Spillovers of Digitization

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Abstract

Property-rights reforms can reshape institutions far beyond their administrative mandate. I exploit the staggered introduction of land-record digitization to trace its spillovers across rural institutions in Punjab, Pakistan. Drawing on 45,347 confidential anti-corruption incidents, administrative records on agricultural credit, court cases, and taxation, I show that digitization expands agricultural credit, raises corruption detection, improves land-case resolution, and activates rental markets. These gains do not stem from shifts in the landownership patterns. Rather, digitization renders land records public and transparent, breaking local officials' exclusive control over them and reshaping how rural institutions function. (*JEL: D73, G21, H11, K42, O13, Q15; Keywords: Digitization, Land-record Reform, Corruption, Credit, Agriculture*)

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1 Introduction

Secure and verifiable property rights are a foundation of economic development. They encourage investment, support the use of land as collateral, and underpin the orderly resolution of disputes (Libecap, 2018; Angeles, 2011; Goldstein and Udry, 2008; Kerekes and Williamson, 2008; Field, 2007; North, 1990). Digitizing the records of the state has likewise been shown to reduce agency problems between bureaucrats and their principals (Dal Bó et al., 2021; Lewis-Faupel et al., 2016; Duflo et al., 2012) and to improve the targeting of welfare recipients (Muralidharan et al., 2016). A growing number of governments have sought to strengthen property rights by digitizing land records, replacing handwritten registers with centralized digital systems. Between 2010 and 2019, more than fifty economies computerized their land registries, often at considerable expense (World Bank, 2019). Yet a land registry is not a standalone administrative artifact. The same records that establish who owns a parcel also serve as the collateral on which lenders secure agricultural credit, the evidence on which courts adjudicate ownership, the base on which the state assesses land taxes, and the source of discretionary power for the local official who controls them. A reform that changes how land is recorded can therefore propagate well beyond the registry, into the markets and institutions that rely on those records.

These spillovers need not point in the same direction, and existing evaluations of land-records digitization have studied them one at a time. In the same reform I study, Beg (2022) shows that digitization improved the allocative efficiency of land and labor, Ullah and Hussain (2023) finds that it changed the resolution of land disputes, and Aman-Rana and Minaudier (2026) documents that it *reduced* the state's collection of agricultural taxes by reorganizing the local bureaucracy. Read in isolation, these results are difficult to reconcile: the same reform appears development-enhancing on some margins and capacity-eroding on others. Whether a single registry reform generates a coherent set of spillovers across credit markets, the accountability of the bureaucracy, dispute resolution, and the fiscal apparatus of the local state—and whether these spillovers share a common mechanism—remains an open question.

In this paper, I study the spillovers of a land-records digitization reform across the markets and institutions that depend on those records. Using the staggered rollout of land records digitization in Punjab, Pakistan, I establish two main results. First, digitization expanded agricultural credit. It raised both the number of borrowers and the volume of credit disbursed, with the new lending concentrated among the smallest, previously collateral-constrained borrowers. Second, it increased the *detection and prosecution* of corruption in the land bureaucracy: recorded complaints, enquiries, cases, and convictions against land-administration officials all rose. I argue that both findings, together with the legal, household, and fiscal evidence, trace to a common source: the reform dismantled the informational monopoly that a single local official—the village land officer, or *patwari*—had exercised over the only legally recognized record of ownership. Breaking that monopoly made titles cheaply verifiable, which expanded collateralized lending. It created a centralized audit trail that rendered the conduct of land officials documentable and attributable, which strengthened accountability. It also stripped the same official of the informal leverage he had used to enforce tax payment.

The setting offers an unusually clean opportunity to trace these spillovers. Before the reform, rural land rights in Punjab were recorded in hand-written registers held at the level of the revenue estate (*mouza*, which consists of one or more villages) by the *patwari*, who alone could issue the ownership certificate (*fard*) required to sell, mortgage, or inherit land. Beginning in 2011, the Punjab Land Records Authority (PLRA), with World Bank support, digitized the records of roughly 90 percent of rural estates and migrated their custody to a network of service centers and an online platform (World Bank, 2019; Gonzalez, 2016). Because the same record underpins credit, the courts, taxation, and the discretion of the officials who keep it, a reform to its custody lets me follow a single shock as it propagates across distinct domains of the economy and the state.

