The Long-run Development Impacts of Agricultural Productivity Gains: Evidence from Irrigation Canals in India*

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Abstract

How do investments in agricultural productivity translate into development and structural transformation? An extensive literature has addressed these questions, dating back to the earliest days of development economics. In this paper, we estimate the long-run impacts of India's irrigation canals, which span over 300,000 km and deliver water to over 100,000 villages. Drawing on high-resolution data on every household, firm, village, and town in India, we use three empirical strategies to characterize the direct and spillover effects of large increases in agricultural productivity. First, we exploit the gravity-driven nature of canal irrigation in a regression discontinuity design with elevation as the running variable. Second, we study spillovers by comparing untreated settlements close to canals to those farther away. Third, we use a 100-year panel of urban populations to estimate the effects of canals on regional urbanization. In the long run, canal access drives substantially higher irrigation intensity and land productivity. These changes result in higher population density, but treated areas experience no structural transformation: there are no changes in the share of the workforce outside of agriculture, or even in agro-processing. Consumption gains accrue only to landowners; we estimate a tight null effect on the consumption of the 60% of the population with little or no land. Structural transformation does occur, but through the growth of regional towns rather than through sectoral reallocation of labor within treated villages or their near neighbors. Our findings are consistent with a model where labor is mobile in the long run, and where urban areas have productivity advantages that cause most non-farm growth to occur in towns. In the long run, the substantial productivity effects of canals were equilibrated through the movement of labor across space rather than within locations across sectors.

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1 Introduction

The link between agricultural productivity and structural transformation has long been a central concern of development economics (Lewis, 1954; Johnston and Mellor, 1961; Mellor, 1986; Schultz, 1964). Authors such as Johnston and Mellor (1961), echoed later by Mellor (1986) and Timmer (1988), argued that agricultural productivity growth was an essential precursor for broader structural transformation and long-run economic growth. This early literature held that productivity growth in agriculture could have the seemingly paradoxical effect of shrinking the agricultural sector as a share of the total economy. Building on the insight that food is an essential good for the poor, agricultural development economists invoked a class of models in which countries that are unproductive in agriculture must devote large shares of labor and other resources to meet their food needs.[1] A critique of these models is that they depend heavily on a closed economy assumption. Matsuyama (1992) showed that in an open economy, increases in agricultural productivity can cause *specialization* in agriculture via comparative advantage.[2]

When testing these theories empirically, the extent to which goods and labor are mobile will depend on the geographic scale of analysis and the time horizon studied. This paper is concerned with understanding the long run effects of technical change in agriculture on the broader economy, with a particular focus on how individuals move across sectors and across space at different geographic scales.

We study the impacts of one of the most significant episodes of agricultural productivity change of the past two centuries: the construction of India's massive irrigation canal network. This network of canals—essentially artificial rivers that carry water into dryland areas—spans over 300,000 km and serves over 130,000 villages, nearly one in four in India. Canals have been especially important in enabling irrigation during the relatively dry winter cropping season. Canals were historically the most important source of irrigation in India, and even in the 21st century they are the second largest

[1] Schultz (1953) referred to this phenomenon as the "food problem". The same mechanism lies at the heart of more recent work, which relied on non-homothetic preferences as the main driver of structural transformation (Gollin et al., 2002, 2007; Alvarez-Cuadrado and Poschke, 2011). The link between agricultural productivity growth and structural change also emerges in other models where productivity growth leads to endogenous changes in the relative price of agricultural goods (Ngai and Pissarides, 2007).

[2] Bustos et al. (2016) also use an open economy framework, where agricultural productivity growth leads to an expansion of agricultural output, but whether this increases or reduces the use of labor in agriculture depends on the factor bias of technical change.

source of irrigation in India after groundwater, providing water to agricultural regions with over a quarter billion inhabitants. In 2011, fully 57% of rural Indians lived within 10 km of a canal.[3]

These canals are an ideal context for studying the long-run impacts of technical change in agriculture for two reasons. First, they drive large and sustained differences in productivity across otherwise similar locations. Second, they were built many decades ago: half before 1981 and many before 1900. Other agricultural interventions tend to diffuse across space over time, making it more difficult to study their long-run impacts.

Studying the effects of canals on the geography of structural transformation requires detailed, high resolution data. We combine microdata from business and household censuses, administrative records, geospatial datasets, and satellite imagery to measure irrigation, agricultural activity, living standards, and non-farm economic activity for all of India's 600,000+ settlements (villages and towns). Our main outcomes were recorded in 2011-2013, over 40 years after the beginning of construction for the median canal and 30 years after the median canal was declared complete. We therefore study the long-run impacts of India's canal network.[4]

We can think of canals as having effects at four different geographies: (i) direct effects in the settlements that they serve with surface irrigation; (ii) indirect effects in nearby unserved settlements; (iii) effects in regional urban markets; and (iv) effects at much broader geographies that could diffuse across the entire country or world. We employ distinct identification strategies to measure effects (i), (ii), and (iii); like much of the literature on the effects of place-based policies, we are unable to provide empirical evidence on universal effects.

To measure direct effects on canal-irrigated areas, we use a regression discontinuity design (RDD) that exploits the gravity-driven nature of canal water distribution, with elevation relative to the nearest canal as the running variable. Canal placement is determined by engineering specifications and topography, and water from canals only flows downhill, treating settlements topographically below

[3] We study India's network of major and medium canals, for which data is maintained by the national Ministry of Water Resources. Smaller surface irrigation projects, such as channels diverting water from village tanks (small artificial reservoirs) or streams to farmers' fields are not included in this analysis.

[4] We are unable to estimate short-run effects as most canals with more recent completion dates appear to have been built many decades ago but have been rehabilitated more recently.

the canal. Settlements a short distance away from a canal but only a few meters higher than the canal will thus experience no irrigation benefit and can serve as a control group for the irrigation treatment.

The RDD analysis tests for long-run differences between below-canal settlements and above-canal settlements. This comparison does not capture the full effects of canals if there are spillovers into above-canal settlements. In the presence of local labor or goods market linkages, untreated settlements above the canal could increase demand for both agricultural and non-agricultural labor. Alternately, canals could recharge regional groundwater tables, increasing access to pump irrigation above the canal.

To measure these more diffuse effects, we compare settlements directly above canals to settlements that are in the same district but are more distant from canals, using entropy balancing (Hainmueller, 2012) that reweights distant settlements to ensure a comparison group with similar distributions of natural characteristics (climate, topography, and agricultural potential).[5]

We expect most spillovers to decay with distance; estimating differences between untreated settlements near and far from canals thus provides a test of the existence of spillovers, as long as they are smaller for more distant settlements.[6] Finally, to capture concentrated effects of canals in nearby urban areas, we use a hundred year panel of town populations, the only high resolution panel data available in the era of canal construction. We use a difference-in-differences design that studies town growth before and after regional canals are built, following Callaway and Sant'Anna (2020).

The RDD analysis reveals sharply improved agricultural outcomes in the settlements directly treated by canals. Treatment settlements have more irrigated acreage, increased land under cultivation, a higher likelihood of growing water-intensive crops, and greater estimated yields.[7] The yield effects are observed entirely in the relatively dry winter (*rabi*) season: canals improve water access in a second cropping season but generate no significant differences during the summer (*kharif*) growing season, when monsoon rains generally provide sufficient water. There are no spillover effects

[5]We show robustness to using an alternate methodology for reweighting distant settlements, coarsened exact matching (Iacus et al., 2012), which discretizes continuous geophysical variables and reweights according to these coarsened bins, discarding distant settlements that do not fall into bins containing canal settlements.

[6]If spillovers extend frictionlessly to the entire country, then they are impossible to measure given the kind of cross-sectional data that is available.

[7]In the absence of high resolution directly-measured yield data, we use a satellite-derived proxy that estimates biomass added in a village over the course of a growing season.

on agricultural outcomes: irrigation levels, yields, and land use in above-canal settlements are highly similar to those in more distant settlements.[8] The sharp differences in agricultural outcomes between above-canal and below-canal settlements have been sustained over many decades, making them a useful natural experiment for studying what happens to the rest of the economy after large-scale gains to agricultural productivity.[9]

Turning to measures beyond agriculture, we find that the agricultural changes brought about by canals cause substantial population growth but ultimately little local structural change. Below-canal settlements have sharply higher population density, with no measurable spillovers into above-canal areas. Below-canal, above-canal, and distant settlements have highly similar shares of workers employed in manufacturing, services, and even in agro-processing. There is evidently an increased demand for labor (since population density is higher in canal-irrigated areas), but these highly agricultural settlements are not developing substantial non-farm sectors. Our town panel analysis, however, finds concentrated population gains in proximate urban areas in the decades following nearby canal construction. We do not observe the historical sectoral allocation of work in the town panel, but can observe that towns have an average 0.248 non-farm jobs per adult compared to 0.131 per adult in the more non-agricultural villages. Therefore town growth implies at least a regional shift in the economy toward non-farm work. A back-of-the-envelope calculation suggests that the net population gain is considerably higher in rural than urban areas, and thus that canals increase the agricultural intensity of the regional workforce, despite inducing some urban growth.

Canals have heterogeneous effects on living standards. We estimate consumption for every household in India using small area estimation Elbers et al. (2003). We find canals produce no significant gains to estimated consumption for the 60% of households who own little to no land. In contrast, households with landholdings above 1.32 acres are wealthier, with effects increasing in household

[8] Notably, above-canal settlements do not have higher irrigation from any source, including groundwater. This rules out the possibility of spillovers through a groundwater recharge channel.

[9] The RDD results are robust to a range of alternate specifications, including an alternate regression discontinuity using distance to the officially designated command area boundary of the canal. The command area is the engineers' definition of the total area that theoretically has access to irrigation water from a given canal. This definition exploits finer details of local topography, but risks endogeneity if command area boundaries were drawn such that they intentionally include or exclude certain locations.

land. We do find evidence of some consumption spillovers into above-canal settlements, but only for landed households and with much smaller effects than for landowners in below-canal settlements.[10]

We interpret our results in the context of a multi-sector, multi-location model that is closely related to the prior literature (Matsuyama, 1992; Bustos et al., 2016), but that captures two key features of our context. First, we model labor as fixed in the short run and mobile in the long run. Second, we assume that towns have productivity advantages in non-farm work relative to villages. Our model highlights several insights from our empirical results. In the long-run spatial equilibrium, increased demand for labor is met by an increase in the number of laborers, eliminating differences in wages across space. Workers still benefit, but the gains are spread across a large linked labor market. Returns to land, the fixed factor, remain higher even in the long run. Structural transformation occurs through the growth of towns, rather than through the relative growth of the non-farm sector in rural areas.

This paper extends a substantial literature linking technical change in agriculture to industrialization.[11] Foster and Rosenzweig (1996) and Foster and Rosenzweig (2004b) studied the impact of the Green Revolution on structural change and educational attainment in a panel of villages across India, finding that agricultural gains raised wages and inhibited industrialization. In the US, Hornbeck and Keskin (2015) estimated only short-run benefits to increased access to irrigation due to the tapping of the Ogallala Aquifer, with no long-run structural transformation at the country level. Bustos et al. (2016) found that the introduction of a second harvesting season for maize in Brazil depressed structural change in Brazilian municipalities, whereas the spread of genetically modified soybeans, which represented labor-augmenting rather than land-augmenting technical change, increased the exit of labor form agriculture. Similar to the effects we find on urban growth, Bustos et al. (2020) found that agricultural productivity gains drove urbanization through the flow of land rents to

[10] These spillovers could be due to improved access to irrigation that we cannot detect in our irrigation measures, or increased demand for the types of labor or goods produced disproportionately by landed households. For example, landowners in above-canal settlements may be more likely to own firms that serve the increased demand from below-canal settlements. The limitations of our data prevent us from delving further into this result.

[11] There is also a large body of evidence on responses to transient agricultural productivity shocks due to weather. Emerick (2018) finds that non-tradeable employment increases in districts experiencing positive agricultural productivity shocks, consistent with our model of demand-driven structural change. Adhvaryu et al. (2013) and Colmer (2021) show that flexible labor markets are key to helping workers adapt to negative temperature shocks. This paper speaks less to this literature on transient shocks because we study how people adjust to large, permanent changes in agricultural productivity.

cities.[12] Our study of the direct and indirect effects of canals at different geographic scales may help to resolve some of the differences in this literature. Our empirical and theoretical results show that we should expect differential effects depending on whether areas are rural, where the direct comparative advantage effects of agricultural productivity gains may outweigh demand or capital channels that would induce non-farm growth, while in proximate urban areas the opposite is likely to be true. We study much smaller geographic units than those in much of the literature; it is possible that county- and municipality-level effects in the US and Brazil are the result of both the local and regional spillover effects that we document at high resolution.[13]

We also contribute to the literature on how labor flows respond to economic shocks. A rich body of research documents how both temporary (Imbert and Papp, 2020) and permanent (Greenstone et al., 2010; Allcott and Keniston, 2018) migration respond to economic shocks in both high- and low-income countries. It is notable that much of the prior empirical work has aimed to study competition for workers between the farm and non-farm sectors in models that shut down the labor mobility channel. This is partly for the reason that mobility is typically much lower in the short- to medium-run periods examined in prior studies. Indeed, in an extension of their main results, Bustos et al. (2016) find that about one-third of the shift out of agricultural employment in soybean areas occurred via migration, over only a 10-year sample period. Our much longer-run analysis suggests that it can be the primary adjustment channel to agricultural change. Indeed, the very nature of structural transformation around the world has involved the movement of billions of people from farms to cities, sometimes across large distances.[14]

Our results also generate further evidence on the high barriers to rural industrialization. Asher and Novosad (2020) and Burlig and Preonas (2021) find that major investments in rural roads and electrification respectively have limited effects on non-farm activity in India.[15] Faber (2014)

[12] An example of this capital channel was discussed at length in the context of colonial Bengal in Bose et al. (1993).

[13] Our results also echo the predictions of Foster and Rosenzweig (2004a), which suggested that agricultural productivity shocks have substantially different effects on landowners and the landless, consistent with our findings.

[14] While there is a widespread idea in the literature that permanent migration in India is rare, this claim appears to arise from the set of rural men who migrate for work. Over 25% of women have changed residence at least once in their lives, and lifetime migration rates for men approach 15% (Kone et al., 2018). Since we only observe population density in the present, we cannot distinguish migration from other mechanisms of population change. However, we show that there are at least no contemporary effects of canals on fertility or mortality, suggesting some combination of higher in-migration and reduced out-migration.

[15] Asher and Novosad (2020) find that the main impact of roads is to provide access to non-agricultural labor markets

actually finds that highway construction through peripheral areas in China caused deindustrialization. Infrastructure investments in rural areas may improve well-being and may motivate in- and out-migration, but are unlikely to cause substantial changes in *in situ* non-farm opportunities. Our results are also consistent with long run evidence that the Green Revolution had substantial positive effects on structural change (Gollin et al., 2021); our analysis suggests that this process may have been driven by the growth of cities, rather than in the rural areas directly experiencing agricultural productivity gains.

Finally, our work adds to a growing literature estimating the impacts of access to irrigation. Sekhri (2014) shows that access to groundwater irrigation lowers poverty. Blakeslee et al. (2021b) find that the drying up of wells actually induces agricultural households to provide more non-farm labor. Jones et al. (2020) study canal irrigation in Rwanda using an elevation-based RDD, finding that labor market frictions limit the returns and thus lower adoption of irrigation. In a concurrent paper, Blakeslee et al. (2021a) study canals in India using a command area distance RDD.[16] While they find similar reduced form effects on population density and (lack of) structural change in canal-irrigated villages, their analysis does not consider spillovers and is focused primarily on evaluating canals as infrastructure investments, rather than as drivers of long-run agricultural change.[17]

Our paper proceeds as follows. Section 2 provides background on India's irrigation canals. Section 3 describes our conceptual framework for understanding how agricultural productivity gains spill over into other economic outcomes. Section 4 describes the data sources and Section 5 lays out our multiple empirical strategies. Section 6 presents our results and Section 7 discusses their interpretation. Section 8 concludes.

2 Context

As a semi-arid region with a highly variable monsoon climate, South Asia has long depended on irrigation for its agricultural productivity. For much of history, this has primarily involved gravity flow

outside the village. This result is suggested by our model, where towns have productivity advantages for non-farm work.

[16] This is analogous to the secondary identification strategy we use to show robustness of the primary elevation-based RDD.

[17] Blakeslee et al. (2021a) differ from us in arguing that canals *reduce* city size directly in their command area. Our analysis uses a time series of urban populations and finds that new towns emerge in the vicinity of canals and that canals boost overall population in treated areas.

surface irrigation through canals of various types. It has been estimated that, at the end of the 19th century, India's 12 million hectares of irrigated land amounted to 4 times that of the United States and 6 times that of Egypt (Shah, 2011). The British oversaw the construction of vast canal networks, often privately funded and yielding high returns, until the end of the Raj in 1947. Canals were used to divert water from India's major rivers to its arid regions, where they facilitated settlement of otherwise uninhabitable land, such as with the Punjab Canal Colonies (Douie, 1914). After gaining independence, the Government of India prioritized canal-building as it sought to avoid mass hunger during a period of high population growth (Mukherji, 2016). Later, canals were built to provide irrigation for the input-intensive high-yielding varieties of food crops that powered India's Green Revolution.

While groundwater eclipsed canals as India's preeminent source of irrigation by the 1970s (Shah, 2011), surface irrigation remains critical to the livelihood of millions of farmers across India. In recognition of the importance of canals, the central government launched the Accelerated Irrigation Benefit Program (AIBP) in 1997. By 2011 it had spent more than $7.5 billion to help finish stalled large-scale irrigation projects (Shah, 2011). More recently, states such as Madhya Pradesh have improved canal irrigation by increasing maintenance, distributing water more equitably, investing in last mile distribution networks, reducing political interference, and building cooperation with farmer organizations (Shah, 2011). According to the most recent estimates, canals still account for approximately one-fourth of the net irrigated area in India (Jain et al., 2019), although estimates vary according to the methodology.

Figure 1 shows the distribution of completion dates of India's major and medium canals, which are the focus of this study.[18] Construction rates increased following India's independence in 1947, although post-independence canals are generally shorter than those constructed under the British Raj in the 19th and early 20th century. By 2012, the main year in which we measure outcomes, 51% of India's 600,000 settlements were within 10 km of a major or medium irrigation canal, with a median canal construction start year of 1968 and completion year of 1981. Given that our primary outcomes are measured in 2011-2013, we thus study the impacts of canals that are typically at least thirty years old.[19]

[18]Major canals are defined as serving 10,000 or more hectares while medium canals serve areas 2,000–10,000 hectares. Canals serving 2,000 hectares or less are termed minor canals and are not included in this study.

[19]In fact, median canal age is likely much greater, as our manual validation reveals that official canal construction dates often apply to rehabilitation projects rather than original construction, especially for those reporting completion af-

3 Conceptual Framework

Our theoretical framework builds on a substantial literature modeling the effects of agricultural productivity change on the non-farm sector (Johnston and Mellor, 1961; Matsuyama, 1992; Bustos et al., 2016; Foster and Rosenzweig, 1996, 2007). Perhaps the most familiar models in this literature are those in which an increase in agriculture's productivity (crucially, in a closed economy) leads to a decline in the relative price of agricultural goods. This in turn lowers the returns to inputs used in this sector and induces a movement of productive resources into non-agricultural sectors. This mechanism lies at the heart of Johnston and Mellor (1961) and similar subsequent papers (including, for example, Gollin et al. (2002)).

