The Long Shadow of Feudalism:

Concentration of Land and Labor Market Power in India*

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ABSTRACT. Land is power: both state and non-state actors have understood this for centuries, but the causal impacts of land concentration are notoriously difficult to study. We study how differences in village land concentration stemming from the granting of feudal titles hundreds of years ago affect present-day labor markets in India. We exploit variation in land tenure systems at a more granular level than is seen in the literature on the long-run effects of land tenure systems, implementing a regression discontinuity along historically feudal borders that no longer correspond with modern administrative boundaries. Large discontinuities in land concentration persist across these boundaries, with the smallest land parcels in more concentrated areas 19% larger than their analogs in less concentrated areas. These differences are associated with 7% lower agricultural wages for women, but not men who are more able to travel and seek outside options. Importantly, these differences in wages persist despite no differences in yields, aggregate labor demand or supply, output prices, or other non-labor agricultural inputs. Village elected bodies in feudal areas scuttle the implementation of the key workfare program designed to provide agricultural labor with an outside employment option, with 71% fewer person-days offered during peak agricultural months when large landowners demand labor, and no difference the lean season when the program is most active. We show that the effect on the workfare program likely operates through caste-based links between large landowners and village elected representatives. This work emphasizes the effects of land inequality on local labor markets as a key mechanism through which inequities persist, and the value of outside options for workers where employers have market power.

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1 Introduction

Land ownership is the main source of political power and control in agrarian economies, and a core driver of rural inequality is the labor market power of large landowners (Hornbeck and Naidu 2014). Despite seventy-five years of state-led efforts to dismantle feudal legacies and reduce inequality in India, progress has been uneven and incomplete. Both the emergence of economic opportunities outside agriculture through economic growth, and social welfare policies, should in theory reduce inequality (Foster and Rosenzweig 2004; Harris and Todaro 1970; Lewis 1954; Muralidharan, Niehaus, and Sukhtankar 2023). However, the effectiveness of these processes critically depends on local institutions, which can be compromised by elite capture (Anderson, Francois, and Kotwal 2015). In India, historical land tenure arrangements in particular have significantly shaped present-day inequalities and rural development trajectories (Banerjee and Iyer 2005; A. Banerjee, Gertler, and Ghatak 2002) echoing the global literature on the institutional roots of inequality (Genicot and Ray 2017; Dell 2010; Kim and Wang 2024). In this paper, we extend beyond documenting the mere persistence of inequality and instead focus on how land concentration leads to elite capture undermining social welfare and labor markets.

Resolving persistent rural inequality has long been a central, yet often unfulfilled, goal of development policy in low- and middle-income countries. Among the most visible and politically fraught dimensions of this inequality is the concentration of land ownership, which remains a key determinant of wealth, power, and social stratification in agrarian societies. Land redistribution has historically been rare and typically occurs only in moments of profound political upheaval, such as the aftermath of colonial rule or revolutions (Binswanger-Mkhize, Bourguignon, and Brink 2009; Deininger 2003). In the absence of land reform governments devise rural welfare programs, but these can be undermined by a landed local elite. In this paper, we demonstrate that while direct redistribution remains elusive and social protection is partially sabotaged, the expansion of external economic opportunities – through labor mobility, non-farm employment, or government programs – can play an important role in mitigating the long-run consequences of entrenched land inequality.

We leverage variation induced by the historic boundaries of feudal land institutions in the princely state of Hyderabad to compare villages with differential exposure to land reform efforts in the early 1950s. By comparing adjacent villages within the same modern districts, we isolate granular spatial variation in historical land institutions that no longer correspond to administrative boundaries. We find that differences in land concentration persist to this day: at the 10th percentile of the landholding distribution, plot sizes are 20% larger in historically feudal areas. Strikingly, however, these differences in land concentration do not coincide with differences in yield, agricultural labor demand, agricultural investments, or other key indicators of economic structure. Despite these similarities in both the outputs and other factors of production, we uncover large and statistically significant differences in wages for agricultural workers with limited outside options, particularly women. After conditioning on crop and season, women's wages are 8% lower in villages that experienced weaker land reform due to their feudal legacy. In contrast, wages for male agricultural workers, who typically have greater spatial mobility and access to non-local labor markets, are not systematically different across these areas. We also find that the government's primary rural employment guarantee scheme, which functions as the main reservation wage option in this context (Kaur 2019), is implemented far less actively during peak agricultural seasons in formerly feudal areas. Specifically, the number of person-days of work provided during peak months is 70% lower in these villages. Notably, this gap disappears during the lean season when the program is not in direct competition with landowners, suggesting a strategic suppression of workfare when it could otherwise increase workers' bargaining power.

While historical institutions can influence development through multiple channels, we marshal a range of empirical evidence to argue that the primary driver of persistent rural inequality in our setting is the continued concentration of land and local political power in areas that were less exposed to land reform due to their feudal legacy. First, as a descriptive fact, we document that landholding and political authority remain tightly aligned. In a random subsample of villages, 71% of elected leaders belong to the same jati (sub-caste) as the single largest landowner, and 91% share a jati affiliation with at least one of the top twenty landowners. This pattern of social overlap between landed elites and political representatives is consistent with elite capture of decentralized institutions (Bardhan and Mookherjee 2000; Acemoglu and J. Robinson 2008), and mirrors findings in other settings where kinship and caste shape the distribution of local power (Munshi and Rosenzweig 2016). Second, we find that villages with higher land concentration systematically provide fewer workfare person-days during the peak agricultural season, and their leaders express a clear preference for reducing program intensity when surveyed. This aligns with prior evidence that powerful elites may resist redistributive programs when such interventions threaten their economic interests (Anderson, Francois, and Kotwal 2015; Alatas et al. 2012). The fact that workfare provision converges across villages during the lean season (when demand is highest and program implementation is less politically salient) suggests that these differences are not driven by capacity constraints or variation in demand, but rather by elite manipulation of local implementation. Third, we show that these effects are more pronounced in remote villages farther from district headquarters, where administrative oversight is weaker, and local political distoritions are less likely to be reigned in.

We also rule out the possibility that the observed differences in female wages are driven by productivity differences across the historical boundary. Male agricultural wages, crop yields, and labor demand remain largely comparable between feudal and non-feudal areas, suggesting that the marginal productivity of labor is not systematically lower in the former. Our analysis compares wages for clearly defined tasks on specific crops during specific seasons, thereby holding constant task-specific labor productivity. Moreover, these comparisons are made across highly local spatial discontinuities, distances that are economically arbitrageable for most agricultural inputs and outputs. The institutional setting of agriculture, where neighboring farmers' landholdings and planting choices are publicly observable, creates conditions conducive to tacit collusion in labor demand (Smith 1992; Foster and Rosenzweig 1994). For instance, farmers can strategically stagger planting schedules to avoid simultaneous labor demand peaks, thereby suppressing wage pressures. However, we argue that reduced provision of public employment through the workfare scheme is also a key driver of depressed female wages. By weakening the reservation wage and constraining workers' outside options, reduced program implementation enhances employers' monopsony power, with gendered consequences given women's more limited mobility. In support of this outside options lever, we also find lower wages for men where credit access is more constrained, indicating that employer market power affects workers who are more intrinsically tied to the frictions in the agricultural labor market.

To distinguish between two candidate mechanisms, i.e. direct collusion among landowners to suppress labor demand, and indirect suppression of wages via elite capture of public employment programs, we develop a simple model of oligopolistic competition among farmers of different landholding sizes. These size classes serve as a proxy for caste-based social groupings that often coordinate behavior in rural labor markets. The model illustrates how wage-setting behavior can emerge from market concentration, and allows us to assess whether the observed wage differentials are plausible given the empirical differences in landholding patterns. Crucially, we find that the magnitude of the wage discounts observed in historically feudal areas can only be rationalized when reduced provision of workfare is taken into account as a constraint on workers' labor supply elasticity. In other words, the wage suppression we observe is quantitatively consistent with a model where both collusive behavior *and* weakened outside options – due to elite interference in workfare – jointly depress wages. This finding highlights the broader role that workfare programs can play not just in poverty alleviation, but in counteracting employer monopsony and elite capture in rural labor markets (Muralidharan, Niehaus, and Sukhtankar 2023; Sokolova and Sorensen 2021).

This paper departs from much of the literature on historical persistence, including that on land tenure in India, in several important respects. First, rather than comparing broad administrative units, we exploit highly localized variation – the most granular possible – by comparing villages located in close geographic proximity, often within the same modern economic and political jurisdictions. In our preferred specification, 71% of matched villages share the same agricultural output markets, implying similar price exposure, and a comparable share fall under the same district headquarters. This ensures that producers on either side of the historical boundary operate under essentially the same market and governance conditions. By contrast, most prior work in the Indian context has relied on variation at the district level, which we explicitly orthogonalize in our identification strategy (Banerjee and Iyer 2005; Lee 2019).

Second, while migration remains a standard concern in geographic regression discontinuity designs, we document no meaningful differences in contemporary out-migration across the historical boundary. Migration flows in our context have historically been directed toward larger cities outside the study area, and we find no evidence of differential population dynamics between treatment and control areas over the last several decades.

Third, we find that the primary effect of land reform in this setting was to increase the number of small landholders, without altering the prevalence or scale of large farms.

As a result, we observe no meaningful treatment effects on agricultural yields, labor demand, irrigation rates, or input investments in areas with greater land concentration. This strengthens the case that observed wage differences stem not from productivity gaps, but from differences in labor market structure and employer concentration. Our design therefore allows for a focused examination of monopsony and elite capture as key channels of persistence, while ruling out several competing mechanisms.

Finally, whereas other studies find substantial effects on consumption and welfare outcomes (Batra 2024), we observe relatively modest impacts on household consumption and poverty. We similarly detect no large changes in education or health outcomes, though there is some evidence of fewer primary schools and healthcare centers in feudal areas—patterns that may reflect broader elite resistance to redistributive public investment.

Unlike much of the literature that compares areas under varying degrees of colonial administration, our analysis exploits variation in land tenure within the quasi-independent princely state of Hyderabad. This region, which today spans much of south-central India and encompasses over 72 million people as of 2025, was never directly governed by the British colonial state. Instead, it was ruled by the Nizam, who retained considerable autonomy in matters of taxation and governance. While some parts of the state were directly administered and taxed by the Nizam's officials, substantial areas were granted as feudal estates (*Jagirs*) – primarily in the early 19th century – as rewards for loyalty and military service. The boundaries of these estates were not rooted in geographic or cultural logic, but reflected political considerations designed to consolidate elite power. Crucially, these areas never developed the bureaucratic land administration systems seen in British-ruled provinces, leaving small farmers without formal land records or tenure security at the time of post-Independence reforms.

When land reform legislation was introduced in the 1950s, partly in response to the armed peasant uprisings of 1947–48 targeting exploitative landlords[1], farmers in these former feudal areas were less likely to benefit. Princely Hyderabad was marked by extreme inequality: the Nizam was reputedly the wealthiest man in the world in 1937 (Time Magazine 1937), deriving much of his wealth from land-based taxation. Historical survey data from 1954 confirms that small farmers in former Jagir villages were significantly less likely to gain ownership under the land reforms (Khusro 1958).

To study the long-run consequences of this differential exposure to land reform, we georeference detailed historical maps of feudal land tenure boundaries and link them to modern administrative data. We then use village-level landholding records (compiled by an agricultural insurance firm using scraped and processed government digitized land data across Karnataka, Telangana, and Maharashtra) to quantify and compare contemporary land concentration on either side of these historical borders.

For our primary outcome data, we conduct a large-scale phone survey covering over 2,000 randomly selected villages from a universe of approximately 20,000 villages within

[1]This movement is widely seen as a precursor to the Maoist insurgency that remained active in parts of India well into the 2010s.

our spatial regression discontinuity bandwidth. These surveys target both elected village representatives and local workers, allowing us to collect detailed data on agricultural outputs (such as yields) and inputs (including labor demand and wages). Crucially, wage and labor demand data are disaggregated by task and gender, enabling us to capture heterogeneity across work types and to focus on the gender segmentation that characterizes agricultural labor markets.[2] Crop choice and seasonal variation are explicitly recorded, allowing us to adjust for these potentially confounding factors throughout the analysis.

In addition to survey data, we compile administrative information on the quality and quantity of workfare provision at the village-month level using publicly available implementation records from the National Rural Employment Guarantee Scheme (NREGS). We further augment this dataset with a wide array of village-level indicators of service delivery from secondary data sources commonly used in the literature, including census and infrastructure surveys, allowing us to benchmark our results against broader development outcomes.

Our work contributes to several strands of literature. First, we add to a growing body of research on monopsony power in labor markets (Azar and Marinescu 2024; Sokolova and Sorensen 2021), with particular attention to its manifestation in rural agricultural settings. We document wage markdowns within the global range of monopsonistic labor markets, consistent with recent work quantifying labor supply elasticities and employer concentration (Felix 2024; Sharma 2023). Our findings align with emerging evidence of employer collusion and labor market power in the Indian context specifically (Sharma 2025). Importantly, we extend this literature by tracing the historical origins of present-day monopsony, showing how land concentration created durable asymmetries in employer power that persist through institutional and social channels.

Second, we contribute to research on land markets and tenancy reform in developing economies. Prior work has emphasized the productivity effects of land redistribution and the institutional tradeoffs between tenure security and labor market flexibility (A. Banerjee, Gertler, and Ghatak 2002; Besley, Leight, et al. 2016; Deininger 2003). In contrast, our setting reveals a decoupling between landholding structure and agricultural productivity: we find no meaningful differences in yields, labor demand, or investment across villages with differing historical exposure to land reform. Instead, the most salient outcome is the suppression of wages for unskilled agricultural workers, especially women, who face the weakest outside options in the labor market.[3]

Our focus on sub-populations most exposed to monopsony – specifically, female agricultural workers engaged in low-skill, immobile tasks – offers a new lens for evaluating

[2]The motivation for this granular focus is that agricultural labor markets in India are sharply segmented along gender and task lines (Brownstone 2025).

[3]The absence of positive effects in productivity in areas with more land concentration is contrary to what one might expect based on Foster and Rosenzweig (2022). We reconcile this by focusing on the source of the variation we exploit: the number and size of large plots of land is no different in previously feudal and non-feudal areas. The difference we exploit lies in the bottom of the distribution. For context, even the average land plot in the top quartile in our setting is substantially smaller than the benchmark laid out in Foster and Rosenzweig (2022) for productivity gains to kick in.

the distributional impacts of institutional legacies in rural labor markets. By leveraging gender-task disaggregation and high-resolution spatial variation, we document not only the persistence of inequality but also the mechanisms – elite capture and constrained outside options – through which it operates.

We further contribute to the literature on elite capture of local redistributive institutions. A growing body of research has documented how local elites may undermine public programs, particularly in settings with weak state capacity and limited administrative oversight (Finan, Olken, and Pande 2017; Mitra 2021). These risks are especially acute when service delivery depends on discretionary decisions by local political actors, creating opportunities for politically mediated allocation and exclusion. Consistent with this literature, and following Anderson, Francois, and Kotwal (2015), we show that landed elites strategically suppress the implementation of workfare programs.

Most directly, this paper contributes to the literature on the long-run effects of historical institutions, with a specific emphasis on the enduring consequences of land ownership structures in India (Banerjee and Iyer 2005; Lee 2019; Batra 2024). Our first key contribution is methodological: we leverage spatial regression discontinuities along multiple historical land tenure borders that no longer align with modern administrative boundaries. This approach offers stronger internal validity by reducing reliance on any single border comparison, and makes our estimates less vulnerable to geographic or institutional confounding. Moreover, by exploiting fine-grained spatial variation, we are able to focus on localized labor market dynamics in a setting where broader development indicators have largely converged over time.

Second, we expand the empirical focus of the historical institutions literature by turning attention to labor market outcomes rather than solely on service delivery or aggregate welfare. Even studies that exploit similarly granular variation tend to rely on administrative data and emphasize broad welfare differences, which are often uniformly poor across rural areas (Ratnoo 2024). In contrast, we document how land inequality continues to affect labor market functioning through large landowners' control over social services and the wage-setting process. This mechanism is particularly consequential for low-mobility workers, especially women, who lack viable alternatives and thus face structurally lower wages.

Our findings carry clear implications for contemporary policy debates on rural inequality and labor market vulnerability (Breza and Kaur 2025). While redistributive land reform is politically and administratively difficult to implement at scale, our results show that entrenched land inequality can sustain unequal outcomes even in the absence of productivity differences. Addressing these disparities may require expanding outside options for the most vulnerable—such as through effective public employment schemes, improved credit access, or reduced barriers to labor mobility. However, these mechanisms are themselves susceptible to elite capture, echoing the core challenge highlighted in classic work on local governance and redistribution (Bardhan and Mookherjee 2000). In this context, investments in state capacity and administrative autonomy—such as those studied in Muralidharan, Niehaus, and Sukhtankar (2023) and Sharan and Kumar (2021)—are especially salient, as they may help insulate redistributive programs from the influence of dominant local actors.