I assembled ten administrative datasets from Pakistan, each merged to the rollout. Two contain the main outcomes: district-by-year agricultural credit from the State Bank of Pakistan, and confidential incident-level anti-corruption records from the Punjab Anti-Corruption Establishment, which I restructure into a balanced *mouza*-by-month panel of complaints, enquiries, and cases. Rest supplies the rollout itself and the mechanism and downstream evidence: household land and labor markets from the Pakistan Household Integrated Economic Survey, land-related court cases, district-level crime records, Kissan card, and administrative agricultural-tax records. Two support identification: scheme-level development budgets, used as a placebo for concurrent public investment, and pre-reform election returns, used to test whether the rollout followed political lines.

My empirical strategy exploits the staggered rollout of digitization across Punjab's 36 districts in three phases, with cohorts treated in 2012, 2013, and 2014 (Section 3.1). Because the realized village-level rollout could respond to time-varying local conditions correlated with the outcomes, I assign treatment at the coarser phase level and present intent-to-treat estimates of the government's decision to digitize. I report two difference-in-differences estimators—a stacked estimator (my preferred specification) and a conventional two-way fixed-effects estimator that is exposed to the biases of staggered adoption (Goodman-Bacon, 2021; Sun and Abraham, 2021; Cengiz et al., 2019; De Chaisemartin and d'Haultfoeuille, 2020)—together with an instrumental-variables version of each and an event-study specification.

I begin by documenting how the digitization reform expanded agricultural credit. The reform increased the number of agricultural borrowers by roughly 19,000—larger than the mean of the estimation sample—and disbursed lending by PKR 5 to 7 billion, with instrumental-variables estimates, which scale the effect by the realized share of villages digitized, about sixty percent larger than the intent-to-treat estimates. The expansion is concentrated among subsistence farmers and small non-farm borrowers rather than the already-banked larger holdings, and it operates through wider reach and improved loan recovery rather than larger loans—the signature of a collateral channel under which verifiable titles lower the cost and risk of secured lending and screen in previously rationed borrowers (Deininger and Goyal, 2012; Field, 2007). The event study displays parallel pre-trends followed by a gradual increase that builds over the years after a district's rollout, the pattern expected from a reform that works through the slow accumulation of verifiable titles rather than a one-time shock.

I then turn to the paper's second main result. In the *mouza*-by-month panel, digitization roughly tripled the recorded incidence of corruption involving the land bureaucracy, and the increase runs through the entire enforcement funnel: complaints, enquiries, cases, and convictions all rise. The effect is concentrated precisely where the mechanism predicts—in incidents involving the Revenue Department and the land-administration staff (*patwari*, *girdawar*, *kanungo*, and *tapedar*) whose discretionary control over records the reform removed—and is absent in unrelated departments. Because these records measure enforcement actions rather than the underlying incidence of bribery, I interpret the result as evidence of improved *accountability*: the centralized audit trail rendered previously unverifiable malfeasance documentable, escalatable, and punishable. That the effect propagates all the way to convictions—rather than dissipating as unfounded complaints—and that survey evidence on the same reform finds routine bribery for land services[1] *fell* (Aman-Rana and Minaudier, 2026), together indicate that digitization curtailed everyday extraction while surfacing and sanctioning the malfeasance that remained.

[1] In a survey of households carried out before the reform, 82 percent of respondents indicated that the way to "remedy the problems faced in accessing land records" was to give a bribe, and 65 percent reported that they could not access land-record services without unofficial payments (Gallup Pakistan, 2009).

