class.

3.5 Endogenous Investment

Let the marriage market economy be comprised of individuals who live for two periods. In period one, all men are in the low sanitation class, but can choose to invest in sanitation at cost c, which converts them into a high sanitation class individual in period two. All investment decisions occur in the first period and all marriage decisions occur in the second period. Assume lifetime utility is additive across periods. If men never marry and do not invest, their lifetime two-period utility is given by $2f(l,0)$. If they do invest, then in the first period they consume the output associated with a low sanitation person, $f(l,0)$, and in the second period consume $f(h,0)$ if they remain single.

The *unmarried individual's return* to investing is the difference between individual output as a high and low sanitation class person:

$$\phi_x = f(h,0) - f(l,0) \tag{4}$$

Putting this potential single individual's return together with the marriage market return to investment, the investment decision rule of males can be written as:

$$f(h,0) + f(l,0) + \max\left[U_h + \theta_x, 0\right] - c > 2f(l,0) + \max\left[U_l + \theta_x, 0\right] \quad (5)$$

The left hand side of this inequality gives the total output consumed by men of high sanitation class after investing in period one; the right hand side gives total output conditional on not investing.

As discussed above, decisions to marry are determined by the agents' value of θ. In particular, if the individual emotional gain from marriage is sufficiently small ($\theta_x < -U_h$), then even the largest possible share of the marital surplus will be insufficient to entice men to marry. Similarly, if the draw of θ_x is larger than $-U_l$, then the male will *always* marry, irrespective of investment decisions. Finally, there is an intermediate range of θ_x for which men marry on condition that they invested in the first period, and they remain single if they do not build a latrine ($-U_h < \theta_x < -U_l$).

3.6 Equilibrium

Equilibrium in this model is established by two criteria. First, a clear prerequisite for any stable matching profile is that equal numbers of men and women must marry. Formally,

$$r\left[1 - F(-V_h)\right] = \left[1 - F(-U_l)\right] + \int_{-U_h}^{-U_l} G(\phi_x + U_h + \theta_x)h(\theta)d\theta \quad (6)$$

where the left-hand side is simplified due to the assumption that female cost to being of high sanitation class is sufficiently low as to not impede females developing strong preferences for toilets.[7]

The first term on the right-hand side gives the proportion of men for whom the idiosyncratic gain from marriage, θ_x, is sufficiently large that they always marry. The second term reflects the proportion of men who marry because they made the premarital investment. The sum of these two terms must equal the sum of females, given on the left-hand side, who marry. This quantity is mediated by r, which specifies the sex ratio of females to males. If $r \neq 1$, then incentives must adjust in order to equilibrate the numbers of available women and willing men to marry.

The second criterion for equilibrium characterizes the relative proportions of men and women in high and low sanitation classes. Even if there are equal populations of men and

[7] More generally, if there is a cost to females of becoming high sanitation class, then they will face an investment decision rule similar to 5. Later, when I discuss the "No Toilet, No Bride" campaign, this case can be modeled as a shock that reduces dramatically the cost of females being high class. For example, if social norms previously made it rare or difficult to negotiate over latrines, then "No Toilet, No Bride" made is less costly for women to express their preferences for sanitation.

women, it could be the case that, in equilibrium, the number of men or women with high sanitation class differs from the other sex. Therefore, some high sanitation class individuals must marry a low sanitation class partner (if they marry at all). Because of complementarity in types, it must be the case that either (i) there are equal numbers of high sanitation men and women, in which case equilibrium displays perfect positive assortativeness, (ii) some high sanitation men marry low sanitation women, or (iii) some low sanitation men marry high sanitation women. Formal characterization of the equilibrium can be found in the appendix.

3.7 Predictions

The equilibrium conditions generate empirically verifiable predictions, which I test in later sections. In particular, the model delivers two important results on the impact of shifting women's preferences on male investment in latrines; these model implications are stated in Proposition 1.

Proposition 1. *Given the transferable utility marriage market with endogenous investment discussed above:*

(i) An increase in the fraction of women with strong preferences for sanitation causes males' marriage market return to sanitation investment to increase.

(ii) The marriage market return to males' sanitation investment associated with any given increase in the proportion of women is larger when women are scarce than with equal populations; this return is smallest when women outnumber men.

Proof. See Section A.3 in the appendix.

Intuitively, as the proportion of women with high sanitation class increases so that there are more high sanitation class women than men, then men obtain their largest possible return to latrine investment. This increased return to building a latrine will, on average, raise latrine ownership; this testable prediction will be evaluated against the data in subsequent sections. Further, if women are scarce, then some men must remain unmarried. Due to transferable utility, these men can bid away the entire marital surplus that low sanitation level men would have obtained if there were equal numbers of men and women. Thus, married males with low sanitation level receive no surplus, and so the marital return for sanitation investment is larger than in the case of equal male and female populations. Investing in a latrine will, except in extreme cases of sex imbalance, ensure men marry at least a low sanitation class woman. In contrast, if women are abundant, then some women must remain unmarried and men with a low sanitation level receive the entire surplus associated with marrying a woman of low sanitation level. Their incentives to invest in sanitation are consequently diminished as compared to the equal populations or scarce women cases.

As discussed above, dowries are a common feature of marriage markets in this empirical context. If women (or their families) can simply adjust the dowry amount depending on their sanitation class, how would the predictions from Proposition 1 be affected? The second proposition provides an answer to this question and clarifies that, in the transferable utility framework studied here, dowries have no impact on the predictions from Proposition 1.

Proposition 2. *If dowry amount is independent of spousal characteristics, then premarital investment decisions are fully separable from decisions over dowry amount. If dowry depends on spousal traits, then dowry amount is determined in equilibrium as one component of the marital surplus. Dowry thus has no effect on male premarital investment.*

Proof. See Section A.4 in the appendix.

This result confirms that dowries are fully internalized in the transferable utility framework, and thus do not impact the empirical predictions from the model. Whereas in later sections I confirm the predictions from Proposition 1, I am unable, given data limitations, to test empirically whether dowry adjusts as a response to the "No Toilet, No Bride" campaign.[8] Later in the discussion of my empirical results, I present limited evidence on changes in dowry as a result of the program.

4 Empirical Context

4.1 Overview of the Empirical Argument

To examine the predictions presented in Section 3, I develop an empirical strategy that takes advantage of a natural policy experiment in the Indian state of Haryana. In 2005, Haryana authorities decided to implement a state-level messaging campaign, which was inspired by the work of a local NGO, that linked potential brides' bargaining power over marriage with the state's low levels of sanitation. Women and their families were encouraged to demand from potential suitors a latrine prior to marriage. In this way the campaign created a new link between long-standing customs related to arranged marital negotiations and one particular good that women value.

The empirical argument proceeds in the following steps. I first explain why latrines are much more valuable to women than men, i.e. why they can be considered a type of female good. The second step discusses the sanitation campaign known as "No Toilet, No Bride" (henceforth, NTNB) and explains its primary effects in terms of the theory outlined

[8] In the context of Bangladesh, however, Arunachalam & Naidu (2006) find that dowries adjust to anticipated changes in bargaining over fertility. Although this is a different setting, it does provide some of the only evidence available that expected bargaining can impact premarital behavior.

above. By focusing on women's ability to demand latrines, the program provides a means of studying their bargaining power on the marriage market. Subsequently, I show evidence that the policy has indeed caused an increase in latrine ownership, that this effect is mediated by the marriage market, and that sex ratios appear to be driving the program effect, further supporting the predictions of the marriage market model. Finally, I present complementary evidence that the program appears not to have changed male preferences as opposed to female preferences, and evidence that the program caused an increase in latrine ownership *only* through the marriage market.

4.2 Sanitation, Gender, and the "No Toilet, No Bride" Program

4.2.1 Sanitation as a Female Good

In rural India, a large majority of people lack access to sanitation and must defecate in the open. In a recent household survey conducted in Madhya Pradesh, for example, 80% of respondents reported that their primary places of defecation were fields, bushes, rivers/streams, and other public spaces rather than an improved latrine (Patil & Salvatore (2010)). Access to sanitation, and the lack thereof, affects all people but is of particular significance to women. It is, first of all, a matter of convenience to have a private toilet at home, to be used at one's whim with little effort; this value exists for all members of the household. For women, however, private latrines also provide significant benefits in terms of personal dignity and physical security. The impact of sanitation on female dignity is reflected well in the comments of a sixteen-year-old girl, who explained that "the toilet campaign is like a liberation. . . I would feel so conscious and ashamed [setting off in the mornings toward the open fields]. But just before my brother got married, we got a toilet in the house."[9] To mitigate embarrassment, Indian women often relieve themselves before sunrise or after dark, putting them at greater risk of sexual assault and other attacks from either humans or, in many rural areas, dangerous wild animals.[10]

These strong preferences for privacy result in uncomfortable strategies to minimize exposure. It is common for women to refrain from drinking during the day in order to avoid needing to use a toilet before sunset. Another respondent elaborated on this behavior: "You can spot men all over the hills and in the main town parking themselves on the side of the roads. But when we go down. . . we keep in mind that we shouldn't consume too much liquids, or else we might have to use the dirty loos. We have got used to holding it for-

[9]Source: Tehelka Magazine (Indian weekly), Vol. 7, Issue 29, July 24, 2010.

[10]One respondent explained: "During the monsoons it is worse. In the dark when we visit the water logged field overgrown with grass and floating with night soil, the danger of getting bitten by snakes and scorpions is also high." Source: Lesley D. Biswas, The Women's International Perspective, October 1, 2010.

ever." These coping mechanisms have psychological and physical consequences. "Women suffer the most [from lack of sanitation] since there are prying eyes everywhere", said Ashok Gera, a doctor who works in a one-room clinic in Haryana. "It's humiliating, harrowing and extremely unhealthy. I see so many young women who have prolonged urinary tract infections and kidney and liver problems because they don't have a safe place to go". Despite these health effects, women rarely report health concerns as a motivation for toilets; their rationales are most frequently framed in terms of privacy and dignity. This is evidence of a strong female preference for privacy in a social context characterized by routine male efforts to view any uncovered women. Finally, menstruation provides another significant reason for why women value private latrines: toilets provide females with the privacy, time, and comfort necessary to attend to personal hygiene (World Bank (2005)).[11] Thus, because of the high and gender-specific value that women ascribe, latrines can be understood as a type of private female good.

4.2.2 The "No Toilet, No Bride" Program

In Haryana state, local authorities initiated a massive media campaign in 2005 organized around the message of respecting the right of women to use latrines in privacy and security. This campaign is part of India's Total Sanitation Campaign (TSC), a national initiative of the Government of India whose primary objective is to ensure access to and use of sanitation facilities in rural areas. Although a federal initiative, states shoulder a portion of the costs and have substantial flexibility in local design and implementation.

This information campaign encouraged the families of women to demand of boys' families that they construct a latrine prior to the woman marrying and relocating into the boys' family compound (Haryana, like the rest of northern India is predominantly patrilocal). Slogans such as "no loo, no I do" and "no toilet, no bride" were disseminated via radio, banners, and other advertising channels. In particular, village walls were painted with the message: "I won't allow my daughter to marry into a home without toilets." This initiative thus emphasized a novel linkage between social norms around the marriage market and access to sanitation.

Popular media reports suggest widespread exposure to these ideas.[12] In an interview conducted by the Washington Post, a young male, age 22, who was hoping to marry soon, explained: "I will have to work hard to afford a toilet. We won't get any bride if we don't

[11]Many authors have argued this strong preference might drive absenteeism among girls in secondary school, despite the null findings of Oster & Thornton (2011), who do not report on the presence of sanitation facilities in their sample schools in Nepal and/or whether latrines influence take-up of menstrual cups.

[12]See, e.g., The Times (UK): "Show us your loo before you woo, men are told" (March 26, 2009) and the Washington Post: "In India, more women demand toilets before marriage" (October 12, 2009).

have one now." "I won't be offended when the woman I like asks for a toilet," he added. As part of the information and education campaign, blank building walls were converted into billboards and painted with the slogan (in Hindi): "I won't get my daughter married into a household which does not have a toilet". A recurring radio jingle sang a tune with the lyrics: "no loo, no I do." The founder of Sulabh International, an NGO that designs low-cost improved latrines, states: "The 'No Toilet, No Bride' program is a bloodless coup. When I started, it was a cultural taboo to even talk about toilets. Now it's changing. My mother used to wake up at 4am to find someplace [in the fields or rivers] to go quietly. My wife wakes up at 7am and can go safely in her home." These vignettes help to characterize the social context in which the "No Toilet, No Bride" campaign operates.

In addition to anecdotal evidence, administrative data from the Haryana health department suggest a large increase in latrine ownership in recent years. According to state officials, 1.42 million toilets were built between 2005 and 2009. Among this total, 950,000 latrines were built by families above the poverty line and 470,000 by households below the poverty line. Further, household survey data provides additional support for the claim of increased latrine coverage. According to data from two rounds of the District-Level and Household Survey (these data will be described in greater detail below), the proportion of households that owned improved latrines increased from 29% in 2004 to 41% in 2008.

Note that latrines are moderately costly capital investments. The average cost of an improved latrine (e.g. a pit latrine with protective slab or a flush toilet to septic tank) typically ranges between 1000 and 2000 rupees (approximately $20–40 USD). For purposes of comparison, Haryana's state-mandated minimum wage for "Scheduled Appointments" of unskilled laborers was 135 rupees in 2004. According to the Indian NSS, Haryana has the second highest daily wage rate for agricultural labor (195 rupees). Therefore, the cost of typical latrine will range from five to 14 days of paid labor for these two unskilled groups. However, the Government of India provides through the Total Sanitation Campaign subsidies that reimburse households for up to 80% of latrine costs if they possess a Below Poverty Line (BPL) card. Given this incentive scheme, households below the poverty line (BPL) are able to construct an improved latrine at an actual cost of approximately Rs. 200–300 (roughly $4.50–6.75 USD). This amount would be only two days labor for an unskilled worker at Haryana's public position minimum wage or for an agricultural daily wage laborers with BPL card (NSS 2010).

5 Empirical Strategy

Haryana's "No Toilet, No Bride" campaign can be understood as generating exogenous variation in the proportion of women with strong preferences for sanitation. Thus, by studying how latrine adoption responds to the program, it is possible to test empirically the predictions from the theoretical model.

I identify program effects by exploiting the following intuition. The program is targeted to girls and their families in the sense that the female side of the marriage market must accept the campaign's message and decide to take action. The female side of the market is the first step in the sequence of behavioral change related to latrine ownership. However, if the program is effective and women either express their preference or demand a latrine from potential suitors, then the program will exert disproportionate pressure, which is plausibly exogenous, specifically on those households that have boys of marriage age, i.e. households with boys active or nearly active on the marriage market. To study whether women are able to demand and obtain latrines, therefore, I explore changes in latrine ownership among these particular households with marriageable boys, who comprise the actual treatment group in which the relevant outcome can be measured. After exposure to the program, households with boys of marriageable age can be expected to have a higher probability of latrine adoption as compared with households without marriageable boys.

Since the campaign began in 2005, households are unable to choose the number of marriageable boys as a response to program incentives.[13] In this sense, program exposure in Haryana is plausibly exogenous to the presence of a marriageable boy. Still, households with marriageable boys might differ systematically from non-marriageable boy households, which raises concerns about endogeneity in any simple comparison of these two groups over time.

To address these econometric concerns, I propose two complementary analyses. Estimation begins with a difference-in-difference (DD) specification, which controls explicitly for potential differences in marriageable boy and non-marriageable boy households. I lay out the identifying assumptions required for this analysis, discuss unresolved issues, and propose an additional method based on significantly weaker assumptions. In particular, I use a difference-in-difference-in-difference (DDD) specification, which captures the change in the difference between households with and without marriageable boys on ownership of a latrine after the program was implemented, using northern Indian states other than Haryana as a comparison group. As discussed in Section 2, these states are an appropriate choice for comparison with Haryana because of their relative similarity on matters of son preference and sex ratios as compared to southern Indian or the easternmost Indian states. For these reasons, the factors that mediate women's bargaining power and marriage market processes are likely to be comparable across treatment and comparison households. Estimates of the

[13]Households could choose, of course, how and when to become *active* on the marriage market. The manner in which I construct my marriageable boy variable, explained in greater detail in Section 5.2, addresses this concern explicitly.

NTNB program effect are shown to be consistent and similar in magnitude across the DD and DDD specifications, despite being based on different assumptions.