The rest of the paper proceeds as follows. In section 2, we discuss the institutional historical background. In sections 3 and 4, we discuss the data and empirical strategy respectively. In section 5, we present the first-stage effects on land concentration, while in section 6, we discuss effects on the labor market. In section 7, we discuss effects on other service delivery outcomes. We conclude in section 8.

2 Institutional background

Within Hyderabad state a feudal land tenure and taxation system dating back to the 18th century was allowed to persist until 1949. While most of the land was directly administered and taxed by the *Nizam*, the leader of the Hyderabad presidency, a significant amount of agricultural land was controlled by nobles who were given land grants in exchange for aiding the *Nizam's* ancestors in 18th century battles (Figure B.1). Even though Hyderabad state stopped fighting wars by the early 19th century, these fiefs persisted. In general, the boundaries of these fiefs do not coincide with any modern administrative boundaries which allows for a sharper focus on village level variation in institutions. Even the Hyderabad Princely state itself was divided between three modern Indian states: Maharashtra, Karnataka, and Telangana.

There were broadly four types of feudal estates in princely Hyderabad: *Jagir*, *Samasthans*, *Paigas*, and *Sarf-i-Khas*. Similar to the feudal estates still present in modern Britain, the boundaries of these territories is largely the product of innumerable historical clashes between great families. *Samasthans* were ancient Hindu kingdoms predating the *Nizam's* rise to power in the 1700s. These Hindu rulers were given a degree of feudal autonomy over many aspects of governance including taxation in exchange for helping the first *Nizams* consolidate power. The second type of feudal land was a *Jagir*. A *Jagir* was simply a tract of land whose public revenue was assigned to an individual in exchange for rendering some service to the state. In most cases this had to do with raising troops, but it was possible to be granted a *Jagir* for other services to the Nizam. These grants were often hereditary although sometimes an additional tax needed to be paid to maintain the *Jagir* across generations. The *Paiga* lands were hereditary lands given to a particular *Mughal* general in the early 1700s who was helpful in the first *Nizam's* military campaigns and later divided among his descendants. The final category of land was the *Nizam's* personal estates for the maintenances of his own family, the *Sarf-i-Khas*. These areas typically faced higher land rents and greater land concentration. One account suggests some *Jagirs* had thousands of acres and charged tenants ten times the rents of non-feudal areas. As of 1949, 36% of the area, 34% of the villages, and 29% of the population of the Hyderabad presidency belonged to one of the three types of feudal estates (Khusro 1958: 2). For the purpose of our paper, we pool together *Jagirs*, *Samasthans*, *Sarf-i-Khas* and *Paigas*, since each of these is associated with a degree of feudal control awarded to local elites in the historical record. More importantly, these areas were not part of British advised revenue modernization efforts which included the development of land records and a land administration bureaucracy [4]. The boundaries of these regions were primarily drawn due to political realities rather than cultural or economic factors.

When India gained independence from the British in 1947, the Hyderabad presidency refused to join the Indian union and was invaded by the Indian army. At the same time the *Nizam* was facing an armed communist rebellion from peasants opposing domination by the feudal landed gentry. Thus, when Hyderabad state was finally integrated into India, the abolition of the *Jagirdars* and land reform was an early priority. Notably, the peasant revolt became the roots of an armed Maoist rebellion in rural Telangana that persisted until the 2000s. Although under-powered, we find no evidence that the formerly feudal areas have more political violence or are systematically more likely to elect left-wing politicians (Tables C.2 - C.3). There is some evidence that districts with more feudal land do have more Maoist violence (Mukherjee 2021), but this wider regional variation would be controlled for in our primary regression discontinuity specification.

As written, the laws placed ceilings on land ownership levels and established protections for tenant farmers, expecting land titles to be transferred to those who had cultivated these lands in feudal areas for a period of six years leading up to the legislation. In practice, this was not implemented in earnest. As we discuss below, large shares of tenant farmers were eased off of the land they were cultivating, with ownership reverting to local elites. Importantly, the formerly feudal areas including the *Sarf-i-Khas* lacked a modernized revenue system with local revenue bureaucrats. Survey evidence from 1953 suggests farmers in former feudal areas faced more barriers accessing the tenure they were entitled to by the land reforms.

> "A significant degree of evasion is noticeable with respect to tenancy legislation and the law regarding ceilings on land. ... The tendency is for the smaller tenants to be more readily evicted than the larger ones and purchases of land have been undertaken more by the larger tenants than the smaller ones." (Khusro 1958: 167)

The same research surveys showed greater outmigration from the formerly feudal regions at the time of the reforms but also higher birthrates. Thus in the present day, these communities have roughly the same populations but have fewer households farming small landholdings.

Importantly, the boundaries of these feudal areas do not correspond with modern district or constituency boundaries, allowing us to isolate village level effects from other persistence channels that work through higher-level institutions. This is a key difference in our setting relative to other work on colonial land tenure systems, wherein the variation resides primarily at the level of administrative districts, the borders of which are largely time-invariant. Instead, we look at fine village-level variation over very narrow geographic bandwidths of up to 20 kilometers on either side of the border.

[4] An additional issue is that visually differentiating *Jagirs*, *Samasthans*, *Sarf-i-Khas* and *Paigas* holdings in the primary historical map we use is challenging

3 Data

We procured land concentration data from a remote sensing and agricultural insurance company, who shared with us village-level indicators for our sample area across all three states. The company had invested significant effort in scraping digitized government land records back when those websites made it possible to do this in bulk.[5] We use a range of publicly available datasets to estimate treatment effects on service delivery outcomes, including the population censuses of 1991, 2001 and 2011; the economic censuses of 2005 and 2013, the socio-economic of 2012; and a village-level report of basic infrastructure (called the Mission Antyodaya data set) from 2020.[6] In addition, we scraped data from the implementation monitoring portal of India's workfare program, the National Rural Employment Guarantee Scheme, detailing the take up and implementation quality in the villages in our sample area.

We also conducted a phone-based primary survey among 1641 village elected representatives and 459 agricultural laborers identified by these representatives. This survey was conducted July 2024 - September 2024 among respondents across all states and formerly feudal areas, focusing primarily on wages in agricultural labor markets. We stratify our sample across border line segments of 25 kilometers. Each survey is conducted with a member of the village elected body, and they are asked to report typical wages and other indicators summarizing labor market dynamics in their respective villages. The sample of villages covered in these phone surveys is shown in the map in Figure B.2 and the sample is detailed Table C.7. Each elected leader was asked to provide phone numbers of agricultural laborers hired. We sampled a subset of these numbers to verify the accuracy of leaders' reported wages (Table B.3). Unfortunately, this process largely yielded male workers so for the primary specification comparing genders we primarily rely on the employer reported wages. However, we use to laborer surveys to verify the accuracy of male wages reported by the representatives.

A novel aspect of our survey is that we collect wages separately by task and gender for each of the two main agricultural seasons: rabi and kharif. In particular, we chose two common crop-agnostic agricultural tasks. For men, we asked about wages for fertilizer application. For women, we asked about wages for weeding. This dis-aggregated look at the agricultural labor market is rare in the vast literature on agriculture in India. This literature tends to consider agricultural laborers as a monolith, but in fact their reliance on these labor markets, what their outside options are, and what wages they are offered are all likely segmented by whether tasks are skilled and the gender of the workers. Focusing on wages for specific tasks for specific crops at specific times also allows us to reduce the noise in our wage data. To further increase precision we ask

[5]We never received access to land parcel or owner-level data. Instead, they were able to run our analysis codes on their raw data to generate village-level aggregate indicators of land concentration, and percentiles of landholding which we use in our primary analysis of concentration. We did ask for the raw data for two villages to verify the companies data matched the online land records which are possible to query individually.

[6]These data sets were procured from the SHRUG repository (Asher, Lunt, et al. 2021). For a subset of villages, we also obtained individual land ownership data with names from the raw socioeconomic and caste census data.

for both peak and averages wages to better capture prevailing wages in these villages since temporary labor shortages and wage spikes can be common.

We also asked the political leaders about their opinion on the workfare program. In particular, we asked what they thought the optimal wage for this program should be. While leaders trying to improve the welfare of poor people in their villages might select a higher wage, those concerned about the program driving up wages farmers have to pay workers might select a lower wage.

Finally, the survey contained a questions on the overall village land and labor market to supplement the available data from other sources. We ask about village size and out-migration, the number of employees and employers, and the number of land transactions.

4 Empirical strategy

In order to accurately delineate the historical boundaries between territories that belonged to *Jagir*, *Samasthans*, *Paigas* and *Diwan-i-Khas*, i.e. feudal and non-feudal areas respectively, we georeferenced maps obtained from the National Archives of India, originally drawn by the Superintendent of the Revenue Survey and Settlement of the Hyderabad Presidency in 1854. This process involved matching known geographic landmarks such as rivers, coastlines, and towns from historical maps with present-day GPS coordinates. Given the potential for measurement error inherent in historical georeferencing, we implement a "donut-hole" design following standard best practices (Moscona, Nunn, and J. A. Robinson 2020), omitting observations within two kilometers on either side of the historical boundaries. This strategy helps mitigate potential misclassification issues arising from mapping inaccuracies. [7] We present our georeferenced study area in figure 1, with the original map in figure B.1, and the extent of our study area relative to the spatial extent of the entire country in figure B.4.

We follow Dell (2010) and Moscona, Nunn, and J. A. Robinson (2020) in implementing a spatial regression discontinuity design. In particular, this is the specification used in Moscona, Nunn, and J. A. Robinson (2020) who follow Gelman and Imbens (2019). We test a wide variety of different definitions of land concentration.

Our core specification for our land concentration and outcomes from secondary data is as follows:

$$Y_{id} = \beta_1 \text{Feudal}_{id} + \beta_2 \text{Distance}_{id} + \beta_3 \text{Distance}_{id} * I_{Feudal_{id}=1} + \beta_4 X_{id} + \phi_d + \gamma_i + \varepsilon_{id}$$

[7] Our results are robust and qualitatively similar regardless of the radius of the donut hole, but we lose considerable power for much larger distances.

where Y_{id} denotes the outcome variable for village i in district d. The key explanatory variable, Feudalid, is an indicator taking the value one if a village historically belonged to a feudal region and zero otherwise. The running variable, Distanceid, measures the geographic distance from the historical boundary in kilometers, coded positively for feudal and negatively for non-feudal regions. We allow different slopes on either side of the boundary through the interaction term Distance$id \times I$Feudal$id = 1$. To increase precision, we include a rich set of time-invariant characteristics as controls Xid, where appropriate, district fixed effects ϕ_d, and local boundary segment fixed effects γ_i. The boundary segments are created by splitting the border between feudal and non-feudal areas into segments of 25 kilometers each. We use heteroskedasticity-robust standard errors by default, clustering by village in cases with multiple observations per village. [8]

Note that we use present-day 2022 district fixed effects in our main specification, allowing us to isolate the small effects of cross-village differences while holding fixed district-level institutions. Much of the literature has previously used variation at the district level itself to evaluate the effects of land concentration on development outcomes. Importantly, present-day districts do not overlap with the borders between feudal and non feudal areas that drive our identifying variation. We estimate a total border between our feudal and non-feudal areas that is 3972 km long. Only 544 km (13.7%) of these borders fall within a 5 km bandwidth of the present-day district borders.[9]

For each unique data source (the Population Census, the Socio-economic and Caste Census, the Mission Antyodaya data, the Demographic and Health Surveys, and the scraped NREGS dashboard), we define geographic units separately and estimate distances from the border separating feudal and non-feudal areas, as well as identifying which line segment they are closest to and which present-day district they belong to. This process minimizes the need to create crosswalks across different data sources, as each strand of analysis is fully self-contained, and relies only on spatially matching a data source to the original georeferenced map.

There are some departures from this basic preferred specification that are worth noting. When analyzing primary data from our own surveys, we follow the same specification as above but with two modifications. First, we exclude line segment fixed effects, since there are too few observations per line segment to make crop fixed effects feasible. Further, our sampling strategy ensures the data is balanced by line segment. Second, when analyzing observations that are reported over multiple agricultural seasons and/or crop types, we deploy additional fixed effects over these dimensions and cluster our standard errors at the respondent level. Lastly, when working with data from the Demographic and Health Surveys (DHS), we revert to a simple OLS specification. This

[8]Given the geographic nature of our data, spatial autocorrelation could bias standard errors. As an additional robustness check, we also estimate Conley standard errors, allowing for arbitrary spatial correlation within a radii ranging from 5 to 20 kilometers. Our results remain robust to these alternative inference methods, providing confidence in the statistical validity of our findings.

[9]The overlap remains low at more granular administrative levels as well. At the sub-district level, only 1166 km (29.4%) of the borders overlap with the border between feudal and non-feudal areas.

is because the georeferencing process that we use to match DHS clusters only yields a handful of clusters in our study area and within our preferred bandwidth. As a result, we treat estimates from this exercise as suggestive as opposed to causal.

The validity of our core empirical strategy hinges on the assumption that villages close to the historical boundaries are comparable in all respects except for their historical feudal status. To test this assumption, we conduct a series of balance checks across multiple observable covariates, such as elevation, temperature, precipitation, soil fertility, and distance to urban centers. In figure 2, we present tests of continuity at the border for a range of variables to establish balance. This establishes broadly that most geographic features and time-invariant characteristics of feudal and non-feudal areas vary continuously across the threshold in our bandwidth of interest. Our results remain robust to the inclusion of these variables.

Our empirical strategy relies critically on the choice of bandwidth around the historical boundaries. Following Gelman and Imbens (2019), we employ an optimal bandwidth selection procedure based on minimizing mean squared error (MSE). Our baseline results use a 20 kilometer bandwidth, which is also an economically meaningful distance: on average, it takes about one hour on a bicycle to traverse 20 kilometers in rural India, making it a highly arbitrageable distance to commute to work. Additionally, we provide robustness checks using alternative bandwidth choices ranging from 2 kilmeters to 30 kilometers, with and without the donut hole (Tables F.1 - F.3). Results remain stable across these specifications, indicating that our findings are not driven by the choice of bandwidth.

5 Effects on land concentration

The historical distinction between feudal and non-feudal regions has significant implications for contemporary land concentration. Using detailed, village-level land parcel data, we find a pronounced difference in land concentration measures across the historical feudal boundaries. We first focus on aggregate indices of land concentration. In table 1, we show the treatment effects on a range of land concentration measures, following our primary specification. In columns 1 and 2, we show that treated areas do not have significantly different normalized Herfindahl-Hirschmann Index (HHI) or GINI indicators for landholdings. We find no significant on the normalized HHI, and a small decrease in the GINI (4%, p-value < 0.1). In columns 3 and 4, we report that the pareto shape parameters for the (80-20) and (90-10) splits are both 4% (p-value < 0.05) and 3% (p-value < 0.05) higher respectively in feudal areas.

While aggregate indicators like the HHI and GINI coefficient show modest differences, the distributional patterns offer richer insights. One reason the HHI and GINI are insufficient to detect localized effects is that they aggregate ownership patterns across the entire distribution, diluting pronounced differences at the bottom of the distribution.[10]

[10]The inability of these aggregate measures to detect and measure the extent of concentration driven

We know from anecdotal evidence and reports from around the time of the reform that this is where we should expect differences in the feudal and non-feudal areas (Khusro 1958).

To address this limitation, we introduce a novel measure: "class HHI,". This indicator is calculated at the level of landowning classes rather than individual landowners. These classes are defined as groups of landowners segmented by land size categories modeled off the categories in the agricultural census, specifically 0–0.5 acres, 0.5–1 acres, 1–2 acres, 2–3 acres, 3–4 acres, and greater than 4 acres. This indicator is particularly useful in our context, as it captures concentration within distinct landholding size groups, biasing towards the lower landowning classes where most of the mass of our population distribution lives. As a result, the class HHI is sensitive to variations at the lower end of the distribution, where historical feudal systems predominantly influenced outcomes. We report treatment effects on this indicator in column 5, finding that feudal areas are more concentrated in their landownership by 6% (p-value < 0.01) on this dimension.

Overall, we see strong evidence of a more compressed distribution of landholdings in feudal areas. Importantly, however, all of these measures mask differential impacts *across* the distribution of landholdings in both treatment and control areas. In order to address this question more directly, we evaluate differences in feudal and non-feudal areas across percentiles of landownership. In table E.1, we show the treatment effect on key points of the landholdings distribution in our sample in hectare terms, while table C.1 represents these treatment effects in terms of logged landholdings. The first percentile of landholdings in feudal villages is 17% higher (p-value < 0.01) than non-feudal villages. This effect dampens to being only about 4% at the 90th percentile (p-value < 0.1), decreasing largely monotonically across the distribution of landholdings.

We also show this dampening in the treatment effect over the distribution of (logged) landholdings in figure 3, and the RD plot on four points of the distribution in figure 4. While there is a sharp discontinuity at the border when comparing the 1st percentile, the effect diminishes markedly for the 99th percentile. This pattern is consistent with the history of this period. The abolition of the *jagirdari* system was implemented imperfectly at best, and seemed to lead to the easing out of smaller tenants disproportionately in feudal areas.