However, this simple story of structural transformation is sensitive to variation in a few central assumptions. The first key element is whether an economy is open or closed. As Matsuyama (1992) pointed out, agricultural productivity growth in a small open economy will not generate any price effect; and indeed, comparative advantage tends to imply that an economy with high agricultural productivity will specialize (partly or fully) in agriculture. A second central assumption relates to the consumption substitutability of agricultural goods for other goods. As shown in Ngai and Pissarides (2007), if agricultural and non-agricultural goods are gross substitutes, then agricultural productivity growth will tend – even in a closed economy – to lead to specialization in agriculture. If the two types of goods are gross complements, then resources will tend to flow into the slow-growing sector; so agricultural productivity growth will drive structural transformation. A third key assumption relates to the production technology and the demand for inputs. As shown in Bustos et al. (2016), patterns of structural change will depend on how new agricultural technologies affect the relative demand for labor and land. A fourth key assumption concerns the supply of inputs; as shown in Foster and Rosenzweig (2007), the impacts of productivity growth in agriculture will differ in an economy where capital moves freely from one where capital is fixed, and where non-agricultural goods face corresponding limits to demand.

The sensitivity of theoretical predictions to these assumptions implies a need to choose a model

ter 1990. Additionally, irrigation projects often comprise multiple elements. Completion dates indicate the year in which the last element was finished, meaning other elements from the project could have been completed and functioning earlier.

structure that is appropriate for the context under consideration. For this reason, we choose a model structure in this paper that reflects two key characteristics of our empirical context. First, we model an economy in which labor flows freely across space in the long run, although not in the short run. This offers a contrast to many models that allow for labor mobility across sectors but not across locations.[20] Second, we allow for spatial variation in non-agricultural productivity, such that larger settlements within a region have a productivity advantage in producing non-agricultural goods. These two assumptions together give rise to a rich theoretical framework that broadly mirrors our empirical setting.

We focus on a representative rural region. This corresponds to a stylized view of India's rural economy; we think of this rural economy as consisting of a large number of predominantly local sub-economies that are embedded in a larger national economy. Each rural region features an expanse of agricultural land, divided into villages, typically with a larger market town that serves as an economic center. The villages that surround each market town are mostly small; in 2011, the median village population in India was approximately 1000. Most villagers work in agriculture. Agricultural land is in general privately owned and managed. Most farms are small (Foster and Rosenzweig, 2017), and many landowners work on their own land. Farms may also hire workers from a large pool of landless workers, who comprise the majority of the labor force.

These observed features of the data give shape to our conceptual framework. In this section, we describe the assumptions and implications of our model. The full model is explained and solved in Section A.

Our model economy consists of two spatial units: villages and towns. A region consists of a single town and its surrounding villages, with their agricultural land. These regions are embedded in a larger economy. The economy has three goods: an agricultural good, a non-agricultural non-tradeable good that can only be consumed in the region, and a manufactured good that is not produced in the region. Both the agricultural and manufactured goods are tradeable, and the region is a price taker with respect to both. The production of the agricultural good requires both labor and land, while the non-tradeable good is produced only using labor. Agricultural and non-agricultural productivity

[20] In this respect, we are most closely related to Foster and Rosenzweig (2007), who recognize the importance of factor mobility – although in their case, the mobile factor is capital rather than labor.

is allowed to vary across locations, with the assumption that towns are more productive than villages in the production of the non-agricultural good. We assume log-linear preferences over all three goods.

We are interested in the impacts of irrigation canals both in the short run and the long run. As other researchers have argued, labor mobility in rural India faces significant frictions in the short run (Foster and Rosenzweig, 2007; Munshi and Rosenzweig, 2016) although Foster and Rosenzweig (2007) find male out-migration from villages of over 20% when considering a longer (17 year) period. Our conceptual framework thus considers three distinct time periods: a baseline before the construction of the canals; a short run after canals have been constructed but before labor has adjusted to the changes in productivity; and a long run in which a spatial equilibrium holds, and in which real wages are equalized across locations. This long-run labor mobility is a key feature of our theoretical framework and also of our empirical work. Allowing for labor mobility leads to important differences relative to prior models.

In our framework, positive agricultural productivity shocks, such as the investments in irrigation canals studied in this paper, will lead to short-term growth in local demand for land and labor, driving up wages and land rents, and leading to higher incomes in treated villages for both landowners and landless workers. This generates a rise in the price of the local non-tradeable good, inducing the reallocation of labor in non-treated villages from agricultural to non-agricultural production. Put simply, in the short run, we expect to see labor flow into agriculture in treated villages and out of agriculture in non-treated villages.

However, in the long run, the higher returns to labor are dissipated due to an influx of workers. Real wages equilibrate for labor, as the mobile factor, and the treated communities end up with higher population density. Returns to land, the fixed factor, remain higher even in the the long run. The higher incomes of landowners, along with the increased population, result in higher demand for non-agricultural goods. What determines where the production occurs to meet this demand? The model implies that increasing demand for non-agricultural goods within a region will be met by some combination of production in villages and in their central town, with the precise allocation depending on the relative productivity levels. Due to their productivity advantage, towns produce a disproportionate share of non-agricultural goods and capture much of the growth in non-agricultural demand. It should be noted that in our model, the long-run share of workers in the non-agricultural sector is fixed, although we observe that modifying preferences to be non-homothetic would result in structural transformation. The disproportionate share of in-migrating non-agricultural workers flowing to the town is even larger if we allow for learning-by-doing productivity growth in the non-agricultural sector, as in Matsuyama (1992).

This simple model illustrates the two major contributions of this paper. First, the long-run impacts of agricultural productivity gains vary with geography: treated villages gain agricultural workers while most of the non-agricultural growth will occur in urban areas if their sectoral productivity is much higher. Second, agricultural productivity shocks induce in situ reallocation of labor across sectors in the short run (when labor is immobile), but in the in the long run (when labor can overcome spatial frictions), spatial equilibrium is restored without local structural change. In Section 7, we discuss how the higher mobility of labor in the long run can help to reconcile our empirical findings with the existing literature.

4 Data

To estimate the impacts of canal irrigation on local economic outcomes, we assemble recent high-resolution data on the universe of firms, households, and settlements in India, building on data from the Socioeconomic High-resolution Rural-Urban Geographic Dataset on India (SHRUG) (Asher et al., 2021). Because the reclassification of (rural) villages into (urban) towns is an endogenous outcome based on population density and administrative discretion, we use a pooled dataset where the unit of observation is a town or village (as provided by the SHRUG), which we call a settlement henceforth.

The 2011 Population Census provides demographic variables along with village-level data on cultivated and irrigated land area in every village in India. It also records the main three crops grown in each village, from which we create an indicator variable for villages that grow a water-intensive crop (cotton, sugarcane, or rice).[21] Since settlements vary in their physical area, our preferred measure for population is population density, which we measure as inhabitants per km^2.[22]

The 2012 Socioeconomic and Caste Census (SECC) is an asset census undertaken in all of India to

[21] Note that Population Census data on agricultural outcomes are generally available only in villages.

[22] We calculate population density as settlement population divided by the area of the settlement GIS polygon shape (in km^2) as opposed to the noisier area reported in the Population Census.

determine eligibility for means-tested programs (Asher and Novosad, 2020). From SECC microdata, we generate the share of adults aged 20–65 who have completed primary, middle, and secondary school, as well as predicted consumption per capita using small area estimation based on the income and asset variables in the SECC. The latter follows the methodology of Elbers et al. (2003) and is described in detail in Asher et al. (2021).[23] Because the SECC is recorded at the household level, we can calculate these outcomes separately for landowners and landless households, enabling estimation of canal effects on returns to capital and labor.

The 2013 Economic Census is a complete enumeration of all non-farm economic establishments in India, which we use to measure non-agricultural economic activity for each settlement. We calculate employment as a share of the adult population, as recorded in the 2011 Population Census.[24] We use the National Industrial Classification codes of firms in the Economic Census to calculate the share of the adult population specifically employed in manufacturing, services, and agro-processing.[25]

In the absence of directly-measured settlement-level agricultural productivity data, we use the Enhanced Vegetation Index (EVI), a satellite-derived measure of biomass with that has been widely used as a proxy for agricultural productivity (Wardlow and Egbert, 2010; Kouadio et al., 2014; Son et al., 2014). We calculate productivity for both the monsoon (*kharif*) season, late May through early October, and winter (*rabi*) season, late December through late March (Selvaraju, 2003). For each season, we define the productivity value by subtracting the mean of the first six weeks of the season from the maximum value reached during the entire season following Rasmussen (1997) and Labus et al. (2002). This measure has better prediction accuracy for yield than a raw biomass measure, as the latter may pick up forest land, which registers as high biomass, but does not change as much as agricultural land during the cropping season. We calculate the mean of this measure for years 2011–13 (corresponding

[23] For a secondary measure of educational attainment, we use the settlement literacy rate from the Population Census.

[24] As the Population Census only reports age-disaggregated numbers for the population aged 0–6, we estimate the population aged 0–17 by multiplying the 0–6 population by 18/7. We then subtract the estimated 0–17 age group from the total population to get the adult population. This calculation reflects the fact that the Indian population pyramid in 2013 is close to uniform for ages 0–30.

[25] Manufacturing employment contains NIC 2-digit codes 10–35 (excluding only the 3-digit code 131) while services contains NIC 2-digit codes 36–93 and 131. Agro-processing is defined as a subset of manufacturing employment codes, specifically NIC codes 10 and 12.

to our other outcome datasets), and log transform it to address outliers and simplify interpretation.[26]

Spatial data on canals and their command areas comes from the Ministry of Water Resources. The India Water Resources Information System (WRIS), a part of the Management Information System of Water Resources Projects of the Central Water Commission in India, provides geospatial data on canals and their command areas.[27] A canal command area is the area determined by canal engineers for which canal irrigation is feasible. The command area begins sharply at the canal (as areas above the canal cannot be gravity-fed) and ends along a threshold that is determined by a combination of canal flow, terrain, and soil type. The WRIS provides dates of canal construction and completion; however, our research of individual canals suggested that recent start and end dates in WRIS often represented canal rehabilitation rather than new canal construction.[28] It is therefore challenging to identify exact construction dates of recent canals. Older construction dates appear to be more credible, as canal investments in the independence period and earlier were more often new canals rather than maintenance maintenance of existing infrastructure.

Using settlement polygon GIS data from ML Infomap, we extract the distribution of elevation in each settlement from Shuttle Radar Topography Mission (SRTM) raster data. Following Riley et al. (1999) and Nunn and Puga (2012), we calculate the ruggedness of a location's topography using the Terrain Ruggedness Index (TRI); TRI measures ruggedness as the average square difference in elevation between a pixel and its eight surrounding pixels. We take the average TRI value across all pixels in a settlement to characterize ruggedness. Using these same data, we compute the distance from every settlement centroid to the nearest canal, command area, river, town, and coast.

Table 1 reports means for all variables used in the analysis. There are 589,950 settlements (villages and towns) in our dataset that contain non-missing population data. As our outcome data is from 2011 onwards, we exclude from our analysis sample any settlements whose closest canal is listed

[26]We find similar effects if we use different years, which is expected given that we are studying equilibrium effects of canals, and similar effect significance if we use EVI levels rather than logs. See Asher and Novosad (2020) for more details on construction of this measure.

[27]The database can be found at `https://indiawris.gov.in/wris/`.

[28]The WRIS database often reports construction dates only in terms of a 5-year planning period, meaning dates are only known within a 5-year window. Note that we augmented and verified dates from the database by manually searching for canal construction dates reported in government documents, news articles, ministry reports, and academic papers.

as incomplete as of 2011.

5 Empirical Strategy

Testing for the long-run impacts of increasing agricultural productivity is challenging for two primary reasons. First, the placement of canals is likely to be endogenous: large, costly infrastructural investments tend to be targeted to areas with political favor and high returns to irrigation. Second, as our theory demonstrates, canals are likely to have very different effects at different geographic scales. We use three empirical strategies to overcome these challenges. To estimate the direct effects on locations receiving increases in agricultural productivity, we exploit the gravitational nature of canal irrigation, which creates arbitrary differences in irrigation availability in proximate settlements directly above and below the canal. To test for the presence of spillovers onto nearby untreated locations, we compare both above- and below-canal settlements to settlements that have similar geophysical characteristics but are further away from canals. Finally, to test for effects on regional urban areas, we use a hundred-year panel of town populations and a difference-in-differences estimator.

5.1 Regression Discontinuity Estimates of the Direct Effects of Canals

Canals provide water to fields through a system of gravity-driven trenches, pipes, and secondary canals. Because water delivery depends physically on gravity, fields must be at a lower elevation than a canal in order to be irrigated with canal water. Settlements above the canal will not be able to access canal water. Our main identification strategy thus compares settlements close to canals with elevations that put them either just above or just below the threshold that would give them access to canal water. For this analysis, below-canal settlements are treated by canals and above-canal settlements serve as controls.

A settlement polygon is characterized by a set of pixels with a distribution of elevation values. We define the polygon elevation as the 5$^{\text{th}}$ percentile of the polygon pixel distribution; this value strongly predicts the difference in canal irrigation between treatment and control areas (see Appendix Figure A1).[29] To calculate canal elevation, we select the elevation of the nearest point on the canal

[29] Results are similar if we use the 25$^{\text{th}}$ or median elevation to define above/below canal thresholds (see Appendix Tables A4, A5, A6, and A7). We chose the 5$^{\text{th}}$ percentile in order to have a control group with close to zero canal irrigation; when we estimate spillover effects below, it is particularly desirable for the above-canal group to experience no direct treatment by canal water.

closest to a given settlement.

Equation 5.1 describes the regression discontinuity design (RDD) specification, following Imbens and Lemieux (2008) and Gelman and Imbens (2019):

$$\begin{aligned} y_{i,s} = \beta_0 + \beta_1 1\{REL\_ELEV_{i,s} < 0\} + \beta_2 REL\_ELEV_{i,s} + \\ \beta_3 REL\_ELEV_{i,s} * 1\{REL\_ELEV_{i,s} > 0\} + \beta_4 X_{i,s} + \nu_s + \epsilon_{i,s}, \end{aligned} \tag{5.1}$$

where $y_{i,s}$ is an outcome in settlement i and subdistrict s and $REL\_ELEV_{i,s}$ is settlement elevation minus canal elevation (such that a negative value means that the settlement lies below the canal, and thus can receive its water), and $X_{i,s}$ is a vector of geophysical controls (ruggedness, mean annual rainfall, distance to the nearest river, distance to the coast, and the GAEZ crop suitability measure for irrigated rice and wheat).[30] ν_s is a subdistrict fixed effect, which restricts our above/below canal comparison to settlements in the same subdistrict. A subdistrict consists of approximately 100 settlements, with a total population of about 250,000 people. Standard errors are clustered at the subdistrict level to account for spatial correlation. In the absence of spillovers to untreated settlements, the effect of canal irrigation is captured by β_1, which is the difference in outcomes between settlements just below and just above the canal.

The analysis sample includes settlements less than 10km of distance and 50m of vertical elevation from the nearest canal.[31] We limit the sample to subdistricts that have at least one settlement in the treatment group and one settlement in the control group. Settlements very close to the treatment threshold have an ambiguous treatment status — for example, a settlement could have some of its land above the canal (and thus not treated) and some of its land below the canal (and thus treated). Inclusion of these settlements would bias RDD estimates toward zero; we therefore exclude a "donut hole" of settlements within 2.5m in elevation of the nearest canal in either direction. Finally, to avoid comparing lowland irrigated areas with rugged hilly areas, we impose a balance restriction

[30] We use these as our best measures of overall agricultural fertility and potential returns to irrigation, which could have hypothetically guided canal placement. As agriculture in India tends to use some inputs but not nearly as much as rich countries, we use the intermediate input variables from the FAO GAEZ. We do not include any socioeconomic controls, because they are available at the settlement level only after 1990, by which time they are plausibly affected by canals.

[31] It is rare that villages further than 10km from a major or medium canal branch show economically meaningful access to canal irrigation, even if they are below the elevation of the canal.

on the terrain ruggedness index. We allow a maximum 25% difference in mean ruggedness between below-canal and above-canal settlements in a given subdistrict; if the percent difference is greater, the entire subdistrict is dropped from our sample. Table 1 shows the sample size and summary statistics for each subset of the data. We use the ruggedness-balanced sample for our primary analysis, but show robustness in the Appendix to alternate sample definitions. The ruggedness-balanced analysis sample is representative of the universe of settlements in India on most dimensions. Around half of agricultural land is irrigated, about 60% of village land is dedicated to agriculture, there is approximately 1 non-farm job for every 10 adults, and just under half of adults have completed primary school.

RDD validity requires that there are no pre-treatment differences at the threshold between above- and below-canal settlements. Since canal infrastructure in India was largely built in the 19th and early-mid 20th centuries and treatment status is determined at the settlement level, there are no high-resolution socioeconomic or agricultural data available to test this assumption. However, we can test for differences in time-invariant geophysical measures, which could proxy for natural advantages that might have affected canal placement and economic outcomes. Table 2 shows estimates of Equation 5.1 on geophysical fundamentals (with the specific outcome excluded from $X_{i,s}$ in each regression), demonstrating that there are no significant differences between above- and below-canal settlements in ruggedness, distance to coast, or crop suitability for rice or wheat. We do estimate that below-canal areas receive 3.6mm *less* annual rainfall on average (on a mean of 1167mm). This effect is tiny in magnitude, would have the opposite effect on agricultural productivity as canal irrigation. We also find that treatment settlements are approximately 10% closer to a river than control settlements, but this should not bias our estimates as distance to river and all other geophysical fundamentals are included as controls in all specifications.

As a robustness check, we use a secondary regression discontinuity design that compares settlements just inside and just outside of the canal command area.[32] We define the running variable as the distance between settlement centroid and command area boundary, defining it negatively inside the command area.[33] The estimation is otherwise similar to that above, but we additionally divide each

[32]This is similar in design to the strategy used in concurrent work by Blakeslee et al. (2021a).

[33]The analysis sample contains settlements within 25km of the command area boundary, and the donut hole

command area boundary into 10km segments and include a fixed effect for each segment, ensuring that we are comparing settlements across the same stretch of each command area. Standard errors are clustered by these segments. This strategy exploits the variation in the xy-plane, whereas the relative elevation strategy exploits variation in the z-axis. The identifying assumption is that settlements just inside and just outside the command area boundary would have similar outcomes if the canal had not been built. We prefer the relative elevation strategy, as boundaries of command areas may be subject to some discretion by officials, who might have incentives to mark one settlement or another as within the command area.[34] We test for balance with this command area boundary strategy in Table A3, finding no evidence for any imbalance.

5.2 Testing for spillovers into above-canal areas

The regression discontinuity design exploits arbitrary differences in access to canal water in proximate above- and below-canal settlements. Given that we are estimating long-run effects of canals, spillovers in such a small geographic area are a distinct possibility. For example, if above- and below-canal settlements are part of integrated labor markets, then labor market effects of canal irrigation could be expected to diffuse across the treatment boundary; if labor mobility was high enough, we could estimate zero differences between these areas in the RDD analysis even in the presence of true effects. Alternately, enhanced agricultural productivity below canals could motivate structural change not only below the canals but also above, attenuating the RDD estimate. More directly, canals could recharge aquifers, improving access to pumped groundwater in above-canal areas.

To test for spillover effects, we define an alternative sample of control locations: distant settlements within each district, which lie at least 15km from the nearest canal but have similar geophysical characteristics. This strategy is predicated on the assumption that any mechanism driving spillovers is likely to decay with distance from treated areas. These settlements are 5km further from the nearest canal than any treatment or control settlement in the RDD sample. By comparing settlements directly above the canal to these more distant settlements, we can test for the spillover effects of

excludes those within 2.5km of the boundary. Results are similar with different exclusion criteria.