5.1 Data

This paper uses two rounds of household microdata from the District Level Household and Facility Survey (DLHS), a nationwide survey implemented by the Government of India to track the national Reproductive and Child Health Program.[14] The primary survey module interviews a representative sample of ever-married women and gathers household information on maternal and child health outcomes, family planning and reproductive health, utilization of health care services, access to health facilities, and health knowledge. Additional modules focus on household, village, and health facility characteristics, but I do not use them in my analysis. The data form a repeated cross-section that is representative at the district level for 601 districts in 34 Indian states and territories. I use the two latest survey rounds, DLHS-2 (2004) and DLHS-3 (2008/9), which provide data immediately preceding the project period as well as after three/four years of program exposure.[15]

I restrict the sample to focus on rural households from northern states, which are those states characterized by the strongest cultural preference for sons, as discussed in Section 2.3.[16] Using these restrictions, my 2004 data contains information on roughly 220,000 households, including 12,500 in Haryana; the 2008 sample contains data on approximately 370,000 households, including about 16,000 Haryana households.

5.2 Variable Construction

In my empirical analysis, I construct the following variables. *Latrine* is a binary indicator that assumes the value of one if household i owns a private latrine that prevents contact between humans and excreta, as per the standard definition of the Joint Monitoring Programme of WHO and UNICEF; note that shared latrines and pit latrines without slabs do not meet

[14]DLHS is an initiative of India's Ministry of Health and Family Welfare and is implemented by the International Institute for Population Sciences in Mumbai.

[15]For the remainder of the paper, I will simply refer the DLHS-3 survey year as 2008.

[16]The 16 states included in my sample are: Jammu and Kashmir, Himachal Pradesh, Punjab, Chandigarh, Uttaranchal, Haryana, Delhi, Rajasthan, Uttar Pradesh, Bihar, West Bengal, Jharkhand, Orissa, Chhatisgarh, Madhya Pradesh, and Gujarat. My empirical results are robust to alternative sample selection that includes only Haryana and adjacent states as well as a regional criterion that includes all states in the northwest quadrant of the country. However, the policy that I examine in my empirical section is at the state-level. Therefore, due to matters of inference using clustered data, it is desirable to include the largest number of states that could serve as plausible controls. Given the close relationship between son preference and women's outcomes in society, the most appropriate control group is comprised by those states with similar levels of stated son preference.

these criteria. I use this definition due to its operational relevance to governmental and non-governmental sanitation programs, including the Total Sanitation Campaign and "No Toilet, No Bride". Moreover, the requirement that toilets be private to a household is closely related to women's concerns around privacy and dignity, and is thus important in the context of the "No Toilet, No Bride" program.

The marriageable boy and girl variables, *mboy* and *girl*, are based on the gender-specific mean age of marriage $+/-$ one standard deviation (and rounded to the nearest integer). This variable adopts a value of one for any household that has a boy/girl of marriageable age, irrespective of marital status. Given my empirical strategy, I am implicitly defining the "No Toilet, No Bride" treatment group as those households with boys of marriageable age, the vast majority of which have been active on the marriage market during the program. I considered alternative definitions of the marriageable criterion, including one based exclusively on single, unmarried children, one based on strictly married men, as well as one using different intervals around the gender-specific mean. Increasing the interval size around mean age at marriage is undesirable because it includes larger numbers of households who might not be affected by treatment. Observe that the definition using singles excludes by construction any households with marriageable boy(s) who married after the program began, thereby eliminating from treatment sample exactly those households most likely to have responded to the program. At the same time, the use of strictly married young men as *mboys* would exclude households with marriageable boys who purchased a toilet in anticipation of marriage.[17] My preferred definition, therefore, is the gender-specific mean age at marriage $+/-$ one standard deviation because it best balances these concerns.

To account for unobserved household fertility preferences, I also construct another *mboy*-oriented variable that is the total number of *mboys* in the household divided by the total number of living children. This variable provides a more robust test of the effect of *mboys* even if there are unobserved changes in household fertility in Haryana, which are potentially correlated with presence of an *mboy*. While I report the results from regressions that use this *fraction of mboys* variable instead of simply the presence or absence of *mboys*, it will be seen that this modified use of *mboys* does not alter in either a qualitative or quantitative manner the central findings.

My empirical definition of *marriage market* builds on the discussion in Section 2, where I reviewed evidence that (i) nearly all women marry within their caste group, and (ii) nearly all women move, upon marriage, to villages that are between 30 and 70 kilometers away from their home villages. Taken together, these facts provide a natural means of defining a given household's marriage market. Unfortunately, the DLHS data does not contain

[17]Numerous popular media accounts contain interviews in which young men report they are building a latrine *in preparation* for the marriage market, even if a potential spouse is not yet identified.

geocoded data on households, nor does it identify previous residences, so I am unable to define marriage markets in this explicitly spatial way. Instead, I assume marriage occurs predominantly within one's administrative district. Districts in India are heterogeneous in terms of area, but their size ranges are comparable to the ranges reported in the studies of marriage migration. For example, the largest district in Haryana is roughly 70 kilometers across from the western to eastern administrative boundary, while the smallest district is roughly 17 kilometers in diameter. Thus, districts provide a reasonable approximation to the distance across which marriages typically form.

The second descriptive fact from Section 2 used when defining marriage markets is caste endogamy, which refers to practice of marrying within one's own caste group. For the purposes of this marriage institution, the relevant grouping is the *jati*, which is sometimes referred to, imprecisely, as sub-caste. The *jati* is a community that plays the principal role in providing one's social identity, including providing potential marital partners, providing some forms of insurance against consumption risk, and serving as a professional network across labor markets (Munshi & Rosenzweig (2006)). In the absence of this detailed, *jati*-specific data, I use the DLHS question on broad caste grouping. This variable represents an aggregation of finer social categories, but it still divides the sample population into four categories (scheduled caste, scheduled tribe, other "backward", and other).[18] Finally, because the relatively large caste category of "other" might include more than one religion (and marriages almost never happen across religions), I also include religion in my marriage market definition. Thus, a *marriage market* for the purposes of this paper will be those households in household *i*'s home district with marriageable boys/girls of the same caste grouping and religion.

Finally, the variable for *sex ratio* is the ratio of women to men in a particular marriage market. I exclude households in marriage markets where either the number of marriageable boys or girls is less than twenty individuals; this omits unusual and pathological (e.g. missing) values for the sex ratio. There exists substantial variation in the sex ratios across marriage markets, despite the overall sex imbalance in the population.

Table I presents summary statistics on key variables for Haryana and comparison states in each round of the survey. These two groups are comparable across a wide range of relevant observables, including household size, the fraction of households with and without *mboys*, male age at marriage, age of the household head, etc. Given the severity of sex imbalance in the Punjab region, which includes Haryana, there is a few percentage point difference in the ratio of women to men, although the trend in similarly declining (i.e. becoming more skewed against women) over time in both Haryana and control states. Note that the sex ratio is greater than one in Haryana in 2004 and in control states in both 2004 and 2008.

[18] These categories encompass 19.4%, 13.3%, 39.9%, and 27% of my sample, respectively.

There are two reasons why this is the case. First, the marriage market definition internalizes the average age gap between men and women at the time of marriage. On average, men marry girls that are 3.5 years younger than them. With population growth, each successive, younger cohort is larger than its predecessor. Thus, by defining marriageable boys and girls in this way, the fact of sex imbalance due to son preference is countervailed by the impact of population growth. The second reason is that the variance of the distribution of female age at marriage is lower than that for males. Hence, when I define the marriage market in respect to male and female mean ages at marriage $+/-$ one standard deviation, the age range for males is two years larger for males than females. This additionally causes more males to be included in a marriage market, thereby increasing the sex ratio.

Latrines are also substantially different across Haryana and comparison states. In data from both survey rounds, control states have mean latrine ownership that is nearly 10 percentage points lower than in Haryana. One reason for this is that Haryana is wealthier than most of the states in the comparison group, and wealth is correlated with latrine ownership. Moreover, the overall trends in latrine ownership in Haryana and control states differ as well. Observe that in the comparison states sample latrine ownership has actually declined between 2004 and 2008. For this reason a difference-in-difference analysis that simply compares Haryana and controls over time would be inappropriate. But when I disaggregate latrine ownership by *mboy* status, it can be seen the decline in the control group is actually driven by the non-*mboy* households; *mboy* households in this group have increased their latrine ownership, on average, but to a lesser degree than in Haryana. In other words, there is a two percentage point increase in latrines among *mboy* households in comparison states, but this increase is much greater in Haryana due to the incentives established by NTNB. Note that in the DDD framework, which is explained in more detail below, these differential trends across *mboy*/non-*mboy* households are explicitly controlled for, and so pose no threat to identification.[19]

5.3 Empirical Specification

To estimate the impact of Haryana's "No Toilet, No Bride" campaign on improved latrine ownership, I begin with a difference-in-difference specification that compares latrine ownership between Haryana households with and without *mboys* before and after program exposure. This analysis highlights the core intuition driving the empirical strategy, namely that NTNB targeted the behavior of *mboy* households in particular. I run a regression of the

[19] As an extra robustness check, I run the entire analysis on a sample of households that excludes any states that have declining latrine coverage. All estimates remain unchanged.

following form:

$$\text{Latrine}_{it} = \alpha + \beta_1(\text{mboy}_i \times \text{post}_t) + \beta_2(\text{mboy}_i) + \beta_3(\text{post}_i) + \epsilon_{it} \tag{7}$$

where *mboy* is an indicator variable that adopts the value of one if household i has a male household member between the ages of 19–27 (males' mean age at marriage $+/-$ one standard deviation) and ϵ_{it} is a household-specific iid error term that satisfies $\text{E}(\epsilon_{it}|\mathbf{X}) = 0$.

In addition to the primary definition of *mboys*, I use an alternative *mboy* variable that is the fraction of *mboys* in the household divided by the total number of children. This alternate definition addresses potential concerns that unobserved household fertility could be correlated with women's status. This DD specification controls for unobserved time-invariant traits of *mboy* and non-*mboy* households, as well as secular trends in Haryana. The coefficient of interest β_1 is therefore identified from changes in latrine ownership among *mboy* households over time. Consistent identification in this case depends on the common trends assumption for *mboy* and non-*mboy* households, i.e. observed changes in latrine ownership between these two groups of households would have been identical in the absence of the program.

One concern with this approach, which would invalidate the identifying assumption, is that an unobserved shock in Haryana is positively correlated with latrine ownership in *mboy* households or negatively correlated with latrine ownership in non-*mboy* households. For example, since *mboy* are on average slightly wealthier than non-*mboy* households, any economic shock that differentially affects wealthier households could affect latrine ownership as well.

I address this concern about potential endogeneity by using a triple difference (DDD) regression specification, where the three differences are households with and without marriageable boys, in Haryana and comparison states, before and after (three to four years of) program exposure. I regress a binary variable for latrine in household i in state j at time t on a set of interactions and fixed effects:

$$\begin{aligned}\text{Latrine}_{ijt} = \alpha &+ \beta_1(\text{mboy}_i \times \text{state}_j \times \text{post}_t) + \beta_2(\text{mboy}_i \times \text{post}_t) + \beta_3(\text{mboy}_i \times \text{state}_j) \\ &+ \beta_4(\text{state}_j \times \text{post}_t) + \beta_5(\text{mboy}_i) + \beta_6(\text{haryana}_i) + \beta_7(\text{post}_i) + \epsilon_{ijt}\end{aligned} \tag{8}$$

where *mboy* is defined as both an indicator and a fraction, as explained above, and ϵ_{ijt} is a household-specific iid error term with $\text{E}(\epsilon_{ijt}|\mathbf{X})$ assumed $= 0$ given the full set of fixed effects and interactions $\mathbf{X}$. The fixed effects control for unobserved time-invariant factors at the state level and time-varying factors across both states. The double interaction terms allow the relationship between marriageable boys and improved latrines to vary across states and across time, in addition to capturing state-specific linear time trends. In this formulation, the primary coefficient of interest is β_1 on the triple interaction, which captures the change in the effect of marriageable boys on latrine adoption in Haryana between 2004 and 2008 relative to the change in effect of marriageable boys on latrine adoption in control states between 2004 and 2008. This is the period during which the "No Toilet, No Bride" campaign likely generated additional social pressure on these households. Because I condition on state-year fixed effects, *mboy*-state, and *mboy*-year interactions, β_1 is identified through Haryana-specific changes over time in differential rates of latrine ownership between households with and without marriageable boys.

Consistent estimation of this saturated linear probability model requires that $\mathrm{E}(\epsilon_{ijt}|\mathbf{X}, \delta_{jt}) = 0$, where $\mathbf{X}$ is a vector comprised of the *mboy* variable interacted with state and year dummies, and δ_{jt} reflects state-year fixed effects.[20] This assumes that changes in this differential across states and time are orthogonal to unobserved determinants of latrine ownership. In assessing the validity of this identification strategy, note that the most likely explanations for a positive effect of marriageable boys on latrine ownership can be ruled out by this empirical strategy. For example, if households with young male adults typically enjoy higher income, which allows them to purchase latrines, we would expect to see a positive correlation between marriageable boy households and latrine ownership across both states, but we would not expect a Haryana-specific change over time. Another rationale for an observed positive correlation between marriageable boy households and latrine ownership is that transfers associated with marriage, such as dowry and gift-giving, could also facilitate latrine ownership. A similar counterargument, however, can also rule out this hypothesis: we would expect this story to affect households with marriageable boys equally in Haryana and control states.

For the identifying assumption to be invalid, an unobserved factor must cause the trend in the difference in latrine ownership between households with and without marriageable boys to diverge across Haryana and control states. In such a case, this factor would cause the common trends assumption to be violated, i.e. the trend in the differential between *mboy*/non-*mboy* households would inaccurately reflect the counterfactual scenario in Haryana in the absence of the program. This identifying assumption would be violated if there are unobserved Haryana-specific shocks that covary with latrine adoption *and* the presence of a marriage age boy. This assumption is impossible to accept with certainty, but it is difficult to generate hypotheses on the types of shocks on Haryana's marriageable boy households that would undermine identification. With that said, Section 6.3 explores this robustness

[20] The linear probability model is particularly appropriate in this context because the fully saturated specification implies the conditional expectation function of latrine ownership is linear. Still, I run similar regressions using probit and logit specifications, which yields nearly identical results.

of this assumption to concerns about Haryana and *mboy*-specific shocks related to wealth and household size. Finally, I present additional findings that lend further support to the marriage market channel interpretation of observed changes in latrine ownership among marriageable boy households, thereby providing further, indirect support for this identification strategy.

6 Empirical Results

The empirical analysis presented in this section tests the two implications derived from the model of marriage matching with endogenous investment.

6.1 Marriageable Boys and Household Latrine Adoption

I focus first on the main program effect of "No Toilet, No Bride" on latrine ownership; this analysis corresponds to the first theoretical prediction from Proposition 1. Recall that because the program generates plausibly exogenous variation that increases the proportion of women with a strong preference in toilets, the model predicts that latrine adoption should rise.

The first test of this prediction uses the DD specification given by (7); estimates are presented in Table III. The DD estimates suggest that NTNB has increased *mboy*'s investment in latrines by between 0.057 and 0.061 percentage points over a baseline mean of 0.27, i.e. NTNB increased latrine ownership by approximately 22%. When using the *mboy* fraction variable that controls for household fertility, which is reported in columns (3) and (4), results are similar, although the point estimates when including controls is somewhat smaller.

As suggested earlier, however, any changes in *mboy* households, e.g. wealth shocks, would violate the identifying assumption in this DD framework and yield inconsistent estimates. Therefore, I turn to the DDD analysis, which relies on significantly weaker assumptions than the DD framework. Table II provides an intuitive preview of my main findings regarding the treatment effect of "No Toilet, No Bride" on households with marriageable boys. This table contains mean latrine ownership among the eight possible groupings implied by the DDD strategy. For exposition, I compare Haryana only with Punjab, which is the state most similar in terms of culture and economy. Punjab is, in fact, the ideal control for Haryana due to their long and common history as part of the greater Punjab region. This evidence must be considered suggestive; statistical inference is complicated by the fact that policy variation occurs at the state-year level.

For this reason, the preferred DDD analysis includes a much larger sample of states, which have similar son preferences to Haryana. Table IV presents DDD estimates of the

NTNB campaign on latrine adoption. The primary coefficient of interest is the first triple interaction, which is positive and statistically significant ($\beta = 0.043$). The program increases latrine ownership by 4.3 percentage points above a baseline mean of 0.28 for Haryana's *mboy* households, i.e. a 15% increase among those households likely to be affected by females demanding/desiring improved sanitation.

These results support the argument that the more simple difference-in-difference specifications (instead of the full DDD implemented here) would provide inconsistent estimates of the program effect. For example, Haryana increased state-level latrine ownership at a faster rate than control states, violating the common trends assumption in a DD framework. Moreover, *mboy* households in Haryana have, on average, a 3% lower probability of latrine ownership, which again suggests a DD analysis at the Haryana/control level is problematic. These changes are explicitly controlled for as part of my identification strategy, as discussed above.