As has been documented in successful land reform contexts in India—most notably West Bengal—small farmers who gained tenure rights were significantly more successful in retaining their holdings compared to their counterparts in areas with weak or absent reforms (Besley and Burgess 2000; A. Banerjee, Gertler, and Ghatak 2002). In our setting, we find strong evidence of a persistent difference in the number of small farmers between feudal and non-fuedal areas. In the next section, we test how these differences translate to key development outcomes, the implementation quality of arterial public programs, and local labor markets.

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by ownership at either end of the distribution have been discussed previously in the inequality literature (Inoua 2021; Clementi et al. 2019).

6 Effects on village labor markets

We conducted detailed wage surveys with village elected leaders in our sample, carefully distinguishing between task and gender combinations. Focusing on these differences in wages is crucial, since agricultural labor markets are not uniform. Wage levels across villages differ by the skill level involved, and we use the task and gender to distinguish between these. In table 2, we report results from outcomes at the village - crop - season level. Each survey respondent is asked to report both peak and typical wages for both male and female tasks, for the primary and secondary crops across the two most recent agricultural seasons. This allows us to apply fixed effects at the crop and season level in our main regression discontinuity specification. Thus each column contains data across two seasons and crops. We find that while wages are lower in feudal areas across the board, the magnitudes of the differences are much larger and statistically significant among females: women in feudal areas earn an average of 6-7% lower for the same task on the same crop in the same season in feudal areas, relative to non-feudal areas. In contrast, male average wages are the same and peak wages are an insignificant 2% lower. The difference between male and female wages is statistically significant for both peak and average wages and when they are pooled in a regression with interactions (Table D.5). Results are robust but slightly noisier when wages reported by female workers are included in the regression (Table D.6).

Women in these settings tend to have limited mobility. These mobility constraints may have discontinuous impacts along the village or neighborhood border (Cheema et al. 2024) [11]. These mobility constraints faced by women may allow them to be disproportionately more exposed to local monopsony power in these labor markets. While the incidence of this monopsony may have reflected in lower wages even for male workers in contexts where male workers also faced constraints on mobility and migration, either due to travel costs or credit frictions (Jayachandran 2006a)[12].

Is this level of monopsony power reasonable? One approach, following Muralidharan, Niehaus, and Sukhtankar (2023), is to compare the markdown to benchmarks in the literature. In Appendix A we place our results on wages and land concentration in a standard model of oligopolistic competition to calculate markdowns. This procedures highlights that to quantitatively reconcile the results workers in feudal areas need to have worse outside options which we provide evidence for below. We estimate workers receive 77% of their marginal product in feudal areas and 84% in non-feudal areas. To contextualize, a review by Sokolova and Sorensen (2021) estimating elasticities across many global labor markets finds an average elasticity of 7.1, implying that workers receive 88% of their marginal product, with a 95% confidence interval from 64% to

[11] Cheema et al. (2024) show that rural women in Pakistan are four times as likely to visit a training center when they are based in their own village, with over half the penalty incurred upon crossing the village border. Field and Vyborny (2022) increased women's job search in Pakistan by providing a women-only transport service to and from work. Among rice transplanting women in Telangana, 47% report their husbands would not approve of them working outside their village including neighboring villages (Brownstone 2025.)

[12] We currently lack historical data on male wages to evaluate these effects.

93%. Thus, the markdowns we estimate are well within the global range.

A key determinant of rural labor market dynamics in this setting is the availability of outside options for workers. We see that the effects of land concentration on wages extend to men in labor markets where they have limited outside options.

The most important outside option in our setting is a job in a government program. In table 6 we present a range of outcomes related to NREGS, the rural workfare program that provides a statutory guarantee of 100 days of paid work every financial year to at least one member of any household who seeks it. A vast literature on NREGS shows that demand for this work is high across rural areas and implementation varies substantially (Imbert and Papp 2015). In our setting, NREGS implementation quality serves as a test of how healthy agricultural laborers' outside options are. Muralidharan, Niehaus, and Sukhtankar (2023) show that improving the quality of NREGS implementation drives up households' earnings by 14%, but that 86% of these gains come from increases in real wages *and* employment in the private agricultural labor market. This is reflected in increases in workers' reservation wages. In their setting, these effects on the private agricultural labor market are accentuated in villages where land ownership is more concentrated, suggesting that these labor markets are operating under monopoly power in the status quo prior to the improvement in NREGS implementation quality.[13] In column (2) of table 6, we show that feudal areas, where landownership is more concentrated, has 42% fewer active NREGS job cards, even thought the number of registered job cards is (noisily) similar. In column (3), we show that the number of households employed in NREGS on the extensive margin is 58% lower, and the total person-days employed in feudal areas is 67% lower. These effects point to depressed NREGS implementation quality in feudal areas overall. These results could plausibly be explained by those with monopoly power in agricultural labor markets being able to exert influence on local elected bodies to constrain workers' outside options.

7 Alternate Explanations

There is substantial evidence that the wage effects we describe above are driven by modern day collusion among large landowners and elected village leaders, rather than other candidate mechanisms such as differences in the marginal product of labor. We will first present evidence for our preferred mechanism, and then highlight the absence of evidence for other potential causal channels.

Although noisy, the wage results appear to be much larger during the monsoon growing season, known as Kharif, which is the season where cultivation is more labor intensive (Table D.7). This is a consistent feature of rural labor markets in this context: wage increases in specific seasons driven by periods of intense labor demand. This feature of agricultural labor markets makes it highly incentive compatible for landowning elites

[13] Muralidharan, Niehaus, and Sukhtankar (2023) is based on a large-scale RCT in the undivided state of Andhra Pradesh, which neighbors (and minimally overlaps with) our study sample.

to oligopolistically collude in order to constrain labor demand, avoiding these wage increases.

However, these landowning elites can only successfully collude if workers' outside options are limited. In our primary results, we show that lower wages are concentrated among female agricultural laborers, driven primarily by their worse outside options that tie them more closely to the frictions in the local labor markets. Importantly, we find suggestively similar results for men who are also constrianed in their non-agricultural labor market activity. We use variation in the roll-out of local bank branches, which is plausibly exogenous and has previously been shown in the literature to be associated with driving credit access in rural areas (Burgess and Pande 2005). Prior literature from rural labor markets also shows that access to local credit is associated with *decreased* labor supply elasticity dampening wage responses to agricultural productivity shocks (Jayachandran 2006b). Credit access likely improves the range of economic opportunities available in a village over time, most directly through self employment opportunities or providing liquidity for local migration opportunities. Notably, much of the work on bank branch expansion in India focuses on district level variation, which we control for in our setting. We find that in villages that got bank branches later, and thus are likely to have a more muted credit supply, there are significant wage markdowns for men as well as women (Table 3). This highlights the role women's lack of outside opportunities likely plays in driving the gendered nature of our main results. We explore other measures of outside economic opportunity (tables D.1 - D.6). While these additional results are not always statistically significant owing to the level of noise in these estimates, we find that the insights are qualitatively similar and directionally consistent with our preferred specification.[14]

Importantly, we also find that the wage effects are concentrated in more remote areas – both further from the district headquarters (Table 4) and in areas with lower local population density (Table D.3). In our setting, district headquarters are notional towns or cities where the enforcement of local-level state institutions is most active. Greater distance from a district headquarters has been consistently associated in the literature with diminished state oversight and administrative reach, granting local elites greater de facto autonomy (Asher, Nagpal, and Novosad 2019).[15]

So far, we have argued that our primary results showing significant wage markdowns are more pronounced for those with worse outside options, and that landowning elites have incentives to compromise workers' ability to access these outside options. However, we are yet to establish a connection between landowning elites and the village polity. To do so, we conducted an exercise where we matched the names of the largest landowners in a random subset of villages in our sample to the names of the political leadership of the village. We found that 71% of village in this sample have at least one

[14]In addition to the noise, one might also be concerned about potential endogeneity in the roll-out of these other dimensions of market access: major roads and special economic zones, unlike the bank roll-out which has previously been established as exogenous in the literature.

[15]Asher, Nagpal, and Novosad (2019) show that villages located farther from the district administrative center receive fewer roads, schools, and health centers, and exhibit poorer economic outcomes, reflecting reduced enforcement capacity by the state.

elected leader who belongs to the same *jati* (sub-caste) as the single largest landowner in the village, and 91% of the villages have at least one elected leader who belongs to the same *jati* as at least one of the top twenty largest landowners. Economic interests are closely aligned within sub-caste groups, and this is concrete evidence of the alignment of narrowly defined incentives between the political and landowning elites.[16] This results in the chronic underprovision of NREGS during peak agricultural months in feudal areas that we discuss above.

Notably, the fact that these effects only appear during the peak agricultural months when the program is less active and easier to implement (Table 7) is evidence that the withholding of days may be strategic and not an artifact of steady-state state capacity differences between feudal and non-feudal areas.[17]. Even stronger evidence that village leaders are strategically manipulating the supply of NREGS scheme comes from our survey of village leaders. When asked about the ideal wage for the scheme, leaders in feudal villages were significantly more likely to suggest a wage below the current statutory wage, or even zero (Table 9). Elite desire to subvert workfare is also consistent with Anderson, Francois, and Kotwal (2015), who evaluate a workfare scheme in Maharashtra which was a precursor to the nationwide NREGS. In addition, just like for our main wage results, we find that the NREGS effects we see are also stronger for more remote villages, in line with the fact that elite capture can be worse where local elites have more influence (Table 8).

A natural concern following these results is that feudal and non-feudal areas have different labor demand levels and/or different productivity. In the survey, we asked village leaders to detail the labor demand (in person-days) across all tasks for the primary and secondary crops in the most recent agricultural season for a typical 10 acre plot in their villages. We find no differences in the total labor demand across feudal and non-feudal areas, or in demand specifically for the tasks that we find the (gendered) wage effects on (Table 12). We similarly elicit the typical yields per acre for the primary and secondary crops across the two most recent seasons, as well as the acreage in the entire village dedicated to planting the crops (Table 13). Across all of these outcomes, we find no significant differences across feudal and non-feudal areas despite deploying the same specification as in the wage result.

We additionally collect information on a range of variables characterizing the aggregate labor markets in rural areas, including the total number of households (Table B.5), workers (Table B.7), and (recent and longer-term) migrants (Table C.5). We find no systematic differences across feudal and non-feudal areas in the thickness of the labor markets, as reported in table C.5, reinforcing the explanation that any differences in wages are likely downstream of differential monopsony power. We also do not find differences in the number of recent land transactions in feudal areas (Table C.5), reflecting the stickiness in land markets in this setting (Sood 2020; Sarap 1996; Rawal

[16] We find no significant treatment effects on the extent of caste overlaps between political and landowning elites in our sample (Table 5).

[17] While there are many pathways to strategically decrease NREGS provision by slowing projects, it is difficult for local officials to strategically increase NREGS provision through new projects (Muralidharan, Niehaus, and Sukhtankar 2016)

2001).

Beyond NREGS, we find some evidence of modestly reduced village public goods provision in education, health, and agriculture across feudal and non-feudal areas as potentially further evidence of elite capture. Namely, we find that feudal areas have a 1% greater share of the population without a primary education (Table C.9), fewer non-primary schools (Table C.10), and a substantially farther distance to access primary and community health care centers (Table C.10). Feudal areas also have fewer government fertilizer shops, seed centers, and soil testing services which are theoretically more useful to small(er) farmers (Table 10). The small education differences that we see are unlikely to be meaningful for the unskilled labor markets we study. Similarly, the agriculture infrastructure differences do not appear to appreciably effect yields or labor demand at the village level. Finally, despite differences in health access we do not find they are large enough to lead to differences in health outcomes (Table C.12). As a result we do not expect productivity differences through the health channel to be meaningful.

An additional concern is that despite survey reports of similar labor demand, differential agricultural investments lead to different agricultural production functions in feudal and non-feudal areas. In this context, the lower wages in feudal areas could simply be a result of lower labor productivity. Using data from the 2012 SECC, we can reject decreases in mechanization or irrigation of more than 3 percentage points from bases of 83% and 88% respectively (Table 10). In our surveys, we elicit from GP heads information on how many households in their GPs own tractors and have made investments greater than INR 1 lakh (USD 1200) on their land. We do not see any systematic or statistically significant differences across feudal and non-feudal areas (Table 11).

These differences in inputs and the quality of agricultural investments have modest impacts on yields themselves, as discussed previously in Table 13. While we lack the ability to establish mechanisms for these results conclusively with individual level landholding-linked data on agricultural inputs or yields, we contend that these aggregate effects on agricultural outcomes mask substantive distributional differences that are downstream of land concentration. Larger landowners have the ability to drive higher mechanization in their own fields given economies of scale (Foster and Rosenzweig 2022). However, the particular land reform in this paper primarily increased the number of small farms rather than increasing the size of large ones. The 75th percentile landholding, where the effects begin to taper, is 13 acres, which is just below the 14 acre threshold where Foster and Rosenzweig (2022) estimate productivity begins increasing. The most important take-away from these results is that the feudal areas are not more productive than the non-feudal ones as a result of their land concentration, nor are the productivity differences large enough to explain within season and within crop wage gaps.

Lastly, a remaining concern with our results is that changes in the composition of the population and differential migration may be linked to the quality of service delivery. If differential land concentration and agricultural labor market activity pushes certain types of households out of feudal areas, we might expect lower demand for key government programs, affecting the quantity and quality of their delivery. There is some evidence of differential migration in the early years of the reform:

> "There has been a marked tendency for families to leave ex-*jagir* villages and such an exodus has accounted for a reduction in families by 8.6% in these villages" (Khusro 1958: 170)

In order to address this concern, we evaluate long-term population trajectories in our study areas, comparing inflows and outflows in feudal and non-feudal villages. We show trends for the overall population in Figure B.5, and trends specifically for the working age population in Figure B.7.[18] These results suggest that differences by population, both overall and working age, are relatively stable over the course of 1991 and 2011 as reported in three rounds of the population census.[19] The historical literature suggests that the period immediately following the land reform legislation saw movement out of feudal areas, but there has not been meaningful *additional* movement in the last four decades. This is consistent with a broader literature that finds frictions in agricultural labor markets constraining worker mobility across space and sectors (Baysan et al. 2024).

Overall, our results provide a consistent story through numerous pieces of evidence from orthogonal data sources. The aggregate wage markdowns we observe are driven by the lack of outside options. Importantly, (political and landowning) elites have the ability to strategically suppress the ability of workers to use non-agricultural work to either exit agricultural labor, or bargain for better wages in agricultural labor.

8 Conclusion

In this paper, we evaluate the long-run effects of historical differences in land tenure systems in princely Hyderabad, and the flawed application of land reform legislation in this region in the post-colonial period, on a range of rural service delivery outcomes as well as agricultural labor markets. We show that land concentration levels remain persistently elevated more than 100 years later in regions where land parcels were gifted to local elites. These areas also demonstrate poorer service delivery outcomes on a range of health and education variables.

Moreover, we find significant evidence of elite capture and monopsony power in rural labor markets. We find that these regions have worse implementation of the arterial workfare program, meant to provide an outside option for primarily landless agricultural laborers. Scuttling the implementation of this workfare program allows large landowners in feudal areas to sustain dramatically lower wages for agricultural workers in their own lands. The wage markdowns are significantly worse for workers with

[18]Importantly, we also do not see any differential trends in the caste and tribal composition of populations in this period (panels 2 and 3 of Figure B.5).

[19]Regardless, we normalize indicators by population (either in per capita terms or in terms of the total number of households) where appropriate.

even poorer outside options owing to constraints on their mobility: in our setting, these are women working on unskilled tasks.

Our work extends recent advances in our understanding of monopsony as well as elite capture in rural labor markets, and highlights the importance of persistent land distributions in understanding the long run effects historical institutions on development.