The remaining evidence connects these two results and probes the common mechanism. Consistent with clearer titles and lower-friction transfers, digitization increased the formalization and successful resolution of land cases through the courts (Ullah and Hussain, 2023) and reduced the violent resolution of disputes—recorded murders and attempted murders fell in digitized districts while crimes unrelated to land were unchanged. It also raised household formal-loan take-up and rental-market activity—the margins through which land is reallocated toward more productive users (Beg, 2022)—while leaving outright purchases and sales unchanged. The digitized record was also used for the Kissan card in 2024 to disburse loans based on land sizes, and the manipulation was negligible. The spillovers, however, are not uniformly benign. Aman-Rana and Minaudier (2026) shows that digitization *reduced* agricultural tax collection; the same loss of discretionary control that strengthened accountability also stripped the local staff of the informal leverage they had used to enforce payment. The reform that expanded credit and improved accountability thus carried a fiscal cost, and evaluating it requires weighing these opposing spillovers against one another.

Taken together, the results show that a reform narrowly aimed at modernizing a record-keeping system reshaped credit access, the accountability of the land bureaucracy, the resolution of land cases and the violence surrounding them, and tax collection through replacing the local official's informal, discretionary authority over a multipurpose record with a transparent and formal system. Where formalization complemented the activity—credit, the detection of corruption, the resolution of cases—outcomes expanded; where the activity had instead relied on the official's informal leverage—tax collection—the outcome contracted. Two features of the setting drive these spillovers. A single record served many functions at once (collateral, evidence, and tax base), and control over it was concentrated in one official whose discretion was the common source of both the frictions the reform relieved and the leverage it removed. Spillovers of this kind are likely largest where one record and one official sit at the intersection of credit, the courts, and the fiscal state, and smaller where these functions are already separated or formalized.

These findings carry a broader lesson for the evaluation of institutional reform. Because a single reform can simultaneously relieve frictions in one market and remove complementarities in another, its components cannot be assessed in isolation. A welfare accounting must net the gains from expanded credit and stronger bureaucratic accountability against the fiscal cost of weakened collection. More generally, the paper shows that the returns to digitizing the state depend not only on the function being digitized but on the web of decisions—private and public—that the digitized records support.

This paper contributes to three strands of existing literature. First, it speaks to the literature on technology, digitization, and development, much of which studies the direct effect of technology on the productivity and accountability of bureaucrats and on service delivery (Debnath et al., 2023; Callen et al., 2020; Hanna and Wang, 2017; Muralidharan et al., 2016; Lewis-Faupel et al., 2016; Fujiwara, 2015; Banerjee et al., 2014; Duflo et al., 2012). Where most of this work evaluates the targeted function, I show that digitizing one record generates first-order spillovers onto markets and state functions it was not designed to change. Second, the paper contributes to the literature on property rights, collateral, and the misallocation of resources, which links the security of land rights to investment, credit, and the efficient allocation of factors and that clarifying rights reduces the violent contestation of ownership (Beg, 2022; Adamopoulos and Restuccia, 2014; Deininger and Goyal, 2012; Galiani and Schargrodsky, 2010; Goldstein and Udry, 2008; Hsieh and Klenow, 2009; Besley, 1995). I provide direct evidence that improving the verifiability of titles expands formal agricultural credit at scale and at the bottom of the distribution. Third, the paper contributes to the literature on corruption, bureaucratic rent-seeking, and state capacity (Colonnelli et al., 2020; Khan et al., 2019; Naritomi, 2019; Khan et al., 2016), by showing that removing a local official's informational monopoly strengthens the detection and prosecution of corruption in the function that was digitized, even as it can erode the informal capacity that supported other state functions. By bringing the credit, corruption, dispute-resolution, and fiscal margins into a single framework, I show that the welfare consequences of digitizing the state turn on the full set of spillovers it sets in motion.

2 Background

This section describes the institution at the center of the paper—the rural land-records bureaucracy of Punjab—and the reform that digitized it. I begin by explaining how land records were maintained before the reform and why their custody conferred substantial discretionary power on a single local official (Section 2.1). I then describe the reform and its phased rollout (Section 2.2). Finally, I explain why a change confined to the land registry plausibly spills over into credit, corruption, the resolution of disputes and the violence surrounding them and land markets—the outcomes I study (Section 2.3).