The key result from this specification is the marked shift in the effect of marriageable boys on improved latrines, specific to Haryana after the NTNB campaign. Specifically, there was a 4.3 percentage point change in the differential over time between Haryana and control households with and without marriageable boys. Because the program targeted specifically those households whose boys are on the marriage market, and having a marriageable boy is plausibly exogenous to household decisions regarding improved latrines, conditional on the full set of interactions and fixed effects, these results provide evidence that either (i) a positive fraction of marriageable women in Haryana have shifted their preferences *and* pressured men into sanitation investments, or (ii) men have anticipated this pressure and responded by increasing their premarital investment in latrines. There is an additional possibility, outside of the bargaining interpretation, which suggests that new couples invest in latrines as a form of health-seeking behavior and health investment in children.[21] While the DDD specification cannot rule out this possibility, the following sections present evidence on each of these channels and confirm that the marriage market hypothesis is driving these results.

6.2 Marriage Markets, Female Preferences and Latrine Adoption

The second testable prediction from the model states that the impact of rising proportions of women with a preference for toilets on premarital investment will be mediated by the sex ratio. To study this cross-partial effect of how sex imbalance in the marriage market mediates

[21] This hypothesis of unitary household preferences for investment in children is ruled out by the analysis of sex ratios below. If women favor child investments more than men, however, then Haryana women demanding toilets, and therefore program effects, could be explained more by child health than private female benefits. This interpretation is fully consistent with the bargaining interpretation of the empirical results.

male investment responses to changing female preferences, I use regression specification (8) in two subsamples, where one is comprised of households in marriage markets with an oversupply of women and one with an undersupply of women. This formulation is desirable for expositional purposes, but it is equivalent to interacting the sex ratio indicator (for r greater or less than unity) with the set of interactions and fixed effects from (8). As before, I first present estimates from the DD (using *mboy*/non-*mboy* and pre/post treatment) and then turn to the preferred DDD analysis.

Tables V and VI report estimates from these analyses. When women are abundant, the estimated average treatment effect of NTNB is statistically indistinguishable from zero. By contrast, when woman are scarce and the marriage market is highly competitive for men, the treatment effect is nearly double the estimate from the entire sample; this point estimate is large and highly statistically significant (at the 99 percent level). The difference in magnitude is nearly double the estimated effect from Table IV, which is consistent with the relative sizes of the subsamples, these results, and the earlier estimate that ignored the sex ratio.

These results provide strong evidence that skewed sex ratios mediate the impact of women's ability to demand latrines on the marriage market. When women are scarce, they are able to negotiate successfully for latrines, but when they are abundant, men have less incentive to invest and women are unable to obtain latrines to the same degree. In this sense, the phenomenon of missing women in a marriage market appears to have increased female bargaining power, conditional on survival to marriage age. Finally, these results lend additional support to the marriage market hypothesis because evidence of marriage market-driven latrine adoption bolsters the case that NTNB exerted disproportionate pressure on marriageable boys.

One issue that arises when interpreting these results is the role of migration. Perhaps males elect to move out of tight marriage markets with dim prospects, or alternatively, they import brides from other marriage markets. Similarly, males could seek brides from younger cohorts, which will be larger than older cohorts, on average, because of population growth. There are in fact a wide variety of possible means by which men could relax the constraints imposed on them in a particular marriage market. Unfortunately, the DLHS data do not contain information that allows me to identify such migration. To the extent men are able to alleviate the pressure they experience on the marriage market, however, my estimates of the program effect will underestimate the program effect in the absence of migration across marriage markets.

6.3 Investigating Competing Hypotheses

The previous sections provided a series of results that together provide compelling evidence that male premarital investments respond to both changing distributions of preferences in the female populations and constraints generated by local sex imbalances. In this section I consider three competing interpretations of my main results. The first two hypotheses explore distinct mechanisms by which the observed changes in latrine ownership might arise, which are outside the marriage market considerations reflected in the theoretical framework. The third hypothesis is a placebo test that examines whether any unobserved factors related to household size and/or fertility are driving my results. Given the tests conducted here, I can strongly reject each of these competing hypotheses regarding the NTNB effect.

6.3.1 Does the Program Change Male Preferences?

Depending on how NTNB messages are received by men, the main empirical result from Section 6.1 could arise not because women are exerting pressure via the marriage market on male investments, but because men's preferences have changed in response to the program. Consider a scenario whereby NTNB changed young adult male preferences for sanitation among both households with boys active on the marriage market as well as those with relatively recent experience on the marriage market. Perhaps the program raised the salience of sanitation in Haryana, changed men's preferences, and thereby caused an increase in latrine adoption. In this way households with young men, which are relatively wealthy, became convinced about the value of sanitation and made the sanitation investment.[22] Then even in my triple difference empirical framework it might be possible to observe a program effect, yet this hypothesized shift would operate entirely outside of the marriage market.

I test this hypothesis directly by studying whether NTNB has caused any change in latrine adoption among households with men slightly older than marriageable age. Recall that my definition of marriageable boy is $+/-$ one standard deviation from males' mean age at marriage; this yields an age range of 19 to 27. Here I create a new indicator that takes the value of one if a household has anyone in the age range 27 to 34 years. This age group is young and close enough in age to serve as a reasonable comparison group to very late teens and twenty-something year olds, but due to their age are almost certainly married already and therefore immune from marriage market pressures generated by the program. I run a regression using the same DDD specification as above, but substitute the *oldboy* variable for the *mboy* variable in all interactions and fixed effects.

As can be seen in Table VIII, the main coefficient of interest is statistically zero. These households, like *mboy* households, are more wealthy on average than households without

[22] Wealth and *mboys* are correlated in my sample ($\rho = 0.11$).

these *oldboys*. Given the focus on a cohort of men who are otherwise likely to be very similar to younger men active on the marriage market (if anything, they should have more income/wealth, on average, which they could use to purchase a latrine), the null result suggests that male preferences have not changed as a result of the NTNB program. This provides additional evidence that the mechanism through which the program operates is the marriage market hypothesis.

6.3.2 Do Changing Female Preferences Drive Latrine Adoption Outside of Marriage?

A second possibility regarding the behavioral mechanism driving latrine adoption is that female preferences are indeed changing as a result of NTNB, but that these preferences affect latrine ownership *outside* of marriage market channels. For example, girls might learn from the NTNB emphasis that latrines are valuable and subsequently push their parents or husbands into buying them. Under this hypothesis, women still push for latrines as a result of the program but they obtain the goods for reasons that have nothing to do with marriage *per se*.

To study this issue, I use a DDD regression that focuses on *mgirls* in place of *mboys*. Yet many households with both *mboys* and *mgirls* will be married, so running this analysis on the entire sample will include all those households in which men built a latrine in order to marry. In this case it would be impossible to distinguish between the effects of premarital bargaining and investment on the one hand and changes in female preferences outside of the marriage market. Instead, I focus on a subsample that excludes households with both marriageable boys and marriageable girls. Some of these households will have both *mboys* and *mgirls* because they are siblings; it is unfortunate that they are excluded but the data does not allow me to differentiate perfectly between sibling pairs and married pairs. The benefit of analyzing this subsample, however, is that it excludes with near certainty those couples who married since NTNB began, thereby focusing the analysis on single *mgirls*.

Table IX reports the results from this analysis. In this case, the coefficient of interest is again statistically indistinguishable from zero. This suggests that among girls who are active on the marriage market, and who are thus a primary target of the NTNB message, there is no program-induced ability to obtain latrines. These results provides an instructive counterpoint to the results from Table IV. In that case, women are able to obtain latrines through marriage, in accordance with their preferences. But when these marriages are excluded from the sample, we observe no program effect at all. Therefore, the evidence indicates that NTNB causes women's preferences to shift, but that this causes a shift in male premarital investments *only* through marriage market pressures.

6.3.3 Omitted Variables Correlated With Household Size

Earlier I noted that the key identifying assumption underlying my econometric specification could be violated if there exists an unobserved, differential trend that affected only marriageable boy households in Haryana. For example, if Haryana instituted an anti-poverty program between 2005 and 2008, which raised incomes of marriageable boy households, then increased toilet coverage could be explained by an income rather than marriage market effect associated with the program. If this were the case, $\mathrm{E}(\epsilon_{ijt}|\mathbf{X}, \delta_{jt}) \neq 0$ and estimates of β_1 from eq. (8) would be biased.

Household structure might play a particularly important role in this context because of the strong son preference in the greater Punjab region. One form this son preference can take is fertility behavior that follows a stopping rule, i.e. when families have children until they have a boy, who will continue to live in the family compound and care for the parents in old age (girls will typically marry and move away from the village). If households practice a stopping rule, then households with any girl children will be, on average, larger and slightly older than households without girls. This reasoning suggests that the demographic and age structure of the household might vary systematically with the gender composition of the children. Additionally, because household size is correlated with poverty, any Haryana-specific anti-poverty program after 2005, such as (hypothetically) workfare or low-income educational stipends, could also cause a spurious correlation between marriageable boy households in Haryana in 2008 and latrine ownership.

My alternative definition of *mboy* as the fraction of *mboy*s among total children in the household provides on strong test of the influence of unobserved fertility on latrine adoption. In addition, I test the validity of this hypothesis by modifying eq. (8) by adding the full suite of interactions and fixed effects for two different categories of household size. Specifically, I create a dummy variable for *small households* that indicates whether the household is smaller than the mean household size in the sample; *large households* are those larger than mean household size. My main empirical specification, modified in this manner, tests simultaneously for both potentially confounding stories just outlined.

As can be seen in Table VII, the primary coefficients of interest are, as before, only the triple interaction terms that reflect the DDD for marriageable boys, small households, and large households in Haryana in the post-treatment period. I find small and statistically insignificant effects for both small and large households, and the estimate of the NTNB treatment effect, captured in the *mboy* triple interaction, remains positive and significant at the five percent level. These robustness results provide evidence rebutting the notion that factors related to the age or demographic structure of households are confounding my estimates of the program effect.

7 Bargaining Power and the Intrahousehold Allocation

The theoretical literature on collective household and marriage models suggests that marriage market conditions, such as sex imbalance or divorce laws, should affect the intrahousehold allocation by shifting the resource sharing rule toward the scarcer sex. Thus, theoretical investigations predict that female scarcity should result in greater bargaining power among the remaining women. In this sense, sex ratios are a prototypical distribution factor (Browning & Chiappori (1998)), i.e. a condition that alters the household sharing rule without changing the joint budget set or individual preferences.

Until now, this paper has focused on how marriage market conditions affect male investments in a good women value highly. I presented evidence that the program appears to operate by changing female preferences in conjunction with marriage market pressures related to sex imbalance. But these results are consistent with two possible mechanisms, both of which operate *through* the marriage market. Does the program indeed cause a shift in female preferences for sanitation, making them stronger than before? Or does the program's emphasis on female bargaining power on the marriage market actually increase that power directly? In other words, the program might shift the distribution of female types and, by so doing, drive male premarital investment, or it might act as a sort of distribution factor that itself shifts the intrahousehold sharing rule.

It is less plausible that an information campaign can act in a distribution factor than serve to shift preferences. Yet these two mechanisms carry strong implications for how household behavior responds to the program. In particular, the former interpretation suggests the program taps into the relative bargaining power held by women when they are scarce, changes women's preferences over the bundle of assets and qualities of marriage that they bargain over, and so causes women to negotiate more readily over latrines as compared to other desired goods. If this interpretation is correct, then we would expect to see that the observed treatment effect on latrines results in a form of compensating differential with respect to other goods, i.e. women must trade off some good(s) in order to obtain a toilet at the time of marriage. And the second interpretation is that by highlighting women's right to demand certain goods, particularly if not only in the context of female scarcity, the program provides an exogenous, positive shock directly to women's bargaining power rather than to female preferences. In this case, we would expect to see an unconditional increase in latrines as a net gain for women, i.e. without any tradeoffs.

I examine the first interpretation that the program changed female preferences, redistributing relatively more weight to latrines and less weight to other items in the bundle of goods that women value. Greater latrine ownership in response to the program should occur when women have sufficient bargaining power to obtain goods they particularly desire.

Moreover, if the program causes an alteration of the woman's weighting scheme over her preferred goods, then the program effect should also cause a decline in ownership of some other favored good. We should therefore find evidence that women must trade off some goods they prefer in order to secure latrines during the marriage bargaining process.

Note that if one is prepared to assume that women would never agree to a trade (of a latrine for some other good or bundle thereof) that lowered her utility, then one can immediately conclude that the increase in latrine adoption reflects a shift in the intrahousehold allocation toward women that has unambiguously improved female welfare. Absent this assumption, it is possible to explore a range of household goods and characteristics for which we have reason to believe women have a strong preference. Given the cultural context of Haryana, such marriage qualities might include the educational levels of one's spouse (conditional on own education), the age gap between husband and wife (younger brides relative to men are associated with lower status of women in the household; see, e.g., Desai & Andrist (2010)), influence over fertility, and/or increased access to household assets such as improved cooking fuels, sewing machines, washing machines, etc.

I test for program effects for each of these outcomes using the DDD approach, when there is data on the particular asset or outcome for both survey rounds, or using DD (with the differences being *mboy*/non-*mboy* households in Haryana and control states), when there exists data only from the 2008 round. Table X summarizes the findings from these regressions. The unifying result is that along a range of numerous possible goods and desirable traits, there is no evidence that women have been forced to tradeoff other desirable goods in order to secure a latrine.

In contrast, these results provide suggestive evidence that the relative position of women has improved, in addition to latrines, along two key dimensions: the education level of the husband, conditional on women's education, and the age at marriage/living with husband. With respect to the education difference between husband and wife, a simple analysis of the DDD estimates does not yield statistically significant differences. Disaggregating the data into those households with and without latrines, however, indicates an increase in 0.1 years in the differential between male and female schooling specifically among *mboy* households in Haryana post-treatment; in these same households without latrines, there appears to have been a 0.03 year decline in the gap but this is not statistically significant; the statistically significant difference between these estimates is 0.13 years. The evidence therefore suggests a slight improvement in the average quality of males for a given female only in those treatment households with latrines.

With respect to female age at marriage, a trait which is positively associated with female status, a pattern similar to the education gap emerges. That is, the DD estimate of changes in age at marriage that arise among *mboy* households in Haryana is not statistically significant, but it appears to mask heterogeneity across households with and without latrines. In particular, there appears to be an increase of 0.16 years in the mean age at marriage, which is specific to households that have latrines who are therefore likely to have responded to the program. In *mboy* households without latrines, however, no such evidence emerges. Hence, women in *mboy* households in Haryana after treatment appear to get married slightly later (0.24 years) in those households that have latrines as compared to Haryana *mboy* households without latrines. A similar result was found with respect to the age at which the female first cohabits with the husband.

One interesting result to emerge is that there has been an increase in motorbike ownership. Motorbikes are a good that males value and that form a common part of dowry among wealthier families. The observed increase among *mboy* households in Haryana after program exposure occurs for both *mboy* households that have latrines as well as those that do not, but those with latrines have a (statistically significant) two percentage point higher probability of motorbike ownership than households without latrines after the program. This quantity reflects a 7% increase in motorbike ownership associated with being a treatment household *and* having a latrine. This result provides suggestive evidence that marriage negotiations over toilets have been associated with men being compensated in other ways for the expense.

Taken together, these suggestive results indicate that NTNB and bargaining over latrines might have caused a number of shifts in dowry outcomes, but there is no evidence that females were forced to substitute items they care about in favor of toilets. On the contrary, it appears that women have improved their relative position in the household on account of marrying older and marrying better educated men, while Haryana's *mboys* have increased their ownership of motorbikes, especially if they built a latrine. Dowries, which are unobserved, could play a fundamental role in driving these findings. It seems likely that several items commonly involved in dowry and marriage negotiations might have shifted in response to the program, but without additional data and assumptions on the household's behavior it is impossible to empirically determine a shift in the intrahousehold allocation.

8 Conclusion

This paper focuses on an innovative natural policy experiment known as "No Toilet, No Bride", which highlighted the link between latrines, for which women have a strong preference due to concerns about privacy and security, and marriage markets in Haryana state in the historical Punjab region. Because the program encouraged girls' families to demand from boys' families a latrine prior to marriage, it generated disproportionate pressure to construct a latrine specifically among those households whose boys were of marriageable age and seeking a bride. I demonstrate that marriageable boy households were indeed affected disproportionately by the program, and I estimate the "No Toilet, No Bride" treatment effect to have increased latrine ownership by 15% over the baseline mean of Haryana households with marriageable boys in 2004. In addition, estimates of latrine adoption in Haryana post-treatment are four times larger in marriage markets characterized by a scarcity of women as compared to marriage markets with more women than men.