[34] In practice, many of the treatment and control areas are defined similarly under the two strategies, since the command area is mostly below the canal elevation.

canals. If spillovers do not decay over this distance, they are more difficult to measure. For example, if landless labor is perfectly mobile across all of India, then a new canal could have a small positive impact on wages in the entire country, but there would be no control group against which such an effect could be measured. While we cannot rule out universal effects like these, our empirical design will identify non-zero spillovers as long as they have a non-zero gradient in distance, but they will be underestimated to the extent that those spillovers extend into the distant control group.

Our preferred specification employs entropy balancing (Hainmueller, 2012) to assign weights to distant settlements so that the distributions (first, second, and third moments) of all geophysical variables in distant settlements match the distributions of these variables in above-canal settlements. Entropy balancing is increasingly favored over other methods because it does not require functional form assumptions on the propensity weights and thus achieves better balance than propensity-score matching methods.[35] Following the literature, we enforce common support by dropping outliers (top and bottom 2.5% for each of the geophysical variables). As an alternate strategy, we use coarsened exact matching (Iacus et al., 2012) to define and weight a matched sample of distant settlements to above-canal settlements. This method coarsens our geophysical variables into discrete bins and matches settlements on all coarsened variables. Distant settlements who do not perfectly match any above-canal settlements on these binned geophysical characteristics are discarded, with the remaining distant settlements re-weighted to match the characteristics of the above-canal settlements. We test for spillovers using the following estimating equation:

$$y_{i,d} = \gamma_0 + \gamma_1 1\{ABOVE\_CANAL_{i,d}\} + \gamma_2 1\{BELOW\_CANAL_{i,d}\} + X_{i,d} + \nu_d + \epsilon_{i,d}, \tag{5.2}$$

where above-canal and below-canal settlements correspond exactly to the set of settlements used in Section 5.1. Distant settlements are the omitted group. $X_{i,d}$ is the same vector of time-invariant geophysical controls as in the RDD specification above. To compare to more distant villages, we use a district fixed effect ν_d instead of the subdistrict fixed effect in the RDD, and standard errors

[35] See Athey and Imbens (2017) for a discussion of how this and similar methodologies seek to create comparable treatment and control groups in the absence of exogenous variation. For recent examples of empirical work using entropy balancing, see Basri et al. (2021) and Guriev et al. (2021).

are clustered at the district level. The coefficient γ_1 describes the difference between above-canal settlements and distant settlements. If there are substantial spillovers from canal-irrigated areas into above-canal settlements, we expect γ_1 to be non-zero.

Note the difference between γ_2 here and the RDD estimate β_1 from Equation 5.1. The RDD estimate describes the difference *at the threshold* between above- and below-canal settlements; γ_2 is the estimate of the average difference between below-canal settlements and distant settlements. If there are no spillovers, and there is no relationship between the RDD running variable (elevation) and the outcome, then we will find $\gamma_1 = 0$ and $\gamma_2 = \beta_1$. In practice, the RDD estimator β_1 requires weaker assumptions for causal interpretation than γ_2 and is thus a better estimator of the direct effects of canal irrigation.

5.3 Town growth through time

The empirical strategies thus far estimate differences between canal irrigated settlements, proximate unirrigated settlements, and similar settlements farther away. But they do not capture the possibility of structural change through concentrated urbanization in areas with economic linkages to canal zones, for two reasons. First, a town may be affected by increases in regional agricultural productivity even if is not in or near the irrigation zone. Second, the spillovers analysis above estimates average effects and is not well suited to test for concentrated changes in a small number of urban towns in a sample with majority rural villages. As suggested by our model, agricultural productivity is likely to generate disproportionate growth in settlements with productivity advantages in non-agricultural production, such as those with natural advantages or agglomeration economies. These advantages may not require being in or adjacent to the canal-irrigated zone.

To test whether canals affect regional urbanization, we exploit variation in canal construction dates, and examine the emergence and growth of towns in their vicinity. The available data (from the 2011 Population Census) records the population of each 2011 town in each decade going back to 1901, beginning with the first decade in which the Census defined a location to be urban.[36] Such an analysis is not possible for any other outcome, because urban population is the only variable

[36] We do not observe former towns which do not exist any longer, but given India's rising urbanization, town disappearance is a rare phenomenon.

available in a long panel spanning many decades of canal construction.

To describe whether a town is near a canal, we first draw a 20 km circle around each town. We define canal treatment for town i in year t as the percentage of the circle area that is overlapped by canal command areas. An alternate specification defines a binary treatment variable that takes the value 1 if more than 20% of the 20 km circle is covered by canal command areas.

Equation 5.3 describes a standard two-way fixed effect (TWFE) continuous treatment difference-in-differences model to test whether town growth and emergence are affected by nearby canal construction:

$$y_{i,t} = \alpha_0 + \alpha_1 CANAL_{i,t} + \zeta_i + \nu_t + \epsilon_{i,t}, \tag{5.3}$$

where outcome $y_{i,t}$ is either an indicator for town existence, log(town population), or decadal growth and ζ_i and ν_t are town and decade fixed effects, respectively. When $y_{i,t}$ represents population, we assign towns with zero population in the panel be 2,000 instead—this treats settlements before they became towns as if their size was just below the average population at which towns first appear in the data.[37] For the binary treatment, we use the estimator from Callaway and Sant'Anna (2020), using the not-yet-treated group as controls and defining treatment to be when a town's 20 km radius catchment area is more than 20% covered by canal command areas. Standard errors are clustered at the subdistrict level.

6 Results

6.1 Direct Treatment Effects of Canals: Regression Discontinuity Estimates

We first report RDD estimates of the direct effects of canal irrigation on agricultural outcomes, the mechanism through which we expect all other equilibrium effects to occur. Panel A of Table 3 shows that in canal-treated areas, 7.4 percentage points more of the land under cultivation is irrigated (17% more than in control settlements), and 9.6 percentage points (300%) more land is irrigated by canals. There are no changes in other sources of irrigation. We test separately for effects on tubewell use, which would suggest greater groundwater access (for example, if canals recharge aquifers as

[37] Of the 7,526 towns present in 2011, only 1,502 existed in 1911. We find similar results if we use 1 for the population of locations before they were urban, but we think that 2,000 is more likely to represent the population of pre-urban settlements.

suggested by Shah (2011)) and find no effects.[38]

Panel B in Table 3 reports direct effects of canal irrigation on agricultural outcomes. Canal-treated settlements experience higher agricultural productivity, with effects concentrated in the relatively dry winter (*rabi*) growing season. Treatment settlements have 7.3% higher values of our satellite-derived land productivity measure in the dry season, and positive but much smaller higher productivity in the wet (*kharif*) season (1.7%, $p=0.058$). This is consistent with the primary role of canals being to improve water access during the dry winter growing season, and being less crucial for productivity in the high precipitation summer. The much smaller effect on summer yields is further suggestive evidence that our treatment is orthogonal to settlement agricultural potential. Settlements below canals also cultivate 2.9 percentage points more of their total area, a 5% increase over control settlements, and are also 5% more likely to grow water-intensive crops. We find no evidence of increased capital intensity of agriculture, as measured by the share of households owning mechanized farm equipment.

The key question of this paper is how these major changes in agricultural productivity affect living standards and the growth of the non-farm economy. Panel C presents estimates of the impacts of canals on population density, non-farm employment, and predicted consumption. The only significant effect is on population: by 2011, treatment settlements have 15% more people per square kilometer than control settlements. This population gain could be the result of reduced out-migration, increased in-migration, increased fertility, or reduced mortality. Our data do not allow us to observe migration flows, but we reject current positive effects on fertility and mortality (as proxied by share of the population aged 0-6 and 70+), suggesting that this result is driven by net in-migration (Table A1).[39] Despite large gains in the productivity of the dominant economic sector, we estimate a statistically insignificant positive 0.09% effect of canals on average living standards ($p=0.103$): we can reject a 1.8% increase and -0.2% decrease in predicted consumption in below-canal settlements with 95% confidence. We find no significant effects on structural transformation, as measured by non-farm

[38]Note that the elevation-based "treatment" classification describes something less that full exposure of a settlement to a canal; out of the 133,000 settlements in India with *any* canal irrigation, the median settlement has 45% of its land irrigated by canals.

[39]It is possible that in the years following canal construction there were changes to fertility or mortality that we do not measure in our data, but we hypothesize that any previous fertility or mortality effects are likely to persist into the present.

employment per (adult) capita, nor do we find significant effects when we disaggregate employment into manufacturing or services. Even when specifically isolating agro-processing, the sector with the strongest linkage to agricultural production, we find no effects the employment share (Table A1). Note that total non-farm employment has risen (as would be expected given the increase in population) but the non-farm share of the economy (the outcome of interest) is unchanged. We do find increases in human capital in canal areas, as shown in Panel D of Table 3. Below-canal settlements show small but precise increases of approximately 1 percentage point in the share of the adult population that has completed primary, middle, and secondary school, as well as the population literacy rate.

Due to the simplicity of the RDD, we can inspect our regression estimates visually. Figure 2 shows regression discontinuity binscatters of key outcomes in each of the categories above, with outcomes residualized on fixed effects and geophysical controls. These figures report the treatment effect at the treatment threshold from Table 3. The binscatters confirm the RDD estimates: we see clear jumps at zero relative elevation in overall and canal irrigation, *rabi* productivity, population density, and educational outcomes, while no visual discontinuities are apparent in *kharif* productivity, non-farm employment, or consumption. Figure 3 plots the coefficients and 95% confidence intervals for the RDD coefficients reported in Table 3, normalized by the standard deviation of each variable in the control sample. Canals lead to a nearly 0.2 SD increase in total irrigation, driven by a nearly 0.8 SD increase in canal irrigation. The gain in population density is almost 0.2 SD and the increase in educational attainment is nearly 0.1 SD for each of the different measures.

Guided by our theory, we test for the differential returns to land and labor. Our model in Section 3 suggests that the long-run spatial equilibrium will be characterized by equalization of returns to mobile factors (such as labor), but not to fixed factors (such as land). In the absence of high resolution data on wages and land rents, the returns to these factors can be approximated by estimating canal treatment effects on predicted consumption separately for landless households (who own only labor) and those who own land as well as labor. We can interpret the consumption effects for landless households as the impact of canals on the returns to labor, and the consumption effects on households with land as the combined impacts on returns to labor and land. Note that because our predicted consumption measure is based on the ownership of a wide range of assets, these proxies should be thought of as the real, rather than nominal, returns to labor and land.

Consistent with our theory, we find that only the returns to land increase relative to above-canal settlements. We first study the effect of canals on land ownership. Panel A of Table 4 shows a decline in the share of the population that are landowners in treatment settlements relative to control settlements, with the average landholding size of landowners unchanged. This implies that the population increase in below-canal settlements is disproportionately driven by an increase in the number of landless households. Consumption effects of canals are substantially different for landed and landless households: we find no significant consumption effects for landless households, but landowner consumption is 2.1% higher in below-canal settlements ($p=0.0001$); this result is significantly different from the estimate for landless consumption. Partitioning landowners by nationally-defined land quintiles, effects increase (almost) monotonically by quintile, with zero consumption effects on those owning <1.32 hectares of land (the 1st quintile), and a 3.1% effect on consumption for those in the top quintile owning >6.10 hectares (Panel B).[40] Both landless and landowning households experience gains in educational attainment, but effects for landowners are two to three times higher than for the landless (Table 4 Panel C).

6.1.1 Robustness

In this section, we test the robustness of our findings to the various parameter choices made in our main empirical analysis. First, we show that our results broadly hold up if we omit the sample exclusion restrictions that we put in place to create a more balanced sample. We run our primary estimation on the key outcome variables using the full sample of all canal area settlements, then all canal area settlements with the "donut hole" removed but without imposing any restriction on ruggedness balance. We also estimate these key outcomes with the same restrictions as our main, balanced analysis sample but instead use the median and 25$^{\text{th}}$ percentile measures of elevation to parameterize settlement elevation, rather than the 5$^{\text{th}}$ percentile used in our main analysis. We also use our main analysis sample with the additional restriction of removing all settlements intersected

[40]We define quintiles in the landholding distribution based on national data, to maintain consistent quintile boundaries across settlements.

by a canal and the additional control for distance to the nearest canal, further ensuring that our variation is along elevation and not across distance in the xy-plane. Lastly, we estimate the effects of canals using the alternative command area boundary RDD described in Section 5.1, where distance to the command area boundary is the running variable rather than relative elevation. While we consider this a secondary identification strategy due to the potentially endogenous drawing of command area boundaries, it is instructive to test whether our results are replicated with a different source of variation.

Appendix Tables A4, A5, A6, and A7 show results for the main outcomes for all of these specifications. Table A4 confirms that canals cause large gains to canal irrigation but limited to no changes for other forms of irrigation. Table A5 confirms broad agricultural impacts across specifications on both the extensive (share of village land under cultivation) and intensive (land productivity, crop choice) margins. In some samples we also find some evidence of mechanization of agricultural (proxied by share of households owning mechanized farm equipment). Our main non-agricultural estimates remain across all of the different specifications: the principal effect of canal irrigation is to increase population density and landowner consumption, while there are negligible effects on non-farm employment shares in canal-irrigated settlements. All panels confirm the positive effects of canals on education.

Finally, we test for sensitivity of outcomes to parameter choice in our main analysis specification of the elevation RDD. Table A8 shows that treatment effects are highly stable in magnitude and significance across bandwidths (Panel A), ruggedness balance restrictions (Panel B), and maximum distance to canal (Panel C). The numbers in bold in the first column indicate the parameter values used in the primary analysis.

6.2 Estimates of spillovers of canals to above-canal settlements

The results above may not fully describe the general equilibrium effects of canals in the long run. If above- and below-canal settlements are economically integrated, not unlikely given their geographic proximity, then there could be important spillovers. For example, if labor markets are integrated, then landless wages could rise in both above- and below-canal settlements. It is also possible that canals have effects on above-canal settlements through groundwater recharge. The results above are therefore insufficient to reach the conclusion that canals have had no effect on landless wages or in situ structural transformation.

As described in Section 5.2, we study spillover effects by comparing both above- and below-canal regions to more distant regions, matching on and controlling for various natural settlement features. Table 5 shows pairs of coefficients from Equation 5.2 describing the difference between (i) below-canal (treatment) settlements and distant settlements; and (ii) above-canal (control) settlements and distant settlements. In each case, the coefficient shows the estimated effect of being either a treated or untreated settlement in the canal region relative to distant, untreated settlements.[41]

We focus on the spillover effects ("Above canal" coefficient). We find evidence of small but significant spillovers in agricultural outcomes: above-canal villages have slightly more irrigation and are more likely to grow water-intensive crops relative to distant settlements, although there are no significant differences in total agricultural land or agricultural productivity in either major growing season. We find only limited evidence of non-agricultural effects. No difference between above-canal and distant settlements is significant at the 95% level, although we do find that population density is 3.8% higher in above-canal village (p=0.097), which we interpret as most likely the impact of the limited gains to agricultural outcomes. We find no evidence of significant spillovers in non-agricultural economic activity, even when disaggregated into manufacturing and services, nor is consumption higher in above-canal relative to distant settlements. In Panel D, we zoom in on effects by landholding status, finding no significant spillover effects (at the 95% confidence level) in consumption or education, regardless of land ownership. We provide an interpretation of these results in the following section.

6.3 DiD Estimates of the Effects of Canals on Urban Growth

The evidence presented so far suggests that in this context, there is no broad structural transformation, either in canal-treated settlements or through spillovers to nearby above-canal settlements, in response to canal-induced agricultural productivity growth. Instead, these agricultural productivity changes are largely absorbed in the long run by net changes in population. However, structural change is typically a geographically concentrated process (Michaels et al., 2012) and closely linked with

[41] The difference between the two coefficients is analogous to the RDD treatment effects in Section 6.1, but it represents an OLS estimate of the difference rather than the better-identified RDD estimate presented above.

urbanization, at least in countries not specialized in mineral extraction (Gollin et al., 2016). If regional agricultural productivity improvements caused concentrated changes in settlements with both market linkages to canal areas and high productivity in non-farm activities, our estimates above would likely not have the precision to capture such an effect.

In this section, we use panel data on urban populations in India going back to 1901, in combination with canal construction dates, to test whether town growth responds to nearby canal construction. For each town, we define a circular catchment area with a radius of 20 km in which to observe canal construction through time. Figure 5 provides suggestive evidence that towns grow more quickly after canals are constructed in their catchment areas. For the set of towns that can be observed for 30 years before and after a canal is built within its catchment area, log population of the town is plotted against the decade relative to the date of canal construction. There is a clear increase in the rate of population growth in the 30 years following canal construction relative to the 30 years preceding construction.

We define a continuous treatment measure to be the share of the town's catchment area that is inside a command area and a binary treatment that considers a town to be treated when more than 20% of the land in its catchment area is inside a canal command area. We use a difference-in-difference estimator with both treatment measure (Equation 5.3) with results shown in Table 6. We find that towns are more likely to appear, defined as crossing the 5,000 population threshold, (Columns 1,2) and are more likely to have larger populations (Columns 3,4) following nearby canal construction. We also find that overall population growth is faster for canal-treated towns, with the estimate from the continuous treatment suggesting that towns entirely surrounded by command areas grow 7.4% faster than similar towns not treated by canals (Column 5), both highly statistically and economic significant. The binary treatment also yields a positive, albeit statistically insignificant, coefficient on town growth (Column 6, p=0.26).

7 Discussion

Our results have two parts. First, differences between the settlements receiving canal water and those nearby that do not (the RDD estimates) demonstrate that the direct land productivity effects of canal irrigation are sustained in the long run. Decades after the canals were built, there is a sharp discontinuity in irrigation and agricultural output between canal treatment and control areas. Landless wages (proxied by consumption) are fully equilibrated across this boundary; the sharp difference in population density across the canal boundary suggests that net population movement is the primary factor behind this wage equalization. Landowners in canal areas are better off, presumably benefiting from the higher land rents brought by increased access to irrigation. There is no sign of structural change at this scale; below-canal settlements have as few non-agricultural jobs per capita as those above the canal.

Second, our spillover estimates clarify that these close-range effects of canals are not hiding a broader pattern of structural change in rural areas. One could imagine that canal-treated areas experienced structural change in both above- and below- canal settlements, in which case they might have higher landless wages and different non-farm industry shares from settlements further away and less plausibly affected by the canal. But we find no evidence of these spillovers: distant settlements have as few non-agricultural jobs per capita as canal treatment or control settlements. Instead, we find evidence for agricultural productivity gains translating into urbanization: towns grow faster following proximate canal construction.

These results are rationalized by our model, where non-agricultural productivity advantages cause structural transformation to be spatially concentrated. The exact links between growing urban markets and greater agricultural productivity in canal areas are difficult to disentangle in our empirical setting, where historical panel data is only available for town population and not for other outcomes. Bustos et al. (2020) shows that landowners in Brazil invested land rents in urban areas, sometimes at substantial distance, which is a plausible driver of structural change in our setting as well. Clemens (2014) suggests that land rents could be used to finance migration, another channel for urbanization and wealth accumulation among landowners. Due to data limitations, we must stay agnostic as to the exact channel(s) driving the urbanization we document.