2.1 The Records

In Punjab, as across South Asia, rural land rights have historically been recorded in a set of hand-written registers inherited from the colonial revenue administration. Panel A of Figure B1 shows how the land record has been maintained since the colonial era. The two central documents are the *record of rights* (*jamabandi*), which lists owners, cultivators, and the area and class of each parcel, and the *register of mutations* (*intiqal*), which records transfers of ownership through sale, inheritance, gift, or mortgage. These registers were maintained at the level of the revenue estate (*mouza*, which consists of one or more villages), the smallest unit of land administration, and were updated and held by a single village-level official, the *patwari*.

The *patwari* occupied a position of unusual local power. He was the sole custodian of the only legally recognized record of ownership; he alone could issue the ownership certificate (*fard*) that a landholder needed to sell, mortgage, or pledge land, register an inheritance, or contest a boundary; and he recorded the mutations that transferred title within his jurisdiction. A cadastral map used to locate the land is shown in Panel B of Figure B1. Because the records were hand-written, decentralized, and effectively unauditable, the *patwari* could delay entries, demand informal payments to produce a *fard* or process a mutation, or alter records in favor of the higher bidder. The opacity of the system thus generated rents for the official who controlled it and exposed landholders—disproportionately the poor and those with weaker outside options—to extortion, manipulation, and protracted disputes. Land records of this kind have been widely identified as a source of insecure tenure, contested ownership, and the misallocation of land and credit (Deininger and Goyal, 2012).

The custody of land records mattered well beyond the registry itself. A clear, verifiable title is the collateral on which formal agricultural lending rests; it is the evidence on which courts adjudicate ownership and inheritance disputes; and the same records define the base on which agricultural land taxes are assessed and collected. A single official's control over the registry therefore sat at the intersection of credit markets, the courts, and the fiscal apparatus of the local state. Critically, the same revenue hierarchy that maintained the records also assessed and collected the agricultural land tax, and its officials could use their control over titles and mutations—expediting a *fard* here, delaying one there—as leverage to induce payment. The registry was thus not only an economic record but an instrument of informal fiscal enforcement.

2.2 The Reform

Beginning in 2011, the Government of Punjab—through the newly created Punjab Land Records Authority (PLRA), with financing and technical support from the World Bank—undertook to replace this manual system with a centralized digital land-records system, the Land Records Management and Information System (LRMIS) (World Bank, 2019; Gonzalez, 2016). The reform digitized the ownership and cultivation records of roughly 23,000 of Punjab's 25,709 revenue estates—about 90 percent of rural land—and migrated their custody from the *patwari* (Panel C of Figure B1 shows how the records were maintained in the *patwari*'s office) to a network of service centers and an online platform, shown in Panel D of Figure B1. The reform's objective was to increase the reliability and transparency of a system that was prone to errors and corruption.

The reform changed three features of the system that had previously concentrated power in the local official. First, it *centralized custody*: records were moved from individual handwritten registers to a single digital database, so that no individual official was any longer the unique gatekeeper of a *mouza*'s records. Second, it *standardized service*: a landholder could obtain a *fard* or initiate a mutation at a service center or online, against a posted fee and with a documented processing time, rather than through a discretionary and untimed interaction with the *patwari*. Third, it *created an audit trail*: digital records are time-stamped, attributable, and centrally stored, which makes manipulation easier to detect and accountability easier to enforce. Together these changes reduced the informational monopoly and discretionary control that had been the basis of the *patwari*'s rents.

The reform was rolled out across Punjab's 36 districts in three discrete phases, originally scheduled for 2011, 2013, and 2015, rather than all at once.[2] The geographical rollout of the program is shown in Panel A of Figure 1, and the realized digitization—the share of villages digitized by location and over time—is shown in Panel B. By the end of the rollout, the system operated through 151 service centers and an online portal. The phased schedule is the source of the quasi-experimental variation I use throughout the paper: districts that were digitized earlier can be compared, before and after their entry, to districts that were scheduled to be digitized later but had not yet been. Figure 2 shows that there were no statistically significant differences in baseline characteristics between districts digitized