Table 1: Aggregate land concentration

	Land concentration measures				
	Norm HHI	GINI	Pareto 80 : 20	Pareto 90 : 10	Class HHI
Feudal	0.002	-0.011*	0.010**	0.009**	0.027***
	(0.003)	(0.006)	(0.005)	(0.004)	(0.010)
Non-feudal mean	0.017	0.234	0.453	0.601	0.469
R^2	0.12	0.93	0.38	0.35	0.29
Observations	9006	9011	9011	9011	9011

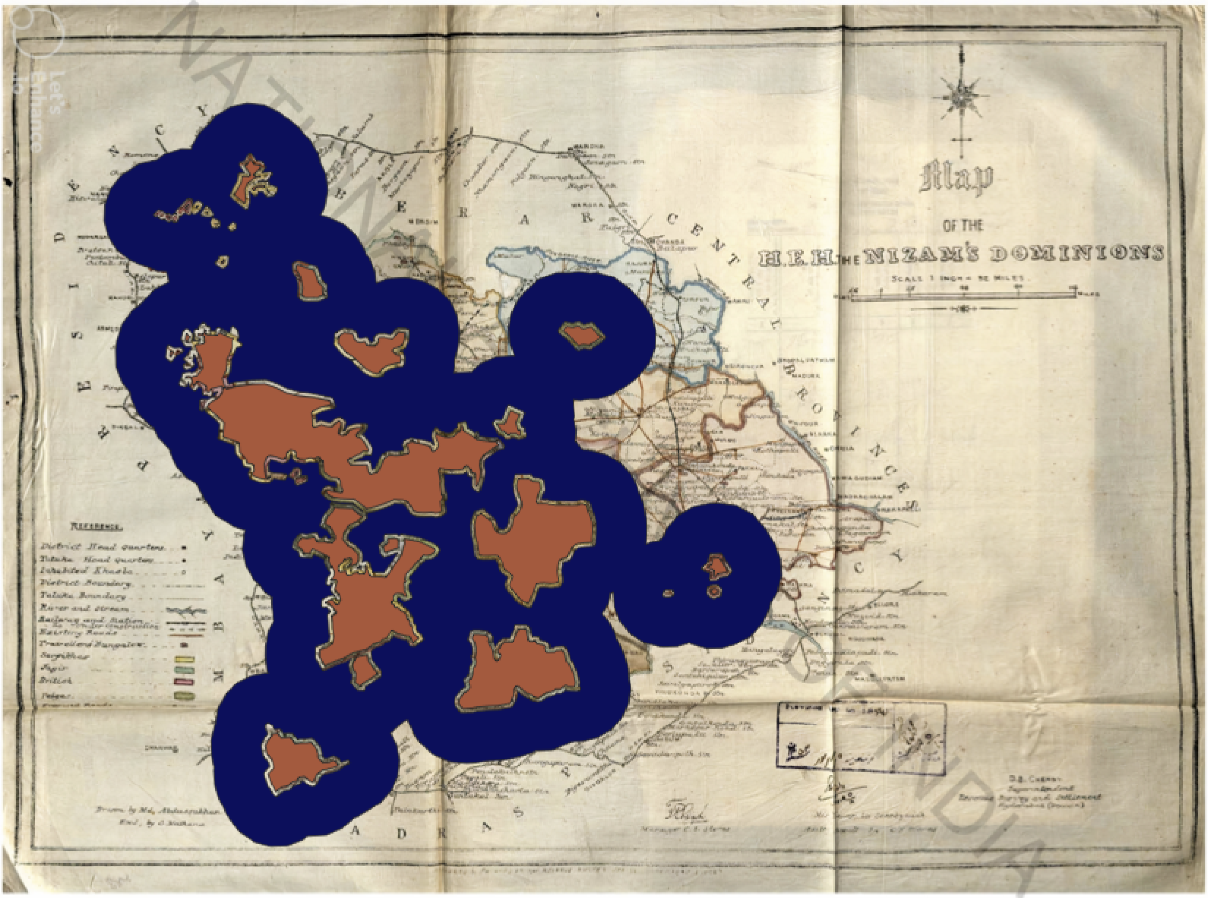
This table shows results from our primary regression discontinuity specification on a range of land concentration measures. In column (1), we show the effects on a normalized Herfindahl-Hirschmann index for land ownership. In column (2), we show the effects on the GINI index. In columns (3) and (4), we show the Pareto shape parameters for 90-10 and 80-20 splits. And in column (5) we show an intuitive measure of the HHI calculated at the landowning-class level, as opposed to the individual land parcel level. All indicators are created using raw land parcel level data procured from a remote sensing company, corresponding to the state of landownership in our study area as of September 2022. Each regression includes fixed effects at the district and border line segment level, and standard errors are robust. $*\, p < 0.1, **\ p < 0.05$, $***\, p < 0.01$

Table 2: Wages for agricultural labor

	Peak male fertilizer		Peak female weeding		Avg male fertilizer		Avg female weeding	
		Ln		Ln		Ln		Ln
Feudal	-13.738	-0.021	-25.939***	-0.067***	-6.334	-0.008	-17.453**	-0.055**
	(10.707)	(0.022)	(9.879)	(0.025)	(9.847)	(0.021)	(7.432)	(0.024)
Non-feudal mean	524.630	6.235	371.358	5.854	475.762	6.128	294.371	5.631
Male vs. Female p-value			0.349	0.067			0.285	0.062
R^2	0.40	0.33	0.53	0.51	0.52	0.47	0.53	0.53
Observations	6935	6935	7313	7313	7603	7603	7792	7792

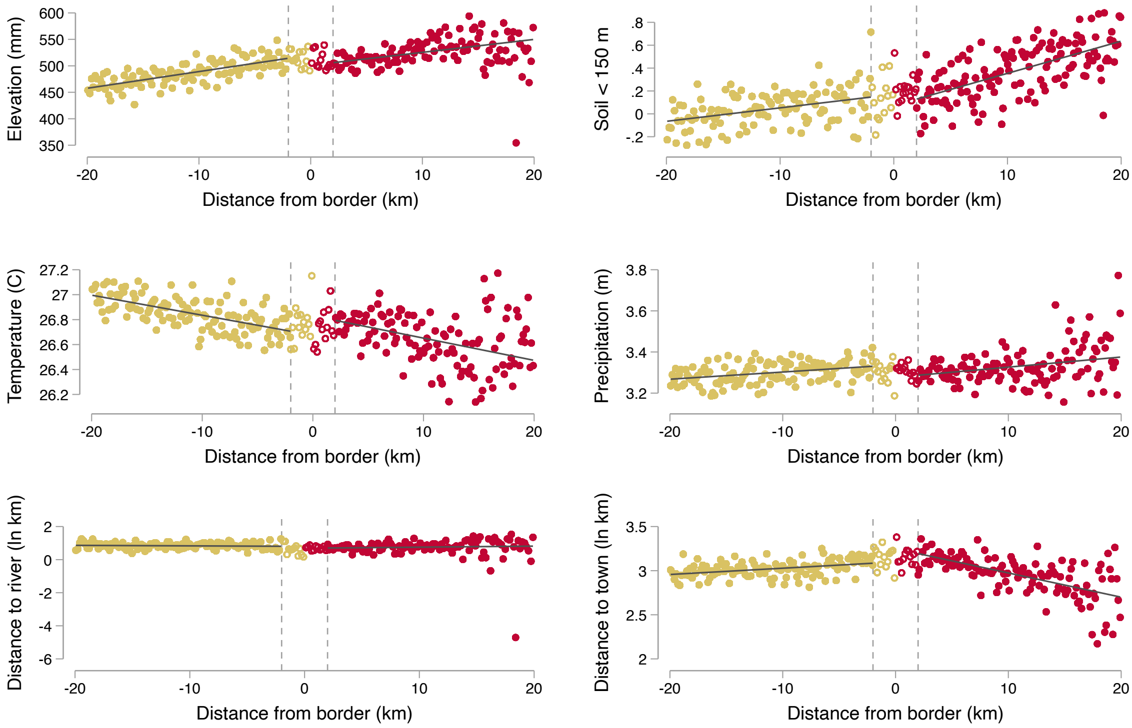
This table shows results from our main specification on wage data that we collected in our phone survey with a sample of N = 2396 elected village representatives in our study area. Within each outcome, the first column reports the absolute wages in INR and the second column reports logged wages. Each regression is at the level of a village-crop-season combination, with district, crop and season fixed effects and standard errors clustered at the village level. The surveys ask both the peak and typical wages for each task-gender combination listed. $*\, p < 0.1, **\ p < 0.05, ***\, p < 0.01$

Figure 1: Georeferencing to identify feudal and non-feudal areas



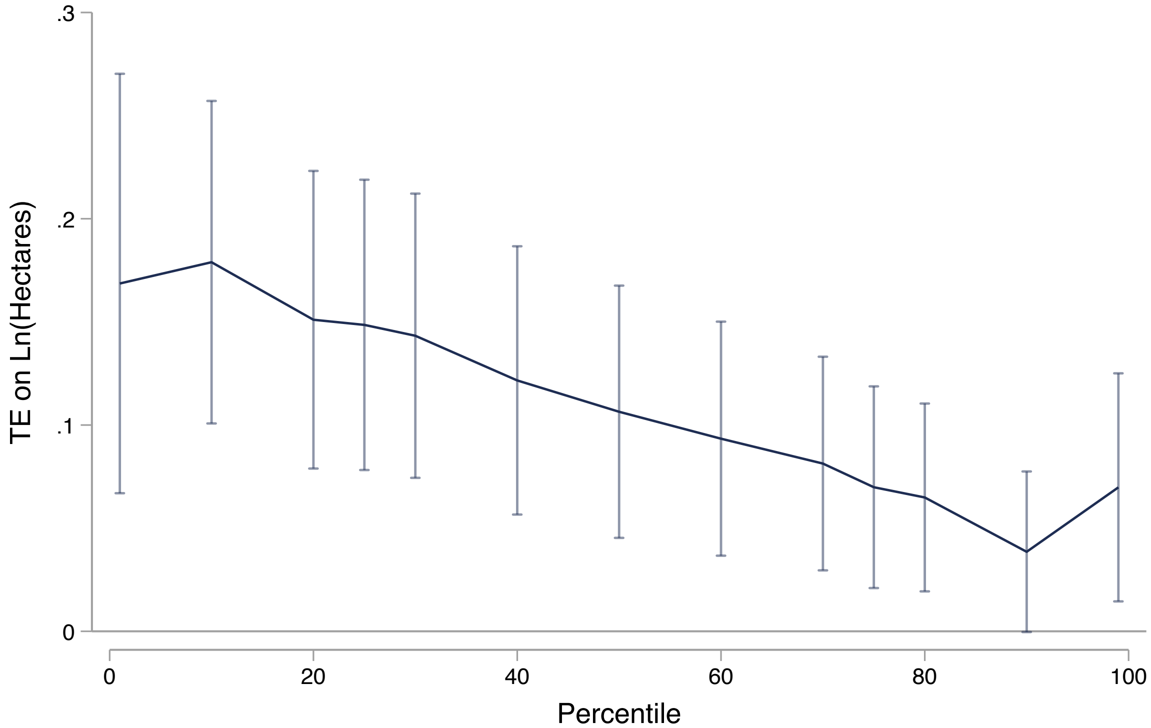
This figure shows our georeferencing of a historical map depicting different land tenure systems in princely Hyderabad. The underlying figure is an officially scanned and watermarked copy of the original map which resides at the National Archives of India in New Delhi (Figure B.1)). We represent in shaded areas the feudal and non-feudal regions idenitified via georeferencing, along with a 2 kilometer "buffer" or "donut hole" to account for measurement error.

Figure 2: Balance tests



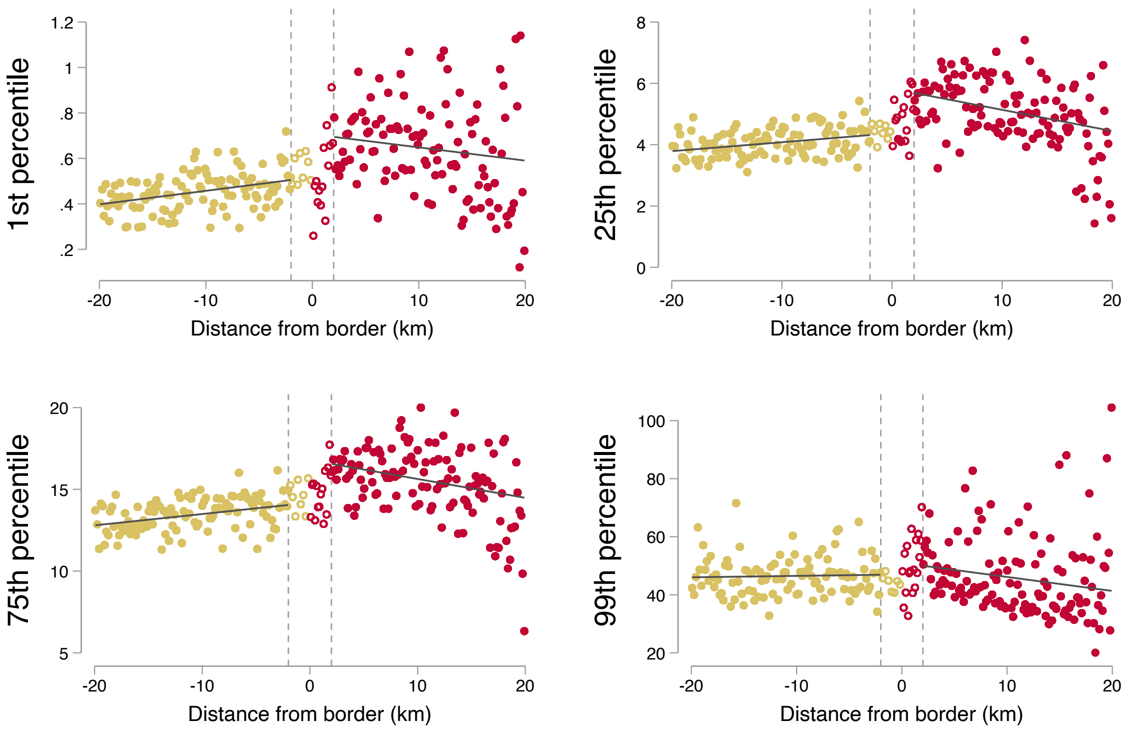
This figure shows RD plots from our key specification for a range of geographic and demographic variables to establish balance. In each RD plot, red and golden solid points indicate feudal and non-feudal areas respectively, while hollow points of each color indicate areas within the 2 kilometer "donut hole" that are not used to estimate the pooled level effects. In the first plot, the elevation data comes from the SRTM (Shuttle Radar Topgraphy Mission) at 1-arc second resolution. In the second plot, the soil depth data comes from the NASA MODIS instruments. In the third and fourth plots, the temperature and precipitation data come from the ERA5 dataset. In the fifth and sixth plots, the data on distance to nearest river and town each come from the population census of 2011. Each of these indicators is accessed through the SHRUG repository (Asher, Lunt, et al. 2021).

Figure 3: Treatment effects on logged landholdings across distribution



This figure plots the point estimates and 95% confidence intervals from our main regression specification, showing how the differences in landholdings across feudal and non-feudal areas are most pronounced at the left tail of the distribution and taper towards zero towards the right tail. Each point on the plot comes from a separate regression which includes fixed effects at the district and border line segment level, and standard errors are robust.

Figure 4: Regression discontinuity plots along landholding distribution



This figure shows regression discontinuity plots from our key specification for four points along the landholding distribution. In each plot, red and golden solid points indicate feudal and non-feudal areas respectively, while hollow points of each color indicate areas within the 2 kilometer "donut hole" that are not used to estimate the pooled level effects. Each plot represents a separate regression which includes fixed effects at the district and border line segment level, and standard errors are robust.