These results are shown to be invulnerable to competing hypotheses that challenge this marriageable boy effect of the program. In particular, I have shown (i) there is no evidence that unobserved factors correlated with household size are driving my estimates and (ii) the program does not appear to have changed male preferences for latrines. The "No Toilet, No Bride" program thus appears to have caused a significant increase in latrine ownership in Haryana specifically through the channel of female preferences in the context of bargaining power. Moreover, I provide substantial evidence that women are not trading toilets for other goods in the context of marriage bargaining toilets. Indeed, the program is associated with an increase along two fundamental dimension for women's status: the mean age at marriage and educational quality of the husband. These findings are consistent with a theoretical framework in which sex imbalance alters marriage market conditions and causes males to increase their premarital investments in their own quality.

The underlying mechanism that drives latrine adoption among households with marriageable boys is thus competition on the marriage market and a household's desire to marry successfully its boys. In exploring the impact of skewed sex ratios on women's bargaining power, as reflected in female demand for latrines under "No Toilet, No Bride", this paper provides evidence that, despite widespread and persistent discrimination, the female bargaining position has improved through heightened competition on the male side of the market.

In addition to the literature on female bargaining power and marriage, this paper also makes an important contribution to the limited evidence that exists on the effectiveness of sanitation campaigns at large scale. In India, nearly 70% of the rural population lacks access to sanitation, and this situation is associated with severe morbidity and mortality. An estimated 1.2 million children under five die each year in the country; most of these deaths are attributed to diarrheal disease and acute respiratory infections, which are both exacerbated by inadequate sanitary behavior and sanitation infrastructure (Black *et al.* (2003)). In this critical policy context, a low-intensity information campaign, "No Toilet, No Bride", cleverly exploited deeply rooted social norms and marriage market conditions in order to increase sanitation. As a result, there are approximately 500,000 more toilets among Haryana's four million households in 2008 than in 2004.[23] By studying this large shift, this paper is infor-

[23] This number reflects the increase in toilets in Haryana that can be attributed to the "No Toilet, No Bride"

mative not only about female bargaining power, but also regarding the design of sanitation policy and behavior change programs more generally.

campaign based on estimates in this paper.

A Theoretical Appendix

A.1 Additional Features of Transferable Utility

The assumption underlying the transferable utility framework is that this marital surplus is divided and shared between spouses. In particular, the surplus is divided according to a sharing rule that is determined not by the individual characteristics of partners in a match, but by the requirements imposed by stable matching. A matching assignment profile is considered *stable* if no two married or unmarried people prefer to be together and no married individual prefers to be single (Gale & Shapley (1962)). This profile will display either positive or negative assortativeness depending on the super- or sub-modularity of the marital output function $f(x, y)$, i.e. whether traits x and y are complements or substitutes.[24] Then the key feature of transferable utility is that a man (woman) with a given level of quality can "bid away" higher quality men (women) by offering the potential spouse on the other side of the market a greater share of the marital surplus. The well-known consequence is that the equilibrium, i.e. the stable assignment profile, must maximize aggregate marital surplus across all men and women (Shapley & Shubik (1972) and Becker (1973)). Moreover, the sharing rule that specifies the division of the marital surplus is determined in equilibrium through the requirements of stable matching. In this way, changes in the relative proportions of men and women, or in the distribution of quality in the male or female populations, alter the sharing rule over marital surplus.

A.2 Further Characterization of Equilibrium

To characterize equilibrium formally, let α denote the fraction of women of high sanitation class; $(1 - \alpha)$ is therefore the fraction of women with low sanitation preferences. Similarly, define β as the fraction of men who have invested in a latrine and so are of high sanitation class; $(1 - \beta)$ gives the complementary set of men who did not invest. The second criterion for equilibrium can then be expressed for each possible scenario outlined above, i.e. $\alpha = \beta$, $\alpha > \beta$, and $\alpha < \beta$.

The first case, when equal numbers of high sanitation class people get married, is the following:

$$\alpha = \underbrace{[1 - F(-U_l)]\, G(\phi_x + U_h - U_l)}_{\text{Always marry and invest}} + \underbrace{\int_{-U_h}^{-U_l} G(\phi_x + U_h + \theta) h(\theta) d\theta}_{\text{Married b/c invested}} = \beta \tag{9}$$

[24] This paper assumes throughout that positive assortativeness holds.

The second and third cases involve the possibility that, in equilibrium, more men or women belong to the high sanitation class and thus some individual(s) in the high grouping marry individual(s) from the low grouping. Formally, these cases are:

$$\alpha > [1 - F(-U_l)]\, G(\phi_x + U_h - U_l) + \int_{-U_h}^{-U_l} G(\phi_x + U_h + \theta) h(\theta) d\theta \tag{10}$$

$$\alpha < [1 - F(-U_l)]\, G(\phi_x + U_h - U_l) + \int_{-U_h}^{-U_l} G(\phi_x + U_h + \theta) h(\theta) d\theta \tag{11}$$

Eq. (10) states there are more women with high preferences for sanitation than men who invested in latrines. Eq. (11) considers the opposite case when more men invested than there are women with high preferences for sanitation. Note that the assumption of complementarity of types implies that only one of these inequalities can hold at once. These expressions combined with equation (9) specify the equilibrium. See Chiappori *et al.* (2009) for a proof of existence and uniqueness of this marriage market equilibrium.

A.3 Division of the Marital Surplus

The equilibrium conditions have strong implications for the sharing rule that divides marital surplus. Consider each of the three cases in turn. In the first case, when $\alpha = \beta$, there is perfect positive assortativeness. Thus, the surplus from marriage of same types, z_{hh} or z_{ll}, must equal the sum of shares from two same-class individuals marrying, that is:

$$z_{hh} = U_h + V_h \tag{12}$$

$$z_{ll} = U_l + V_l \tag{13}$$

Male and female shares need not be equal if the outside options to marriage differ across spouses.

When $\alpha \neq \beta$, then surplus shares must satisfy:

$$U_h + V_l \geq z_{hl} \tag{14}$$

$$U_l + V_h \geq z_{lh} \tag{15}$$

In particular, when $\alpha < \beta$, there are more men who invested than there are women with strong preferences for sanitation, and eq. (14) will hold with equality while eq. (15) will hold as a strict inequality. An immediate consequence of these equilibrium shares is that high sanitation men must relinquish some of the marital return from their investment. In particular, they must receive their lower-bound marital return to investment; otherwise, an equivalent man could bid away any surplus until the minimum bound is restored. Plugging eq. (14) into (12) and using (13) yields:

$$V_h - V_l = z_{hh} - z_{hl} \tag{16}$$

$$U_h - U_l = z_{hl} - z_{ll} \tag{17}$$

Eq. (17) specifies the marriage market return to investment by males, and men get only their marginal contribution to a marriage with a low sanitation class woman, i.e. the lower bound on their investment return. By contrast, women with high sanitation preferences get their entire contribution to the marital surplus of a marriage with a man with a high sanitation class. By contrast, when $\alpha > \beta$, men receive the entire marginal contribution to marital surplus from their investment ($= z_{hh} - z_{lh}$), and women receive the remainder ($= z_{lh} - z_{ll}$).

These bounds on the marriage market returns yield testable results. I gather the key predictions from this discussion into Proposition 1, which is presented in Section 3.7 above.

Proof. I consider each of the two main testable predictions in turn.

Observe that eq. (9) implicitly defines males' marriage market return to investment ($U_h - U_l$) as a function of α. As α increases, it immediately follows that either U_h must increase and/or U_l decrease in order for there to be sufficient men willing to marry (conditional on investing). Thus, an increase in α drives male premarital investment by increasing males' marriage market return.

For the second implication, consider eq. (6) when $\theta_y > -V_h$ and all women want to marry someone.[25] Assume $r < 1$ and women are scarce. This implies that some men must remain unmarried. As r decreases, V_h and U_l must decline and/or U_h must increase, which implies that the marriage market return to male premarital investment increases (and women's marriage market return to being of high sanitation class must decline) to maintain equality in the numbers of men and women who want to marry.

In the case of equal populations, i.e. $r = 1$, an increase in the fraction of females with strong preferences for sanitation causes men to receive their upper bound return on sanitation investment. Now consider a situation of female scarcity. Some proportion $(1 - r)$ men will fail to marry, even if they want to, because of an insufficient number of brides. These potentially unmarried men, who will all be of a low sanitation class, will bid away the entire surplus obtained by the married low sanitation class men, i.e. U_l decreases. The immediate consequence is that $U_h - U_l$ is larger than in the case of equal populations. Conversely, when $r > 1$ and women outsupply men, then low sanitation level men receive the entire surplus in a marriage with a low sanitation type female, i.e. U_l increases as compared to the case of

[25]This situation in which the idiosyncratic gain from marriage for females is sufficiently large to cause all women to prefer marriage is easily justified given the particularly low status ascribed to older single women in Haryana.

equal populations. Therefore, $U_h - U_l$ must be smaller when $r > 1$ than when $r = 1$, which is in turn smaller than when $r < 1$. In this way, the second part of Proposition 1 follows directly from transferable utility and the requirements of pairwise stable matching. □

A.4 Dowries

In a transferable utility (TU) framework, dowries form a part of the transfers that divide the surplus. Thus, a natural way to incorporate dowry is to assume that marital output is comprised of two portions: non-dowry and dowry output, i.e. the marital output $= f(x,y) + d_{xy}$ and d can depend in any way on x and y. A union generates additional output over singles' status both because of complementary traits in household production (from $f(x,y)$ and because it brings in "additional" resources from the bride's family. From the perspective of the bride's family, dowry enters as a cost that diminishes the total gain of marriage; if dowries d are greater than the material and emotional gain from marriage, then the marriage does not occur. The only relevant criterion for males' investment decision is the marriage market return to investment and not how that return is constituted by dowry and other non-dowry marital output. I summarize this argument as Proposition 2, presented in the main body of this paper.

Proof. Consider two cases: when dowry is independent of spousal traits and when dowry depends on spousal traits in an undefined manner.

Note first that if dowry amount is independent of spousal traits, investment decisions are fully separable from dowry considerations. We can rewrite the respective problems as: $U_X = \max_Y [z_{XY} - V_Y + d]$ and $V_Y = \max_X [z_{XY} - U_X - d]$. Thus, the share of male surplus U_X is comprised of two parts: a dowry d and non-dowry amount equal to $\max_Y [z_{XY} - V_Y]$, and males' decisions are simply over the latter (and, of course, whether to marry at all, i.e. $\max\{U_X + \theta_x + d, 0\}$).

Now consider the case when dowry payments are determined by spousal traits. Let d_{XY} denote the dowry given by woman of type Y to man of type X. Ignore considerations of wealth/income or credit constraints. Then we can rewrite eqs. (2) and (3) to explicitly incorporate dowry:

$$u_x = \max\{U_x + \theta_x, 0\} \quad \text{where} \quad U_x = \max_y \left[\gamma_{xy} - V_y\right] \tag{18}$$

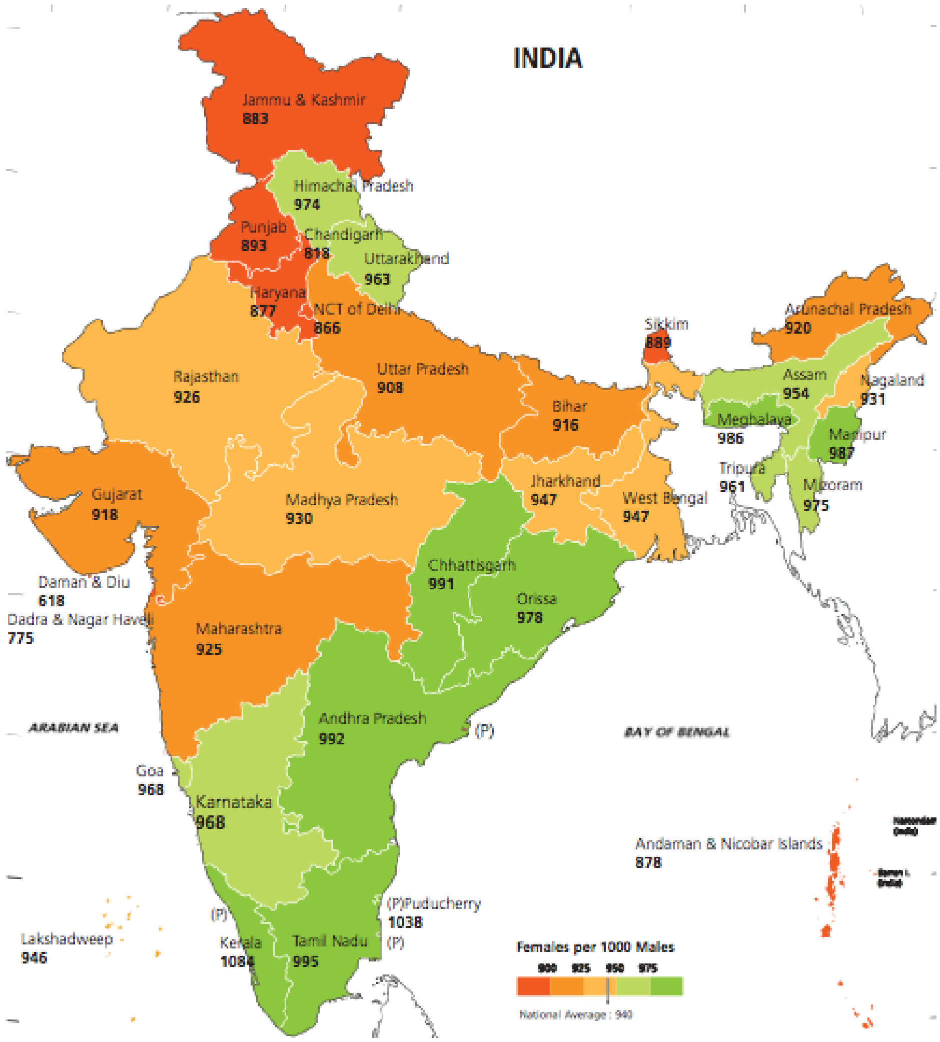
$$v_y = \max\{V_y + \theta_y - d_{xy}, 0\} \quad \text{where} \quad V_y = \max_x \left[\gamma_{xy} - U_x\right] \tag{19}$$

where $\gamma_{xy} = f(x,y) + d_{xy} - f(x,0) - f(0,y) = z_{xy} + d_{xy}$, and z_{xy} on the right-hand side of the last equality is the same surplus object from the non-dowry analysis. Although the dowry and non-dowry portions of marital output are simply added up and shared, consistent with TU, the dowry also enters as an additional term in the decision by the female about whether to marry at all.

Irrespective of how dowry depends on spousal traits x and y, the surplus shares from Section A.3 are easily modified to account for the constraints imposed here. In other words, the sharing rule that divides the marital surplus is still determined in equilibrium, and the bounds on the sum of marital output plus dowry must hold in the same way as before. Thus, from the perspective of male incentives to invest, the inclusion of dowry in this modeling framework is irrelevant. □

B Figures and Tables

FIGURE I: SEX RATIOS ACROSS INDIAN STATES, 2011



Source: Census of India, 2011.

TABLE I: SUMMARY STATISTICS

	Haryana		Control States	
VARIABLE	2004	2008	2004	2008
Latrine	0.285	0.395	0.198	0.185
	(0.451)	(0.489)	(0.398)	(0.388)
Latrine (*w/ mboy*)	0.276	0.419	0.196	0.202
	(0.447)	(0.493)	(0.397)	(0.401)
Latrine (*w/o mboy*)	0.293	0.368	0.199	0.173
	(0.455)	(0.484)	(0.399)	(0.378)
Marriageable boy	0.456	0.465	0.415	0.415
	(0.498)	(0.499)	(0.493)	(0.493)
Marriageable girl	0.450	0.432	0.442	0.419
	(0.498)	(0.495)	(0.497)	(0.493)
HH size	6.263	5.984	6.415	6.041
	(2.656)	(2.527)	(2.942)	(2.626)
Number of males	3.265	3.102	3.260	3.067
	(1.563)	(1.477)	(1.730)	(1.577)
Number of female	2.996	2.883	3.154	2.974
	(1.606)	(1.567)	(1.768)	(1.617)
Age of HH head	43.631	44.452	43.444	43.787
	(13.681)	(13.480)	(13.221)	(13.289)
Male age at marriage	22.975	22.507	22.741	22.397
	(3.608)	(3.485)	(4.625)	(4.306)
Female age at marriage	19.310	19.330	18.368	18.545
	(2.674)	(2.335)	(3.666)	(3.147)
Sex ratio	1.016	0.962	1.087	1.048
	(0.091)	(0.094)	(0.138)	(0.144)
Observations	15220	14108	263079	319449

Standard errors in parentheses. Marriageable boys are household members 18–26 in age; marriageable girls are members aged 15–24. Haryana has the “No Toilet, No Bride” campaign, which started in 2005 and has been ongoing since then.