[2]The phasing reflected the logistical scale of digitizing tens of thousands of estates and standing up service centers across the province, and the order in which districts entered was fixed administratively in advance. I exploit this staggered timing (following Aman-Rana and Minaudier, 2026; Beg, 2022), and the gap between the planned and the realized rollout, for identification; see Section 4. Because the rollout was delayed, treatment is dated by actual implementation (cohorts treated in 2012, 2013, and 2014); see Section 3.1.

during the first two phases of the rollout and those digitized in the final phase. Figure D2 further pools the coefficients for Phases 1 and 2, confirming no significant difference relative to districts digitized in the final phase. Panel (a) of both figures displays baseline district and institutional characteristics; Panel (b) shows socioeconomic characteristics; and Panel (c) presents baseline political characteristics.

2.3 The Effect

The reform was, narrowly, an administrative modernization of a record-keeping system (World Bank, 2019). Its economic significance comes from the fact that the same records underpin a range of decisions outside the registry, so that a change in how land is recorded can propagate into the markets and institutions that rely on those records. This logic motivates the outcomes I examine.

Most directly, more secure and verifiable titles strengthen the collateral that supports formal agricultural credit: when a lender can cheaply confirm ownership and register a charge against land, the cost and risk of secured lending fall, and credit can expand (Kerekes and Williamson, 2008; Feder and Nishio, 1998). At the same time, by dismantling the local official's informational monopoly,[3] digitization removes an important source of bureaucratic rent-seeking,[4] which should be visible in the incidence of corruption complaints and enforcement actions against land-administration staff (Adam and Fazekas, 2021; Potel, 2019). Clearer records and lower-friction transfers should also reduce the ambiguity that gives rise to land disputes, changing the volume and resolution of land-related cases (Ullah and Hussain, 2023). Where claims cannot be verified on paper, they are often contested by force. Land disputes are a leading trigger of violent crime in rural Punjab, so an authoritative record should also reduce the violence through which such disputes are otherwise resolved. Existing work has studied several outcomes. Beg (2022) shows that digitization improved the allocative efficiency of land and labor, and Aman-Rana and Minaudier (2026) document that it can reduce agricultural tax collection by weakening the local staff who enforced it. I bring these margins together and show that a single reform to the land registry simultaneously reshapes credit access, the detection of corruption, the resolution—formal and violent—of

[3]Because the bureaucrats no longer had control over the land-record process, they lost this source of bribes (Aman-Rana and Minaudier, 2026).

[4]Only 2 percent of households reported paying a bribe for land records once those had been digitized, and a majority of households had a good or very good experience with the newly digitized services (Apex Consulting Pakistan, 2016).

land disputes, and land rental markets. Consistent with the mechanism, the share of bureaucrats agreeing that citizens wanted to tip to obtain land titles fell from 48 percent before digitization to 33 percent after (Figure F3).

3 Data

I combine ten datasets from Pakistan, each merged with the phased rollout of the Land Records Management and Information System. Three comprise the main analysis, including the rollout of digitization, agricultural credit, and anti-corruption enforcement. Four are used to examine mechanisms and downstream outcomes, including household land and labor markets, land-related court cases, district crime records, and agricultural tax administration. The remaining two support identification, such as the provincial development budget, used as a placebo for concurrent public investment, and pre-reform election returns, used to test whether the rollout followed political lines. The agricultural credit data is a panel at the district level, covering all districts[5] in Pakistan and spanning 2008 to 2022. The corruption data were originally structured at the incident level, with details such as the type of incident, department, personnel, ranks, and dates. I restructured these data to the *mouza*[6] (also known as revenue circle or revenue village) and monthly level, covering 2000 to 2023. The datasets vary in their unit of observation, frequency, and geographic coverage; Table A1 summarizes these differences, while Table A2 presents the definition, unit, and structure of every variable. Appendix A details the harmonization and construction of each panel, and Appendix C provides descriptive statistics for the main datasets.