The caveat to these spillover results is that it remains possible for linkages to propagate impacts of canals to even more distant parts of India, or even the world. For example, if India were characterized by a single, large, high-mobility landless labor market, then it would be possible that canals raised landless wages equally everywhere, such that distance to the canal has no effect on wages. We expect that this story is at least partially true — if the population change in canal areas was driven at all by migration, then a non-zero labor demand elasticity in the sending locations implies some wage gain there, though it may be too small to detect. We therefore remain agnostic on the aggregate nature of these spillovers; our key result is that the primary equilibrating force following agricultural change was net population movement into treated rural areas to work in agriculture and into regional urban areas to work in the non-agricultural sector. Our regional spillovers analysis suggests only very limited agricultural spillovers to nearby above-canal settlements, and we find no evidence of non-agricultural spillovers to proximate settlements apart from concentrated growth of urban population. Consistent with other work on transportation costs in LMICs, distance is likely to remain a substantial friction for many markets (Atkin and Donaldson, 2015).

Some of our results have implications beyond our stylized model of structural change. The increase in educational attainment among both landowning and landless households fits into a literature examining the relationship between economic change and human capital investments. Several papers have suggested that increased labor demand in agriculture may deter human capital investment, particularly among the poor or landless (Foster and Rosenzweig, 2004b; Shah and Steinberg, 2017). In the context of canals, increased labor demand was met in the long run by net population growth, mitigating these potentially adverse effects, such that human capital increased among both the landed and the landless. This result recalls other scenarios where new economic opportunities resulted in higher educational investments (Jensen, 2012; Heath and Mobarak, 2015; Adukia et al., 2020). Foster and Rosenzweig (2004b) offer the suggestion that demand for school investment among the wealthier land-rich could have resulted in more schools which ultimately provided benefits to the landless. It is a puzzle that despite no consumption gains, we find that landless households experience higher educational attainment in canal-treated settlements. The mechanisms driving these educational gains is beyond the scope of this paper. However, we find that while canal settlements have no more public schools, they do have more private schools (Table A2). This is consistent with the hypothesis that richer landed households drive an outward shift in the educational supply curve, which may lower the cost of education for landless households as well.

8 Conclusion

India's canal systems provide an ideal testing ground for examining the geographic relationship between agricultural productivity improvements and long run structural transformation. Canal irrigation raises agricultural productivity – and especially the returns to land. A unique feature of canals is they create sharp spatial changes in agricultural productivity that can persist for decades after they are built.

In the long run, we find that spatial equilibrium is restored primarily through substantial changes in the size of the laborer population. Decades after the canals were built, there are few differences in living standards between landless workers in canal and non-canal settlements. Nor are there differences in non-farm activity in treated areas. However, structural transformation has taken place, with towns emerging disproportionately near canal-irrigated settlements.

The limitations of our work arise from the impossibility of measuring labor flows directly in our context; we observe higher population levels in canal areas, but the data do not tell us from where these people came. Mobile laborers who settled in canal-irrigated settlements, changes in fertility, or even changes in exogamous marriage patterns could explain what we observe in equilibrium. Disentangling this economic history is beyond the scope of this paper but would be valuable in completing the picture.

Many shorter term studies have found that rising agricultural wages can deter or delay industrialization. Our study suggests that, in the long run, these effects may be tempered by changes in the labor supply. Naturally, it is difficult to compare different contexts in different times and places. Most of India's canals were built during the License Raj period, where manufacturing investments were slow and state-inhibited and may have had difficulties responding to changes in labor demand, potentially enhancing the role of mobile labor. Whether modern agricultural shocks will be equally mitigated by labor flows remains an important question for researchers.

Mobile workers pose challenges for applied empirical researchers by violating assumptions of population stability across treatment and control groups. Yet hundreds of millions of Indians report living in places other than those of their birth, and there are tens of millions of temporary migrants on top of those. Our study suggests that this large mobile population is a powerful economic force that can affect policy outcomes substantially.

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Table 1: Summary statistics

	All India	All canal-area settlements	All canal-area settlements minus donut hole	Balanced analysis sample
Sample Size	589,950	245,531	132,969	91,465
Percent Treatment	–	83	77	80
Total irrigated area (share of ag. land)	0.464	0.584	0.518	0.536
Canal irrigated area (share of ag. land)	0.134	0.177	0.138	0.132
Tubewell irrigated area (share of ag. land)	0.196	0.264	0.225	0.242
Other irrigated area (share of ag. land)	0.142	0.158	0.175	0.179
Agricultural land (share of total village area)	0.577	0.666	0.626	0.644
Kharif agricultural production, EVI-derived (log)	7.560	7.739	7.710	7.688
Rabi agricultural production, EVI-derived (log)	7.231	7.370	7.290	7.292
Any water intensive crop grown	0.586	0.657	0.604	0.605
Mechanized farming equipment (share of households)	0.047	0.062	0.055	0.061
Population density (log)	5.238	5.699	5.514	5.546
Consumption (log)	9.726	9.755	9.750	9.760
Total nonfarm employment (share of adult pop)	0.119	0.105	0.107	0.107
Services employment (share of adult pop)	0.077	0.067	0.067	0.066
Manufacturing employment (share of adult pop)	0.029	0.031	0.032	0.033
Primary school ed attained (share of adult pop)	0.471	0.498	0.490	0.495
Middle school ed attained (share of adult pop)	0.318	0.339	0.329	0.331
Secondary school ed attained (share of adult pop)	0.194	0.212	0.207	0.207
Literacy rate (literate share of adult pop)	0.561	0.577	0.576	0.579

Notes: There are 589,950 settlements in our All-India sample that have population data. All canal-area settlements includes towns and villages $\leq$10km from the nearest canal in distance and within 50m of the nearest canal in elevation. Removing the donut hole removes settlements $\pm$2.5m in elevation from the nearest canal. The balanced analysis sample then imposes a balance criteria on ruggedness by dropping all subdistricts in which there is a $\geq$25% difference in average ruggedness between below-canal (treatment) and above-canal (control) settlements. The mean values shown here for the balanced analysis sample also exclude subdistricts that do not contain at least one settlement in each of the treatment and control groups. All averages are weighted by land area.

Table 2: Balance in the RDD using relative elevation

	Ruggedness	Annual rainfall avg. 2010-2014 (mm)	Distance to coast (km)	Distance to river (km)	Wetland rice (GAEZ)	Wheat (GAEZ)
Below canal	-0.008	-3.557**	0.078	-2.390***	0.007	0.000
	(0.056)	(1.694)	(0.371)	(0.425)	(0.011)	(0.004)
Control group mean	4.754	1166.971	362.341	24.562	2.272	0.767
Observations	91,465	91,465	91,465	91,465	91,465	91,465
R^2	0.594	0.988	0.999	0.874	0.923	0.983

$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

Notes: This table reports the regression discontinuity estimate for geophysical variables following Equation 5.1, dropping each outcome variable from the list of controls. Crop suitability measures are taken from the Global Agro-Ecological Zones (GAEZ) model that estimates expected conditions for agricultural production based on climate, soil, and terrain parameters. GAEZ model estimates made assuming gravity-fed irrigation and intermediate level inputs are used.

Table 3: Regression discontinuity results for main outcomes

Panel A. Irrigation outcomes

	Total irrigated area (share of ag. land)	Canal irrigated area (share of ag. land)	Tubewell irrigated area (share of ag. land)	Other irrigated area (share of ag. land)
Below canal	0.074***	0.096***	-0.005	-0.007
	(0.008)	(0.007)	(0.007)	(0.005)
Control group mean	0.430	0.032	0.210	0.195
Observations	83,182	83,192	83,247	82,385
R^2	0.610	0.380	0.470	0.630

Panel B. Agriculture outcomes

	Agricultural land (share of village area)	Kharif (monsoon) ag. prod (log)	Rabi (winter) ag. prod (log)	Water intensive crops (any)	Mechanized farm equip. (share of households)
Below canal	0.029***	0.017*	0.073***	0.028***	0.002
	(0.005)	(0.009)	(0.012)	(0.009)	(0.002)
Control group mean	0.598	7.686	7.209	0.560	0.056
Observations	90,137	90,096	89,836	70,260	86,115
R^2	0.610	0.820	0.700	0.730	0.300

Panel C. Non-farm outcomes

	Population density (log)	Total emp. (share of adult pop.)	Services emp. (share of adult pop.)	Manuf. emp (share of adult pop.)	Consumption per capita (log)
Below canal	0.150***	-0.063	-0.004	-0.056	0.009
	(0.023)	(0.053)	(0.007)	(0.048)	(0.005)
Control group mean	5.281	0.124	0.066	0.049	9.744
Observations	91,465	85,342	85,342	85,342	86,842
R^2	0.450	0.000	0.000	0.000	0.530

Panel D. Education outcomes

	At least primary (share of adult pop.)	At least middle (share of adult pop.)	At least secondary (share of adult pop.)	Literacy (share of pop.)
Below canal	0.013***	0.012***	0.009***	0.011***
	(0.003)	(0.003)	(0.002)	(0.002)
Control group mean	0.478	0.313	0.197	0.570
Observations	86,068	86,068	86,068	91,465
R^2	0.570	0.560	0.530	0.580

$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

Notes: Results for all outcome variables each separately estimated following Equation 5.1. The β_1 coefficient is reported in the first row for each variable, with the stars indicating its significance and the standard error below in parentheses. The control group mean, number of observations with non-missing data for that outcome, and adjusted R^2 for each regression are each reported.

Table 4: Regression discontinuity results for outcomes disaggregated by landownership

Panel A. Landownership overview

	Landowners (share of households)	Avg. size of land holdings (log hectares, all households)	Avg. size of land holdings (log hectares, land owners)	Consumption pc (log)	
				Landless	*Landowners*
Below canal	-0.025***	-0.053***	-0.004	0.003	0.021***
	(0.005)	(0.018)	(0.014)	(0.006)	(0.005)
Control group mean	0.536	0.750	1.502	9.604	9.811
Observations	86,117	83,796	83,763	83,802	84,108
R^2	0.460	0.460	0.510	0.460	0.560

Panel B. Consumption distribution

	Consumption pc (log) 1^{st} quintile *Landowners*	Consumption pc (log) 2^{nd} quintile *Landowners*	Consumption pc (log) 3^{rd} quintile *Landowners*	Consumption pc (log) 4^{th} quintile *Landowners*	Consumption pc (log) 5^{th} quintile *Landowners*
Below canal	0.001	0.015**	0.014**	0.022***	0.031***
	(0.008)	(0.006)	(0.007)	(0.007)	(0.007)
Control group mean	9.739	9.766	9.805	9.846	9.934
Observations	74,140	76,446	72,323	74,105	68,911
R^2	0.460	0.460	0.410	0.410	0.380

Panel C. Education attainment

	At least Primary (share of adult pop.)		At least Middle (share of adult pop.)		At least Secondary (share of adult pop.)	
	Landless	*Landowners*	*Landless*	*Landowners*	*Landless*	*Landowners*
Below canal	0.010***	0.021***	0.009***	0.021***	0.006**	0.018***
	(0.004)	(0.004)	(0.003)	(0.004)	(0.002)	(0.003)
Control group mean	0.433	0.518	0.270	0.353	0.161	0.003
Observations	83,639	84,064	83,639	84,064	83,639	84,064
R^2	0.470	0.590	0.460	0.580	0.420	0.550

$^{*}p<0.10, ^{**}p<0.05, ^{***}p<0.01$

Notes: Regression discontinuity results following Equation 5.1 separately estimated for landowning and landless households. Panel A summarizes differences in the share of landowners, the size of landowners' plots, and the overall consumption of landowners and the landless in each settlement. Panel B shows the consumption of landowners by quintile of land holding size. The bottom (1^{st}) quintile are the landowners with plots in the 0-20% range of the national distribution while the top (5^{th}) quintile are those landowners with total land holdings in the top 80-100% of the national distribution. The quintile break points in ascending order are 1.32, 2.30, 3.65, and 6.10 acres. Note that all consumption coefficients are in units of log consumption per capita, as they are throughout the paper.

Table 5: Comparison to distant settlements

Panel A. Irrigation outcomes

	Total irrigated area (share of ag. land)	Canal irrigated area (share of ag. land)	Tubewell irrigated area (share of ag. land)	Other irrigated area (share of ag. land)
Below canal	0.074***	0.093***	0.011	-0.018*
	(0.016)	(0.011)	(0.008)	(0.009)
Above canal	0.016*	0.007*	0.018**	-0.009*
	(0.009)	(0.004)	(0.008)	(0.005)
Control group mean	0.381	0.034	0.196	0.156
Observations	80,408	80,572	80,576	79,864
R^2	0.60	0.20	0.39	0.77

Panel B. Agriculture outcomes

	Agricultural land (share of village area)	Kharif (monsoon) ag. prod (log)	Rabi (winter) ag. prod (log)	Water crops (any)
Below canal	0.018**	0.011	0.016	0.055**
	(0.008)	(0.016)	(0.021)	(0.022)
Above canal	-0.002	0.007	-0.029	0.039**
	(0.008)	(0.013)	(0.019)	(0.017)
Control group mean	0.569	7.808	7.329	0.632
Observations	90,055	89,997	89,800	69,287
R^2	0.55	0.86	0.56	0.70

Panel C. Non-farm outcomes

	Population density (log)	Total emp (share of adult pop.)	Services emp (share of adult pop.)	Manuf. emp (share of adult pop.)	Consumption pc (log) (all households)
Below canal	0.191***	0.011	0.006	0.005	0.011
	(0.030)	(0.007)	(0.004)	(0.004)	(0.009)
Above canal	0.038*	0.006	0.003	0.003	-0.008
	(0.023)	(0.007)	(0.003)	(0.004)	(0.008)
Control group mean	5.524	0.109	0.069	0.032	9.636
Observations	91,267	83,986	83,986	83,986	86,640
R^2	0.29	0.01	0.00	0.02	0.43

Panel D. Outcomes disaggregated by landownership

	Consumption pc (log) *Landless*	Consumption pc (log) *Landowners*	Middle school ed. *Landless*	Middle school ed. *Landowners*
Below canal	-0.013	0.018*	0.007*	0.026***
	(0.008)	(0.009)	(0.004)	(0.006)
Above canal	-0.015*	-0.002	0.000	0.004
	(0.008)	(0.009)	(0.004)	(0.005)
Control group mean	9.514	9.739	0.252	0.335
Observations	83,497	84,052	83,303	84,012
R^2	0.38	0.43	0.44	0.53

$^{*}p<0.10, ^{**}p<0.05, ^{***}p<0.01$

Notes: This table shows the results of the spillovers analysis, comparing above- and below-canal settlements to distant settlements far from the canal within the same district, following Equation 5.2. Distant settlements are defined as settlements more than 15km away from a canal. Weights were calculating using entropy balancing to ensure distant settlements are comparable to above-canal villages with respect to geophysical controls following Hainmueller (2012). The coefficients on the dummy variables for being in the below-canal treatment or above-canal control groups are reported here.

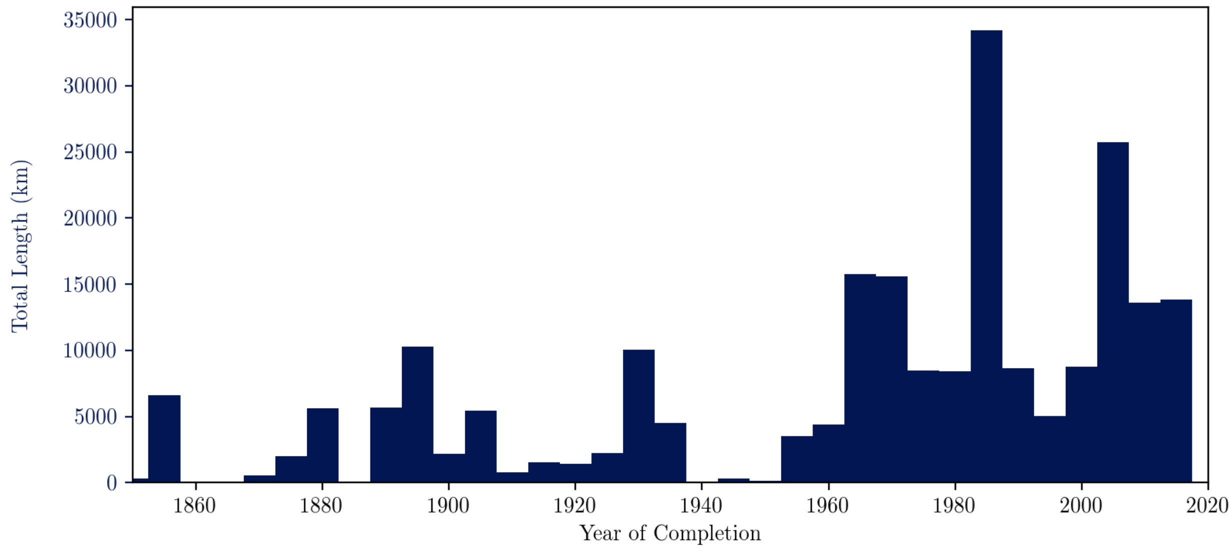
Table 6: Effect of canals on urbanization (event study)

	Town Existence (pop. 5,000)		Population (log)		Growth (decadal)	
	1	2	3	4	5	6
Command area in town catchment area	0.039**		0.084***		0.027	
(binary treatment)	(0.016)		(0.031)		(0.024)	
Share of town catchment area in command area		0.050*		0.319***		0.066**
(continuous treatment)		(0.030)		(0.078)		(0.031)
Observations	26,292	76,872	26,292	76,872	24,101	70,466
R^2		0.67		0.81		0.06

$^{*}p<0.10$,$^{**}p<0.05$,$^{***}p<0.01$

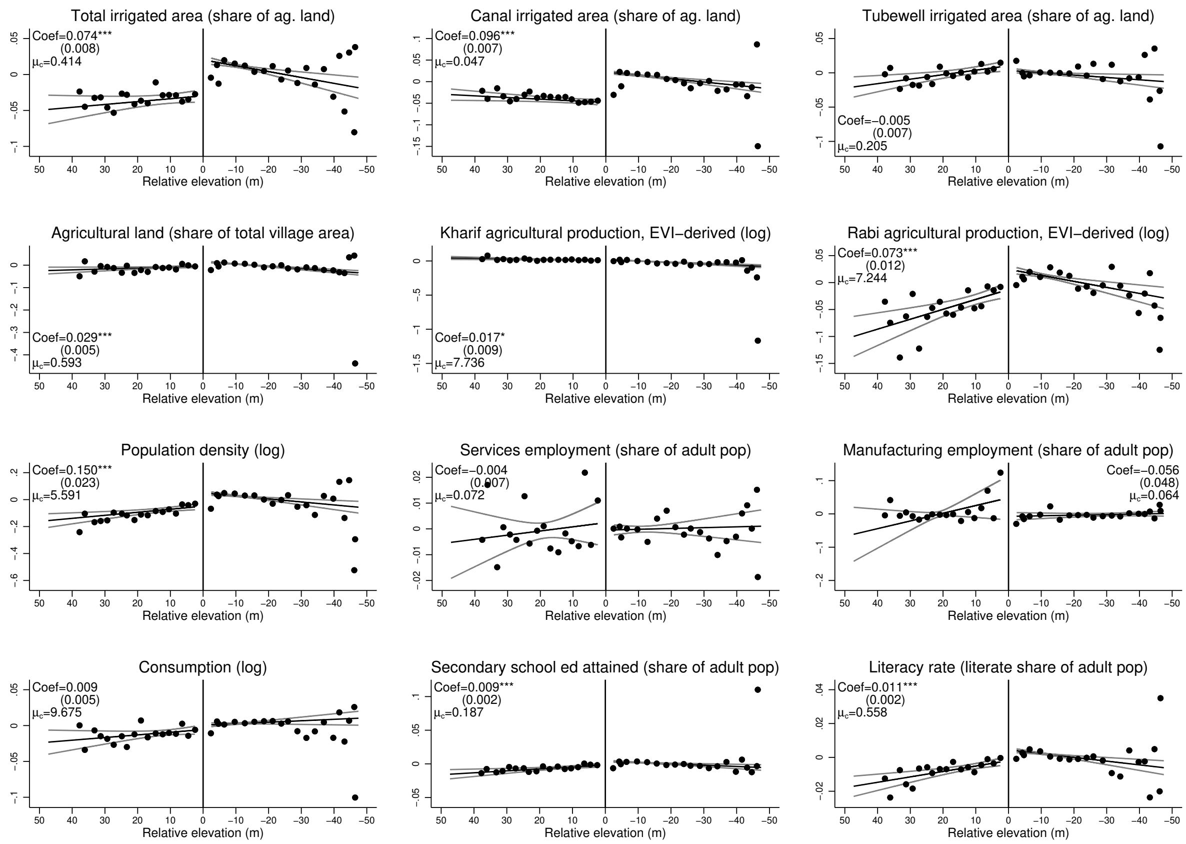
Notes: This table shows the estimated effect of canal area on town growth as estimated in Equation 5.3. Each column reports the β_1 values for various outcome variables. The outcome variable in columns 1 and 2 is the existence of a town with population 5,000 or greater as the outcome variable. In columns 3 and 4 the outcome variable is log population while in columns 5 and 6 it is decadal population growth. All regressions assume that before a town appears in the time series, its is a settlements with a population of 2,000 (smaller than the population required to be declared a town). The first row shows Callaway-Sant'anna results using a binary indicator for canal construction where a town is treated when 20% of catchment area (radius=20km) around it has been covered by a command area. The second row uses a continuous value, the share of the town catchment area (radius=20km) covered by a command area, as the dependent variable.