Table 3: Wages by bank exposure

	Log wages	
Feudal	-0.011	0.019
	(0.022)	(0.021)
Post 2000 bank	0.011	0.032
	(0.027)	(0.025)
Feudal × Post 2000 bank	-0.067**	-0.092***
	(0.032)	(0.031)
Female		-0.379***
		(0.025)
Feudal × Female		-0.062**
		(0.030)
Female × Post 2000 bank		-0.046
		(0.037)
Feudal × Female × Post 2000 bank		0.052
		(0.045)
Non-feudal mean (logged)	6.010	6.010
Non-feudal mean (INR)	450.277	450.277
R^2	0.40	0.67
Observations	14913	14913

This table shows results from our main specification on wage data that we collected in our phone survey with a sample of N = 2396 elected village representatives in our study area. The outcome is stacked log wages, pooling together peak and typical wages, and those for both female and male tasks. We control for whether the wage is peak or typical, and interact with the gender classification of the task. The specification focuses on an interaction with a binary variable which indicates whether or not the village received a bank branch before or after 2000, which is the median year in which bank branches were rolled out to our sample. The data for the year of bank branch expansion was obtained from the RBI dataset in the SHRUG repository (Asher, Lunt, et al. 2021). Each regression is at the level of a village-crop-season combination, with district, crop and season fixed effects and standard errors clustered at the village level. $*p < 0.1, **\ p < 0.05, ***p < 0.01$

Table 4: Wages by distance to district headquarters

	Log wages	
Feudal	0.002	-0.000
	(0.025)	(0.021)
2nd quartile	0.006	0.013
	(0.028)	(0.025)
3rd quartile	0.021	-0.032
	(0.028)	(0.026)
4th quartile	-0.007	-0.079***
	(0.028)	(0.026)
Feudal × 2nd quartile	-0.023	-0.052*
	(0.033)	(0.030)
Feudal × 3rd quartile	-0.087***	-0.058**
	(0.033)	(0.029)
Feudal × 4th quartile	-0.065**	-0.015
	(0.033)	(0.031)
Female		-0.501***
		(0.027)
Feudal × Female		0.011
		(0.031)
Female × 2nd quartile		-0.005
		(0.036)
Female × 3rd quartile		0.110***
		(0.036)
Female × 4th quartile		0.146***
		(0.038)
Feudal × Female × 2nd quartile		0.047
		(0.043)
Feudal × Female × 3rd quartile		-0.063
		(0.042)
Feudal × Female × 4th quartile		-0.106**
		(0.045)
Non-feudal mean (logged)	6.007	6.007
Non-feudal mean (INR)	448.877	448.877
R^2	0.39	0.67
Observations	30992	30992

This table shows results from our main specification on wage data that we collected in our phone survey with a sample of N = 2396 elected village representatives in our study area. The outcome is stacked log wages, pooling together peak and typical wages, and those for both female and male tasks. We control for whether the wage is peak or typical, and interact with the gender classification of the task. The specification focuses on an interaction with a categorical variable which is the quartile of the distance between the village and the district headquarters. Each regression is at the level of a village-crop-season combination, with district, crop and season fixed effects and standard errors clustered at the village level. $*p < 0.1, **\ p < 0.05, ***p < 0.01$

Table 5: Linking large landowners and village leaders through caste

	Any match		Number of matches		Average match rank		Highest match rank	
	Last name	Caste	Last name	Caste	Last name	Caste	Last name	Caste
Feudal	0.022	-0.039	-1.045	-6.479	-0.001	0.917	-1.115	0.231
	(0.068)	(0.045)	(1.544)	(4.991)	(1.037)	(0.783)	(1.183)	(0.833)
Non-feudal mean	0.689	0.948	6.704	45.556	10.792	4.061	9.452	2.815
R^2	0.16	0.12	0.16	0.32	0.20	0.20	0.20	0.15
Observations	238	238	238	238	238	238	238	238

This table shows results from our main specification on the extent of overlap between elected leaders and landowners in our sample villages. The matching is done on two dimensions: by last name and caste. Columns 1 and 2 report binaries for whether there is any match between elected leaders' names and the names of any of the top 20 landowners. Columns 3 and 4 report the raw number of matches. Columns 5 and 6 report the average of the match rank. Columns 7 and 8 report the highest match rank within village. This process is conducted for a sample of 238 randomly selected villages. Ambiguous cases are never treated as matches. Regressions include district fixed effects, and standard errors are robust. $*p < 0.1, **\ p < 0.05, ***p < 0.01$

Table 6: NREGS implementation: registration and household-level access

	Job cards		Households employed	
	Ln(Registered)	Ln(Active)	Ln(Total)	I{Any for 100 days}
Feudal	-0.568***	-0.741***	-1.386**	-0.508**
	(0.206)	(0.259)	(0.554)	(0.210)
Non-feudal mean (logged)	6.830	5.061	4.560	.
Non-feudal mean (raw)	1265.480	270.067	199.131	0.497
R^2	0.58	0.66	0.67	0.27
Observations	4890	4908	4818	4960
No. of villages	4739	4774	4694	4795

This table shows results from our main specification on NREGS implementation outcomes in 2023, using data scraped by us from the NREGS portal. Columns (1) and (2) depict the (logged) total number of registered and active NREGS job cards respectively. Columns (3) depicts the (logged) number of households that received any work through NREGS. Column (4) depicts a binary for whether any households in the village received the statutory guarantee of 100 days of work. Regressions control for the total number of households as reported in the Mission Antyodaya 2020 data. Each regression is at the level of a village, with district and border line segment fixed effects, and robust standard errors. $*p < 0.1, **\ p < 0.05, ***p < 0.01$

Table 7: NREGS implementation: log person days per month

	Annual		Peak ag months		Lean ag months	
Feudal	-0.719**	-0.711**	-0.799**	-0.817**	-0.180	-0.179
	(0.340)	(0.340)	(0.360)	(0.364)	(0.256)	(0.255)
Non-feudal mean (logged)	5.543	5.543	5.209	5.209	6.634	6.634
Non-feudal mean (raw)	669.463	669.463	465.784	465.784	2003.883	2003.883
Year FE		✓		✓		✓
R^2	0.58	0.59	0.53	0.55	0.57	0.59
Observations	49228	49228	48656	48656	43042	43042
No. of villages	6003	6003	6003	6003	5978	5978
No. of years	9	9	9	9	9	9

This table shows results from our main specification on (logged) NREGS person-days per month in the period 2016 - 2023, using data scraped by us from the NREGS portal. Columns (1) and (2) depict the total number of person-days at the monthly level. Columns (3) and (4) depict the number of person-days in peak agricultural months. Columns (5) and (6) depict the total person-days of work provided in lean agricultural months. Regressions have year fixed effects when indicated, in addition to district and border line segment fixed effects, and standard errors are clustered at the village level. $*p < 0.1, **\ p < 0.05, ***p < 0.01$

Table 8: NREGS by distance to district HQ

	Annual		Peak ag months		Lean ag months	
Feudal	-0.432	-0.427	-0.451	-0.465	-0.041	-0.033
	(0.401)	(0.402)	(0.418)	(0.423)	(0.323)	(0.323)
2nd quartile	-0.157	-0.158	-0.181	-0.180	-0.193	-0.187
	(0.148)	(0.149)	(0.145)	(0.146)	(0.128)	(0.128)
3rd quartile	0.186	0.187	0.230	0.233	0.011	-0.001
	(0.173)	(0.174)	(0.165)	(0.167)	(0.148)	(0.148)
4th quartile	0.161	0.159	0.212	0.208	-0.108	-0.111
	(0.172)	(0.173)	(0.169)	(0.170)	(0.158)	(0.159)
Feudal × 2nd quartile	0.029	0.033	0.040	0.041	-0.007	-0.018
	(0.260)	(0.263)	(0.251)	(0.256)	(0.229)	(0.231)
Feudal × 3rd quartile	-0.216	-0.216	-0.339	-0.345	-0.146	-0.155
	(0.293)	(0.296)	(0.284)	(0.288)	(0.259)	(0.262)
Feudal × 4th quartile	-0.318	-0.309	-0.392	-0.389	-0.218	-0.224
	(0.310)	(0.313)	(0.294)	(0.300)	(0.288)	(0.291)
Non-feudal mean (logged)	5.477	5.477	5.171	5.171	6.553	6.553
Non-feudal mean (raw)	662.248	662.248	470.705	470.705	1952.353	1952.353
Year FE		✓		✓		✓
R^2	0.58	0.59	0.53	0.55	0.57	0.60
Observations	44421	44421	43866	43866	38518	38518
No. of villages	5324	5324	5324	5324	5306	5306
No. of years	9	9	9	9	9	9

This table shows results from our main specification on NREGS implementation quality, interacted with the distance between the village and the district headquarters, split into quartiles. Outcomes are defined as in Table 7. Regressions have district and border line segment fixed effects, and standard errors are robust. $*p < 0.1, **\ p < 0.05, ***p < 0.01$

Table 9: Willingness to pay for NREGS

	Unconstrained NREGS wages	Unconstrained NREGS wages as share of statutory wage	Unconstrained wage ≤ statutory wage
Feudal	-22.692	-0.077	0.086*
	(17.730)	(0.059)	(0.044)
Non-feudal mean	425.295	1.372	0.274
R^2	0.27	0.30	0.32
Observations	2394	2394	2394

This table reports village leaders' responses to questions where we ask them to set wages for NREGS work in their village in the hypothetical case that they were not bound by statutory requirements or budget constraints. Column 1 reports the actual reported wages, column 2 reports their reported wages as a share of the statutory wages set by the state government, and column 3 reports a binary for whether the unconstrained wage is below the statutory wage. Each regression has district fixed effects and standard errors clustered at the village level. $*p < 0.1, **\ p < 0.05, ***p < 0.01$

Table 10: Investments in agriculture

	Agricultural equipment			Share of total land that is		
	Any	Mechanized	Irrigation	Irrigated	Irrigated for 2 crops	Unirrigated
Feudal	-0.020	-0.013	-0.014	-0.014	-0.165**	-0.730
	(0.013)	(0.018)	(0.017)	(0.010)	(0.084)	(0.601)
Non-feudal mean	0.939	0.836	0.880	0.406	1.035	2.532
R^2	0.06	0.18	0.08	0.23	0.07	0.03
Observations	9281	9281	9281	9079	9145	9145

This table shows results from our main specification on a range of outcomes denoting the use of agricultural inputs as reported in the 2012 socio-economic and caste census. In columns (2) and (3) we use binaries for whether the village uses mechanized or irrigation equipment respectively, and column (1) equals 1 if they use either. Column (4) reports the share of land that is irrigated, while column (5) reports the share irrigated for 2 crops and column (5) denotes the share of unirrigated land. Regressions have district and border line segment fixed effects, and standard errors are robust. $* p < 0.1, ** \ p < 0.05, *** p < 0.01$

Table 11: Investments in agriculture

	Households with tractors	Households with investments ≥ INR 1 lakh
Feudal	1.468	-2.722
	(3.152)	(3.232)
Non-feudal mean	25.033	18.841
R^2	0.10	0.15
Observations	2350	2334

This table shows results from our main specification on data that we collected in our phone survey with a sample of N = 2396 elected village representatives in our study area. Column (1) depicts treatment effects on the number of households in the village that own a tractor. Column (2) depicts treatment effects on the number of households in the village that have made agricultural investments of at least INR 100,000 (≈ USD 1200). Each regression is at the level of a village representative, with district fixed effects and standard errors clustered at the village level. $* p < 0.1, ** \ p < 0.05, *** p < 0.01$

Table 12: Labor demand per 10 acres

	Total	Task-specific	
		Male (fertilizers)	Female (weedeing)
Feudal	-20.680	2.520	-2.211
	(24.848)	(4.274)	(9.456)
Non-feudal mean	213.800	27.758	70.212
R^2	0.17	0.16	0.12
Observations	2393	2053	2053

This table reports questions about labor demand per acre for the primary crop in the village in the most recent agricultural season. The question used to generate the outcomes listed here asked for the hypothetical labor requirement for tasks required to cultivate ten acres. Note that there are fewer observations here than in the wage regressions because this question was not repeated by season and crop, in order to reduce survey time. Each regression is at the level of a village respondent, with district fixed effects and standard errors clustered at the village level. $*p < 0.1, **\ p < 0.05, ***p < 0.01$

Table 13: Productivity

	Yield		Planting acres	
		Ln		Ln
Feudal	-7.789	-0.079	-82.174	0.114
	(6.255)	(0.058)	(136.365)	(0.110)
Non-feudal mean	107.767	4.426	773.851	5.883
R^2	0.56	0.50	0.25	0.46
Observations	4610	4557	7036	6975

This table reports questions about yield and total acres of the primary and secondary crops in both agricultural seasons. Note that there are fewer yield observations than the wage outcomes since only farmers who grew the crop were asked about the yield for the relevant season. For each outcome, the first column reports the raw outcome, while the second column reports the logged version. Each regression is at the level of a village-crop-season combination, with district, crop and season fixed effects and standard errors clustered at the village level. $*p < 0.1, **\ p < 0.05, ***p < 0.01$

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Appendix

Appendix

A Toy model

Empirical evidence indicates that although overall labor supply and demand in feudal and non-feudal areas are similar, the composition of workers' outside options differs markedly. There are two channels we explore in order to reconcile the magnitude of the effects we see. First, in non-feudal the greater number of small(er) farms makes collusion in wage setting more difficult, whereas in feudal areas the number of small(er) farmers is lower, so farmers are more able to collude to restrict labor demand and drive up wages. We show in a simple ogliplistic framework the difference in land concentration alone is unlikely to generate the wage difference we observe. However, allowing the labor supply elasticity to be lower in feudal villages, relfecting the reduced NREGS provision, rationalizes our results with other estimates of labor supply in this setting.

Ologiplistic wage setting is very natural in agricultural labor markets since farmers know the landholdings and crop choices of their neighbors (and thus aggregate labor demand) to a first approximation. Firms knowing other firms labor demand and optimizinig accordingly is the core assumption for oligopilistic models of the labor market.

In this model, firms are chosing employment to maximize revenues.

$$\max_{L_i} R(L_i) - w(L + L_i^*)L_i$$

The intuition in our case is farmers are choosing labor with an understanding of how their labor demand will affect wages because they know other farmers' labor demand.

The first order condition of each farm leads to the natural markdown of:

$$\frac{MRP - w}{w}$$

The key intuition here is that larger farms' MRP curves are shifted to the right as they are able to derive more revenue per any given level of employment. Note that it is still the case that the marginal worker is more productive on a small farm than a large farm due to diminishing marginal returns. The core idea is simply that the productivity diminishes less steeply the more land a farmer has. As a result, the more large farms are in a village the greater the wage markdown.

Note that the MRP translates directly to the employment share of each farm i conditional on the labor supply elasticity

$$\frac{MRP_i - w}{w} = \varepsilon^{-1}\left[\sum_{i}^{n}\left(\frac{L_i}{L}\right)^2\right]$$

Notably, this is different from the raw HHI in land for two reasons. First, as defined, this measure should be equivalent to the HHI in labor shares. Since small farms use a much greater share of family labor, the HHI in land does not capture this well. Second, larger fields do not operate as individual "firms". In this setting, there is evidence farmers of the same caste working together to restrict hiring to push down wages and optimize profits. As a result, these caste groups can be better conceptualized as a singular firm (Anderson, Francois, and Kotwal 2015).

Rather than using individual farms we use land size categories, adapted from the Indian agricultural census, as an approximation for measures of the concentration of land ownership at the level of caste groups. In particular, we use cut-offs c of 0.5, 1, 2 ,3, and 4 hectares. Then, the markdown becomes:

$$\frac{MRP - w}{w} = \varepsilon^{-1}\left[\sum_{c}^{5}\left(\frac{L_c}{L}\right)^2\right] = \varepsilon^{-1}H$$

When the employment is more concentrated among the large farmers, H increases and the markdown is bigger.

If we assume the labor supply elasticity is the same in feudal and non-feudal areas, focusing on the effect of land concentration, we get the following system of equations:

Non-feudal:

$$\frac{MRP - w}{w} = \varepsilon^{-1}H$$

Feudal

$$\frac{MRP - w'}{w'} = \varepsilon^{-1}\alpha^{-1}H'$$

We can then solve for ε by equating the MRPs

$$\varepsilon = \frac{wHHI - w'HHI'}{w' - w}$$

We can take w, w', H, H' from our data. w, w' are simply the wages at the discontinuity for feudal and non-fuedal areas. H, H' are the categorical versions of the HHI in feudal and non-feudal area. These substitutions yield a ε of -0.127, which naturally is infeasible.

Subsequently, we turn to the second key channel driving our results: the suppression of workers' outside options in the form of NREGS availability. Our empirical results establish that a primary channel through which land concentration affects workers' wages is through large landowners' control of local governments. The large landowners are able to constrain workers' access to NREGS – the public workfare program – which provides a credible outside option to agricultural labor. This can be formalized as reducing workers labor supply elasticity ε by a multiplier $\alpha < 1$, further increasing the markdown on wages.

We now have a system of two equations describing wages in the feudal and non-feudal areas.