3.1 Digitization

The treatment of interest is the digitization of rural land records under LRMIS, implemented by the Punjab Land Records Authority (PLRA) with support from the World Bank (World Bank, 2019; Gonzalez, 2016).[7] The reform digitized ownership and cultivation records for

[5] A total of 149 districts, including 36 from Punjab province. Standard errors are clustered at the district level (149 clusters) throughout the credit panel.

[6] The total number of *mouzas* (revenue circle or revenue village) is 1,287. One *mouza* typically consists of three to five villages. Because digitization is observed at a more granular level in the corruption panel, standard errors are clustered at the *mouza* level throughout the corruption panel.

[7] Program details are drawn from the PLRA portal (`https://www.punjab-zameen.gov.pk`) and World Bank project documentation. Related studies examining this reform include Beg (2022), Aman-Rana and Minaudier (2026), and Ullah and Hussain (2023).

approximately 23,183 of the 25,709 revenue estates (*mouza*) in Punjab—covering roughly 90 percent of rural land. This process replaced hand-written registers maintained by the village land officer (*patwari*) with a centralized digital system capable of issuing ownership certificates (*fard*) and recording transfers (*mutations*) through 151 service centers and an online portal.

Digitization was rolled out across Punjab's districts in three phases, originally scheduled for 2011, 2013, and 2015. However, as the actual implementation was significantly delayed relative to this original plan—to the extent that no districts were digitized according to the intended timeline—I define the treatment variable based on actual implementation progress, utilizing data provided by the Punjab Land Records Management Information System (LR-MIS) project office and the Punjab Land Records Authority (PLRA).

To construct a robust treatment indicator, I define a "phase" as the set of districts scheduled for digitization within the same rollout cohort. A phase is classified as "digitized" in a given year if at least 5 percent of the villages within that cohort have completed the digitization process. I define the beginning of digitization at the phase level, rather than at the individual district level, for two primary reasons. First, this approach allows us to exploit variation in the rollout that is less likely to be driven by unobserved, endogenous district-level characteristics, which could potentially correlate with both the pace of implementation and outcomes. Second, this method preserves an intent-to-treat (ITT) interpretation. A district is considered treated once its cohort crosses the 5 percent threshold, regardless of whether that specific district's own villages are yet digitized.

Under this definition, the treatment variable is a dummy equal to 1 for a district and year if it belongs to a phase that has surpassed the 5 percent threshold. Consequently, Phase 1 is treated starting in 2012, Phase 2 in 2013, and Phase 3 in 2014. Further details on the instrument and the empirical strategy are provided in Section 4.

3.2 Credit

Data on agricultural credit come from the State Bank of Pakistan (SBP), the country's central bank, at the district-by-year level for 2008–2022.[8] The data cover all 149 districts of

[8] State Bank of Pakistan, Agricultural Credit and Microfinance Department, `https://www.sbp.org.pk`. Computerized land registries have elsewhere been shown to expand credit by improving collateral verifiability (Deininger and Goyal, 2012).

Pakistan, so the panel contains a never-treated group. For each district-year I observe the number of unique borrowers (in thousands) and the amounts disbursed, recovered, and outstanding (in billions of Pakistani rupees), separately for the farm and non-farm sectors and, within each, by landholding or enterprise class (subsistence, economic, and above-economic farm holdings; small and large non-farm borrowers). The estimation sample contains 2,235 district-year observations. I also construct several derived outcomes—for example, average loan size, number of borrowers per village, recovery rate, and debt burden; the construction of all variables is given in Table A2. Descriptive statistics for the main variables are in Table C4, and Table C5 reports summary statistics for the disaggregated variables, such as the number of borrowers and loan sizes by farmer and landholding class.