TABLE II: MEAN LATRINE OWNERSHIP BY TREATMENT-STATE-YEAR

	Before	After	Time Trend
	TREATMENT HHS		
Haryana	0.270	0.320	0.050
Punjab	0.522	0.670	0.148
Within Year Diff.	−0.152	−0.250	
Diff-in-Diff	−0.098		
	NON-TREATMENT HHS		
Haryana	0.276	0.276	0.000
Punjab	0.502	0.633	0.131
Within Year Diff.	−0.126	−0.257	
Diff-in-Diff	−0.131		
DDD ESTIMATE	$-0.098 + 0.131 = 0.033$		

This table summarizes the triple difference strategy (in Haryand and Punjab only) by looking at group means of latrine ownership using the interaction of treatment group, state, and time. Treatment households are those with at least one child of marriageable age, defined as $+/-$ one standard deviation from the gender-specific mean age at marriage, which are those who should be affected by the NTNB program. Punjab is the state most similar to Punjab in terms of wealth, culture, and politics.

TABLE III: LATRINE ADOPTION (DIFF-IN-DIFF ESTIMATES)

Latrine	(1)	(2)	(3)	(4)
Mboy x Post	0.061***	0.057***	0.070***	0.038***
	(0.014)	(0.011)	(0.015)	(0.012)
Mboy	-0.017	-0.041***	0.011	-0.039***
	(0.011)	(0.007)	(0.010)	(0.010)
Post	0.082***	0.065	0.015	0.039**
	(0.026)	(0.017)	(0.033)	(0.019)
Constant	0.276***	-0.239***	0.362***	-0.277***
	(0.026)	(0.015)	(0.034)	(0.019)
Controls	N	Y	N	Y
R^2	0.014	0.251	0.021	0.249
N	29345	27815	29328	27815

The dependent variable is a dummy variable for whether household *i* has a latrine. Column (1) reports the basic DD regression using the *mboy* variable. Column (2) adds the following control variables: age and education of the household head, wife/mother's education, household size, and four proxies for wealth (house type, fan, TV, phone, and motorcycle). Column (3) using the *fraction of mboys* variable, which uses total number of *mboys* in the household as a fraction of total children in order to account for household fertility. Column (4) adds the same control variables to the regression from Column (3). The primary coefficient of interest in all cases is the double interaction. All standard errors are clustered at the village-year level. Significance levels: ***p< .01 **p< .05 *p< .1.

TABLE IV: LATRINE ADOPTION (DDD ESTIMATES)

Latrine	(1)	(2)	(3)	(4)
Mboy x Haryana x Post	0.036***	0.043***	0.047***	0.040***
	(0.011)	(0.007)	(0.010)	(0.008)
Mboy x Haryana	-0.011	-0.018***	-0.009	-0.021***
	(0.008)	(0.004)	(0.006)	(0.005)
Mboy x Post	0.019*	0.001	0.023**	0.003
	(0.010)	(0.006)	(0.009)	(0.008)
Haryana x Post	0.110**	0.051*	0.114**	0.060**
	(0.046)	(0.026)	(0.045)	(0.025)
Mboy	0.009	-0.013***	0.020***	-0.023***
	(0.008)	(0.004)	(0.006)	(0.004)
Haryana	0.088***	-0.064***	0.079***	-0.066***
	(0.026)	(0.013)	(0.024)	(0.025)
Post	-0.102**	-0.016	-0.101**	-0.016
	(0.046)	(0.026)	(0.045)	(0.025)
Constant	0.281***	-0.147***	0.282***	-0.150***
	(0.026)	(0.025)	(0.024)	(0.025)
Controls	N	Y	N	Y
R^2	0.019	0.330	0.020	0.333
N	445584	445583	393885	393884

The dependent variable is a dummy variable for whether household *i* has a latrine. Column (1) reports the basic DDD regression using the *mboy* variable. Column (2) adds the following control variables: age and education of the household head, wife/mother's education, household size, and four proxies for wealth (house type, fan, TV, phone, and motorcycle). Column (3) using the *fraction of mboys* variable, which uses total number of *mboys* in the household as a fraction of total children in order to account for household fertility. Column (4) adds the same control variables to the regression from Column (3). The primary coefficient of interest in all cases is the triple interaction. All standard errors are clustered at the state-year level. Significance levels: $^{***}p< .01$ $^{**}p< .05$ $^{*}p< .1$.

TABLE V: SEX RATIOS & MARRIAGE MARKET COMPETITION (DD ESTIMATES)

	LOW SEX RATIO		HIGH SEX RATIO	
Latrine Ownership	Coef.	p-value	Coef.	p-value
Mboy x Post	0.075***	0.000	0.020	0.308
	(0.014)		(0.019)	
Mboy	-0.022*	0.050	0.001	0.906
	(0.011)		(0.012)	
Post	0.133***	0.000	0.034***	0.009
	(0.020)		(0.013)	
Constant	0.241***	0.000	0.348***	0.000
	(0.000)		(0.008)	
R^2	0.034		0.002	
N	18399		10568	

The dependent variable is a dummy for whether household *i* owns a latrine. I run this specification separately for households in a competitive marriage market (from the marriageable boy's perspective), which is defined as having more marriageable boys than marriageable girls, and for households in a less competitive marriage market. These are the low and high sex ratio columns, respectively. The coefficients of interest (the double interactions) are statistically different from each other at the 99% level. Standard errors, clustered at the village-year level, are reported in parentheses. Significance levels: ***p< .01 **p< .05 *p< .1

TABLE VI: SEX RATIOS & MARRIAGE MARKET COMPETITION (DDD ESTIMATES)

	LOW SEX RATIO		HIGH SEX RATIO	
Latrine Ownership	Coef.	p-value	Coef.	p-value
Mboy x Haryana x Post	0.065***	0.007	0.018*	0.061
	(0.024)		(0.009)	
Mboy x Haryana	-0.044**	0.038	-0.018	0.199
	(0.021)		(0.014)	
Mboy x Post	0.005*	0.443	0.036**	0.028
	(0.006)		(0.015)	
Haryana x Post	0.099*	0.056	-0.110***	0.007
	(0.042)		(0.036)	
Mboy	0.016***	0.002	0.007	0.611
	(0.004)		(0.012)	
Haryana	0.056	0.226	0.090**	0.018
	(0.046)		(0.034)	
Post	0.110***	0.000	-0.170***	0.000
	(0.021)		(0.037)	
Constant	0.136***	0.000	0.291***	0.000
	(0.010)		(0.012)	
R^2	0.028		0.028	
N	249100		270938	

The dependent variable is a dummy for whether household *i* owns a latrine. I run this specification separately for households in a competitive marriage market (from the marriageable boy's perspective), which is defined as having more marriageable boys than marriageable girls, and for households in a less competitive marriage market. These are the low and high sex ratio columns, respectively. The coefficients of interest (the triple interactions) are statistically different from each other at the 99% level. Standard errors, clustered at the state-year level, are reported in parentheses. Significance levels: ***p< .01 **p< .05 *p< .1

TABLE VII: HOUSEHOLD STRUCTURE

Improved Latrine	Coef.	SE	p-value
Mboy x Haryana x Post	0.037**	0.016	0.019
Mboy x Haryana	-0.024**	0.011	0.035
Mboy x Post	0.007	0.011	0.499
Mboy	0.023***	0.007	0.003
Small HH x Haryana x Post	0.024	0.018	0.176
Small HH x Haryana	-0.045***	0.016	0.004
Small HH x Post	-0.072***	0.020	0.000
Large HH x Haryana x Post	-0.002	0.030	0.958
Large HH x Haryana	-0.042***	0.016	0.008
Large HH x Post	-0.003	0.020	0.888
State-Year FE	Yes		
R^2			0.0645
N			442824

The dependent variable is household latrine ownership. *Small HH* indicates that household *i* had four or less children; *large HH* indicates household *i* had more than four children. Standard errors are clustered at the state-year level. Significance levels: ***p< .01 **p< .05 *p< .1.

TABLE VIII: "NO TOILET, NO BRIDE" AND MALE PREFERENCES

Latrine Ownership	Coef.	SE	p-value
Oldboy x Haryana x Post	0.003	0.009	0.705
Oldboy x Haryana	-0.002	0.010	0.724
Oldboy x Post	-0.082***	0.014	0.000
Haryana x Post	0.136***	0.045	0.006
Oldboy	0.100***	0.006	0.000
Haryana	0.075***	0.016	0.000
Post	0.000	0.045	0.996
Constant	0.184***	0.018	0.000
R^2			0.010
N			461730

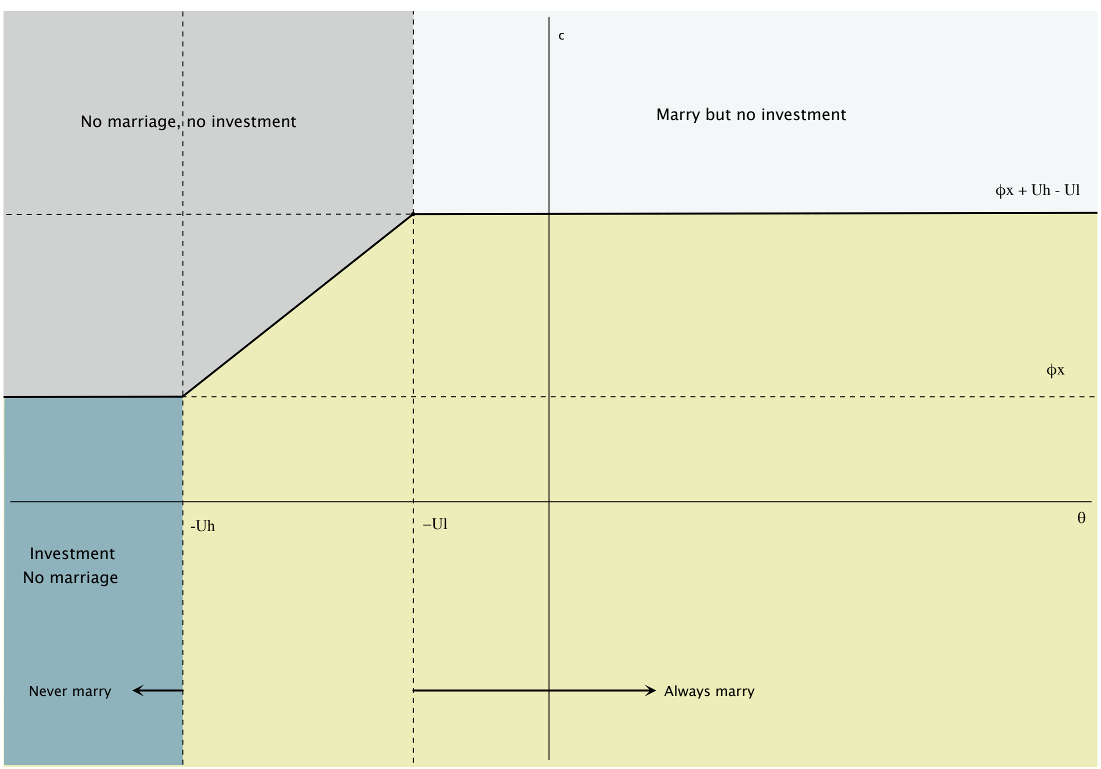
The dependent variable is latrine ownership at the household level. *Oldboy* is an indicator variable for whether the household has a male slighter older than being of marriageable age (i.e. between 28 and 36 years of age). All standard errors clustered at the state-year level. Significance levels: ***p< .01 **p< .05 *p< .1.

TABLE IX: FEMALE PREFERENCES OUTSIDE OF MARRIAGE

Latrine Ownership	Coef.	SE	p-value
Mgirl x Haryana x Post	0.005	0.011	0.669
Mgirl x Haryana	0.020***	0.006	0.003
Mgirl x Post	-0.003	0.111	0.798
Haryana x Post	0.115**	0.042	0.010
Mgirl	0.022***	0.006	0.001
Haryana	0.079***	0.018	0.000
Post	-0.015	0.042	0.724
Constant	0.192***	0.018	0.000
R^2			0.007
N			455113

Sample focuses on single *mgirls* only by excluding households with both mboys and mgirls. All standard errors clustered at the state-year level. Significance levels: ***p< .01 **p< .05 *p< .1.

FIGURE II: MARRIAGE AND INVESTMENT INCENTIVES



The idiosyncratic gain to marriage, θ, is along the x-axis. Individual costs of investment are along the y-axis. The yellow region indicates those men who will invest. Graph is adapted from Chiappori, Iyigun, and Weiss (2009).

TABLE X: FURTHER EVIDENCE ON INTRAHOUSEHOLD ALLOCATION

Dep. Var.	Full	Latrine	No Latrine	Diff.
Education gap	0.016	0.099*	-0.032	0.131***
	(0.031)	(0.050)	(0.032)	
Age gap	-0.012	0.026	-0.003	0.029
	(0.053)	(0.113)	(0.956)	
Age at first birth	0.208**	0.105	0.188*	-0.083
	(0.091)	(0.078)	(0.098)	
Pregnant	-0.013**	-0.013*	-0.007	-0.006
	(0.006)	(0.006)	(0.007)	
Contraceptive use	-0.027*	-0.002	-0.022	-0.020
	(0.014)	(0.013)	(0.170)	
Marriage age	-0.042	0.163***	-0.079	0.242***
	(0.064)	(0.051)	(0.073)	
Living with husband	0.145*	0.291***	0.013	0.278***
	(0.069)	(0.049)	(0.077)	
Sewing machine	0.035***	0.029**	0.021**	0.008
	(0.008)	(0.012)	(0.008)	
Washing machine	0.022***	0.011**	0.017***	-0.006
	(0.002)	(0.004)	(0.001)	
Cooker	-0.005	-0.003	0.003	0.006
	(0.005)	(0.006)	(0.007)	
Fan	0.012	0.003	0.031	-0.028***
	(0.006)	(0.008)	(0.007)	
Television	-0.014**	-0.019	-0.023***	-0.004
	(0.005)	(0.016)	(0.007)	
Motorcycle	0.052***	0.049***	0.027***	0.022**
	(0.008)	(0.018)	(0.008)	

Reported coefficients are from the triple interaction of *mboy*-haryana-post. The coefficients for marriage age, age at which first living with husband, washing machine, and cooker are all based on the difference-in-difference using *mboy* status and Haryana/control (i.e. with no time dimension) due to variables being missing from the 2004 round. The first column reports the basic DDD or DD estimates. Column "Diff" reports the statistical significance of the difference between the coefficients from columns (2) and (3) based on $p > \chi^2$. All standard errors are clustered at the state-year level. Significance levels: ***p< .01 **p< .05 *p< .1.

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(Adverse) General Equilibrium Effects of Cash Transfers[†]

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If general equilibrium effects are taken into account, cash transfers to eligible households indirectly decrease utility of ineligible households living in the same neighborhood. A hike in local prices outweighs the income effect from higher local wages. Households with low labor endowments are particularly affected. Our results contrast sharply with the common notion that this class of programs generates positive multiplier effects on the local economy.

JEL Classification: E21, H43, I38, O12, O17
Keywords: Policy evaluation, general equilibrium effects, structural estimation

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1 Introduction

The general notion in the existing literature is that welfare programs generate positive spillover effects on the local neighborhood. Miguel and Kremer (2004) show that anti-worm treatment to some individuals - because of reducing disease transmission - generates large benefits for other individuals not receiving the treatment. Kaboski and Townsend (2011) find Microfinance to increase local wages. Angelucci and De Giorgi (2009), Bobonis and Finan (2009) and Lalive and Cattaneo (2009) report positive effects of *Oportunidades*, Mexico's flagship conditional cash transfer program, on food consumption and human capital investments of program-ineligible households residing in the neighborhood.

The results presented in this paper contrast with this notion. When general equilibrium effects are taken into account, cash transfers indirectly decrease utility of ineligible households living in the same neighborhood.

We study the effects of *Oportunidades* within a general equilibrium model. The model is simple in the sense that, unlike Todd and Wolpin (2006) and Attanasio et al. (2012), it is static and ignores fertility and educational choices. Instead, the model puts emphasis on equilibrium effects in factor and commodity markets. The basic set-up consists of a village populated by poor and non-poor households. Each household is endowed with staple (food) as well as labor, and chooses a consumption bundle (food, non-food, leisure) which maximizes utility subject to a budget constraint. Poor households receive a social security cash payment by the federal government. The staple (food) is produced *inside* the village, while the non-food commodity is produced *outside*. The village exports a fraction of its staple and imports non-food commodities. These assumptions reflect that rural areas specialize in staple production, while importing industrialized products from urban areas.[1] Importing implies transactions costs, in particular labor. The village price of non-food consists of an exogenous component ('world market' price) *plus* an endogenous component (labor costs linked to importing non-food to the village). Labor costs are endogenous because the procurement of non-food requires village residents' labor.[2] In equilib-

[1]Household data for rural Mexico, for example, shows that half of rural households' monthly consumption consists of imported hygiene products (e.g. soap, combs, tooth and hair brushes, detergents, whiteners), household utensils (plates, towels, combs etc.), clothing, tennis shoes and boots, school supplies (pens and paper), and energy (batteries, gas, petrol).