Figure 1: Canal construction through time



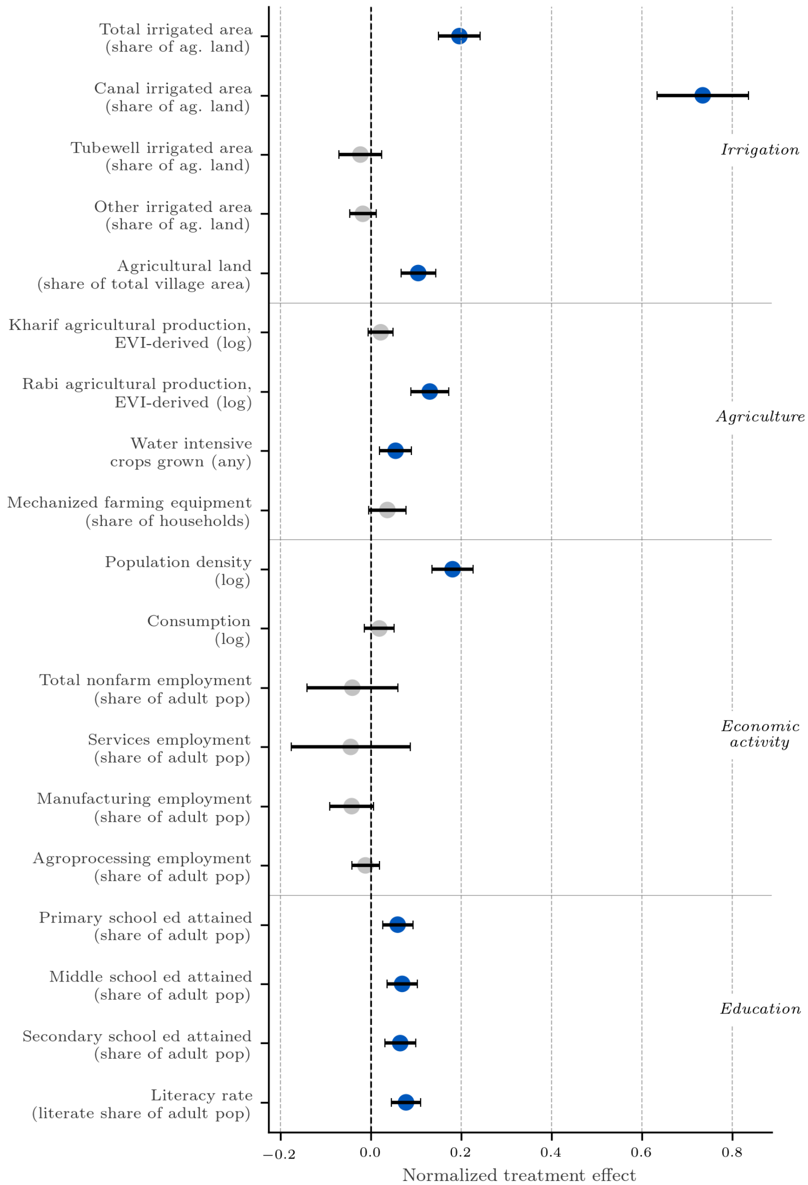
Notes: The total length of medium and major canals constructed in India from 1850-2013. Any canals with dates older than 1850 are coded as 1850 while any canals not completed before 2013 are not included. Note that 217 of the 1442 total canal projects reported, or 9% of total canal length in the geospatial canals data, have an unknown date of completion and are not included in this plot. Additionally, 236 projects totaling 22% of total canal length in the data were not completed as of 2013 (the last date of our major outcome variables) and so are not included in this plot.

Figure 2: Regression discontinuity binscatters for key variables



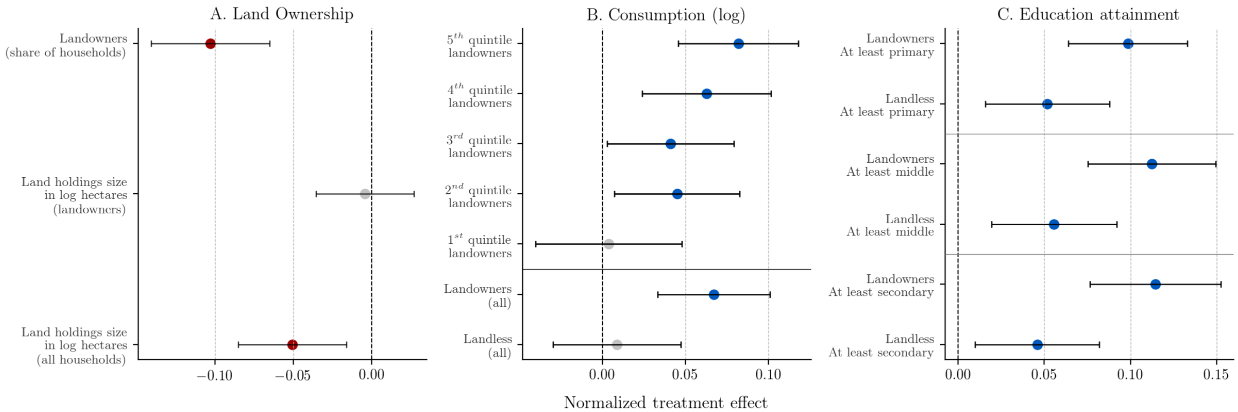
Notes: Each figure shows the binned scatterplot relationship between an outcome of interest and the RDD running variable (elevation relative to the nearest canal), after residualizing on the geophysical controls and subdistrict fixed effects. Treatment settlements that lie below the canal have negative relative elevation while untreated settlements that lie above the canal have positive relative elevation. All regressions were done following Equation 5.1. The regression discontinuity coefficient (Coef) for each variable is reported with stars indicating the significance and the standard error in parentheses below. The control group mean is also reported (μ_c).

Figure 3: Regression discontinuity coefficients for main outcomes



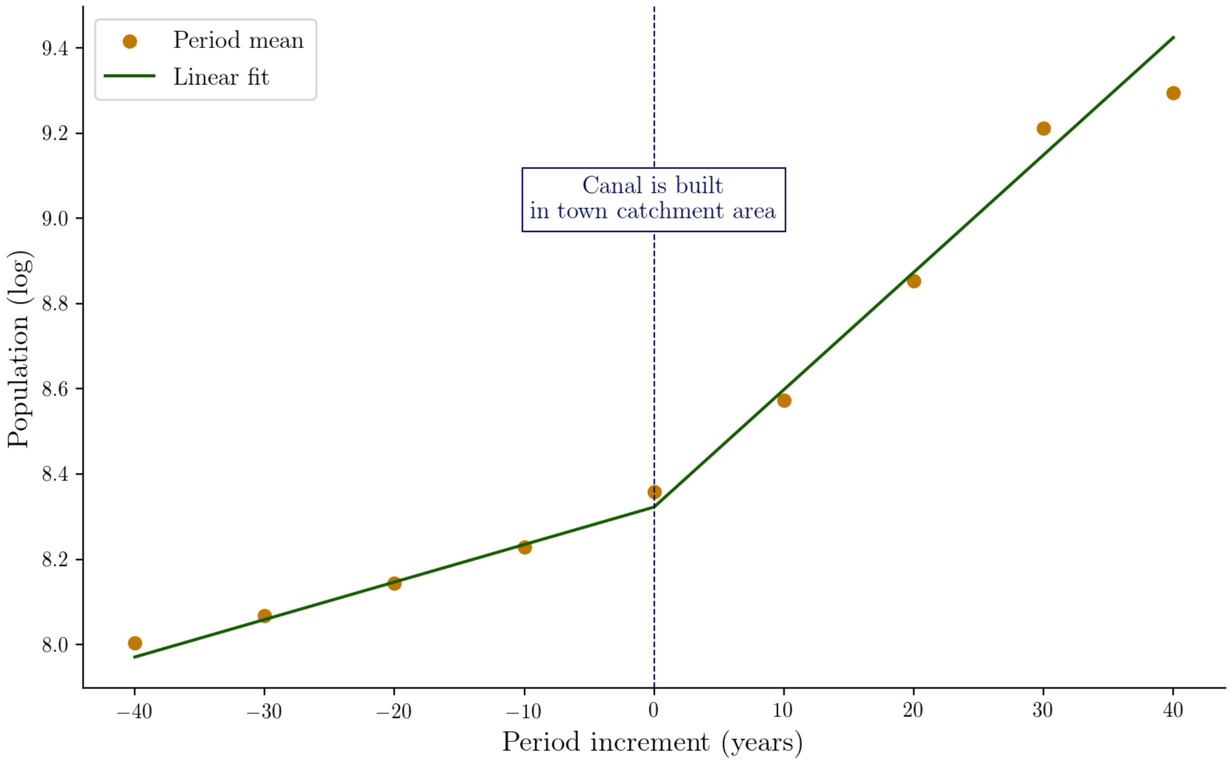
Notes: This figure shows the regression discontinuity results for our main outcomes using our preferred specification and following Equation 5.1. Blue points indicate normalized treatment effects above 0, red below zero, and gray indicates an insignificant result. The normalized treatment effect is calculated by dividing the regression discontinuity coefficient by the standard deviation of the outcome variable in the sample. Error bars indicate the 95% confidence interval for each estimate.

Figure 4: Land ownership outcomes



Notes: This figure shows the regression discontinuity coefficients for outcomes disaggregated by land ownership. Following Equation 5.1, we estimate the RDD coefficients for outcomes based on land ownership status. Blue points indicate normalized treatment effects above 0, red below zero, and gray indicates an insignificant result. The normalized treatment effect is calculated by dividing the regression discontinuity coefficient by the standard deviation of the outcome variable in the sample. Error bars indicate the 95% confidence interval for each estimate.

Figure 5: Trend break in town population growth after canal construction



Notes: This figure shows the trend break that occurs in town population after canal construction. Towns are aligned by period, where period 0 is the decade in which a command area first appeared in the town catchment area, with the 20 km town radius used in this figure. Period increment 10 indicates 1 decade after the command area appearance while period increment -10 indicates 1 decade before the first command area appearance.

A Theoretical model

A.1 Model environment

In the model economy, a region consists of a single town and a set of surrounding villages. The town has higher population than the villages and no agricultural land of its own. Each village, by contrast, is the center of an area of agricultural land. Let V denote the number of villages, and let v_i denote the ith village, $i = 1,2,...,V$.

The economy produces two goods: an agricultural good that is traded beyond the region and a non-agricultural good that is traded within the region but faces prohibitively high trade costs that make it effectively non-tradeable in relation to other regions. This might correspond to non-tradeable services, such as haircuts; but it also allows for manufactured goods with low value per unit transport costs, such as bricks.[42] The agricultural good is produced on the land surrounding villages, and the non-agricultural good can be produced either in villages or in towns. These characteristics of the non-agricultural good reflect the fact that in much of non-urban India, villages and towns produce a mix of non-tradeable services (e.g., wholesale and retail trade, food services, entertainment, government administration and public sector work, construction, repair services, and personal care) and relatively non-tradeable manufacturing (e.g. brick making, metal fabrication, and carpentry).

Within the model economy, people consume the two goods that are produced locally and also consume a third good: a traded non-agricultural good that is only available from the rest of the economy. This might correspond to goods that require production capabilities that are not available within the rural economy (e.g., mobile phones) or perhaps some raw materials that are also unavailable locally (e.g., refined petroleum products). The rural region pays for these "imported" goods through "exports" of its agricultural production.

Land and people

Each of the region's villages has an endowment of land, L_i, and the sum of all the land area in the region is $\sum_{i=1}^{V} L_i = \bar{L}$. In the initial period, the economy has a population of N^0 people, where

[42] This is consistent with the fact that the vast majority of manufacturing firms in India have under five employees, and are thus unlikely to be serving a very large market.

N^{0i} is the population of village v_i and N^{0T} is the population of the town. Thus, $\sum_i N^{0i} + N^{0T} = N^0$. For simplicity, we assume that land rents accrue to the representative household. In principle, it would be possible to model a separate class of land owners, but with homothetic preferences, this adds little. All individuals provide one unit of labor to the market, inelastically.

A.1.1 Preferences and utility

The representative consumer has preferences over the three consumption goods: agriculture (a), non-tradeables (c), and manufactured goods (m). These preferences can be represented by a log linear utility function:

$$u(a,c,m) = \alpha \log a + \beta \log c + \gamma \log m \tag{A.1}$$

As noted above, we abstract for simplicity from a non-homothetic representation of preferences; this is convenient for aggregation and does not require us to address issues related to (for example) the distribution of land across households.

Production and trade

The representative region produces two goods. The region's farmland is used to produce the agricultural good, a, which can be consumed as food or can be traded to the rest of the economy in return for the "imported" good, m. We limit our analysis to the case where this economy is a net exporter of agricultural goods. The model economy also produces a non-tradeable non-agricultural good, c; production of this good does not require land; it can be produced and in any settlement (town or village) and can be consumed in any location within the region, but cannot be traded beyond the region.

Production technologies are simple. The agricultural technology is given by the Cobb-Douglas technology $Y_a = A(N_a)^{\theta} L^{1-\theta}$, where N denotes labor and L represents land, and $0 < \theta < 1$. The non-agricultural good is produced with diminishing returns to labor (but uses no other inputs); the technology can be written as $Y_c = C(N_c)^{\varphi}$, where $0 < \varphi < 1$.

Since both the agricultural good and the manufactured good are traded frictionlessly with the rest of the economy, the representative region is a price taker for these two goods. The relative price p_m is the price of this imported manufactured good in terms of agricultural goods, which are the numeraire.

Factor mobility

Land is fixed within locations; we abstract from any extensive margins that might be available to grow the supply of land. Land is used only in the production of the agricultural good; the non-tradeable good does not require any land. Labor is freely mobile across sectors, within a rural region. Workers can move frictionlessly between different villages, and between villages and the town.

In the long run, the economy is open to flows of labor. Thus, in the initial period, which represents the long-run equilibrium before the the construction of a canal, the representative region takes the wage w as given from the rest of the economy. However, labor does not flow instantaneously across regions. This means that a shock to production or demand may create a short-run departure from the wage w. These short-run deviations from the economy-wide wage will be eroded in the long run by labor mobility; in the long run, a labor market equilibrium will obtain in which wages are equalized across locations.

Long-run equilibrium

An equilibrium in this economy consists of an allocation of labor across sectors (agriculture and non-agriculture) in each village, N_{a1},N_{a2},...,N_{aV} and N_{c1},N_{c2},...,N_{cV} as well as the labor assigned to the town, N_{cT}. In addition, the equilibrium consists of consumption allocations for all individuals in the economy and production allocations for each sector and settlement.

Because the economy faces no externalities or market imperfections, and because production is fully competitive, the first and second welfare theorems hold, and we can solve the social planner's problem to arrive at the same equilibrium allocations that would obtain in a competitive equilibrium. Moreover, since preferences are homothetic, we can focus on the problem of a representative consumer who receives the average consumption allocation. For further simplicity, we first consider the case of a single representative village; we then briefly summarize how results would change with multiple heterogeneous villages.

An equilibrium in this economy consists of an allocation of labor across sectors (agriculture and non-agriculture) in each village, N_{a1},N_{a2},...,N_{aV} and N_{c1},N_{c2},...,N_{cV} as well as the labor assigned

to the town, N_{cT}. In addition, the equilibrium consists of consumption allocations for all individuals in the economy and production allocations for each sector and settlement.

The social planner's problem can be expressed as follows:

$$\max_{\{N_a,N_c,N,a,c\}} \alpha \log a + \beta \log c + \gamma \log m$$

subject to:

$$aN + p_m mN \leq A(N_a)^\theta \tag{A.2}$$

$$cN \leq \sum_j C_j (N_{cj})^\varphi \tag{A.3}$$

$$N_a + N_c = N \tag{A.4}$$

where p_m, w are given, and a, c, m, N_a, $N_c \geq 0$.

The social planner will choose the quantity of agricultural labor so that the marginal value product is equated with the wage; i.e., $A\theta(N_a)^{\theta-1} = w$, which gives the equilibrium quantity of labor in agriculture: $N_a^* = \left(\frac{A\theta}{w}\right)^{\frac{1}{1-\theta}}$. This in turn yields the equilibrium quantity of agricultural production: $Y_a^* = A\left(\frac{A\theta}{w}\right)^{\frac{\theta}{1-\theta}} = A^{\frac{1}{1-\theta}}\left(\frac{\theta}{w}\right)^{\frac{\theta}{1-\theta}}$.

The corresponding condition for the non-tradeable good is more complicated, however, as the price of this good is determined simultaneously with the quantity; the price and quantity depend in turn on the total population of the economy, which is endogenous.