Non-Feudal:

$$\frac{MRP - w}{w} = \varepsilon^{-1} H$$

Feudal

$$\frac{MRP - w'}{w'} = \varepsilon^{-1} \alpha^{-1} H'$$

Now, rather than solving for ε, we can take ε as the 3.07 estimated in Muralidharan, Niehaus, and Sukhtankar 2023 and solve for an α that rationalizes our data.

$$\alpha = \frac{wHHI - w'HHI'\varepsilon^{-1}}{MRP - w'}$$

$$\alpha = \frac{wHHI - w'HHI'\varepsilon^{-1}}{wHHI\varepsilon^{-1} + w - w'}$$

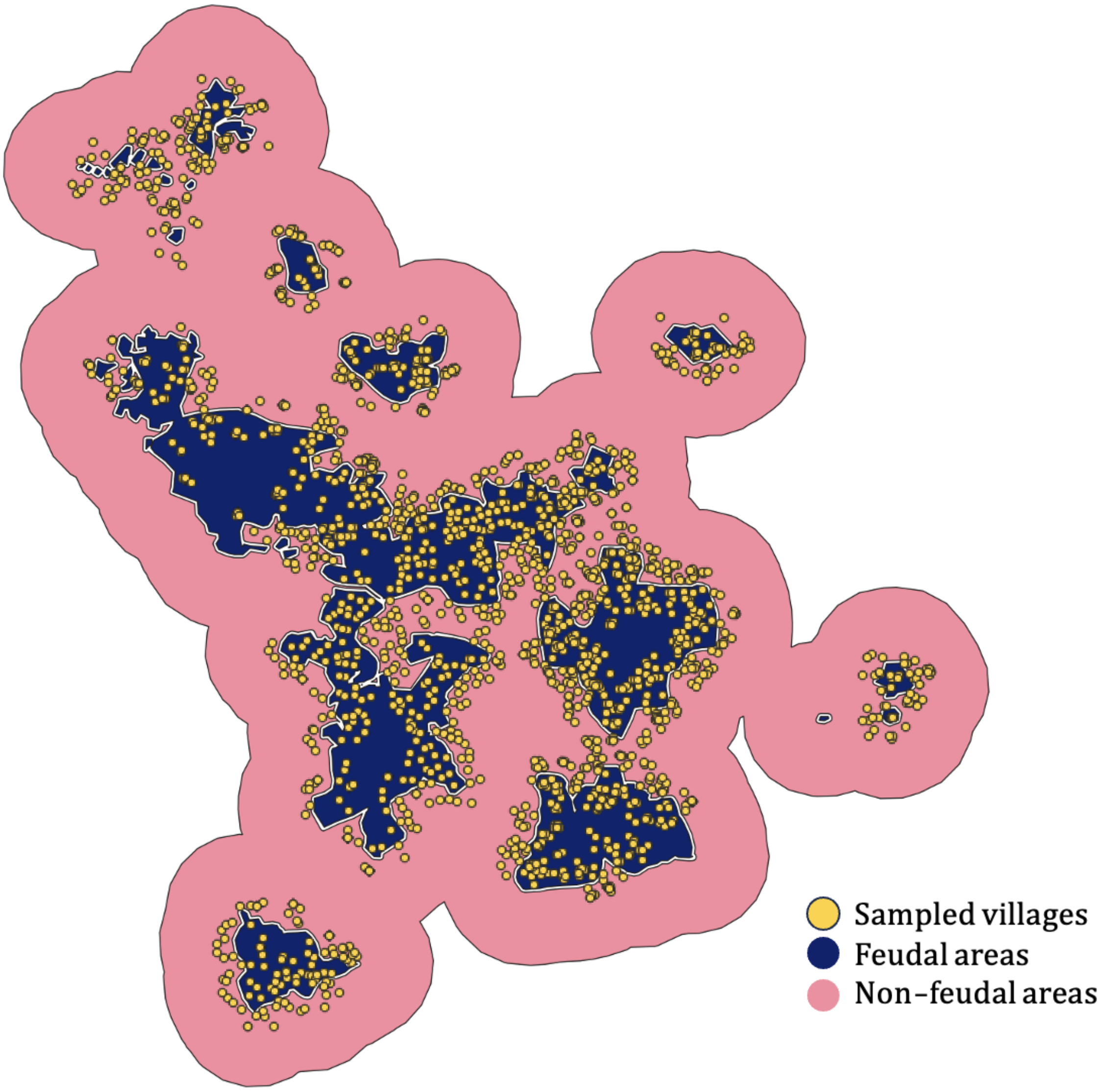
The resulting value for α, 0.72, suggests that labor supply elasticity is 28% lower in-feudal villages which provide 70% fewer NREGS person days during the peak agricultural season. This value aligns with the limited literature exploring the sensitivity of labor supply elasticities to outside options. For instance, Caldwell and Oehlsen 2023 estimate an α of 0.53, with the elasticity for working at ride-hailing firms decreasing from 1.5 to 0.8 when workers have no outside firm option.

B Supplementary figures

Figure B.1: Map of Hyderabad Princely State Showing Different Tenure Systems

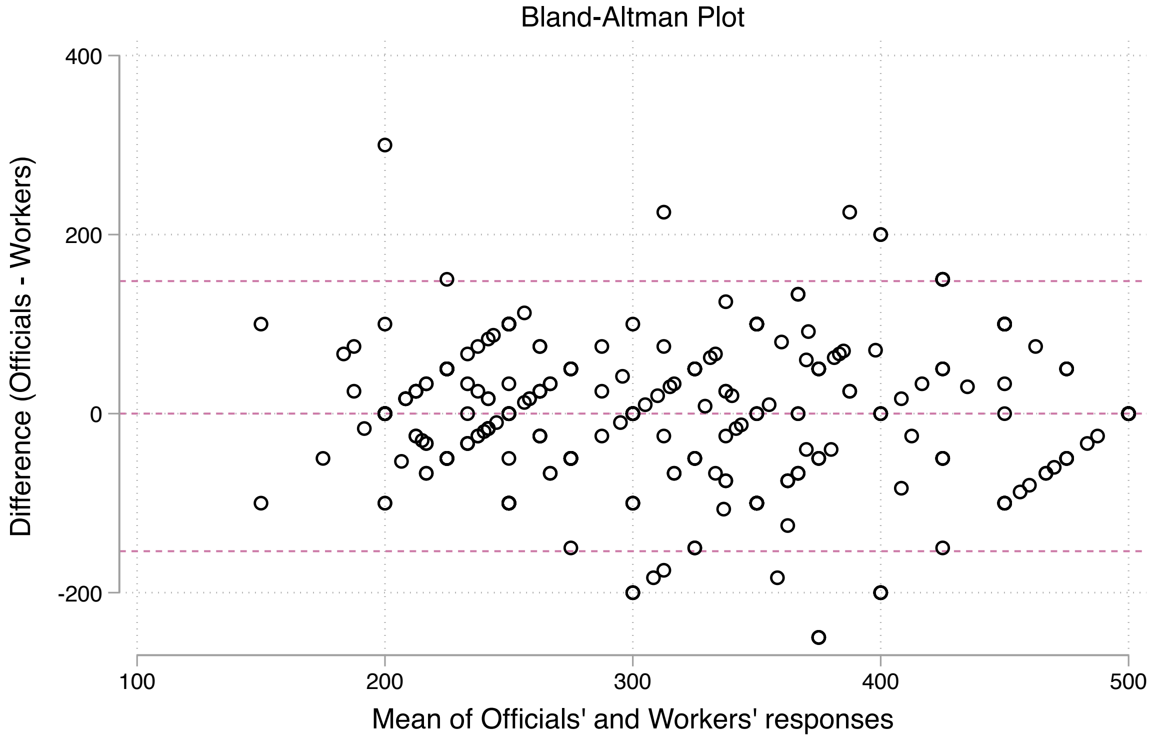
This figure shows the official scanned and watermarked copy of the original map from the National Archives of India in New Delhi that we used for our georeferencing. The georeferenced version of the map is in Figure 1.

Figure B.2: Phone survey sample



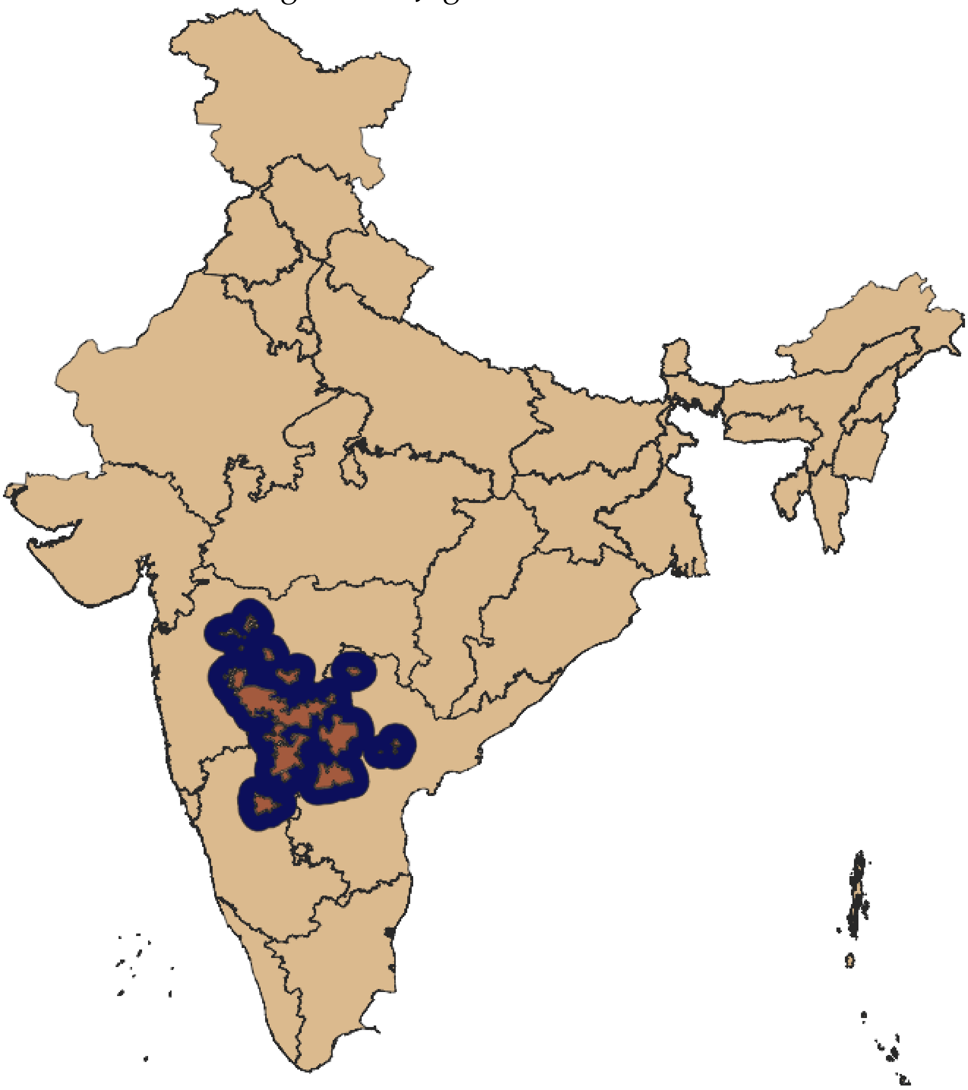
This figure shows on our map of feudal and non-feudal regions points that represent villages we sampled for phone surveys with elected representatives and worekrs, which form the basis of our primary data that we use to estimate treatment effects on labor market outcomes.

Figure B.3: Accuracy of wages



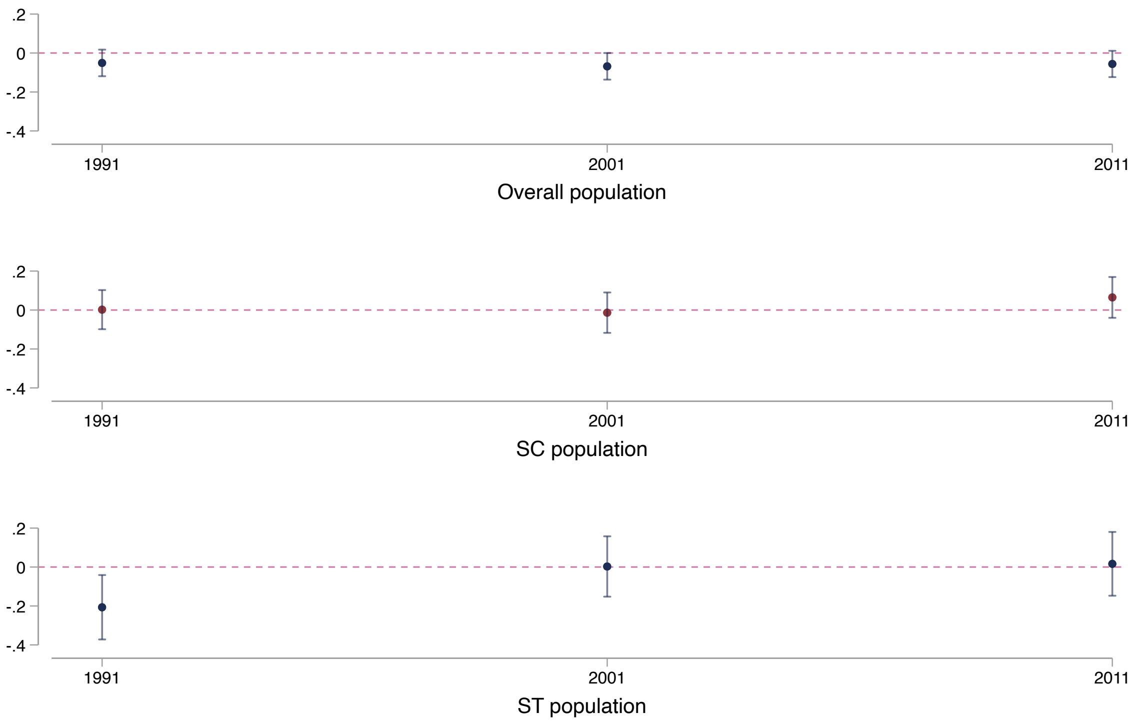
This figure shows a standard Bland-Altman plot, comparing responses from village representatives ("officials") and workers on the prevailing agricultural wages at the crop-season-task-gender level.

Figure B.4: Jagirs within India



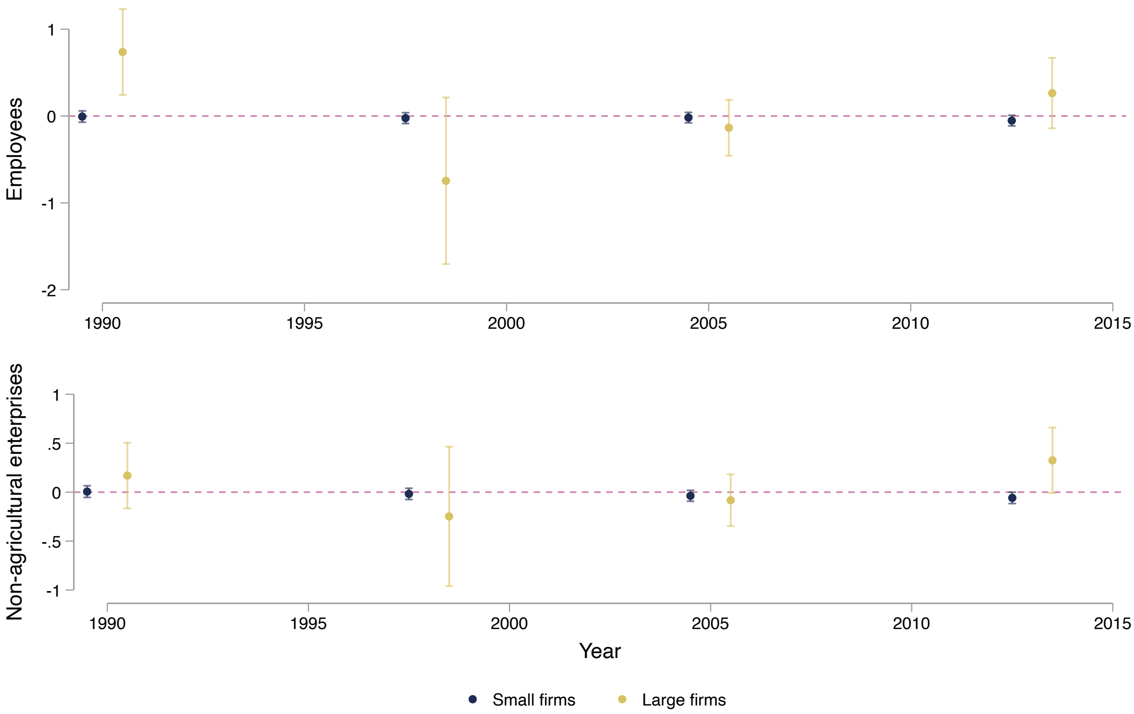
This figure shows our study area with reference to the extent of the entire country, indicating that this is a sizeable and significant part of the country.

Figure B.5: Population dynamics



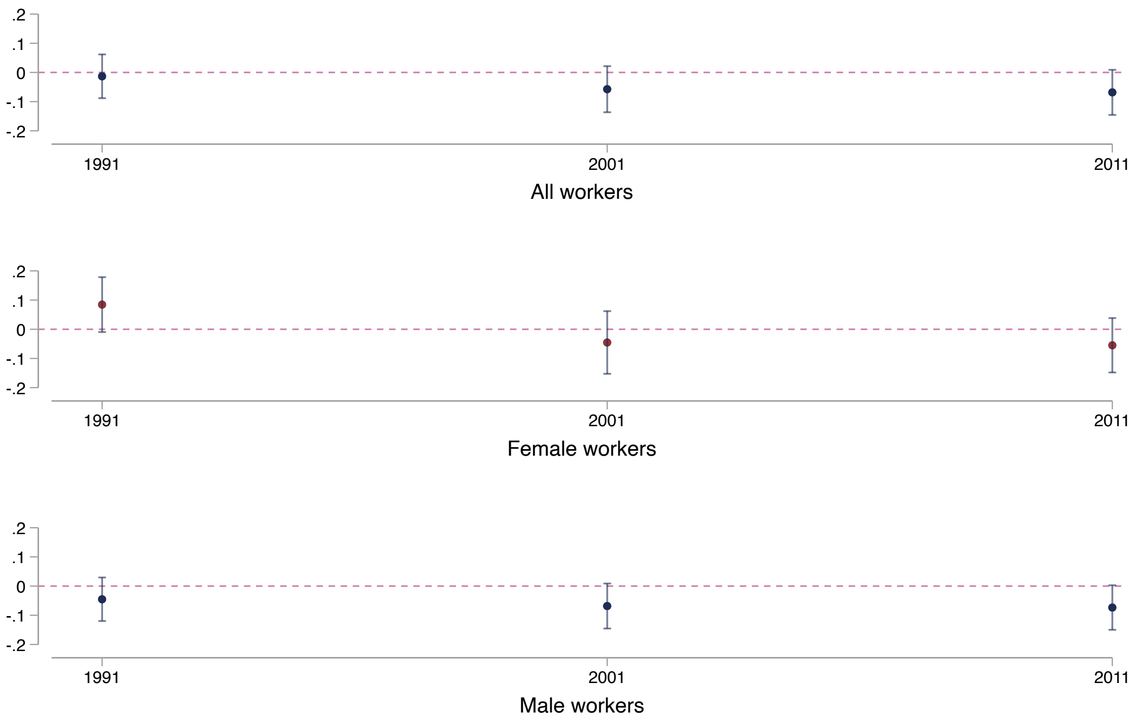
This figure plots the trends in logged village populations over the course of the three most recent population census rounds. The first plot represents treatment efects on the overall population. The second and third plots represent treatment effects on the scheduled caste and scheduled tribe populations. Each point on the plot comes from a separate regression which includes fixed effects at the district and border line segment level, and standard errors are robust.