[2]Major national grocery chains, or other forms of non-food procurement which do not require village

rium the village's labor market must clear, which determines the village's wage rate hence the village price of non-food. Food prices are determined in a similar vein but, because food is exported, its village price consists of an exogenous component *minus* labor costs linked to export food. If, as is common in practice, food is sold at the farmgate (hence no village residents' labor is required for export), food prices become exogenous. In this case, if wages go up, non-food prices increase while food prices remain unaffected.

The model predicts the cash payment to increase recipients' demand for non-food items. The additional labor requirements for procurement cause growth of village wages. Higher wages have an ambiguous effect on non-food consumption of non-poor households. On the one hand, income hence demand for non-food items increases. On the other hand, the costs of importing non-food items go up. Non-food consumption decreases if the price effect outweighs the income effect. Conditions are derived under which this is the case. Food prices do not increase. The income effect from higher local wages, therefore, increases non-poor households' consumption of food yet not necessarily of non-food.

We then bring empirical evidence for the predictions of the model, using data collected for the evaluation of the rural component of *Oportunidades.* 506 villages were randomized into treatment and control villages, and households in these villages were classified as either poor or non-poor. Poor households in treatment villages would receive regular cash payments by the government.[3] The existing literature finds indeed no effect of *Oportunidades* on food prices, and higher food consumption of non-poor households in treatment villages (Angelucci and De Giorgi, 2009).

Little, on the other hand, is known about *Oportunidades*'s impact on non-food prices and non-food consumption. Lack of data is a major challenge: Only non-food expenditures are observed from the data, yet neither prices nor quantities. This paper approaches the data problem in two different ways, the first using reduced-form and the second using structural estimations. The reduced-form approach compares changes in non-food expenditure at the extensive margin, and finds that non-poor households in

residents' labor, do not usually exist in villages.

[3]Payments were of substantial size, about 20% of the poors' pre-program household income. See Hoddinott and Skoufias (2004) for a detailed description of the program.

treatment villages have a lower probability of purchasing non-food items, suggesting a decrease in non-food consumption.

The structural approach uses an empirical version of the model - calibrated to the *Oportunidades* data - to simulate changes in consumed non-food quantities. In a first step, the parameters of the model are calibrated to data available on households residing in control villages. In a second step, out-of-sample forecasts are conducted: The cash transfer is added to the income of poor households in the control village sample. To test the performance of the model, the resulting simulated moments are compared to the actual moments observed in the treatment village sample. Thus, in the vein of Todd and Wolpin (2006), the experimental design of *Oportunidades* is used as a source of model validation. The model is able to replicate reasonably well the moments of the treatment group sample. This increases confidence in by the model simulated impacts of *Oportunidades* on non-food consumption. The exercise suggests a decrease in non-poor households' non-food consumption of about seven percent. Overall, utility of the average non-poor household falls by roughly two percent.

Lastly, we study heterogenous effects on non-poor households' *per capita* non-food consumption. Analytical expressions are derived which show how the sign and magnitude of the effect depend on household level parameters (endowments, preferences), as well as parameters that describe the program (size of cash payments) and local context (number of payment recipients, land distribution, aggregate agricultural productivity). Model and the data, for example, suggest that non-food consumption and utility are more likely to decrease in non-poor households with few labor endowments, i.e. in households with below average number of adult household members.

2 Theory

A. The model

Consider a village populated by $g = 2$ groups of households: poorer households ($\mathbf{P}$s) and somewhat richer households ($\mathbf{R}$s)

$$g \in \{\mathbf{P}, \mathbf{R}\}$$

Table 1: Notation of the model's variables and parameters

Notation	Description
Variables:	
p_L	village wage rate
x_i	non-food consumption of household i
q_i	staple (food) consumption of household i
l_i	leisure consumption of household i
L_i	labor supply of household i
Village level parameters:	
$\bar{p}_q$	market price of food
$\bar{p}_\chi$	market price of non-food
$\bar{Q}$	village's staple endowment (agricultural productivity)
Household level parameters:	
$\bar{\lambda}_i$	household i's share on the village's staple endowment
$\bar{L}_i$	labor endowment of household i
$\bar{\alpha}_q$	preference food item
$\bar{\alpha}_x$	preference non-food item
$\bar{\alpha}_l$	preference leisure
$\bar{T}_i$	cash transfer

Household i in group g has two sources of initial endowment. First, a share $\bar{\lambda}_{\{i,g\}}$ of the village's staple endowment $\bar{Q}$ (the implications of endogenizing staple production are discussed at the end of this section). Second, its stock of labor $\bar{L}_{\{i,g\}}$, net of labor needed to produce the household's staple endowment.

The household consumes staple, $q_{\{i,g\}}$, and a non-food commodity $x_{\{i,g\}}$.

Assumption A.1 The staple is produced inside *the village, while the non-food commodity is produced* outside *the village.*

Thus, the non-food commodity needs to be imported into village. Assumption A.1 reflects the fact that rural areas usually specialize in agricultural production, while importing manufactured and services from urban areas items (e.g. batteries). Household data from rural Mexico (see online appendix), for example, shows that at least 80 percent of the adult village population report agriculture as their main occupation. At the same time, about half of the monthly value of consumption are non-food items such as hygiene products (e.g. soap, combs, tooth and hair brushes, detergents, whiteners), household utensils (plates, sheets, towels, blankets etc.), industrialized clothing, tennis shoes and boots, school supplies (pens and paper), and energy (batteries, gas, petrol). Given the latters' industrialized nature and the aforementioned high share of labor force employed in agriculture, these non-food items are unlikely to be produced by the village. Importing these items implies transactions costs:

Assumption A.2 For each unit of consumption of x it is required one unit of labor.

The village price of x is consequently $p_x = \bar{p}_\chi + p_L$, where $\bar{p}_\chi$ is the factory price of x and p_L being the price of labor. The interpretation of x is not necessarily limited to consumption of imported non-food commodities. It can also be thought of as a consumed *service*. Think of, for example, a carpenter service: In this case $\bar{p}_\chi$ may be the remuneration of the wood and tools that the carpenter is using, and p_L the remuneration of the carpenter's labor.

Assumption A.3 Utilities are comparable between households, and each household maximizes a utility function that represents its reflexive, transitive, complete, continuous, and convex preferences.

The utility function of i writes $u_{\{i,g\}}(q_{\{i,g\}}, x_{\{i,g\}}, l_{\{i,g\}})$ where $l_{\{i,g\}}$ is consumption of leisure.

Assumption A.4 All agents treat prices as parametric, and no trade is permitted to take place except at equilibrium prices.

Household i chooses a consumption bundle $\{q_{\{i,g\}}, x_{\{i,g\}}, l_{\{i,g\}}\}$ which maximizes its utility function subject to the household's budget constraint:

$$\max_{q_{\{i,g\}}, x_{\{i,g\}}, l_{\{i,g\}}} u_{\{i,g\}}(q_{\{i,g\}}, x_{\{i,g\}}, l_{\{i,g\}}) \quad s.t.$$
$$p_x \times x_{\{i,g\}} + \bar{p}_q \times q_{\{i,g\}} \equiv [\bar{L}_{\{i,g\}} - l_{\{i,g\}}]p_L + [\bar{\lambda}_{\{i,g\}} \times \bar{Q}]\bar{p}_q + \bar{T}_{\{i,g\}}$$

where $\bar{T}_{\{i,g\}}$ is a cash transfer granted by the government *exclusively* to $g \in \mathbf{P}$, i.e. $\bar{T}_{\{i,P\}} > 0$ and $\bar{T}_{\{i,R\}} = 0$. Under the maintained assumption of strict quasi concavity of the utility function, the solution of the household's maximization problem will result in a demand function for the food item

$$q_{\{i,g\}} : (p_L, \bar{T}_{\{i,g\}}, \mathbf{\Omega}) \rightarrow \Re$$

the non-food item

$$x_{\{i,g\}} : (p_L, \bar{T}_{\{i,g\}}, \mathbf{\Omega}) \rightarrow \Re$$

and a labor supply function ($L_{\{i,g\}} = \bar{L} - l_{\{i,g\}}$)

$$L_{\{i,g\}} : (p_L, \bar{T}_{\{i,g\}}, \mathbf{\Omega}) \rightarrow \Re$$

as functions of the village's wage rate (p_L), the parameter vector $\mathbf{\Omega}$={$\bar{p}_\chi$, $\bar{p}_q$, $\bar{L}_{\{i,g\}}$, $\bar{\lambda}_{\{i,g\}}$, $\bar{Q}$}, and the cash transfer $\bar{T}_{\{i,g\}}$.

B. Predictions

Lets first consider the benchmark case in which there are no local general equilibrium effects (p_L is exogenous).

Proposition 2.1 ***Under assumptions A.1-A.4, we have that*** $\partial x_{\{i,P\}}/\partial \bar{T}_{\{i,P\}} > 0$ ***and*** $\partial q_{\{i,P\}}/\partial \bar{T}_{\{i,P\}} > 0$***. For*** $g \in \mathbf{R}$***, however,*** $\partial q_{\{i,R\}}/\partial \bar{T}_{\{i,P\}} = x_{\{i,R\}}/\partial \bar{T}_{\{i,P\}} = 0$***.***

Proof. A utility function which fulfills the preference requirements of assumption A.2 is the Cobb-Douglas utility function[4]

$$u_{\{i,g\}}(q_{\{i,g\}}, x_{\{i,g\}}, l_{\{i,g\}}) = q_i^{\{\bar{\alpha}_q\}}\, x_i^{\{\bar{\alpha}_x\}}\, l_i^{\{1-\bar{\alpha}_q-\bar{\alpha}_x\}} \quad \text{with } \; 0 < \bar{\alpha}_q + \bar{\alpha}_x < 1.$$

Utility maximization then yields demand and labor supply functions of the form

$$\begin{aligned} q_{\{i,g\}} &= \bar{\alpha}_q[\bar{L} \times p_L + \bar{\lambda}_{\{i,g\}} \times \bar{Q} \times \bar{p}_q + \bar{T}_{\{i,g\}}]/\bar{p}_q && (1) \\ x_{\{i,g\}} &= \bar{\alpha}_x[\bar{L} \times p_L + \bar{\lambda}_{\{i,g\}} \times \bar{Q} \times \bar{p}_q + \bar{T}_{\{i,g\}}]/[\bar{p}_\chi + p_L] && (2) \\ L_{\{i,g\}} &= \bar{L}_{\{i,g\}} - [1 - \bar{\alpha}_q - \bar{\alpha}_x][\bar{L} \times p_L + \bar{\lambda}_{\{i,g\}} \times \bar{Q} \times \bar{p}_q + \bar{T}_{\{i,g\}}]/p_L && (3) \end{aligned}$$

Deriving with respect to $\bar{T}_{\{i,P\}}$ yields $\partial q_{\{i,P\}}/\partial \bar{T}_{\{i,P\}} = \bar{\alpha}_q/\bar{p}_q > 0$, and $\partial x_{\{i,P\}}/\partial \bar{T}_{\{i,P\}} = \bar{\alpha}_x/[p_\chi + p_L] > 0$, and $\partial L_{\{i,P\}}/\partial \bar{T}_{\{i,P\}} = -[1 - \bar{\alpha}_q - \bar{\alpha}_x]/p_L < 0$. For $g \in \mathbf{R}$ we have $\partial x_{\{i,R\}}/\partial \bar{T}_{\{i,P\}} = \partial q_{\{i,R\}}/\partial \bar{T}_{\{i,P\}} = 0$ and $\partial L_{\{i,R\}}/\partial \bar{T}_{\{i,P\}} = 0$. ■

The cash transfer generates a positive income effect for $g \in \mathbf{P}$. Demand for $q_{\{i,P\}}$, $x_{\{i,P\}}$ and $l_{\{i,P\}}$ increases.

The income effect for $g \in \mathbf{R}$ is zero and, consequently, demand remains unchanged. Existing empirical evidence, however, rejects these predictions. Angelucci and De Giorgi (2009), for example, show that food consumption

[4]Proposition 2.1, however, holds for every other utility function which fulfils the preference requirements of A.2.

of non-poor households increases.

Now, lets allow for local general equilibrium effects. The village's labor market equilibrium writes

$$\sum_g \sum_i L_{\{i,g\}} \equiv \sum_g \sum_i x_{\{i,g\}} \tag{4}$$

where the left hand side is the village's aggregate labor supply. By assumption A.2, the right hand side is the village's aggregate labor demand. Equation (4) assumes that the village labor market is local (i.e. limited to the village's population). This assumption is corroborated by, first, data from the 2002 *Encuesta Nacional de Hogares Rurales* (a representative household survey of rural Mexico), where only six percent of adult village residents report to do non-agricultural work in a different village. We are not aware of any studies looking at *cross-village* migration in Mexico. Existing studies exploit the Mexican census, where respondents are asked the state in which they were born. It is however difficult to conclude from cross-state migration about cross-village migration, because it is unclear to which extent cross-state migration simply reflects rural-to-urban migration. But several factors suggest the magnitude of cross-village migration to be rather low. First, land markets are often imperfect, which may constrain the acquisition of land of emigrants (Finan et al., 2005). Second, formal credit and insurance markets are imperfect and informal insurance networks within the village a dominant source of insurance (Fafchamps and Lund, 2003). Thus, emigration is costly, because it may disconnect emigrants from these networks.

Second, there are usually no large supermarket/retail chains (which receive products from its urban area headquarters) in rural villages. This implies that village residents' labor is needed to import non-food (industrialized) products from urban areas.

PROPOSITION 2.2 ***(Endogenous wages) Under assumptions A.1-A.4,*** $\partial q_{\{i,R\}}/\partial \bar{T}_{\{i,P\}} > 0$***. However,*** $\partial x_{\{i,R\}}/\partial \bar{T}_{\{i,P\}} > 0$ ***only if (1) labor endowment of*** $i \in \mathbf{R}$ ***is sufficiently large, or (2) staple endowment of*** $i \in \mathbf{R}$ ***is sufficiently low.***

Proof. Substituting (2) and (3) into (4) and solving for p_L yields

$$p_L^* = \frac{\bar{p}_\chi[(\bar{\alpha}_x - 1)(\bar{Q} \times \bar{p}_q + \sum_i \bar{T}_{\{i,P\}})]}{\sum_i \bar{T}_{\{i,P\}} + \bar{Q} \times \bar{p}_q - \bar{\alpha}_x \bar{p}_\chi \sum_g \sum_i \bar{L}_{\{i,g\}}}. \tag{5}$$

Note that the equilibrium wage is positive only if

Assumption A.5: $\bar{\alpha}_x \sum_g \sum_i \bar{L}_{\{i,g\}} > [\sum_i \bar{T}_{\{i,P\}} + \bar{Q} \times \bar{p}_q]/\bar{p}_\chi$,

i.e. if the village's aggregate labor endowment is large enough to allow the village's aggregate consumption demand for x to be satisfied. Substituting (5) into (1) and deriving with respect to $\bar{T}_{\{i,P\}}$ yields $\partial q_{\{i,R\}}/\partial \bar{T}_{\{i,P\}} > 0$. Substituting (5) into (2) and deriving with respect to $\bar{T}_P$ yields

$$\frac{\partial x_{\{i,R\}}}{\partial \bar{T}_{\{i,P\}}} = \frac{[\bar{L}_{\{i,R\}}\bar{p}_\chi - \bar{\lambda}_{\{i,R\}}\bar{Q}\bar{p}_q][\bar{\alpha}_x - 1]\sum_g \sum_i \bar{L}_{\{i,g\}}}{-(\sum_g \sum_i \bar{L}_{\{i,g\}}\bar{p}_\chi - \bar{Q}\bar{p}_q - \sum_i \bar{T}_{\{i,P\}})^2} \tag{6}$$

By assumption A.5 the denominator in equation (6) is always negative. Since $\bar{\alpha}_x < 1$, the term $[\bar{\alpha}_x - 1]$ in the numerator is always negative. Consequently, the sign of $\partial x_{\{i,R\}}/\partial \bar{T}_{\{i,P\}}$ will depend on the sign of the first term in brackets in the numerator. We have that

$$\frac{\partial x_{\{i,R\}}}{\partial \bar{T}_{\{i,P\}}} > 0 \;\text{ if } \bar{L}_{\{i,R\}}/\bar{\lambda}_{\{i,R\}}\bar{Q} > \bar{p}_q/\bar{p}_\chi. \quad \blacksquare \tag{7}$$

The intuition behind proposition 2.2 is the following: The cash grant increases cash recipients' demand for non-food items. Importation of these items requires labor. The village's labor demand increases, raising the village's equilibrium wage.[5] A higher wage, however, has an a priori ambiguous effect on non-food consumption of the remainder of the village population not receiving cash transfers. On the one hand, higher wages imply a positive income effect which ceteris paribus increases consumption of non-food items. On the other hand, because $p_x = p_\chi + p_L$, higher wages raise the village price of these non-food items, making their consumption more expensive. Non-food consumption of non-poor households decreases if the price effect outweighs the income effect. Whether this is the case, according to condition (7), depends on a non-poor household's endowments. In section 5 we will discuss in detail the crucial role of endowments.