To solve this, we begin by noting that the social planner's problem, along with (A.2), implies that total agricultural output will be partitioned between domestic consumption and exports such that (using Y_a^* to denote the optimized value of agricultural production):

$$aN = \left(\frac{\alpha}{\alpha+\gamma}\right) Y_a^*$$

$$mN = \left(\frac{1}{p_m}\right)\left(\frac{\gamma}{\alpha+\gamma}\right)Y_a^*$$

Note that the right-hand side of each equation is entirely given by the parameters of the model. For notational convenience, let us rewrite these as $aN = \sigma_a$ and $mN = \sigma_m$. Given that the quantity of agricultural output is essentially determined from exogenous parameters of the model, these equations show that an increase in population reduces the per capita consumption of a and m. The social planner's challenge is thus to balance this dilution effect against the benefits of producing more of the non-tradeable good, which of course requires labor. We can rewrite the social planner's problem now as:

$$\max_{\{N_c,N,c\}} \alpha \log\frac{\sigma_a}{N} + \beta \log c + \gamma \log\frac{\sigma_m}{N}$$

subject to:

$$cN \leq C(N_c)^{\varphi} \tag{A.5}$$

$$N_a^* + N_c = N \tag{A.6}$$

The first order conditions with respect to labor give:

$$\text{FOC(N)}: \left(\frac{\alpha N}{\sigma_a}\right)\left(-\frac{\sigma_a}{N^2}\right) + \left(\frac{\gamma N}{\sigma_m}\right)\left(-\frac{\sigma_m}{N^2}\right) = \lambda[c - \varphi C(N - N_a)] \tag{A.7}$$

Similarly, the first-order conditions with respect to c give:

$$FOC(c): \left(\frac{\beta}{cN} = \lambda\right) \tag{A.8}$$

From (A.7) and (A.8) together, we get:

$$\left(\frac{\alpha+\gamma}{N}\right) = \left(\frac{\beta}{cN}\right)\left[\frac{\varphi Y_c}{N - N_a} - c\right]$$

$$(\alpha+\gamma)=\frac{\beta\varphi cN}{c(N-N_a)}-\beta$$

$$(\alpha+\beta+\gamma)=\frac{\beta\varphi N}{N-N_a}$$

With some algebra, this gives an expression for the share of non-tradeable labor in the total workforce of:

$$\frac{N_c^*}{N^*}=\frac{\beta\varphi}{\alpha+\beta+\gamma}.$$

This in turn implies that total population can be expressed as:

$$N^*=N_a^*\left(\frac{\alpha+\beta+\gamma}{\alpha+\beta+\gamma-\beta\varphi}\right)=A^{\frac{1}{1-\theta}}\left(\frac{\theta}{w}\right)^{\frac{\theta}{1-\theta}}\left(\frac{\alpha+\beta+\gamma}{\alpha+\beta+\gamma-\beta\varphi}\right)$$

It is easy to see that this is increasing in both A and φ. Put simply, as the agricultural productivity of the region increases, factor-neutral technical change (as modeled here) induces an increase in the marginal productivity of all inputs. In this case, an inflow of workers from outside the region occurs to the point where a spatial equilibrium once again holds. That new equilibrium will feature a higher number of workers in both agriculture and non-agriculture. It will also lead to higher land rents for those village that benefit from irrigation – though not for untreated villages. Even at constant wages, the higher population and higher land rents will lead to higher demand for all goods. Since the prices of a and m are exogenously determined, the demand for these goods increases through a pure income effect. The demand for the non-tradeable non-agricultural good also increases, and since this good is produced only with labor, there is necessarily an increase in the labor allocated to production of the non-agricultural good. The <u>share</u> of workers in this sector does not change – a result driven by the Cobb-Douglas structure of preferences. But if towns are more productive than villages in the technology for non-agricultural goods, then the increase in production will have geographic implications, as shown in the next subsection.

Two locations: Village and town

Consider next the implications of having a town that is more productive in non-agriculture than its surrounding villages. This scenario might correspond to a situation with some sort of learning effects, or urban production externalities. In the following paragraphs, we sketch out the equilibrium for this scenario.

To begin, we note that because the agricultural good is only produced in the village and the fully tradeable good is not produced in the region at all, we can start with a simplified version of the social planner's problem after having solved for a and m. We assume that $N_{cT} > N_v$ for all $v = \{1,2,...,V\}$.

$$\max_{\{N_{ct},N_{cv},N,c\}} \alpha \log\frac{\sigma_a}{N} + \beta \log c + \gamma \log\frac{\sigma_m}{N}$$

subject to:

$$cN \leq C_t(N_{ct})^{\varphi} + C_v(N_{cv})^{\varphi} \tag{A.9}$$

$$N_a^* + N_{ct} + N_{cv} = N \tag{A.10}$$

$$N_{ct} = RN_{cv} \tag{A.11}$$

The final constraint in this problem reflects the fact that the marginal product of labor in non-agricultural production must be equalized between the town and the village, because labor moves freely within the region. This condition is thus a necessary condition for the within-region spatial equilibrium:

$$w = \varphi C_t N_{ct}^{\varphi-1} = \varphi C_v N_{cv}^{\varphi-1}$$
$$N_{ct} = N_{cv}\left(\frac{C_t}{C_v}\right)^{\frac{1}{1-\varphi}}$$

Given that $0 < \varphi < 1$, this condition implies that the town's proportion of the total regional non-agricultural labor will be constant – and the larger is φ, the larger the share that will concentrate in the town.

Consider now the possibility that, as in Matsuyama (1992), the non-agricultural productivity level is linked in a dynamic sense to the non-agricultural output in a previous period. Combined with the previous result, this would imply that over time, non-agricultural labor will steadily concentrate in towns, leading to a growing urbanization process. Some non-agricultural production will always continue to take place in villages, but the concentration effects will ensure that a growing fraction of the non-agricultural labor is in the town. Although we do not fully model the dynamics of this economy, we note that our framework is broadly consistent with a long-run process of urbanization. Moreover, because an agricultural productivity increase generates an expansion in the absolute size of the town, a dynamic model of the kind described here would predict that an agricultural productivity increase would generate a long-run process in which non-agricultural productivity will concentrate in towns, leading to a process of urbanization even without any change in the total share of the non-agricultural labor force, as the sector concentrates spatially in towns. If in addition we allowed for non-homotheticities to increase the share of the non-agricultural sector, these effects would become even more pronounced. However, we do not formally offer this dynamic model in this paper, since we lack the kind of long-run panel data that would be needed to test the model fully.

Short-run equilibrium

The preceding sections address the long-run equilibrium of the economy and its responses to a change in agricultural productivity. As argued above, in the long run, inflows of labor allow for a new spatial equilibrium to emerge, in which workers are indifferent between regions. However, as other researchers have argued, labor mobility in rural India faces significant frictions in the short run (Foster and Rosenzweig, 2007; Munshi and Rosenzweig, 2016), even though there is substantial migration in the long run. (For instance, Foster and Rosenzweig (2007) find male out-migration from villages of over 20% when considering a 17-year period, consistent with a fairly high level of long-run mobility.)

In recognition of this distinction, we consider the model's predictions concerning a short-run period after canals have been constructed but before labor has adjusted to the changes in productivity. In our framework, positive agricultural productivity shocks, such as the investments in irrigation canals studied in this paper, will lead to short-term growth in local demand for land and labor, driving up wages and land rents, and leading to higher incomes in treated communities for both landowners and landless workers. However, in the long run, the higher returns to labor are dissipated due to an influx of workers. Real wages equilibrate for labor, as the mobile factor, and the treated communities end up with higher population density. Returns to land, the fixed factor, remain higher even in the the long run.

The higher incomes of landowners, along with the increased population, lead in treated villages to higher demand for non-agricultural goods. In a model with the standard non-homotheticity of preferences, where landowners spend their increased rents disproportionately on non-agricultural goods, this would lead to even stronger transformation effects. However, for simplicity, we do not impose non-homotheticity on our benchmark model.[43]

The increased demand for non-agricultural goods drives an expansion of this sector within the rural region, since these goods are not traded beyond the region. However, as in the long-run equilibrium, we impose the assumption that productivity in non-agriculture is higher in towns than in their surrounding villages. This means that the absolute increase in non-agricultural production will be larger in towns relative to the increase in villages.

To see these effects more clearly, we turn next to consider the short-run effects of the agricultural productivity change. Again, we solve the social planner's problem, in which an additional constraint is that the total stock of labor remains fixed at the level prior to the productivity shock.

Short-Run Effects of an Agricultural Productivity Shock

Taking the total labor supply in the economy as given – which is essentially how we want to think about the short-run response to a labor supply shock. Let N be fixed. The social planner's problem is to solve:

$$\max_{\{N_a,a,c\}} \alpha \log a + \beta ; \log c + \gamma \log m$$

subject to:

[43]We note that non-homotheticity would also apply to the increased wages that workers would receive in the short run, before migration inflows bring wages into spatial equilibrium with the outside world. Again, however, we abstract from the non-homotheticity of preferences in our simple model.

$$aN + p_m mN \leq A(N_a)^\theta \tag{A.12}$$

$$cN \leq \sum_j C_j (N_{cj})^\varphi \tag{A.13}$$

$$N_a + N_c = N \tag{A.14}$$

where N, p_m are given, and $a, c, m, N_a, N_c \geq 0$.

Note that this is the same as the initial period, except that the population N is now fixed instead of the wage w. Starting on the consumption side, we can see how the agricultural good will be allocated between food and exports (which correspond directly to consumption of the imported good).

$$aN = \left(\frac{\alpha}{\alpha+\gamma}\right) Y_a^*$$

$$mN = \left(\frac{1}{p_m}\right)\left(\frac{\gamma}{\alpha+\gamma}\right) Y_a^*$$

This gives $a = \frac{\alpha}{\alpha+\gamma} A N_a^\theta$ and $p_m m = \frac{\gamma}{\alpha+\gamma} A N_a^\theta$. We can substitute these in to the social planner's problem to get a maximization problem in one variable:

$$\max_{\{N_a\}} \alpha \log\left(\frac{\alpha}{\alpha+\gamma} A N_a^\theta\right) + \beta \log C (N - N_a)^\varphi + \gamma \log\left(\frac{\gamma}{\alpha+\gamma} A N_a^\theta\right)$$

This produces the first order condition:

$$\text{FOC}(N_a): \left(\frac{\alpha\theta}{N_a}\right) - \left(\frac{\beta\varphi}{N - N_a}\right) + \left(\frac{\gamma\theta}{N_a}\right) = 0, \tag{A.15}$$

which gives an expression for the share of agricultural labor in the total workforce of:

$$\frac{N_a}{N} = \frac{\theta(\alpha+\gamma)}{\beta\varphi + \theta(\alpha+\gamma)}.$$

Then

$$\frac{N_c}{N} = \frac{\beta\varphi}{\beta\varphi + \theta(\alpha + \gamma)}.$$

Relative to the comparable expression for the long-run equilibrium, note that the denominator here is strictly smaller, since all three parameters of the utility function are multiplied here by exponents from the production functions, where these exponents are smaller than one; i.e., with $0 < \varphi < 1$ and $0 < \theta < 1$, the denominator here is smaller than was the case for the long-run equilibrium, while the numerator is identical.

What this implies is that, in the short-run equilibrium, the fraction of the workforce engaged in non-agriculture rises. Even though higher agricultural productivity increases the returns to labor in agriculture, the fraction of the workforce in agriculture falls. This seemingly curious result reflects the fact that the productivity increase, with fixed labor force, leads to an increase in the wage relative to the price of agricultural goods and the "imported" good. The resulting income effect leads to an increase in demand for the non-tradeable good. Since this can only be produced within the region, labor must reallocate away from agriculture towards the non-tradeable good. As a result, the general equilibrium effect in the short run is an increase share of workers in the non-tradeable (and non-agricultural) sector.

In the long run, additional workers will flood in to the treated region, restoring the shares of workers across sectors that previously pertained. Thus, with sufficiently detailed data, one might expect to see a modest short-run increase in the fraction of workers in non-agriculture, within treated villages, but no effect on this fraction in the long run.

B Appendix Tables and Figures

Table A1: Additional RDD outcomes

	Settlement is a town (likelihood)	Population age 0-6 (share of pop.)	Population age 70+ (share of pop.)	Agroprocessing emp. (share of adult pop.)
Panel A: Relative elevation				
Below canal	0.009	-0.002***	0.000	-0.024
	(0.007)	(0.001)	(0.000)	(0.024)
Control group mean	0.025	0.140	0.037	0.018
Observations	91,465	91,465	86,111	85,342
R^2	0.150	0.570	0.340	0.040

$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

Notes: Additional outcomes reported for the relative elevation specification of the regression discontinuity design. These results use the balanced, analysis sample following Equation 5.1.

Table A2: School infrastructure outcomes

	Primary schools (number)		Middle schools (number)		Secondary schools (number)	
	Gov.	*Priv.*	*Gov.*	*Priv.*	*Gov.*	*Priv.*
Below canal	0.013	0.057**	0.007	0.032*	0.018	0.006
	(0.066)	(0.028)	(0.033)	(0.019)	(0.019)	(0.015)
Control group mean	1.931	0.303	0.811	0.188	0.352	0.127
Observations	90,285	89,733	90,192	89,658	90,299	90,229
R^2	0.350	0.240	0.270	0.270	0.180	0.130

$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

Notes: This table shows the RD effect on the number of primary, middle, and secondary schools in the settlement by type, either government or private.

Table A3: Balance in the RDD using distance to command area boundary

	Ruggedness	Annual rainfall avg. 2010-2014 (mm)	Distance to coast (km)	Distance to river (km)	Wetland rice (GAEZ)	Wheat (GAEZ)
Below canal	-0.022	-1.130	-0.062	-0.843	-0.008	0.009
	(0.040)	(3.139)	(0.459)	(0.769)	(0.016)	(0.006)
Control group mean	3.602	1317.887	467.054	23.404	2.704	1.187
Observations	48,965	48,965	48,965	48,965	48,965	48,965
R^2	0.640	0.990	1.000	0.930	0.960	0.990

$^{*}p<0.10,^{**}p<0.05,^{***}p<0.01$

Notes: This table reports the regression discontinuity effect on several outcomes we expect to be balanced using the command area boundary RDD, which uses distance to the command area boundary as the running variable instead of relative elevation. Crop suitability measures are taken from the Global Agro-Ecological Zones model that estimates expected conditions for agricultural production based on climate, soil, and terrain parameters. Model estimates assume that crops have gravity-fed irrigation and intermediate inputs.

Table A4: Regression discontinuity irrigation outcomes for a range of robustness specifications

	Total irrigated area (share of ag. land)	Canal irrigated area (share of ag. land)	Tubewell irrigated area (share of ag. land)	Other irrigated area (share of ag. land)
Panel A: All canal-area settlements				
Below canal	0.063***	0.086***	-0.006	-0.008***
	(0.002)	(0.001)	(0.000)	(0.000)
Control group mean	0.435	0.070	0.214	0.158
Observations	230,139	230,190	230,282	228,312
R^2	0.640	0.450	0.470	0.560
Panel B: All canal-area settlements, minus donut hole				
Below canal	0.079***	0.107***	-0.007	-0.009**
	(0.002)	(0.002)	(0.000)	(0.000)
Control group mean	0.387	0.051	0.181	0.161
Observations	122,187	122,246	122,313	120,714
R^2	0.600	0.400	0.480	0.630
Panel C: Canal-area settlements balanced on ruggedness, using median settlement elevation				
Below canal	0.053***	0.071***	-0.007*	-0.005
	(0.003)	(0.004)	(0.000)	(-0.001)
Control group mean	0.469	0.068	0.245	0.162
Observations	90,434	90,435	90,496	89,492
R^2	0.640	0.500	0.500	0.620
Panel D: Canal-area settlements balanced on ruggedness, using 25th percentile settlement elevation				
Below canal	0.073***	0.103***	-0.014***	-0.007*
	(0.002)	(0.003)	(0.000)	(0.000)
Control group mean	0.440	0.055	0.226	0.166
Observations	94,646	94,656	94,713	93,726
R^2	0.650	0.450	0.500	0.620
Panel E: Main analysis sample, excluding villages intersected by a canal				
Below canal	0.074***	0.096***	-0.005	-0.007
	(0.002)	(0.002)	(0.000)	(0.000)
Control group mean	0.414	0.047	0.205	0.169
Observations	83,182	83,192	83,247	82,385
R^2	0.620	0.390	0.470	0.630
Panel F: Main analysis sample, additional control for distance to canal				
Below canal	0.045***	0.048***	0.005	-0.001
	(0.001)	(0.000)	(0.001)	(0.000)
Control group mean	0.414	0.047	0.205	0.169
Observations	83,182	83,192	83,247	82,385
R^2	0.620	0.410	0.480	0.630
Panel G: Command area boundary RDD				
Inside command area	0.118***	0.156***	-0.008	-0.019*
	(-0.001)	(-0.002)	(-0.002)	(0.002)
Control group mean	0.535	0.063	0.333	0.156
Observations	43,244	43,206	43,239	42,752
R^2	0.730	0.510	0.570	0.520

$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

Notes: This table shows irrigation outcomes using a range of samples to test the robustness of our main results of the regression discontinuity. Panel A contains all settlements ≤10km from the nearest canal in distance and ±50m from the nearest canal in elevation. Panel B removes the "donut hole", meaning all settlements ±2.5m from the canal are removed. The samples in Panel C and D use the same sample restrictions as as our main, balanced analysis sample (shown for reference in panel F), but use different values for settlement elevation. Panel C uses the median while Panel D uses the 25th percentile to parameterize settlement elevation when calculating relative elevation. Panel E shows the results of the regression discontinuity using the distance to command area boundary specification.

Table A5: Regression discontinuity results for agricultural outcomes for all specifications

	Agricultural land (share of village area)	Kharif (monsoon) ag. prod (log)	Rabi (winter) ag. prod (log)	Water intensive crops (any)	Mechanized farm equip. (share of households)
Panel A: All canal-area settlements					
Below canal	0.032***	0.028***	0.052***	0.029***	0.003**
	(0.002)	(0.003)	(0.002)	(0.001)	(0.000)
Control group mean	0.594	7.744	7.272	0.698	0.043
Observations	242,163	242,220	240,423	193,861	231,571
R^2	0.590	0.790	0.720	0.730	0.340
Panel B: All canal-area settlements, minus donut hole					
Below canal	0.042***	0.026***	0.066***	0.029***	0.005***
	(0.002)	(0.003)	(0.002)	(0.001)	(0.000)
Control group mean	0.564	7.734	7.238	0.671	0.039
Observations	131,005	131,002	130,591	101,579	125,349
R^2	0.590	0.810	0.680	0.750	0.310
Panel C: Canal-area settlements balanced on ruggedness, using median settlement elevation					
Below canal	0.020***	0.025***	0.039***	0.012*	0.001
	(0.002)	(0.004)	(0.003)	(0.002)	(0.000)
Control group mean	0.630	7.747	7.292	0.691	0.049
Observations	97,507	97,440	97,077	77,316	93,412
R^2	0.610	0.820	0.730	0.730	0.350
Panel D: Canal-area settlements balanced on ruggedness, using 25th percentile settlement elevation					
Below canal	0.029***	0.033***	0.060***	0.023***	0.002
	(0.002)	(0.004)	(0.003)	(0.001)	(0.000)
Control group mean	0.612	7.737	7.273	0.667	0.048
Observations	101,919	101,876	101,595	80,408	97,503
R^2	0.620	0.820	0.730	0.720	0.340
Panel E: Main analysis sample, excluding villages intersected by a canal					
Below canal	0.029***	0.017*	0.073***	0.028***	0.002
	(0.002)	(0.003)	(0.003)	(0.001)	(0.000)
Control group mean	0.593	7.736	7.244	0.649	0.047
Observations	90,137	90,096	89,836	70,260	86,115
R^2	0.610	0.830	0.710	0.730	0.310
Panel F: Main analysis sample, additional control for distance to canal					
Below canal	0.019***	-0.005	0.060***	0.015*	0.001
	(0.001)	(0.002)	(0.002)	(0.001)	(0.000)
Control group mean	0.593	7.736	7.244	0.649	0.047
Observations	90,137	90,096	89,836	70,260	86,115
R^2	0.610	0.830	0.710	0.730	0.310
Panel G: Command area boundary RDD					
Inside command area	0.026**	0.141***	0.061**	0.028	0.008**
	(0.000)	(-0.002)	(0.001)	(-0.002)	(0.000)
Control group mean	0.656	7.611	7.392	0.783	0.047
Observations	48,290	48,344	48,238	41,690	45,953
R^2	0.720	0.810	0.790	0.770	0.360

$^{*}p<0.10, ^{**}p<0.05, ^{***}p<0.01$

Notes: This table shows agricultural outcomes using a range of samples to test the robustness of our main results of the regression discontinuity. Panel A contains all settlements ≤10km from the nearest canal in distance and ±50m from the nearest canal in elevation. Panel B removes the "donut hole", meaning all settlements ±2.5m from the canal are removed. The samples in Panel C and D use the same sample restrictions as as our main, balanced analysis sample (shown for reference in panel F), but use different values for settlement elevation. Panel C uses the median while Panel D uses the 25th percentile to parameterize settlement elevation when calculating relative elevation. Panel E shows the results of the regression discontinuity using the distance to command area boundary specification.