Figure B.6: Non-agricultural economic activity



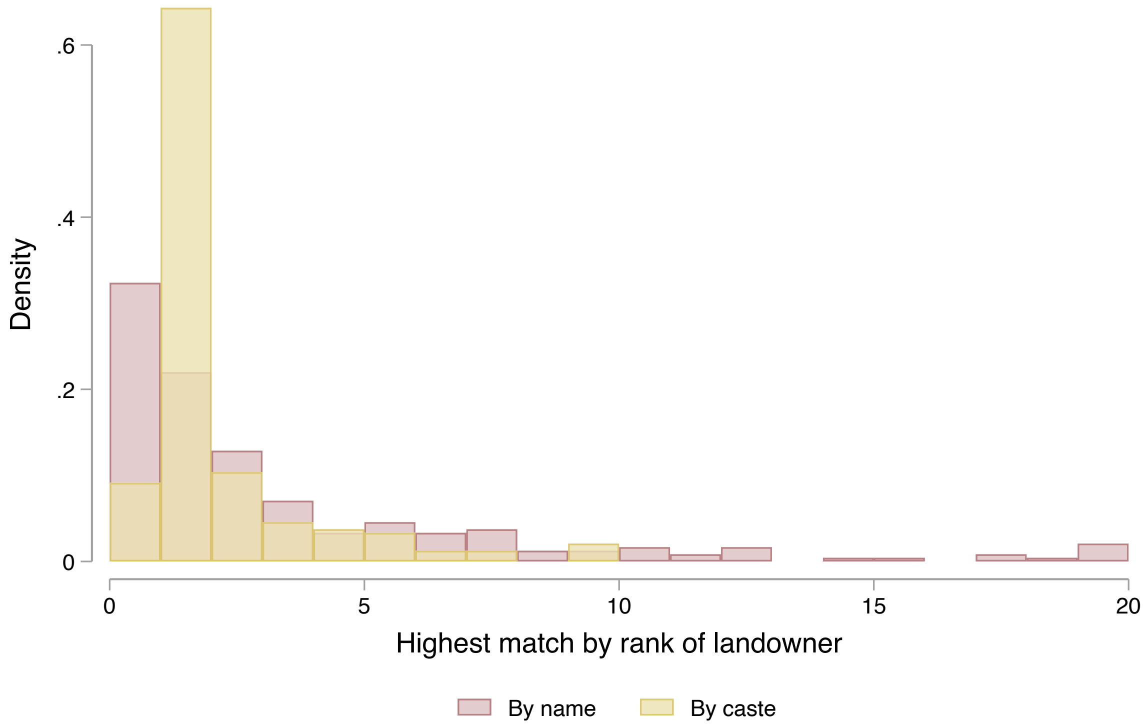
This figure plots the trends in non-agrilcultural economic activity in the villages in our sample. The first plot represents treatment efects on the number of employees in non-agricultural enterprises, indicated separately for large and small firms split by the median firm size in the sample. The second plot represents treatment effects on the raw number of non-agricultural enterprises in the village, again split by firm size. Each point on the plot comes from a separate regression which includes fixed effects at the district and border line segment level, and standard errors are robust.

Figure B.7: Working population dynamics



This figure plots the trends in logged village working-age populations (aged 18-60) over the course of the three most recent population census rounds. The first plot represents treatment efects on the overall working-age population. The second and third plots represent treatment effects on these populations split by female and male totals, respectively. Each point on the plot comes from a separate regression which includes fixed effects at the district and border line segment level, and standard errors are robust.

Figure B.8: Working population dynamics



This figure plots the distribution of the average rank of the match between elected village leaders and landowners. The x-axis represents the rank of the landowner in the village with whom the elected leaders get matched, either by last name or caste. A rank of zero implies that there is no match among the 20 largest landowners.

C Supplementary results from secondary data

Table C.1: Logged landholdings

	Percentiles of Ln(landholdings)						
	1st	10th	25th	50th	75th	90th	99th
Feudal	0.169***	0.179***	0.149***	0.106***	0.070***	0.039*	0.070**
	(0.052)	(0.040)	(0.036)	(0.031)	(0.025)	(0.020)	(0.028)
Non-feudal mean	6.866	8.514	9.364	10.135	10.732	11.176	11.918
R^2	0.41	0.43	0.44	0.45	0.41	0.34	0.19
Observations	8928	8924	8925	8933	8943	8949	8929

This table shows results from our primary regression discontinuity specification on percentiles across the distribution of logged landholdings in our study regions. All indicators are created using raw land parcel level data procured from a remote sensing company, corresponding to the state of landownership in our study area as of September 2022. Each regression includes fixed effects at the district and border line segment level, and standard errors are robust. $*p < 0.1$, $**\ p < 0.05$, $***p < 0.01$

Table C.2: Differences in violent events

	ACLED			UCDP	
	Total conflict	Land conflict	Fatal conflict	All conflicts	All deaths
Feudal	-1.193	-0.022	-0.006	-0.003	-0.006
	(1.001)	(0.017)	(0.005)	(0.010)	(0.012)
Non-feudal mean	0.167	0.005	0.001	-4.595	-4.594
R^2	0.00	0.00	0.00	0.01	0.01
Observations	9854	9854	9854	9854	9854

This table shows results from our primary specification on geo-coded violence data from public repositories housed at the Armed Conflict Location and Event Data (ACLED) and the Uppsala Conflict Data Program (UCDP). Each outcome depicts the logged number of total (columns 1 and 4), land-related (column 2) and fatal (columns 3 and 5) conflicts. We add a notional value of 0.01 to zeroes before taking logs. A major limitation is that many ACLED events appear to be coded to district headquarters as opposed to the event locations, which would naturally induce some measurement error. $*\ \ p < 0.1$, $**\ \ p < 0.05$, $***\ \ p < 0.01$

Table C.3: Assembly election results

	I{Left party win}		
Feudal	0.007***	0.006***	0.007***
	(0.002)	(0.002)	(0.002)
District FE	✓	✓	✓
Year FE		✓	✓
Constituency type			✓
Non-feudal mean	0.031	0.031	0.031
R^2	0.25	0.28	0.28
Observations	196969	196967	196967

This table shows results from our main specification on results from 42 distinct state-level assembly elections in the period between 1977 - 2021. Each column depicts a different regression specification, with the same outcome which is a binary for whether the left party won the election. In column 1, we report our base specification with district and border line segment fixed effects. In column 2, we additionally control for year fixed effects. In column 3, we also control for whether the constituency was reserved for minority representatives. Elections data were procured from the SHRUG repository (Asher, Lunt, et al. 2021). Standard errors are always clustered at the village level. $*p < 0.1$, $**\ p < 0.05$, $***p < 0.01$

Table C.4: Balance table

	Elevation		Temperature	Precipitation	Soil depth (sd)	Distance to nearest			
	Mean	SD				river (ln km)	town (ln km)	a8	a9
Feudal	-1.318	0.065	0.011	-0.028***	0.003	-0.187***	0.112***	0.003	-0.039
	(1.319)	(0.195)	(0.008)	(0.004)	(0.019)	(0.043)	(0.017)	(0.009)	(0.033)
Non-feudal mean	459.932	10.747	26.986	3.292	-0.000	0.782	3.029	2.089	0.571
R^2	0.90	0.25	0.88	0.84	0.54	0.35	0.33	0.12	0.08
Observations	19467	19467	20171	20171	20171	18225	19201	5234	18572

This table shows results from our primary regression discontinuity specification on the balance indicators that are described in figure 2. Each regression includes fixed effects at the district and border line segment level, and standard errors are robust. $*p < 0.1, **\ p < 0.05, ***p < 0.01$

Table C.5: Treatment effects on village composition

	# HHs			# workers	# land txns	# migrant HHs	
	Current	Recent exits	Employers			Cyclical	Permanent
Feudal	17.772	1.956	53.735	237.664	-6.220	-11.509	-13.761
	(91.131)	(4.108)	(38.439)	(344.169)	(10.488)	(18.527)	(16.551)
Non-feudal mean	1010.216	22.623	142.883	1146.251	67.671	140.532	82.028
R^2	0.49	0.11	0.18	0.09	0.07	0.41	0.18
Observations	2366	2339	2330	2321	2209	2346	2356

This table shows results from our main specification on data that we collected in our phone survey with a sample of N = 2396 elected village representatives in our study area. Columns 1, 2 and 3 report the total number of households in the village, the number of households that have left the village in the most recent five years, and the number of households that employ agricultural laborers. Column 4 reports the number of agricultural laborers in the village. Column 5 reports the number of land sales that have taken place in the most recent five years. Column 6 and 7 reports the number of households that send cyclical and permanent migrants out of the village. Each regression is at the level of a village representative, with district fixed effects and standard errors clustered at the village level. $*p < 0.1, **\ p < 0.05, ***p < 0.01$

Table C.6: Wage agreement between workers and leaders in survey

Variable	Concordance correlation coefficient		95% CI	Mean difference		Observations
	Estimate	SE		INR	%	
Peak male wages	0.544	0.034	[0.477, 0.610]	14.822	2.34	426
Peak female wages	0.598	0.031	[0.538, 0.658]	33.675	6.88	425
Typical male wages	0.631	0.029	[0.574, 0.689]	6.641	1.29	425
Typical female wages	0.688	0.026	[0.638, 0.738]	2.851	0.91	425

This table reports concordance on wage reports between the responses of village representatives (N = 2396) and a sample of workers in a subset of these villages (N = 426). Concordance correlation coefficient ranges from -1 to 1 and refers the degree of agreement between workers and leaders reports for the same villages.

Table C.7: Characteristics of survey respondents

Variable	Mean	SD	Min	Max	Observations
Panel A: *Workers survey*					
Number of surveys/village	1.475	0.768	1	5	459
Share of male respondents	0.961	0.182	0	1	459
Average respondent age	40.990	9.556	18	76	456
Share of married respondents	0.928	0.244	0	1	459
Share of respondents in cultivation last season	0.902	0.279	0	1	459
Share of upper caste respondents	0.209	0.387	0	1	459
Share of SC/ST respondents	0.196	0.376	0	1	459
Share of other caste respondents	0.400	0.463	0	1	459
Panel B: *Officials survey*					
Number of surveys/village	1.460	0.869	1	5	1641
Village head surveyed	0.593	0.457	0	1	1641
Village vice-head surveyed	0.057	0.212	0	1	1641
Village elected member surveyed	0.350	0.443	0	1	1641

This table reports demographic and time-invariant characteristics of respondents for the wage survey. In panel A, we report characteristics for workers, anchoring to the villages they represent in the sample. In panel B, we report the analogous for elected village representatives.

Table C.8: SECC earnings

	Solid		Population share	Consumption (INR)	Poverty rate
	Roof	Wall	$\geq$ 10K INR		
Feudal	-0.011	-0.014	0.003	355.090	-0.000
	(0.010)	(0.010)	(0.007)	(219.308)	(0.006)
Non-feudal mean	0.725	0.759	0.086	20534.407	0.164
Adj. R^2	0.51	0.40	0.17	0.69	0.30
Observations	9153	9153	9153	9281	8272

This table shows results from our main specification on aggregate earning outcomes, as reported in the 2012-13 socio-economic and caste census. Columns 1 and 2 report the share of households in the village with solid roofs and walls. Column 3 reports the share of village households that reported earning more than INR 10,000 (USD 115) in the previous year. Column 4 reports the household-level consumption expenditure in INR. Column 5 reports the share of the village population classified as being below the poverty line. Regressions have district and border line segment fixed effects, and standard errors are robust. $*p < 0.1, **\ p < 0.05, ***p < 0.01$

Table C.9: Aggregate education levels

	Share of population with ... or above		
	Sec school	middle school	primary school
Feudal	-0.002	-0.007	-0.013**
	(0.004)	(0.005)	(0.006)
Non-feudal mean	0.189	0.294	0.493
R^2	0.46	0.47	0.38
Observations	9153	9153	9153

This table shows results from our main specification on educational attainment outcomes, as reported in the 2012-13 socio-economic and caste census. Each outcome is a continuous variable for the share of residents in the village with at least the lsited level of education. Regressions have district and border line segment fixed effects, and standard errors are robust. $*p < 0.1, **\ p < 0.05, ***p < 0.01$

Table C.10: Health and education infrastructure

	School in village		Distance to ... (km)	
	Primary	Non-primary	PHC/CHC	Hospital
Feudal	-0.000	-0.549***	3.948**	2.324
	(0.025)	(0.203)	(1.969)	(2.884)
Non-feudal mean	0.973	0.579	4.299	6.821
R^2	0.18	0.23	0.22	0.26
Observations	5062	5062	5062	5062

This table shows results from our main specification on educational attainment outcomes, as reported in the 2020 Mission Antyodaya dataset. Columns (1) and (2) are binaries that indicate whether there is a primary or non-primary school in the village, respectively. Columns (3) and (4) indicate the distance to health centers and hospitals in kilometers, respectively. PHC and CHC stand for primary health center and community health center respectively. Regressions have district and border line segment fixed effects, and standard errors are robust. $*p < 0.1, **\ p < 0.05, ***p < 0.01$

Table C.11: Agriculture infrastructure

	Ag infrastructure			Water infrastructure		Other
	Seed centre	Fertilizer shop	Soil testing	Watershed	Rainwater harvesting	Ag coop society
Feudal	-0.300*	-0.408**	-0.119*	-0.005	-0.344	-0.240
	(0.159)	(0.202)	(0.070)	(0.178)	(0.309)	(0.302)
Non-feudal mean	0.127	0.276	0.057	0.302	0.507	0.318
R^2	0.22	0.26	0.23	0.26	0.32	0.29
Observations	5062	5062	5062	5062	5062	5062

This table shows results from our main specification on a range of outcomes denoting infrastructure supporting agricultural practices as reported in the 2011 population census. Columns (1), (2) and (3) report binaries for the presence of a center to buy subsidized seeds, a shop for subsidized fertilizers, and a soil testing center respectively. Columns (4) and (5) denote whether there is a watershed or rainwater harvesting infrastructure available. Column (6) denotes whether the village has an agricultural cooperative society. Regressions have district and border line segment fixed effects, and standard errors are robust. $*p < 0.1$, $**\ p < 0.05$, $***p < 0.01$

Table C.12: DHS health

	Weight (kg)		Height (cm)		WAZ		WHZ		HAZ	
Feudal	0.080	0.074	0.026	-0.126	-0.086	-0.143*	0.010	-0.031	0.084*	0.081
	(0.125)	(0.146)	(0.568)	(0.636)	(0.068)	(0.075)	(0.049)	(0.053)	(0.048)	(0.050)
Female		-1.790***		-8.669***		-0.081		0.023		0.133**
		(0.136)		(0.604)		(0.079)		(0.057)		(0.056)
Interaction		0.122		0.893		0.173		0.120		0.001
		(0.214)		(0.890)		(0.117)		(0.082)		(0.081)
Dep var mean	10.522	10.522	83.858	83.858	-1.349	-1.349	-1.650	-1.650	-1.041	-1.041
R^2	0.00	0.06	0.00	0.07	0.00	0.00	0.00	0.00	0.00	0.00
Observations	3961	3961	3948	3948	3653	3653	3653	3653	3661	3661

This table reports results from a simple OLS regression where the dependent variable is a binary indicator for feudal status, and the outcomes are children's health indicators from the Demographic and Health Surveys of 2019-20. We interact feudal status with a binary indicating whether the child is female. Columns 1 and 2 report the child's weight in kilograms. Columns 3 and 4 report their height in centimeters. The remaining columns denote the weight-for-age z score, the weight-for-height z score, and the height-for-age z score respectively. Regressions have district fixed effects, and standard errors are clustered at the survey cluster level. $*p < 0.1$, $**\ p < 0.05$, $***p < 0.01$

Table C.13: DHS wealth index

	I{top two quartiles}			Wealth index		
Feudal	-0.012	-0.038	-0.013	-0.310	-1.100	-0.709
	(0.062)	(0.072)	(0.066)	(0.958)	(1.119)	(1.049)
Agricultural laborer HH		-0.159***			-3.439***	
		(0.040)			(0.649)	
Feudal × Agricultural laborer HH		0.040			1.314	
		(0.053)			(0.937)	
Female agricultural laborer			-0.190***			-4.000***
			(0.036)			(0.585)
Feudal × Female agricultural laborer			0.024			1.380
			(0.053)			(1.011)
Dep var mean	0.235	0.235	0.235	2.765	2.765	2.765
R^2	0.15	0.17	0.18	0.21	0.24	0.25
Observations	1151	1151	1151	1151	1151	1151