[5]

$$\frac{\partial p_L^*}{\partial \bar{T}_{\{i,P\}}} = \frac{\bar{\alpha}_x[\bar{\alpha}_x - 1]\bar{p}_\chi^2 \sum_g \sum_i \bar{L}_{\{i,g\}}}{-[\bar{\alpha}\sum_g \sum_i \bar{L}_{\{i,g\}}\bar{p}_\chi - \sum_i \bar{T}_{\{i,P\}} - \bar{Q} \times \bar{p}_q]^2} > 0$$

C. Discussion

The model is simple in that there is no credit and insurance market, and schooling conditionalities that come with Oportunidades are disregarded. Furthermore, the model does not allow for changes in agricultural production, and assumes away transaction costs for staple exports. In the following we discuss the implication of these omissions.

(i.) School enrollment, classroom attendance, and social interactions
Higher payments are made to households that send their children to school. Paul Schultz (2004) finds a significant impact of *Oportunidades* on school enrollment and school attendance. In our model, school enrollment can be interpreted, at least in the short term, a as reduction in a household's net labor endowment. Consider the stylized case where the loss of child income corresponds exactly to the value of the *Oportunidades* payment. In this case, a poor household's budget constraint does not change. Demand for food, non-food, and leisure remains unaffected, but labor supply decreases. Lower labor supply drives up equilibrium wages. As in proposition 2.2, for non-poor households, higher wages imply an increase in food consumption but not necessarily in non-food consumption.

Bobonis and Finan (2009) and Lalive and Cattaneo (2009) find that, due to peer effects and social interactions inside the village, Oportunidades also increase school enrollment of non-poor households residing in the same neighborhood. If non-poor households enroll their children in school, the income effect from higher local wages will be lower.

(ii.) Risk sharing
Angelucci and De Giorgi (2009) find poor households to partially share the Oportunidades payment with their extended family. This would increase non-poor households' income, on top of the increase in wages. The effect on food consumption would be higher, and a decrease in non-food consumption less likely.

(iii.) Exogenous vs. endogenous agricultural production, and farmgate selling
Exogenous staple production may be a reasonable assumption in the short but not the long-run. The implications of endogenizing staple production will depend on assumptions about the agricultural market. First, con-

sider the standard separable agricultural household model setting (Singh et al., 1986), with a perfect village labor market, no transaction costs for selling staple, and a surplus-producing village, i.e. exogenous agricultural prices. The increase in local wages resulting from the presence of *Oportunidades* would cause agricultural production to fall. Second, consider the case where the village is not producing a surplus. The price of the staple becomes endogenous. Higher demand exhibits upward pressure on prices which, ceteris paribus, increases production. Agricultural production falls if higher wages outweigh the price effect. Third, in the case of transaction costs for exporting the staple, higher wages imply a decrease in a farmer's selling price hence production.

Should agricultural production fall, this would dampen the income effect from higher wages. Consequently, the increase in non-poor households' food consumption would be smaller, and a decrease in non-food consumption even more likely.

On the other hand, some non-poor household may be credit constrained and invest additional wage income into agricultural production (Gertler et al., 2012; Bianchi and Bobba, 2013). In this case, the increase in food consumption would be higher, and a decrease in non-food consumption less likely.

The model assumes that there are no transaction costs for exporting staple. Fafchamps and Hill (2005) show that farmgate selling is the most common selling method of farmers in Uganda. For Mexico, we are not aware of any quantitative study that documents the most common selling method of farmers. In field work the author conducted in about twenty *Oportunidades* villages, farmers reported to sell their harvest directly to a crop merchant who visits the village with a truck after harvest. This suggests small transaction costs for farmers to sell their produce. There may, however, be villages or regions were, for some reason, farmgate selling is not common. In this case, farmers need to transport their produce to the next regional market. If labor is the only source of transaction costs - and assuming, for ease of exposition, that the export of one unit of staple requires one unit of the village's labor - then the village price of staple is $p_q = \bar{p}_Q - p_L$, where $\bar{p}_Q$ is the exogenous market price of staple. If a non-poor household is a buyer of staple then transaction costs for food exports do further reinforce the increase in food consumption, because higher wages reduce the price of staple. The opposite reasoning holds if a non-poor household is a seller of staple.

3 Empirical Evidence: Income vs. Price Effect

A. Commodity Price Effects

As a net exporter of staple, a single village is a staple price taker. *Oportunidades* can thus not be expected to differently affect staple prices in the 320 and 186 control villages. This has been confirmed empirically by Angelucci and De Giorgi (2009), who compare staple prices in treatment and control villages, and do not find statistically significant differences.

Less is known regarding *Oportunidades*'s impact on non-food prices, because the latter are not observed from the data. We attempt to infer changes in non-food prices by looking at the extensive margin of non-food expenditure. The following linear probability model is estimated:

$$\text{expense(yes/no)}_{i,t}^{\text{non-food}} = \text{const.} + \theta\,\text{treat village}_i + \gamma X_i^{'} + \epsilon_{i,t} \quad \text{if} \quad i \in \text{ineligible} \tag{8}$$

where $\text{expense(yes/no)}_{i,t}^{\text{non-food}}$ is a dummy which takes the value 1 if household i had positive expenditures for non-food items during the past month, zero otherwise. The subscript t denotes the post-baseline data waves March 1999 (12 months after baseline) and November 1999 (18 months after baseline), respectively. The variable treat village_i is a dummy that indicates whether household i lives in a treatment village, i.e. a village where *Oportunidades*-eligible households do receive cash transfers (as opposed to control villages, where eligible households do not). Only *Oportunidades*-ineligible households are included in the regression. $X_i^{'}$ is set of controls, including state and time dummies. Standard errors are clustered at the village level in order to take into account the intra-village correlation of the individual error term $\epsilon_{i,t}$.

In the presence of price effects on non-food items one would expect, ceteris paribus, that $\theta < 0$. Higher non-food prices imply, everything else equal, a reduction in demand for non-food items - which will eventually lead to a corner solution (zero expenditure) for some households.

Table 2 displays the OLS estimates of the linear probability model in equation (8). The first column shows that *Oportunidades*-ineligible households in treatment villages are significantly more likely to have zero non-food expenditure. From column (2) to (5), which break down the result by non-food category, we conclude that the effect reported in column (1) is mainly driven by a reduction in expenditure of hygiene and households supply products. This may be seen as additional evidence for the model of section 2, which predicts price hikes for items which a village imports. Hygiene products (e.g. soap, shampoo, etc.) and households supplies (e.g. detergents) are typical examples of items that are not produced by the village, but which have to be imported from outside the village.

A note on identification: Behrman and Todd (1999) show treatment and control group samples to be balanced at baseline, and Angelucci and De Giorgi (2009) report no differential attrition rates. In terms of measurement error, ineligible households in treatment villages may underreport their expenditure in order to appear eligible for *Oportunidades.* If this would be true then one should expect ineligibles' reported non-food expenditure to be lower in treatment villages. This, however, is not the case.

B. Income Effect

Previous studies have estimated the impact of *Oportunidades* on wages. Angelucci and De Giorgi (2009) report no changes in wages. Their wage measure, however, include all sources sources of labor supply, including agricultural wage labor. Because agricultural wage labor constitutes a large share of total hours worked, yet largely being seasonal work on some commercial farm away from the village, these wage measures are not likely to capture the village's wage rate.

We construct a wage proxy which is based on economic activities that occur inside the village. Our wage proxy, ω, is calculated as the sum of reported daily profits from within-village activities (e.g. petty sales, tailoring, washing and ironing, etc.). It is then checked whether this wage proxy, ω_i, is different between treatment and control villages. The following model is estimated:

Table 2: Extensive margin treatment effects: Monthly non-food expenditure of *Oportunidades*-ineligible households (linear probability model estimates)

		by expenditure category			
	(1) non-food expenditure all categories	(2) hygiene and home supplies	(3) toys	(4) clothing	(5) shoes
treat village	-0.009***	-0.015***	0.004	-0.007	-0.001
	(0.003)	(0.006)	(0.006)	(0.016)	(0.018)
controls	Yes	Yes	Yes	Yes	Yes
number of obs	9646	9646	9646	9646	9646
R-squared	0.011	0.014	0.013	0.029	0.056

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Table shows OLS estimates for the model $\text{expense(yes/no)}_{i,t}^{\text{non-food}} = \text{const.} + \theta\,\text{treat village}_i + \gamma X'_{i,t_0} + \epsilon_{i,t}$ if $i \in$ ineligible. $\text{expense(yes/no)}_{i,t}^{\text{non-food}}$ is a dummy which takes the value 1 if household i had positive expenditures for non-food items during the past month, zero otherwise. Subscript t denotes the post-baseline data waves March 1999 (12 months after baseline) or November 1999 (18 months after baseline). treat village$_i$ indicates whether household i lives in a treatment village. X_i' is set controls, including state and time dummies. Standard errors clustered at village level.

$$\omega_{i,t} = \text{const.} + \theta\,\text{treat village}_i + \gamma X_i' + \epsilon_{i,t} \quad \text{if} \;\; i \in \text{ineligible} \tag{9}$$

Column (1) and (2) in table 3 report the resulting treatment effects. The Tobit estimate is our preferred estimate given the relatively large frequency of left-censoring in the data. The latter suggests that daily service profits of ineligible households increase by, on average, 2.3 Mexican *Peso*. The OLS estimate is lower than that, yet still statistically significant.

Columns (3) to (6), which report estimates of equation (9) using different measures of labor supply as dependent variable, provide further evidence for higher village wages. The results suggest that ineligible households in treatment villages work more hours per day and more days per months in the above mentioned within-village commercial activities.

A raise in village wages also seems in line with the results of Attanasio et al. (2012), who find an increase in child wages. Since children are more likely to work inside the village, these studies' findings may be interpreted as additional evidence that *Oportunidades* raised local wages.

Table 3: Treatment effect on Oportunidades-ineligible households' daily service profits

	dependent variable:					
	daily income		hours per day		days per months	
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	Tobit	OLS	Tobit	OLS	Tobit
treat village	0.098*	2.298**	0.034	1.080**	0.260	4.264**
	(0.053)	(0.920)	(0.053)	(0.519)	(0.170)	(1.906)
controls	Yes	Yes	Yes	Yes	Yes	Yes
number of obs	9511	9511	9529	9529	9553	9553

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Table shows estimates of the model $x_{i,t}$=const.+θ treat village$_i$+$\gamma X_i'$+$\epsilon_{i,t}$ if $i \in$ ineligible. In columns (1) and (2), the dependent variable is daily profits (past week average) made from non-agricultural within-village commercial activities (e.g. petty trade, tailoring, washing and ironing, etc.) of household i, measured in Mexican *Peso*. The exchange rate in 1999 was roughly 1 US Dollar=10 Mexican *Peso*. In columns (3) and (4), the dependent variable is a household's daily hours worked (past week average) in these activities. In columns (5) and (6), the dependent variable is a household's monthly days worked in these activities. Subscript t denotes the post-baseline data waves March 1999 (12 months after baseline) or November 1999 (18 months after baseline). treat village$_i$ indicates whether household i lives in a treatment village. Only *Oportunidades*-ineligible households are included in the regression. X_i' is set of controls, including state and time dummies. Standard errors at the village level. The top percentile of the dependent variable is excluded.

4 Effects on Consumption and Utility

A positive income effect - in the absence of a price effect on food - implies a positive effect of *Oportunidades* on food consumption of *Oportunidades*-ineligible households. That *Oportunidades* has indeed had a positive effect on food consumption of ineligible households is documented in Angelucci and De Giorgi (2009), who find positive treatment effects on food consumption of program-ineligible households.

Estimating *Oportunidades*'s effect on non-food consumption of program-ineligible households is complicated by the fact that only non-food expenditure is observed from the data, yet neither non-food prices nor quantities. In section 3, we have already provided some suggestive evidence that *Oportunidades* may have lead to a decrease in non-food consumption: *Oportunidades*-ineligible households in treatment villages are significantly less likely to have positive expenditure for non-food items (Table 2).

Another way of inferring changes in non-food quantities is by using an empirical version of the structural model introduced in section 2. In a first step, we calibrate the parameters of the model to the data available on households residing in control villages. In a second step, we conduct out-of-sample forecasts: The cash transfer is added to the income of eligible households in the control village sample. In order to test the performance of the model, we compare the resulting simulated moments to the actual moments observed in the treatment village sample. Thus, as in Todd and Wolpin (2006), we use the experimental design of *Oportunidades* as a source of model validation. The model is able to replicate quite well the moments of the treatment village sample. This increases our confidence in by the model simulated impacts of *Oportunidades* on non-food consumption. Next, we are going to describe these steps in greater detail.

A. Model equations

The model described in section 2 can be written as a set of nine equations:

Household full income is given by

$$\begin{aligned} I_P &= \bar{L}_P \times p_L + \bar{Q}_P \times \bar{p}_q + \bar{T}_P \\ I_R &= \bar{L}_R \times p_L + \bar{Q}_R \times \bar{p}_q \end{aligned}$$

Where I_P and I_R denote the full income of the average *Oportunidades*-eligible (Poor) and *Oportunidades*-ineligible (Rich) household, respectively. $\bar{L}_P$ and $\bar{L}_R$ are labor endowments, $\bar{Q}_P$ and $\bar{Q}_R$ are staple endowments, $\bar{T}_P$ is the *Oportunidades* cash transfer, p_L and $\bar{p}_q$ denote the village wage rate and staple price, respectively.
Labor supply is given by

$$\begin{aligned} L_P &= \bar{L}_P - \bar{\alpha}_l \times I_P/p_L \\ L_R &= \bar{L}_R - \bar{\alpha}_l \times I_R/p_L, \end{aligned}$$

where $\bar{\alpha}_l$ is the Cobb-Douglas preference for leisure.
Demand for non-food items is given by

$$\begin{aligned} x_P &= \bar{\alpha}_x \times I_P/p_x \\ x_R &= \bar{\alpha}_x \times I_R/p_x, \end{aligned}$$

with $p_x = \bar{p}_\chi + \bar{m} \times p_L$. $\bar{p}_\chi$ is the factory price of non-food items. $\bar{m}$ describes how many units of labor are needed to import one unit of the non-food item. $\bar{\alpha}_x$ is the Cobb-Douglas preference for non-food items.
Demand for food-items is given by

$$\begin{aligned} q_P &= \bar{\alpha}_q \times I_P/\bar{p}_q \\ q_R &= \bar{\alpha}_q \times I_R/\bar{p}_q, \end{aligned}$$

where $\bar{\alpha}_q$ is the Cobb-Douglas preference for food.
A village's labor market equilibrium is given by

$$(\bar{n}_P \times x_P + \bar{n}_R \times x_R)\bar{m} = \bar{n}_P \times L_P + \bar{n}_R \times L_R,$$

where $\bar{n}_P$ and $\bar{n}_R$ denote the number of *Oportunidades*-eligible and *Oportunidades*-ineligible village residents, respectively.

B. Calibration

We calibrate the model exploiting data available on the *control group* of the *Oportunidades* randomized control trial (March 1999 data wave). The vector of model parameters is:

$$\mathbf{\Omega} = \{\bar{p}_\chi, \bar{p}_q, \bar{L}_i, \bar{Q}_i, \bar{\alpha}_{\{q\}}, \bar{\alpha}_{\{x\}}, \bar{m}, \bar{n}_i\} \quad i \in \{\mathbf{P}, \mathbf{R}\}$$

In Mexico, the staple is corn. 78 percent of households in the control group cite corn as their main cultivated crop. Corn is also the dominant ingredient in the food consumption basket of Mexicans. A value for

Table 4: Parameter values

	average eligible HH	average ineligible HH
Village level parameters		
price of food commodity ($\bar{p}_q$)	1.8	
price of non-food commodity ($\bar{p}_\chi$)	0.1	
number of eligible households ($\bar{n}_P$)	35.3	
number of ineligible households ($\bar{n}_R$)	8.8	
labor requirement per unit of x ($\bar{m}$)	2.7	
Household level parameters		
food item endowment ($\bar{Q}_i$)	301.1	481.2
HH efficient units of labor ($\bar{L}_i$)	93.0	63.1
preference food item ($\bar{\alpha}_q$)	0.512	0.512
preference non-food item ($\bar{\alpha}_x$)	0.415	0.415
preference leisure ($\bar{\alpha}_l$)	0.074	0.074

the market price of corn ($\bar{p}_q$) can be observed directly from administrative records (Ministry of Agriculture). Monthly corn production (in kilogram) of the average *Oportunidades*-eligible household in the control group (which amounts to roughly 300kg) is taken for $\bar{Q}_P$. In an analog manner, monthly corn production (in kilogram) of the average *Oportunidades*-ineligible household in the control group (which amounts to roughly 450kg) is taken for $\bar{Q}_R$. The number of eligible households in the average control village, $\bar{n}_P$, is 35.3. The number of ineligible households in the average control village, $\bar{n}_R$, is 8.8.