Table A6: Regression discontinuity results for non-farm outcomes for all specifications

	Population density (log)	Total emp. (share of adult pop.)	Services emp. (share of adult pop.)	Manuf. emp (share of adult pop.)	Consumption pc (log) *Landless*	Consumption pc (log) *Landowners*
Panel A: All canal-area settlements						
Below canal	0.123***	-0.027	-0.002	-0.024	0.004	0.019***
	(0.003)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Control group mean	5.692	0.142	0.078	0.048	9.535	9.735
Observations	245,531	226,911	226,911	226,911	225,320	225,921
R^2	0.510	0.020	0.010	0.020	0.470	0.590
Panel B: All canal-area settlements, minus donut hole						
Below canal	0.185***	-0.041	-0.002	-0.037	0.009*	0.026***
	(0.003)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Control group mean	5.527	0.146	0.076	0.052	9.536	9.726
Observations	132,969	123,496	123,496	123,496	122,037	122,044
R^2	0.450	0.020	0.010	0.020	0.480	0.550
Panel C: Canal-area settlements balanced on ruggedness, using median settlement elevation						
Below canal	0.029*	0.004	-0.003	0.000	-0.001	0.008*
	(0.007)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)
Control group mean	5.802	0.149	0.076	0.061	9.542	9.757
Observations	99,102	92,493	92,493	92,493	91,030	91,235
R^2	0.490	0.020	0.020	0.020	0.500	0.600
Panel D: Canal-area settlements balanced on ruggedness, using 25th percentile settlement elevation						
Below canal	0.109***	-0.018	-0.002	-0.018	0.004	0.021***
	(0.006)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Control group mean	5.696	0.145	0.075	0.056	9.550	9.756
Observations	103,548	96,668	96,668	96,668	94,966	95,298
R^2	0.500	0.020	0.020	0.020	0.480	0.590
Panel E: Main analysis sample, excluding villages intersected by a canal						
Below canal	0.150***	-0.063	-0.004	-0.056	0.003	0.021***
	(0.003)	(-0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Control group mean	5.590	0.151	0.072	0.064	9.550	9.747
Observations	91,465	85,342	85,342	85,342	83,802	84,108
R^2	0.460	0.020	0.010	0.020	0.470	0.560
Panel F: Main analysis sample, additional control for distance to canal						
Below canal	0.095***	-0.060	-0.003	-0.055	-0.001	0.012**
	(0.002)	(-0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Control group mean	5.590	0.151	0.072	0.064	9.550	9.747
Observations	91,465	85,342	85,342	85,342	83,802	84,108
R^2	0.460	0.020	0.010	0.020	0.470	0.560
Panel G: Command area boundary RDD						
Inside command area	0.226***	0.005	-0.004	0.004	0.000	0.034***
	(0.002)	(0.000)	(0.000)	(0.000)	(-0.001)	(0.000)
Control group mean	6.281	0.114	0.080	0.029	9.516	9.770
Observations	48,909	45,098	45,098	45,098	45,029	44,837
R^2	0.650	0.050	0.050	0.030	0.500	0.580

$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

Notes: This table shows non-farm outcomes using a range of samples to test the robustness of our main results of the regression discontinuity. Panel A contains all settlements ≤10km from the nearest canal in distance and ±50m from the nearest canal in elevation. Panel B removes the "donut hole", meaning all settlements ±2.5m from the canal are removed. The samples in Panel C and D use the same sample restrictions as as our main, balanced analysis sample (shown for reference in panel F), but use different values for settlement elevation. Panel C uses the median while Panel D uses the 25th percentile to parameterize settlement elevation when calculating relative elevation. Panel E shows the results of the regression discontinuity using the distance to command area boundary specification.

Table A7: Regression discontinuity results for education outcomes for all specifications

	At least primary (share of adult pop.)	At least middle (share of adult pop.)	At least secondary (share of adult pop.)	Literacy (share of pop.)
Panel A: All canal-area settlement				
Below canal	0.015***	0.015***	0.011***	0.009***
	(0.001)	(0.001)	(0.000)	(0.000)
Control group mean	0.462	0.308	0.185	0.554
Observations	231,452	231,452	231,452	245,531
R^2	0.580	0.560	0.530	0.600
Panel B: All canal-area settlements, minus donut hole				
Below canal	0.021***	0.020***	0.015***	0.013***
	(0.001)	(0.000)	(0.000)	(0.000)
Control group mean	0.447	0.294	0.176	0.545
Observations	125,287	125,287	125,287	132,969
R^2	0.590	0.580	0.550	0.600
Panel C: Canal-area settlements balanced on ruggedness, using median settlement elevation				
Below canal	0.007***	0.005**	0.003	0.003*
	(0.001)	(0.001)	(0.001)	(0.001)
Control group mean	0.482	0.324	0.196	0.567
Observations	93,374	93,374	93,374	99,102
R^2	0.590	0.570	0.540	0.610
Panel D: Canal-area settlements balanced on ruggedness, using 25th percentile settlement elevation				
Below canal	0.012***	0.010***	0.009***	0.008***
	(0.001)	(0.001)	(0.001)	(0.001)
Control group mean	0.475	0.317	0.192	0.564
Observations	97,460	97,460	97,460	103,548
R^2	0.580	0.570	0.550	0.610
Panel E: Main analysis sample, excluding villages intersected by a canal				
Below canal	0.013***	0.012***	0.009***	0.011***
	(0.001)	(0.000)	(0.000)	(0.000)
Control group mean	0.465	0.308	0.187	0.558
Observations	86,068	86,068	86,068	91,465
R^2	0.580	0.560	0.540	0.590
Panel F: Main analysis sample, additional control for distance to canal				
Below canal	0.006	0.006**	0.005**	0.006***
	(0.000)	(0.000)	(0.000)	(0.000)
Control group mean	0.465	0.308	0.187	0.558
Observations	86,068	86,068	86,068	91,465
R^2	0.580	0.560	0.540	0.590
Panel G: Command area boundary RDD				
Inside command area	0.021***	0.021***	0.020***	0.019***
	(-0.001)	(-0.001)	(0.000)	(0.000)
Control group mean	0.471	0.322	0.186	0.562
Observations	45,941	45,941	45,941	48,909
R^2	0.660	0.620	0.570	0.690

$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

Notes: This table shows education outcomes using a range of samples to test the robustness of our main results of the regression discontinuity. Panel A contains all settlements ≤10km from the nearest canal in distance and ±50m from the nearest canal in elevation. Panel B removes the "donut hole", meaning all settlements ±2.5m from the canal are removed. The samples in Panel C and D use the same sample restrictions as as our main, balanced analysis sample (shown for reference in panel F), but use different values for settlement elevation. Panel C uses the median while Panel D uses the 25$^{\text{th}}$ percentile to parameterize settlement elevation when calculating relative elevation. Panel E shows the results of the regression discontinuity using the distance to command area boundary specification.

Table A8: RD analysis sensitivity to bandwidth and canal distance

Panel A. Regression discontinuity bandwidth

Bandwidth (m)	Total irrigated area (share of ag. land)	Rabi (winter) ag. prod (log)	Population density (log)	Literacy (share of pop.)	Balance (ruggedness)	Sample size
25	0.065***	0.054***	0.106***	0.009***	-0.044	88,535
	(0.009)	(0.012)	(0.024)	(0.002)	(0.057)	
50	0.068***	0.080***	0.150***	0.011***	-0.008	91,465
	(0.008)	(0.012)	(0.023)	(0.002)	(0.056)	
75	0.075***	0.071***	0.165***	0.010***	-0.056	90,735
	(0.008)	(0.012)	(0.023)	(0.002)	(0.055)	

Panel B. Percent difference in ruggedness

Percent difference in ruggedness (km)	Total irrigated area (share of ag. land)	Rabi (winter) ag. prod (log)	Population density (log)	Literacy (share of pop.)	Balance (ruggedness)	Sample size
10%	0.065***	0.046***	0.126***	0.006**	-0.011	54,914
	(0.011)	(0.016)	(0.028)	(0.003)	(0.037)	
25%	0.074***	0.073***	0.150***	0.011***	-0.008	91,465
	(0.008)	(0.012)	(0.023)	(0.002)	(0.056)	
50%	0.075***	0.068***	0.167***	0.011***	-0.167***	116,695
	(0.007)	(0.011)	(0.019)	(0.002)	(0.053)	

Panel C. Distance to Canal

Max distance to canal (km)	Total irrigated area (share of ag. land)	Rabi (winter) ag. prod (log)	Population density (log)	Literacy (share of pop.)	Balance (ruggedness)	Sample size
5	0.082***	0.062***	0.179***	0.008***	-0.015	61,217
	(0.012)	(0.016)	(0.028)	(0.003)	(0.052)	
10	0.074***	0.073***	0.150***	0.011***	-0.008	91,465
	(0.008)	(0.012)	(0.023)	(0.002)	(0.056)	
15	0.069***	0.077***	0.143***	0.003	-0.045	109,071
	(0.007)	(0.011)	(0.020)	(0.007)	(0.044)	

$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

Notes: This table shows the sensitivity of our major results to changes in the construction of our sample. Each panel shows the results if one assumption were to be changed. The bolded parameters indicate the values used in our preferred sample. These preferred values are used for the two parameters not being tested in each panel. In panel A, we modify the bandwidth of the regression discontinuity where 50m would include settlements that lie 50m above to 50m below the nearest canal. Here we test 25m and 75m bandwidths in addition to our preferred 50m bandwidth. In panel B, we modify the threshold allowed for the average difference in ruggedness between treatment and control settlements for each fixed effect group. We test 10% (more strict) and 50% (less strict) in addition to our preferred 25% threshold. Lastly in panel C we modify the maximum distance a settlement may lie away from the nearest canal to be considered treated by that canal. Here we test 5km and 15km in addition to our preferred 10km.

Table A9: Spillovers analysis robustness for irrigation outcomes using entropy balance

	Total irrigated area (share of ag. land)	Canal irrigated area (share of ag. land)	Tubewell irrigated area (share of ag. land)	Other irrigated area (share of ag. land)
Panel A. Entropy balance, no outliers dropped				
Below canal	0.070***	0.093***	0.006	-0.018**
	(0.016)	(0.011)	(0.007)	(0.009)
Above canal	0.012	0.005	0.014*	-0.006
	(0.009)	(0.004)	(0.008)	(0.005)
Control group mean	0.383	0.037	0.189	0.162
Observations	104,083	104,268	104,265	103,469
R^2	0.60	0.19	0.39	0.76
Panel B. Entropy balance, 1% outliers dropped				
Below canal	0.072***	0.095***	0.007	-0.019**
	(0.016)	(0.011)	(0.008)	(0.009)
Above canal	0.016*	0.007*	0.017**	-0.007
	(0.009)	(0.004)	(0.008)	(0.005)
Control group mean	0.380	0.036	0.193	0.155
Observations	93,834	94,012	94,014	93,246
R^2	0.59	0.19	0.39	0.76
Panel C. Entropy balance, 2.5% outliers dropped - **preferred specification**				
Below canal	0.074***	0.093***	0.011	-0.018*
	(0.016)	(0.011)	(0.008)	(0.009)
Above canal	0.016*	0.007*	0.018**	-0.009*
	(0.009)	(0.004)	(0.008)	(0.005)
Control group mean	0.381	0.034	0.196	0.156
Observations	80,408	80,572	80,576	79,864
R^2	0.60	0.20	0.39	0.77
Panel D. Entropy balance, 5% outliers dropped				
Below canal	0.069***	0.103***	0.007	-0.028**
	(0.019)	(0.013)	(0.010)	(0.011)
Above canal	0.008	0.006	0.012	-0.010
	(0.010)	(0.005)	(0.009)	(0.007)
Control group mean	0.380	0.029	0.205	0.150
Observations	63,864	64,027	64,020	63,385
R^2	0.63	0.20	0.41	0.77

$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

Notes: This table shows robustness to the spillovers analysis results presented in Table 5 for all irrigation outcomes. Results follow Equation 5.2 using entropy balancing method as described in the main results. Panel A does not drop any outliers while Panel B drops 1%, Panel C drops 2.5% (as in the main text), and Panel D drops 5% outliers.

Table A10: Spillovers analysis robustness for agriculture outcomes using entropy balance

	Agricultural land (share of village area)	Kharif (monsoon) ag. prod (log)	Rabi (winter) ag. prod (log)	Water crops (any)
Panel A. Entropy balance, no outliers dropped				
Below canal	0.017*	-0.002	0.043*	0.068***
	(0.009)	(0.019)	(0.022)	(0.022)
Above canal	-0.005	0.009	-0.021	0.037**
	(0.009)	(0.016)	(0.018)	(0.017)
Control group mean	0.562	7.760	7.329	0.648
Observations	115,251	115,413	115,158	88,658
R^2	0.53	0.83	0.57	0.65
Panel B. Entropy balance, 1% outliers dropped				
Below canal	0.022***	0.006	0.037*	0.069***
	(0.008)	(0.020)	(0.021)	(0.021)
Above canal	0.002	0.011	-0.020	0.046***
	(0.008)	(0.017)	(0.018)	(0.017)
Control group mean	0.565	7.784	7.325	0.642
Observations	104,415	104,384	104,139	80,371
R^2	0.54	0.84	0.56	0.68
Panel C. Entropy balance, 2.5% outliers dropped				
Below canal	0.018**	0.011	0.016	0.055**
	(0.008)	(0.016)	(0.021)	(0.022)
Above canal	-0.002	0.007	-0.029	0.039**
	(0.008)	(0.013)	(0.019)	(0.017)
Control group mean	0.569	7.808	7.329	0.632
Observations	90,055	89,997	89,800	69,287
R^2	0.55	0.86	0.56	0.70
Panel D. Entropy balance, 5% outliers dropped				
Below canal	0.018**	0.018	-0.017	0.050**
	(0.009)	(0.015)	(0.023)	(0.024)
Above canal	-0.004	0.000	-0.040*	0.048**
	(0.008)	(0.012)	(0.021)	(0.023)
Control group mean	0.584	7.826	7.368	0.613
Observations	71,244	71,202	71,069	53,857
R^2	0.56	0.88	0.57	0.70

$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

Notes: This table shows robustness to the spillovers analysis results presented in Table 5 for all agriculture outcomes. Results follow Equation 5.2 using entropy balancing method as described in the main results. Panel A does not drop any outliers while Panel B drops 1%, Panel C drops 2.5% (as in the main text), and Panel D drops 5% outliers.

Table A11: Spillovers analysis robustness for non-farm outcomes using entropy balance

	Population density (log)	Total emp (share of adult pop.)	Services emp (share of adult pop.)	Manuf. emp (share of adult pop.)	Consumption pc (log) (all households)
Panel A. Entropy balance, no outliers dropped					
Below canal	0.182***	0.003	0.004	0.000	0.012
	(0.031)	(0.006)	(0.003)	(0.004)	(0.008)
Above canal	0.016	-0.004	0.000	-0.003	-0.014*
	(0.026)	(0.008)	(0.003)	(0.006)	(0.008)
Control group mean	5.528	0.127	0.080	0.034	9.634
Observations	117,083	107,401	107,401	107,401	111,400
R^2	0.32	0.01	0.00	0.01	0.41
Panel B. Entropy balance, 1% outliers dropped					
Below canal	0.198***	0.006	0.004	0.003	0.014*
	(0.027)	(0.005)	(0.003)	(0.003)	(0.008)
Above canal	0.041*	0.002	0.001	0.002	-0.008
	(0.023)	(0.006)	(0.003)	(0.004)	(0.007)
Control group mean	5.534	0.120	0.077	0.033	9.634
Observations	105,874	97,379	97,379	97,379	100,701
R^2	0.29	0.01	0.00	0.02	0.42
Panel C. Entropy balance, 2.5% outliers dropped - **preferred specification**					
Below canal	0.191***	0.011	0.006	0.005	0.011
	(0.030)	(0.007)	(0.004)	(0.004)	(0.009)
Above canal	0.038*	0.006	0.003	0.003	-0.008
	(0.023)	(0.007)	(0.003)	(0.004)	(0.008)
Control group mean	5.524	0.109	0.069	0.032	9.636
Observations	91,267	83,986	83,986	83,986	86,640
R^2	0.29	0.01	0.00	0.02	0.43
Panel D. Entropy balance, 5% outliers dropped					
Below canal	0.210***	0.011	0.008*	0.004	0.014
	(0.035)	(0.007)	(0.004)	(0.004)	(0.011)
Above canal	0.026	0.008	0.004	0.004	-0.010
	(0.025)	(0.007)	(0.004)	(0.004)	(0.009)
Control group mean	5.492	0.102	0.065	0.029	9.641
Observations	72,192	66,484	66,484	66,484	68,366
R^2	0.28	0.01	0.00	0.03	0.40

${}^{*}p<0.10, {}^{**}p<0.05, {}^{***}p<0.01$

Notes: This table shows robustness to the spillovers analysis results presented in Table 5 for all non-farm outcomes. Results follow Equation 5.2 using entropy balancing method as described in the main results. Panel A does not drop any outliers while Panel B drops 1%, Panel C drops 2.5% (as in the main text), and Panel D drops 5% outliers.

Table A12: Spillovers analysis robustness for outcomes disaggregated by landownership using entropy balance

	Population density (log)	Total emp (share of adult pop.)	Services emp (share of adult pop.)	Manuf. emp (share of adult pop.)	Consumption pc (log) (all households)
Panel A. Entropy balance, no outliers dropped					
Below canal	0.182***	0.003	0.004	0.000	0.012
	(0.031)	(0.006)	(0.003)	(0.004)	(0.008)
Above canal	0.016	-0.004	0.000	-0.003	-0.014*
	(0.026)	(0.008)	(0.003)	(0.006)	(0.008)
Control group mean	5.528	0.127	0.080	0.034	9.634
Observations	117,083	107,401	107,401	107,401	111,400
R^2	0.32	0.01	0.00	0.01	0.41
Panel B. Entropy balance, 1% outliers dropped					
Below canal	0.198***	0.006	0.004	0.003	0.014*
	(0.027)	(0.005)	(0.003)	(0.003)	(0.008)
Above canal	0.041*	0.002	0.001	0.002	-0.008
	(0.023)	(0.006)	(0.003)	(0.004)	(0.007)
Control group mean	5.534	0.120	0.077	0.033	9.634
Observations	105,874	97,379	97,379	97,379	100,701
R^2	0.29	0.01	0.00	0.02	0.42
Panel C. Entropy balance, 2.5% outliers dropped - **preferred specification**					
Below canal	0.191***	0.011	0.006	0.005	0.011
	(0.030)	(0.007)	(0.004)	(0.004)	(0.009)
Above canal	0.038*	0.006	0.003	0.003	-0.008
	(0.023)	(0.007)	(0.003)	(0.004)	(0.008)
Control group mean	5.524	0.109	0.069	0.032	9.636
Observations	91,267	83,986	83,986	83,986	86,640
R^2	0.29	0.01	0.00	0.02	0.43
Panel D. Entropy balance, 5% outliers dropped					
Below canal	0.210***	0.011	0.008*	0.004	0.014
	(0.035)	(0.007)	(0.004)	(0.004)	(0.011)
Above canal	0.026	0.008	0.004	0.004	-0.010
	(0.025)	(0.007)	(0.004)	(0.004)	(0.009)
Control group mean	5.492	0.102	0.065	0.029	9.641
Observations	72,192	66,484	66,484	66,484	68,366
R^2	0.28	0.01	0.00	0.03	0.40

$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

Notes: This table shows robustness to the spillovers analysis results presented in Table 5 for outcomes disaggregated by landownership. Results follow Equation 5.2 using entropy balancing method as described in the main results. Panel A does not drop any outliers while Panel B drops 1%, Panel C drops 2.5% (as in the main text), and Panel D drops 5% outliers.