This table reports results from a simple OLS regression where the dependent variable is a binary indicator for feudal status, and the outcomes are households' wealth indices from the Demographic and Health Surveys of 2019-20. We interact feudal status with binaries indicating whether the household has an any agricultural laborer, and whether the household has a female agricultural laborer. Columns 1 - 3 use as the outcome a binary indicator that is equal to 1 if the household falls within the top two quartiles of wealth. Columns 4 - 6 reports the actual wealth index, which runs from 1 to 5 and has up to five decimal places in the raw data. Regressions have district fixed effects, and standard errors are clustered at the survey cluster level. $*p < 0.1$, $**\ p < 0.05$, $***p < 0.01$

D Supplementary results from primary surveys

Table D.1: Wages by SEZ exposure

	Log wages	
Feudal	-0.031**	0.001
	(0.014)	(0.012)
SEZ exposure	0.034*	-0.003
	(0.019)	(0.019)
Feudal × SEZ exposure	-0.034	-0.061***
	(0.022)	(0.022)
Female		-0.449***
		(0.015)
Feudal × Female		-0.064***
		(0.018)
Female × SEZ exposure		0.076***
		(0.027)
Feudal × Female × SEZ exposure		0.056*
		(0.032)
Non-feudal mean (logged)	6.006	6.006
Non-feudal mean (INR)	448.287	448.287
R^2	0.39	0.67
Observations	31172	31172

This table shows results from our main specification on wage data that we collected in our phone survey with a sample of N = 2396 elected village representatives in our study area. The outcome is stacked log wages, pooling together peak and typical wages, and those for both female and male tasks. We control for whether the wage is peak or typical, and interact with the gender classification of the task. The specification focuses on an interaction with a binary variable which indicates whether or not the village is above the median level of exposure to SEZs, as estimated by the number of SEZs within a 200 km radius of the village. The data for the location of SEZs in India is from (Gallé et al. 2024). Each regression is at the level of a village-crop-season combination, with district, crop and season fixed effects and standard errors clustered at the village level. $*p < 0.1, **\ p < 0.05, ***p < 0.01$

Table D.2: Wages by road exposure

	Log wages	
Feudal	-0.072***	-0.040**
	(0.017)	(0.018)
No major roads	-0.024	0.029
	(0.020)	(0.020)
Feudal × No major roads	0.016	-0.014
	(0.024)	(0.024)
Female		-0.360***
		(0.021)
Feudal × Female		-0.065***
		(0.025)
Female × No major roads		-0.104***
		(0.029)
Feudal × Female × No major roads		0.060*
		(0.035)
Non-feudal mean (logged)	6.021	6.021
Non-feudal mean (INR)	456.876	456.876
R^2	0.40	0.67
Observations	25548	25548

This table shows results from our main specification on wage data that we collected in our phone survey with a sample of N = 2396 elected village representatives in our study area. The outcome is stacked log wages, pooling together peak and typical wages, and those for both female and male tasks. We control for whether the wage is peak or typical, and interact with the gender classification of the task. The specification focuses on an interaction with a binary variable which indicates whether or not the village had a major road as reported in the 2011 population census. Each regression is at the level of a village-crop-season combination, with district, crop and season fixed effects and standard errors clustered at the village level. $*p < 0.1$, $**\ p < 0.05$, $***p < 0.01$

Table D.3: Wages by population exposure

	Log wages	
Feudal	-0.038***	-0.020*
	(0.012)	(0.011)
Above median population exposure	0.047	0.045
	(0.035)	(0.031)
Feudal × Above median population exposure	0.036	0.051
	(0.043)	(0.039)
Female		-0.439***
		(0.014)
Feudal × Female		-0.036**
		(0.016)
Female × Above median population exposure		0.000
		(0.046)
Feudal × Female × Above median population exposure		-0.021
		(0.058)
Non-feudal mean (logged)	5.961	5.961
Non-feudal mean (INR)	425.516	425.516
R^2	0.36	0.68
Observations	26580	26580

This table shows results from our main specification on wage data that we collected in our phone survey with a sample of N = 2396 elected village representatives in our study area. The outcome is stacked log wages, pooling together peak and typical wages, and those for both female and male tasks. We control for whether the wage is peak or typical, and interact with the gender classification of the task. The specification focuses on an interaction with a binary variable which indicates whether or not the village is above or below median in terms of the total population that lives within 20 kilometers as reported in the 2011 population census. Each regression is at the level of a village-crop-season combination, with district, crop and season fixed effects and standard errors clustered at the village level. $*p < 0.1, **\ p < 0.05, ***p < 0.01$

Table D.4: Wages by reservation status

	Log wages	
Feudal	-0.059***	-0.043***
	(0.014)	(0.013)
Ever reserved	-0.036*	-0.063***
	(0.019)	(0.019)
Feudal × Ever reserved	0.028	0.031
	(0.023)	(0.023)
Female		-0.441***
		(0.016)
Feudal × Female		-0.030
		(0.019)
Female × Ever reserved		0.055**
		(0.027)
Feudal × Female × Ever reserved		-0.007
		(0.033)
Non-feudal mean (logged)	6.004	6.004
Non-feudal mean (INR)	447.592	447.592
R^2	0.39	0.67
Observations	30550	30550

This table shows results from our main specification on wage data that we collected in our phone survey with a sample of N = 2396 elected village representatives in our study area. The outcome is stacked log wages, pooling together peak and typical wages, and those for both female and male tasks. We control for whether the wage is peak or typical, and interact with the gender classification of the task. The specification focuses on an interaction with a binary variable which indicates whether or not the village elected body has ever been reserved (i.e. constrained to only be able to elect members of ethnic minorities). Each regression is at the level of a village-crop-season combination, with district, crop and season fixed effects and standard errors clustered at the village level. $*p < 0.1$, $**\ p < 0.05$, $***p < 0.01$

Table D.5: Wages by type and gender

	Logged wages		
Feudal	-0.048**	-0.029	-0.026
	(0.020)	(0.022)	(0.023)
Peak	0.213***		0.164***
	(0.016)		(0.015)
Feudal × Peak	0.004		-0.007
	(0.019)		(0.019)
Female		-0.423***	-0.472***
		(0.017)	(0.018)
Feudal × Female		-0.033	-0.044**
		(0.021)	(0.021)
Peak × Female			0.097***
			(0.017)
Feudal × Peak × Female			0.022
			(0.021)
Non-feudal mean (logged)	6.006	6.006	6.006
Non-feudal mean (INR)	448.287	448.287	448.287
R^2	0.39	0.59	0.67
Observations	31172	31172	31172

This table shows results from our main specification on wage data that we collected in our phone survey with a sample of N = 2396 elected village representatives in our study area. The outcome is stacked log wages, pooling together peak and typical wages, and those for both female and male tasks. We interact with whether the wage is peak or typical, and also interact with the gender classification of the task. Each regression is at the level of a village-crop-season combination, with district, crop and season fixed effects and standard errors clustered at the village level. $* p < 0.1$, $** p < 0.05$, $*** p < 0.01$

Table D.6: Pooling workers' and officials' wage reports

	Peak male fertilizer		Peak female weeding		Avg male fertilizer		Avg female weeding	
		Ln		Ln		Ln		Ln
Feudal	-8.763	-0.006	-27.704*	-0.058*	-5.504	-0.000	-18.538**	-0.058**
	(12.988)	(0.024)	(14.254)	(0.030)	(10.571)	(0.022)	(8.512)	(0.027)
Non-feudal mean	567.073	6.297	405.867	5.912	490.715	6.151	294.894	5.632
Male vs. Female p-value			0.434	0.066			0.702	0.125
R^2	0.46	0.41	0.54	0.55	0.51	0.46	0.48	0.49
Observations	9318	9318	7809	7809	9619	9619	7882	7882

This table shows results from our main specification on pooled wage data that we collected in our phone survey with a sample of N = 2396 elected village representatives and N = 496 workers in our study area. Within each outcome, the first column reports the absolute wages in INR and the second column reports logged wages. Each regression is at the level of a village-crop-season combination, with district, crop and season fixed effects and standard errors clustered at the village level. The surveys ask both the peak and typical wages for each task-gender combination listed. $*p < 0.1, **\ p < 0.05, ***p < 0.01$

Table D.7: Wages by season

	Log wages	
Feudal	-0.038**	-0.040***
	(0.015)	(0.015)
Kharif	-0.013	-0.043**
	(0.018)	(0.018)
Feudal × Kharif	-0.015	0.021
	(0.021)	(0.021)
Female		-0.452***
		(0.018)
Feudal × Female		0.001
		(0.021)
Female × Kharif		0.056**
		(0.025)
Feudal × Female × Kharif		-0.066**
		(0.031)
Non-feudal mean (logged)	6.006	6.006
Non-feudal mean (INR)	448.287	448.287
R^2	0.39	0.67
Observations	31172	31172

This table shows results from our main specification on wage data that we collected in our phone survey with a sample of N = 2396 elected village representatives in our study area. The outcome is stacked log wages, pooling together peak and typical wages, and those for both female and male tasks. We control for whether the wage is peak or typical, and interact with the gender classification of the task. The specification focuses on an interaction with a binary variable for the "Kharif" season, which is the more labor intensive cultivation season in the year. Each regression is at the level of a village-crop-season combination, with district and crop fixed effects and standard errors clustered at the village level. $*p < 0.1, **\ p < 0.05, ***p < 0.01$

E Tabular versions of graphs

	Percentiles of landholdings in hectares						
	1st	10th	25th	50th	75th	90th	99th
Feudal	0.067**	0.413***	0.686***	0.872***	0.982***	0.693*	6.176**
	(0.032)	(0.091)	(0.163)	(0.244)	(0.303)	(0.387)	(2.764)
Non-feudal mean	0.475	1.922	4.085	7.960	13.178	19.521	47.516
R^2	0.29	0.41	0.43	0.45	0.44	0.31	0.16
Observations	8931	8925	8925	8933	8943	8949	8929

Table E.1: Differences in the distribution of landholdings

This table shows results from our primary regression discontinuity specification on percentiles across the distribution of landholdings, specified in hectares, in our study regions. All indicators are created using raw land parcel level data procured from a remote sensing company, corresponding to the state of landownership in our study area as of September 2022. Each regression includes fixed effects at the district and border line segment level, and standard errors are robust. $* p < 0.1, ** \ p < 0.05, *** p < 0.01$

F Robustness tests

Table F.1: Robustness to bandwidths

	GINI											Class HHI										
	(0, 5)	(0, 10)	(0, 20)	(0, 30)	(2, 5)	(2, 10)	(2, 20)	(2, 30)	(5, 10)	(5, 20)	(5, 30)	(0, 5)	(0, 10)	(0, 20)	(0, 30)	(2, 5)	(2, 10)	(2, 20)	(2, 30)	(5, 10)	(5, 20)	(5, 30)
Feudal	-0.007	-0.002	-0.011**	-0.012***	0.030	0.012	-0.011*	-0.017***	0.029	-0.033***	-0.046***	0.023*	0.012	0.022***	0.022***	0.026	0.003	0.027***	0.026***	-0.046	0.033**	0.032**
	(0.008)	(0.005)	(0.005)	(0.004)	(0.020)	(0.009)	(0.006)	(0.006)	(0.024)	(0.010)	(0.009)	(0.012)	(0.009)	(0.007)	(0.006)	(0.035)	(0.015)	(0.010)	(0.009)	(0.042)	(0.016)	(0.013)
Non-feudal mean	0.234	0.234	0.234	0.234	0.234	0.234	0.234	0.234	0.234	0.234	0.234	0.469	0.469	0.469	0.469	0.469	0.469	0.469	0.469	0.469	0.469	0.469
R^2	0.95	0.95	0.93	0.91	0.96	0.95	0.93	0.91	0.95	0.93	0.91	0.37	0.33	0.29	0.28	0.40	0.33	0.29	0.29	0.34	0.29	0.29
Observations	3398	6114	10504	14028	1903	4619	9011	12535	2712	7102	10626	3398	6114	10504	14028	1903	4619	9011	12535	2712	7102	10626

This table represents robustness to a range of different bandwidths for our preferred regression discontinuity specification. In columns 1 - 11, we report treatment effects on the GINI indicator. In columns 12 - 22, we report treatment effects on the Class HHI. Each column header denotes the range for which observations are subset for the specification. Each regression is at the level of a village level, with district and border line segment fixed effects and robust standard errors clustered. $* p < 0.1$, $** \ p < 0.05$, $*** p < 0.01$

Table F.2: Robustness to bandwidths

	1st percentile											25th percentile										
	(0, 5)	(0, 10)	(0, 20)	(0, 30)	(2, 5)	(2, 10)	(2, 20)	(2, 30)	(5, 10)	(5, 20)	(5, 30)	(0, 5)	(0, 10)	(0, 20)	(0, 30)	(2, 5)	(2, 10)	(2, 20)	(2, 30)	(5, 10)	(5, 20)	(5, 30)
Feudal	-0.086**	-0.036	0.027	0.043**	-0.292**	-0.033	0.067**	0.082***	0.007	0.139**	0.141***	0.452**	0.372**	0.588***	0.593***	-0.538	0.107	0.686***	0.682***	0.271	1.076***	0.933***
	(0.042)	(0.030)	(0.023)	(0.021)	(0.125)	(0.052)	(0.032)	(0.028)	(0.141)	(0.054)	(0.043)	(0.208)	(0.150)	(0.115)	(0.104)	(0.613)	(0.264)	(0.163)	(0.141)	(0.738)	(0.262)	(0.209)
Non-feudal mean	0.475	0.475	0.475	0.475	0.475	0.475	0.475	0.475	0.475	0.475	0.475	4.085	4.085	4.085	4.085	4.085	4.085	4.085	4.085	4.085	4.085	4.085
R^2	0.33	0.31	0.28	0.28	0.36	0.32	0.29	0.29	0.35	0.29	0.29	0.47	0.45	0.43	0.43	0.50	0.45	0.43	0.44	0.48	0.44	0.45
Observations	3364	6052	10405	13888	1888	4576	8931	12414	2684	7037	10520	3350	6036	10398	13895	1875	4561	8925	12422	2682	7044	10541

This table represents robustness to a range of different bandwidths for our preferred regression discontinuity specification. In columns 1 - 11, we report treatment effects on the 1st percentile of landownership. In columns 12 - 22, we report treatment effects on the 25th percentile of landownership. Each column header denotes the range for which observations are subset for the specification. Each regression is at the level of a village level, with district and border line segment fixed effects and robust standard errors clustered. $* p < 0.1$, $** \ p < 0.05$, $*** p < 0.01$

Table F.3: Robustness to bandwidths

	75th percentile											99th percentile										
	(0, 5)	(0, 10)	(0, 20)	(0, 30)	(2, 5)	(2, 10)	(2, 20)	(2, 30)	(5, 10)	(5, 20)	(5, 30)	(0, 5)	(0, 10)	(0, 20)	(0, 30)	(2, 5)	(2, 10)	(2, 20)	(2, 30)	(5, 10)	(5, 20)	(5, 30)
Feudal	0.564	0.440	0.710***	0.642***	0.887	0.280	0.982***	0.871***	-1.543	1.278***	1.075***	8.115**	7.574***	6.317***	5.428***	12.360	8.603**	6.176**	5.054**	10.604	6.330	4.936
	(0.382)	(0.276)	(0.215)	(0.197)	(1.101)	(0.482)	(0.303)	(0.267)	(1.299)	(0.482)	(0.394)	(3.223)	(2.421)	(1.994)	(1.833)	(9.237)	(4.087)	(2.764)	(2.413)	(9.098)	(4.460)	(3.507)
Non-feudal mean	13.178	13.178	13.178	13.178	13.178	13.178	13.178	13.178	13.178	13.178	13.178	47.516	47.516	47.516	47.516	47.516	47.516	47.516	47.516	47.516	47.516	47.516
R^2	0.51	0.47	0.44	0.43	0.53	0.47	0.44	0.44	0.48	0.46	0.45	0.30	0.21	0.16	0.14	0.33	0.23	0.16	0.14	0.26	0.17	0.14
Observations	3374	6072	10427	13921	1888	4586	8943	12437	2694	7049	10543	3375	6067	10412	13891	1890	4582	8929	12408	2687	7033	10512

This table represents robustness to a range of different bandwidths for our preferred regression discontinuity specification. In columns 1 - 11, we report treatment effects on the 75th percentile of landownership. In columns 12 - 22, we report treatment effects on the 99th percentile of landownership. Each column header denotes the range for which observations are subset for the specification. Each regression is at the level of a village level, with district and border line segment fixed effects and robust standard errors clustered. $* p < 0.1$, $** \ p < 0.05$, $*** p < 0.01$