Values for the following parameters can neither be obtained from administrative records nor from the *Oportunidades* RCT data:

$$\mathbf{\Lambda} = \{\bar{p}_\chi, \bar{L}_i, \bar{\alpha}_q, \bar{\alpha}_x, \bar{m}\} \quad i \in \{\mathbf{P}, \mathbf{R}\}$$

In order to obtain values for these parameters, we exploit that some of the model's endogenous variables, such as food consumption, non-food expenditure, and labor supply, are observed from the control group data. Denote this vector

$$\mathbf{Y}_{\mathbb{C}}^{RCT} = \{q_i^{RCT}, (p_x \times x_i)^{RCT}, L_i^{RCT}\}$$

where the *RCT* superscript (randomized control trial) and $\mathbb{C}$ subscript is used to indicate sample averages of the *Oportunidades* control group. Denote $\mathbf{Y}^{sim}(\mathbf{\Lambda})$ the vector of from the model simulated values of these variables. We then calibrate $\mathbf{\Lambda}$ by minimizing the standardized squared distance between $\mathbf{Y}_{\mathbb{C}}^{RCT}$ and $\mathbf{Y}^{sim}(\mathbf{\Lambda})$

$$\min_{\mathbf{\Lambda}} E = \left(\frac{\mathbf{Y}_{\mathbb{C}}^{RCT} - \mathbf{Y}^{sim}(\mathbf{\Lambda})}{\mathbf{Y}_{\mathbb{C}}^{RCT}}\right)^2$$

The full calibration writes

$$\begin{aligned}
\min_{\Lambda} E &= \sum_{i=P,R}\left(\frac{q_i^{RCT} - \bar{\alpha}_q \times I_i/\bar{p}_q}{q_i^{RCT}}\right)^2 \\
&+ \sum_{i=P,R}\left(\frac{(p_x \times x_i)^{RCT} - p_x \times x_i}{(p_x \times x_i)^{RCT}}\right)^2 \\
&+ \sum_{i=P,R}\left(\frac{L_i^{RCT} - [\bar{L}_i - \bar{\alpha}_l \times I_i/\bar{p}_L]}{L_i^{RCT}}\right)^2
\end{aligned}$$

$$s.t.$$

$$\begin{aligned}
p_x &= \bar{p}_\chi + \bar{m} \times p_L \\
I_i &= \bar{L} \times p_L + \bar{Q}_i \times \bar{p}_q + \bar{T}_i \\
x_i &= \bar{\alpha}_x \times I_i/p_x \\
(n_P^{RCT} \times x_P^{RCT} + n_R^{RCT} \times x_R^{RCT})\bar{m} &= n_P^{RCT} \times L_P^{RCT} + n_R^{RCT} \times L_R^{RCT} \\
1 &= \bar{\alpha}_x + \bar{\alpha}_q + \bar{\alpha}_l
\end{aligned}$$

where $i \in \{P, R\}$. Table 4 shows all the parameter values of the model.

C. Model predictions (out-of-sample forecasts)

Having obtained values for the parameters of the model, we then add the cash transfer to the income of the Oportunidades-*eligible* household in control villages, and solve the model. This yields a vector of (on control villages) *simulated* outcomes.

Formally, we compute the simulated treatment effect for some outcome Y_j of household i, $\theta_{\{i,j\}}^{sim}$, as the difference between *simulated* control group, $Y_{\{i,j\}}^{sim}|_{\{\mathbf{\Omega},\bar{T}_P=0\}}$, and *simulated* treatment group, $Y_{\{i,j\}}^{sim}|_{\{\mathbf{\Omega},\bar{T}_P>0\}}$.

$$\theta_{\{i,j\}}^{sim} = Y_{\{i,j\}}^{sim}|_{\{\mathbf{\Omega},\bar{T}_P>0\}} - Y_{\{i,j\}}^{sim}|_{\{\mathbf{\Omega},\bar{T}_P=0\}}$$

The predictions of the model are shown in table 5. The model predicts an increase in ineligibles' monthly food consumption of roughly four percent.

Table 5: Structural estimation *Oportunidades*'s consumption effects

	Oportunidades-ineligible Households	
	(1) observed	(2) predicted
Panel I: *Food consumption*		
control group mean (C)	756.85	731.08
treatment group mean (T)	757.34	759.44
$T - C$	14.01	28.35
(s.e)	(11.16)	
$(T - C)/C$	0.019	0.039
Panel II: *Non-food expenditure* [quantity]		
control group mean (C)	679.00[n/a]	592.51[29.17]
treatment group mean (T)	700.44[n/a]	615.49[27.09]
$T - C$	31.94[n/a]	22.98[-2.08]
(s.e)	(24.68)	
$(T - C)/C$	0.047[n/a]	0.039[-0.071]

The first row of column (1) in panel I and II show March 1999 sample means of household monthly food consumption (Mexican *Peso* value) and non-food expenditure (Mexican *Peso* value) of program ineligible households living in treatment villages. In 1999, the exchange rate was roughly 1 US Dollar=10 Mexican *Peso*. The second row of column (1) in panel I and II show March 1999 sample means of household monthly food consumption (Mexican *Peso* value) and non-food expenditure (Peso value) of program ineligible households living in control villages. The third row of column (1) in panel I and II shows the treatment effect (θ) obtained from the treatment effect regression x_i = const.+θtreat village$_i$+ϵ_i $\forall$ $i \in$ ineligible, where x_i is household monthly food consumption (panel I), and household monthly non-food expenditure (panel II), respectively. The fourth row of column (1) in panel I and II show the ratio of treatment effect (third row) over the control group mean (first row). Column (2) in panel I and II show the by the general equilibrium model simulated values. Values in [] are consumed quantities (non-food quantities are not observed from the *Oportunidades* data).

This compares to about two percent observed from the experimental data. The model predicts an increase in ineligibles' monthly non-food consumption *expenditure* of 3.9 percent. This compares to 4.7 percent observed from the experimental data.

The model replicates reasonably well the actual treatment effects. This increases our confidence in the model's simulated changes in non-food quantities. The model, however, predicts a decrease in non-food consumption of about seven percent. Overall, non-poor neighbors' utility falls by two percent.

5 Heterogenous Effects

So far, the analysis has focused on average effects. In this section we turn to heterogenous effects. Proposition 2.2 states that the neighborhood effect is

1. decreasing in a household's labor endowment, in the case of non-food consumption
2. not related to a household's labor endowment, in the case of food consumption

In the model, this is because large-labor-endowment households have lower per capita consumption, ceteris paribus, than low-labor-endowment households. Therefore, if local wages and non-food prices increase, per capita non-food consumption of the low-labor-endowment households will be more adversely affected. To see this, consider a simple household demand function for non-food, $x_i/\bar{L}_i = \bar{\alpha}_x I_i/p_x$, with $I_i = p_L\bar{L}_i + \bar{p}_q\bar{\lambda}_i\bar{Q}$ and $p_x = \bar{p}_\chi + p_L$. The notation is the same as in section 2. In per capita terms, this demand function writes $\tilde{x}_i = x_i/\bar{L}_i = \bar{\alpha}_x[p_L\bar{L}_i + \bar{p}_q\bar{\lambda}_i\bar{Q}]/[\bar{p}_\chi + p_L]\bar{L}_i$. Deriving with respect to p_L yields

$$\frac{\partial\tilde{x}_i}{\partial p_L} = \frac{\bar{\alpha}_x}{\bar{p}_\chi + p_L} - \frac{\bar{\alpha}_x[p_L\bar{L}_i + \bar{p}_q\bar{\lambda}_i\bar{Q}]}{[\bar{p}_\chi + p_L]^2\bar{L}_i}.$$

It is easy to see that $\partial\tilde{x}_i/\partial p_L > 0$ if $\bar{L}_i \to \infty$, and $\partial\tilde{x}_i/\partial p_L < 0$ if $\bar{L}_i \to 0$.

For per capita food consumption, we have $\partial\tilde{q}_i/\partial p_L = \bar{\alpha}_q/\bar{p}_q$. Thus, if local wages increase, per capita food consumption of low-labor-endowment and high-labor-endowment households should be equally affected.

In order to test these predictions, the following regression model is estimated:

$$\begin{aligned} \text{expense\_pcapita}^{\text{non-food}}_{i,t} &= \text{constant} + \theta_1 \,\text{treat village}_i \\ &+ \theta_2 \,\text{treat village}_i \times \text{hhsize\_small} \\ &+ \alpha\text{hhsize\_small} + \gamma X^{'}_{i,t_0} + \epsilon_{i,t} \\ &\quad \text{if} \;\; i \in \text{ineligible}, \end{aligned} \tag{10}$$

where $\text{expense\_pcapita}^{\text{non-food}}_{i,t}$ is per capita monthly household non-food expenditure. We are primarily interested in the regression coefficient on the interaction effect treat village$_i$ $\times$ hhsize_small, where variable hhsize_small

Table 6: Heterogenous consumption effects of *Oportunidades* on ineligible households

	dependent variable:			
	non-food consumption		food consumption	
	(1)	(2)	(3)	(4)
treat village	9.273	23.157**	12.419**	0.857
	(7.862)	(10.048)	(5.193)	(4.863)
treat×hhsize_small		-17.453*		11.324*
		(10.105)		(5.855)
number of obs	9337	9337	9272	9272
R-squared	0.004	0.013	0.008	0.055

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable in columns (1) and (2) is per capita monthly household non-food expenditure, and in columns (3) and (4) per capita monthly food consumption, all measured in Mexican *Peso*. In 1999, the exchange rate was roughly 1 US Dollar=10 Mexican *Peso*. Subscript t denotes the post-baseline data waves March 1999 (12 months after baseline) or November 1999 (18 months after baseline). treat village indicates whether household lives in a treatment village. Only *Oportunidades*-ineligible households are included in the regression. Standard errors are clustered at the village level. *hhsize_small* is a dummy indicating if the number of adult household members is below the sample average. Controls include time and state dummies, household size and land size. The top percentile of the dependent variable is excluded.

is a dummy indicating if the number of adult household members is below the sample average.

Columns (1) and (2) in table 6 report the OLS estimates of equation (10). Column (1) shows that the coefficient on the treatment village dummy is positive (though imprecisely measured) when the interaction effects treat village$_i$ × hhsize is excluded. Including the interaction effects, as column (2) shows, changes the picture. The coefficient on treat village$_i$ × hhsize_small is positive. This suggests that the impact of *Oportunidades* on ineligibles' non-food consumption, $\theta_1 + \theta_2$, is higher for households with above average number of (adult) members.

Columns (3)-(4) show the results of (10) but with per capita food consumption as dependent variable. As expected, the impact of *Oportunidades* on ineligibles' food consumption, $\theta_1 + \theta_2$, is not strongly related to the average number of (adult) household members.

6 Concluding Remarks

This paper studied how redistributive transfers the poorest households of a village affect consumption of the remainder of the village population. This is an important question when measuring the distributional (or overall welfare) impact of cash transfers. Yet, most impact evaluations focus on beneficiaries while 'spill-over' effects remain understudied. From the set of possible channels through which spill-over effects may operate, this paper explored the role of local general equilibrium effects. A structural model of a village populated by poor and non-poor households - calibrated to data from the *Oportunidades* randomized control trial - suggests that transfers to the poor households generate competing local price effects which increase food consumption, yet potentially decrease non-food consumption of non-poor households. Overall, we find non-poor households' utility to decrease by about two percent.

The lack of data on non-food consumption and prices is the main challenge of the empirical analysis. In the absence of such data, we propose a structural approach to estimate non-food quantities. The results rely on the strong assumption that the model is correctly specified. Further research, with observed data on non-food consumption and prices, would be useful to support the model's predictions. Impact evaluations of cash transfer programs usually collect data on non-food expenditure, but not on consumed quantities. We hope the results presented in this paper will encourage researchers to collect such data in the future.

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7 Tables for Online Appendix

Table 7: Descriptive statistics: monthly household consumption

	(1) October 1998	(2) March 1999	(3) November 1999
hygiene products (soap, combs, tooth and hair brushes detergents, whiteners)	40.1	50.3	59.2
household utensils (ollas, platos, cazuelas sartenes, sabanas, toallas y cobijas)	4.6	6.2	9.8
fuels (gas, carbon, petrol)	14.3	10.4	16.7
electricity (batteries, light, etc.)	21.6	24.0	25.3
industrialized clothes	55.4	116.5	105.2
shoes (tennis shoes, boots, etc.)	59.3	114.7	104.6
school supplies (pens, paper, etc.)	15.6	10.0	28.3
total non-food expenditure	344.9	468.9	495.1
total value of consumed food items	513.1	471.5	520.0

Displayed values are sample means of the control group sample. All values are in Mexican *Peso*. In 1999, the exchange rate was roughly 1 US Dollar=10 Mexican *Peso*.

Table 8: Descriptive statistics: Main occupational choice

	(1) October 1998 (in %)	(2) March 1999 (in %)	(3) November 1999 (in %)
agricultural day laborer (*jornalero*)	60.35	60.13	64.42
other employment in agricultural sector	14.93	12.72	14.87
self-employed	10.86	13.4	8.89
family business	5.06	4.72	3.77
ejidatario	6.87	5.87	6.42

Displayed values are sample means of the control group sample.

Table 9: Descriptive statistics: Non-agricultural labor supply

	(1) yes/no (in %)	(2) hours per day	(3) days per week	(4) revenue per month
October 1998				
tailoring	1.14	4.3	4.3	220.2
preparing food for sale	0.72	5.6	3.7	284.9
construction/carpenter	0.79	8.7	5.5	560.1
buying and reselling	2.17	7.0	5.7	478.6
transport	0.15	5.6	2.9	1127.5
fixing items	0.01	8.0	6.0	400.0
wash, iron, cooking for pay	1.19	5.3	3.0	102.0
other	3.84	7.0	5.3	1052.5
March 1999				
tailoring	0.77	4.2	4.1	140.8
preparing food for sale	0.35	5.3	4.2	281.1
construction/carpenter	0.87	8.5	5.2	478.1
buying and reselling	2.42	7.3	6.4	394.4
transport	0.09	5.1	3.5	1162.5
fixing items	0.02	8.0	3.5	1030.0
wash, iron, cooking for pay	1.04	5.5	3.2	164.1
other	2.00	6.5	5.1	442.1
November 1999				
tailoring	0.48	4.0	4.2	242.7
preparing food for sale	0.24	4.7	3.4	275.9
construction/carpenter	0.24	8.1	5.1	900.0
buying and reselling	0.51	7.1	5.0	338.7
transport	0.05	8.2	3.4	466.0
fixing items	0.00	.	.	.
wash, iron, cooking for pay	0.69	5.4	3.0	112.6
other	0.59	6.2	5.0	582.4

Values shows are sample means of the control group. Values in columns (2) to (4) are conditional on 'yes' in column (1).

Table 10: Descriptive Statistics: Counterfactual characteristics

	Eligible Households	Ineligible Households
	(1)	(2)
	Mean	Mean
	[Std.Dev.]	[Std.Dev.]
Household and Community Characteristics		
Gini Index for agricultural land ownership	0.71	
	[120.7]	[124.9]
Pre-program household poverty score	701.6	882.5
	[120.7]	[124.9]
Monthly food consumption (per capita, peso value)	182.5	198.4
	[163.6]	[153.2]
Monthly food expenditure (per capita, peso value)	137.3	169.6
	[130.1]	[145.4]
Monthly non-purchased food consumption (per capita, peso value)	38.85	27.86
	[591.9]	[48.1]
Monthly household disposable income (in peso)	662.1	795.3
	[362.6]	[2129.8]
Cultivated area (in hectare)	0.46	0.75
	[2.77]	[2.31]
Hourly wage rate	5.27	6.97
	[36.14]	[25.12]
Livestock holding (principal component index)	-0.21	0.06
	[2.41]	[3.63]
Household size	5.44	4.82
	[2.60]	[2.53]
Indigenous household head	0.36	0.17
	[0.48]	[0.37]
Education of head		
no	32.55	26.35
primary	62.03	64.52
secondary	4.92	6.95
tertiary	0.51	2.19
N	6857	1949

Calculations are based on the March 1999 Encel survey. Column (1) displays the sample mean of *Oportunidades*-eligible households residing in control villages. Column (2) displays the sample mean of *Oportunidades*-ineligible households residing in control villages. Standard deviations are reported in brackets. Differences between column (1) and (2) are all statistically significant.