Table A13: Spillovers analysis robustness for irrigation outcomes using coarsened exact matching

	Total irrigated area (share of ag. land)	Canal irrigated area (share of ag. land)	Tubewell irrigated area (share of ag. land)	Other irrigated area (share of ag. land)
Panel A. Coarsened exact matching, no outliers dropped				
Below canal	0.056***	0.073***	0.002	-0.015*
	(0.013)	(0.009)	(0.008)	(0.009)
Above canal	0.000	0.004	0.005	-0.010
	(0.009)	(0.005)	(0.007)	(0.007)
Control group mean	0.415	0.041	0.209	0.171
Observations	47,144	47,262	47,240	46,841
R^2	0.55	0.21	0.42	0.66
Panel B. Coarsened exact matching, 1% outliers dropped				
Below canal	0.054***	0.066***	0.002	-0.009
	(0.014)	(0.009)	(0.009)	(0.010)
Above canal	-0.003	0.004	0.002	-0.009
	(0.010)	(0.005)	(0.008)	(0.008)
Control group mean	0.441	0.031	0.228	0.186
Observations	32,185	32,235	32,231	31,981
R^2	0.57	0.21	0.43	0.69
Panel C. Coarsened exact matching, 2.5% outliers dropped				
Below canal	0.056***	0.073***	-0.002	-0.011
	(0.014)	(0.011)	(0.010)	(0.009)
Above canal	0.003	0.009	0.006	-0.010
	(0.011)	(0.006)	(0.009)	(0.008)
Control group mean	0.436	0.032	0.224	0.188
Observations	21,369	21,398	21,388	21,240
R^2	0.58	0.24	0.44	0.68
Panel D. Coarsened exact matching, 5% outliers dropped				
Below canal	0.037**	0.071***	-0.009	-0.022**
	(0.017)	(0.012)	(0.008)	(0.011)
Above canal	-0.002	0.006	0.004	-0.016*
	(0.012)	(0.006)	(0.010)	(0.009)
Control group mean	0.436	0.020	0.223	0.197
Observations	13,147	13,163	13,160	13,071
R^2	0.58	0.22	0.45	0.70

$^{*}p<0.10, ^{**}p<0.05, ^{***}p<0.01$

Notes: This table shows robustness to the spillovers analysis results presented in Table 5 for all irrigation outcomes. Results follow Equation 5.2 employing coarsened exact matching (CEM), which discretizes continuous control variables into bins before drawing balanced groups from across the coarsened distributions. Panel A does not drop any outliers while Panel B drops 1%, Panel C drops 2.5%, and Panel C drops 5% outliers.

Table A14: Spillovers analysis robustness for agriculture outcomes using coarsened exact matching

	Agricultural land (share of village area)	Kharif (monsoon) ag. prod (log)	Rabi (winter) ag. prod (log)	Water crops (any)
Panel A. Coarsened exact matching, no outliers dropped				
Below canal	0.015*	-0.040***	0.011	0.028*
	(0.008)	(0.012)	(0.023)	(0.015)
Above canal	0.005	-0.005	0.001	0.013
	(0.008)	(0.010)	(0.019)	(0.013)
Control group mean	0.602	7.823	7.345	0.616
Observations	53,111	53,074	52,936	40,881
R^2	0.55	0.75	0.59	0.74
Panel B. Coarsened exact matching, 1% outliers dropped				
Below canal	0.009	-0.029**	0.010	0.025
	(0.010)	(0.014)	(0.022)	(0.015)
Above canal	0.005	-0.002	0.002	0.019
	(0.010)	(0.013)	(0.021)	(0.014)
Control group mean	0.645	7.868	7.434	0.584
Observations	36,432	36,402	36,327	27,819
R^2	0.60	0.77	0.62	0.72
Panel C. Coarsened exact matching, 2.5% outliers dropped				
Below canal	0.014	-0.033**	0.019	0.059***
	(0.009)	(0.014)	(0.025)	(0.020)
Above canal	0.008	-0.006	0.011	0.048**
	(0.009)	(0.012)	(0.024)	(0.021)
Control group mean	0.662	7.898	7.397	0.563
Observations	24,115	24,105	24,042	18,269
R^2	0.62	0.79	0.64	0.71
Panel D. Coarsened exact matching, 5% outliers dropped				
Below canal	0.006	-0.034**	0.028	0.033
	(0.010)	(0.016)	(0.024)	(0.022)
Above canal	0.005	-0.001	0.024	0.023
	(0.011)	(0.015)	(0.019)	(0.022)
Control group mean	0.650	7.935	7.400	0.551
Observations	14,644	14,646	14,595	10,998
R^2	0.64	0.79	0.66	0.71

$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

Notes: This table shows robustness to the spillovers analysis results presented in Table 5 for all agriculture outcomes. Results follow Equation 5.2 employing coarsened exact matching (CEM), which discretizes continuous control variables into bins before drawing balanced groups from across the coarsened distributions. Panel A does not drop any outliers while Panel B drops 1%, Panel C drops 2.5%, and Panel C drops 5% outliers.

Table A15: Spillovers analysis robustness for non-farm outcomes using coarsened exact matching

	Population density (log)	Total emp (share of adult pop.)	Services emp (share of adult pop.)	Manuf. emp (share of adult pop.)	Consumption pc (log) (all households)
Panel A. Coarsened exact matching, no outliers dropped					
Below canal	0.115***	0.004	0.008	-0.001	0.018**
	(0.027)	(0.010)	(0.008)	(0.003)	(0.009)
Above canal	0.047**	0.004	0.001	0.005	0.004
	(0.022)	(0.015)	(0.013)	(0.003)	(0.008)
Control group mean	5.546	0.119	0.079	0.033	9.661
Observations	53,865	50,066	50,066	50,066	51,264
R^2	0.32	0.00	0.00	0.02	0.39
Panel B. Coarsened exact matching, 1% outliers dropped					
Below canal	0.088**	0.006	0.011	-0.001	0.005
	(0.034)	(0.012)	(0.010)	(0.004)	(0.009)
Above canal	0.049	0.010	0.008	0.005	-0.003
	(0.031)	(0.017)	(0.015)	(0.004)	(0.009)
Control group mean	5.535	0.102	0.067	0.027	9.704
Observations	36,886	34,448	34,448	34,448	35,274
R^2	0.34	0.00	0.00	0.03	0.39
Panel C. Coarsened exact matching, 2.5% outliers dropped					
Below canal	0.098***	0.004	0.009	-0.001	0.004
	(0.029)	(0.012)	(0.009)	(0.006)	(0.010)
Above canal	0.048*	0.006	0.006	0.004	0.000
	(0.026)	(0.014)	(0.012)	(0.005)	(0.010)
Control group mean	5.508	0.129	0.086	0.035	9.695
Observations	24,419	22,811	22,811	22,811	23,299
R^2	0.33	0.00	0.00	0.04	0.38
Panel D. Coarsened exact matching, 5% outliers dropped					
Below canal	0.084***	0.007	0.008	0.003	-0.007
	(0.031)	(0.014)	(0.012)	(0.006)	(0.010)
Above canal	0.046*	0.008	0.008	0.004	-0.010
	(0.026)	(0.013)	(0.011)	(0.005)	(0.010)
Control group mean	5.446	0.088	0.062	0.021	9.690
Observations	14,822	13,848	13,848	13,848	14,243
R^2	0.36	-0.01	-0.01	-0.01	0.37

$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

Notes: This table shows robustness to the spillovers analysis results presented in Table 5 for all non-farm outcomes. Results follow Equation 5.2 employing coarsened exact matching (CEM), which discretizes continuous control variables into bins before drawing balanced groups from across the coarsened distributions. Panel A does not drop any outliers while Panel B drops 1%, Panel C drops 2.5%, and Panel C drops 5% outliers.

Table A16: Spillovers analysis robustness for outcomes disaggregated by landownership using coarsened exact matching

	Consumption (log) *Landless*	Consumption (log) *Landowners*	Middle school ed. *Landless*	Middle school ed. *Landowners*
Panel A. Coarsened exact matching, no outliers dropped				
Below canal	-0.006	0.033***	0.017***	0.040***
	(0.008)	(0.010)	(0.005)	(0.007)
Above canal	-0.011	0.010	0.008*	0.016***
	(0.009)	(0.010)	(0.005)	(0.006)
Control group mean	9.533	9.765	0.259	0.345
Observations	49,421	49,792	49,319	49,765
R^2	0.32	0.40	0.35	0.48
Panel B. Coarsened exact matching, 1% outliers dropped				
Below canal	-0.014	0.021**	0.010**	0.031***
	(0.011)	(0.010)	(0.005)	(0.006)
Above canal	-0.016	0.004	0.007	0.012**
	(0.010)	(0.010)	(0.005)	(0.005)
Control group mean	9.556	9.809	0.266	0.362
Observations	34,154	34,336	34,094	34,319
R^2	0.33	0.40	0.38	0.48
Panel C. Coarsened exact matching, 2.5% outliers dropped				
Below canal	-0.017*	0.029**	0.019***	0.034***
	(0.010)	(0.012)	(0.006)	(0.008)
Above canal	-0.021*	0.012	0.015**	0.014**
	(0.011)	(0.012)	(0.006)	(0.007)
Control group mean	9.547	9.790	0.257	0.350
Observations	22,573	22,734	22,531	22,721
R^2	0.31	0.38	0.36	0.47
Panel D. Coarsened exact matching, 5% outliers dropped				
Below canal	-0.006	0.007	0.019***	0.035***
	(0.011)	(0.012)	(0.007)	(0.008)
Above canal	-0.010	-0.004	0.013*	0.021***
	(0.011)	(0.011)	(0.007)	(0.008)
Control group mean	9.547	9.786	0.256	0.343
Observations	13,775	13,883	13,741	13,877
R^2	0.32	0.36	0.37	0.47

$^{*}p<0.10, ^{**}p<0.05, ^{***}p<0.01$

Notes: This table shows robustness to the spillovers analysis results presented in Table 5 for outcomes disaggregated by landownership. Results follow Equation 5.2 employing coarsened exact matching (CEM), which discretizes continuous control variables into bins before drawing balanced groups from across the coarsened distributions. Panel A does not drop any outliers while Panel B drops 1%, Panel C drops 2.5%, and Panel C drops 5% outliers.

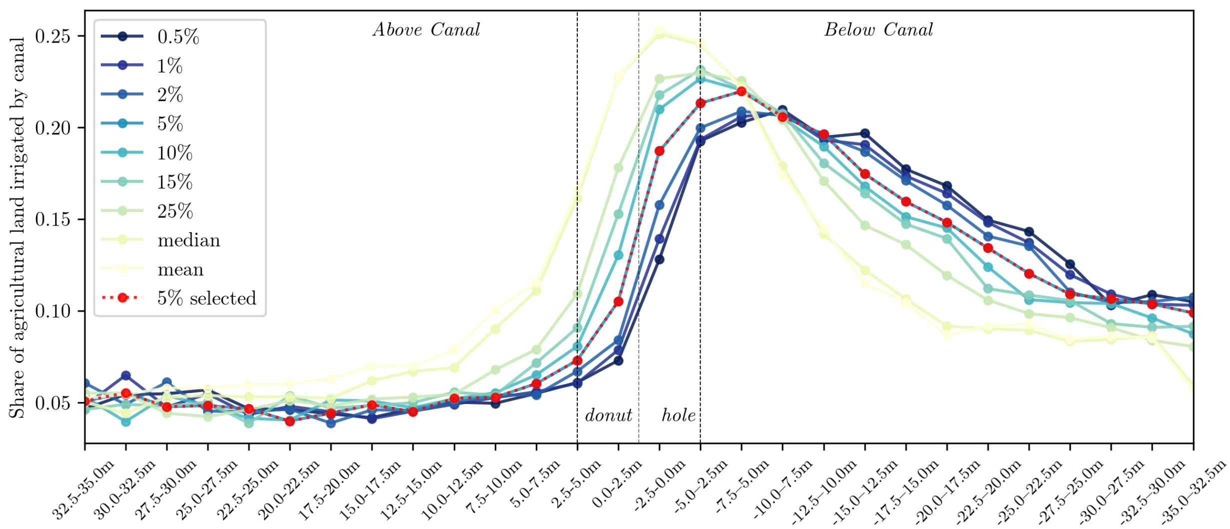
Table A17: Town analysis robustness

	Town Existence (pop. 5,000)			Population (log)			Growth (decadal)		
Panel A. 10km radius									
	1	2	3	4	5	6	7	8	9
Command area in town catchment area	0.030*			0.065**			0.057***		
(binary treatment)	(0.017)			(0.031)			(0.021)		
Share of 0-10km band in command area		0.056*	0.117		0.273***	0.241		0.064**	0.084
(continuous treatment)		(0.032)	(0.079)		(0.070)	(0.151)		(0.025)	(0.060)
Share of 10-20km band in command area			-0.078			0.041			-0.026
			(0.076)			(0.163)			(0.065)
Observations	25,416	74,952	74,952	25,416	74,952	74,952	23,298	68,706	68,706
R^2		0.70	0.67		0.82	0.80		0.15	0.06
Panel B. 20km radius									
	1	2	3	4	5	6	7	8	9
Command area in town catchment area	0.039**			0.084***			0.027		
(binary treatment)	(0.016)			(0.031)			(0.024)		
Share of 0-20km band in command area		0.054*	0.139*		0.334***	0.284*		0.074**	0.202***
(continuous treatment)		(0.032)	(0.079)		(0.084)	(0.171)		(0.032)	(0.070)
Share of 20-40km band in command area			-0.121			0.070			-0.182**
			(0.084)			(0.210)			(0.074)
Observations	26,292	71,520	71,520	26,292	71,520	71,520	24,101	65,560	65,560
R^2		0.70	0.67		0.82	0.81		0.15	0.06
Panel C. 30km radius									
	1	2	3	4	5	6	7	8	9
Command area in town catchment area	0.045***			0.098***			0.037		
(binary treatment)	(0.017)			(0.031)			(0.023)		
Share of 0-30km band in command area		0.056*	0.088		0.390***	0.312**		0.075**	0.194***
(continuous treatment)		(0.030)	(0.063)		(0.093)	(0.144)		(0.033)	(0.070)
Share of 30-60km band in command area			-0.049			0.120			-0.182**
			(0.073)			(0.176)			(0.082)
Observations	26,856	72,732	72,732	26,856	72,732	72,732	24,618	66,671	66,671
R^2		0.70	0.67		0.82	0.80		0.15	0.06
Panel D. 40km radius									
	1	2	3	4	5	6	7	8	9
Command area in town catchment area	0.020			0.032			-0.002		
(binary treatment)	(0.019)			(0.034)			(0.026)		
Share of 0-40km band in command area		0.060**	0.068		0.442***	0.398***		0.048	0.134**
(continuous treatment)		(0.030)	(0.054)		(0.100)	(0.130)		(0.036)	(0.064)
Share of 40-80km band in command area			-0.012			0.068			-0.135*
			(0.066)			(0.146)			(0.075)
Observations	25,824	74,352	74,352	25,824	74,352	74,352	23,672	68,156	68,156
R^2		0.70	0.67		0.82	0.80		0.15	0.06

$^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$

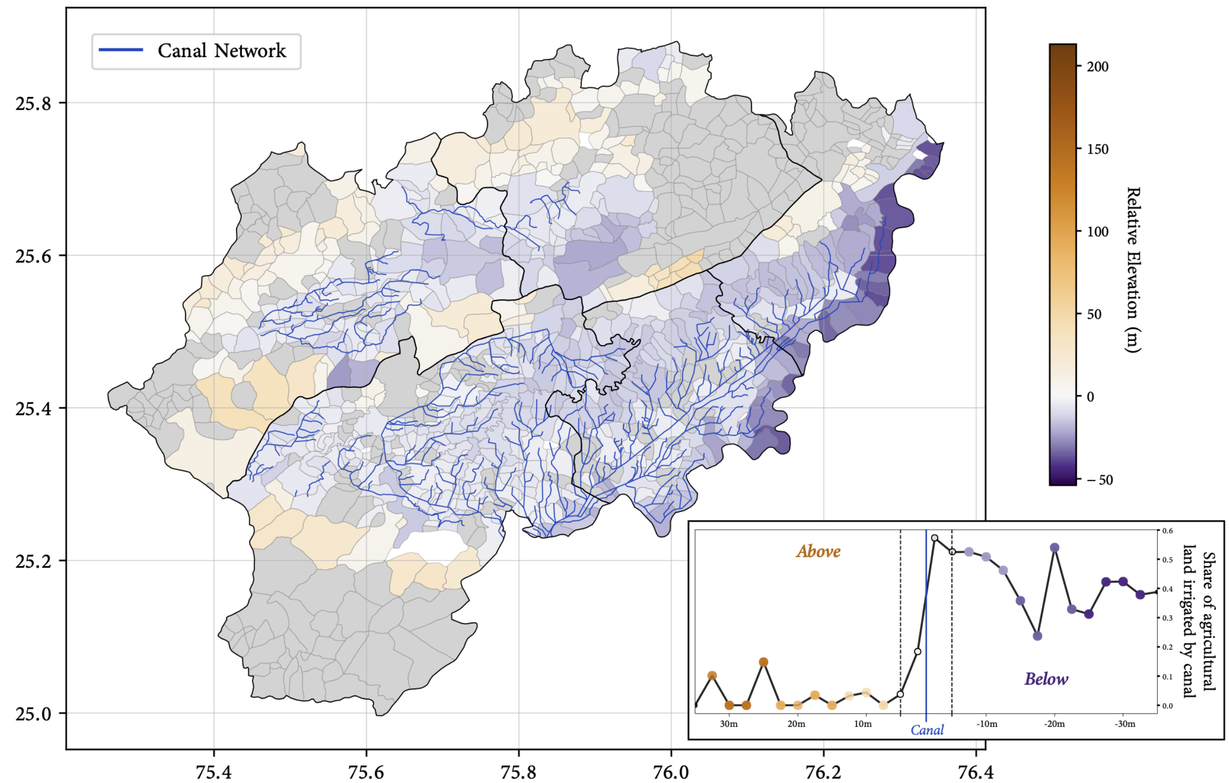
Notes: This table shows the effect of canal construction on town growth reported in Table 6 using various catchment area radii around the town results. An additional analysis includes the command area coverage of a second, outer band around each town as an additional independent variable. Panel B, using the 20km radius to define the town catchment area, is presented in Table 6.

Figure A1: Calculating the relative elevation of each settlement



Notes: Each line in this figure uses a different moment of the distribution of elevation in a settlement polygon to define the relative elevation between that settlement and the nearest canal. The elevation of the nearest canals is parameterized by the elevation of the single closest point. Share of agricultural land irrigated by canal is on the y-axis. Relative elevation is plotted on the x-axis, with negative relative elevation indicating settlements below the canal. We select the 5th percentile to define settlement elevation.

Figure A2: Relative elevation RDD empirical strategy



Notes: This figure illustrates our relative elevation empirical strategy using Bundi district in Rajasthan. Each polygon is a settlement (village or town), with its elevation relative to the nearest point on the nearest canal colored orange for settlements above the canal and purple for those below. Settlements that are more than 10km away from the nearest canal (in distance) or within ±2.5m (in elevation) of the nearest canal are excluded (light gray on the map). The inset plots the share of agricultural area that is irrigated by canal vs. the relative elevation for each settlement. The discontinuity is clear, with settlements topographically above the nearest canal having a significantly larger share of canal-irrigated area.