3.3 Corruption

The corruption data are confidential incident-level records from the Punjab Anti-Corruption Establishment (ACE), obtained under a data-use agreement. The incidents comprise 27,170 complaints, 14,440 enquiries, and 3,737 cases. Records are de-identified prior to analysis, and the construction details are in Appendix A.2. The region comprises 1,287 *revenue circles*[9]. The data consist of three linked registers—*complaints* filed, *enquiries* opened, and *cases* prosecuted. The recorded incidents were compiled in text format. An expert legal team (randomly assigned) systematically analyzed these reports. The team undertook a detailed examination of each incident, extracting pertinent information including the date and location of the incident, the alleged official(s) involved, their respective departments, designations, and grades. Additionally, the analysis encompassed the disposition of each case, as well as the outcomes of any convictions or ongoing court proceedings. Because a single incident can name multiple officials, I record each register at the individual level and then aggregate to a balanced *mouza*-by-month panel spanning 2000–2023, assigning a value of zero to *mouza*-months with no recorded incident. The resulting panel contains 1,287 *mouza* and 370,656 observations. Descriptive statistics for the *mouza*-month panel are in Table C6.

For each *mouza*-month, I construct counts of incidents that are *land-related* along two dimensions: by *department*—incidents involving the Revenue Department, the PLRA, or the Local Government—and by *personnel*—incidents in which the accused holds a land-

[9]The region comprises two of Punjab's nine administrative divisions, which together contain nine districts and 1,287 revenue circles. Crucially for identification, these districts span all three rollout phases.

administration designation (*patwari*, *girdawar*, *kanungo*, or *tapedar*). I also built counts by stage (complaints, enquiries, cases), by grade (Basic Pay Scale), by employment status (currently employed versus retired), and the transition rates along the enforcement funnel (complaint-to-enquiry, enquiry-to-case, and conviction).

3.4 Markets

To investigate the mechanisms and downstream outcomes of increased agricultural credit, I use household-level data on land and labor markets from the Pakistan Household Integrated Economic Survey (HIES[10]) for Punjab. The hypothesis is twofold: if administrative credit rose, household formal-loan take-up should rise as well; and the household data reveal how that credit was used. I therefore observe whether a household obtained a formal loan, sold or purchased agricultural land, rented land in or out, and its total cultivated area, together with household-head characteristics (age, gender, and education). Sample sizes range from 7,256 cultivating or landowning households for land-market outcomes to 19,067 households for credit take-up, across 34 districts. Summary statistics are in Table C8.

3.5 Cases

To measure the formalization of land cases, I use weekly registers from the court-annexed mediation and dispute-resolution centers of the Lahore High Court, Government of Punjab, covering all 36 districts over 2017–2018. For each district-week I observe the total number of land-related cases and their composition—criminal, civil, family, guardian, rent, and other—and, by outcome, the number of cases resolved successfully versus those that fail. The estimation sample contains 2,268 district-week observations. Because the data begin in 2017, after all districts had entered the program, identification here does not come from the onset of treatment but from cross-district variation in digitization intensity—the share of villages digitized and the time elapsed since each cohort's rollout—which I compare across earlier- and later-digitized cohorts. Descriptive statistics for the land-cases data are in Table C7.

[10] The same data are used by Beg (2022) to study household-level effects. They measure farmer-level total factor productivity and the marginal product of land, and document improved allocative efficiency as land is redistributed toward more productive farmers.

3.6 Crime

To test whether the reform displaced the violent resolution of land disputes, I use annual police-recorded crime statistics for all 36 Punjab districts over 2002–2015, covering fifteen offense categories with counts and rates per 100,000 population. I group offenses into *dispute-sensitive* violent crimes—murder, attempted murder, hurt, and rioting—the categories through which contested land ownership typically escalates, and *placebo* property crimes with no land connection—motor-vehicle theft, burglary, and ordinary theft—which should not respond to digitization and serve as a falsification against general policing or reporting changes. Because recorded crime reflects both incidence and reporting, the placebo categories double as a control for reporting behavior.

3.7 Kissan Card

To document how the digitized registry underpins program delivery, I use administrative records from the Punjab Kissan Card, the province's flagship farm-support scheme in 2024, which extends subsidized input credit to farmers and verifies eligibility digitally against PLRA land records. The data cover four districts and comprise 37,095 card recipients and 1,816 non-recipients with holdings of up to 25 acres; for each farmer I observe the district, tehsil, and landholding as recorded in the digitized registry. Eligibility is means-tested at the 12.5-acre subsistence threshold, so the data permit a direct check of whether recorded landholding is manipulated around a high-stakes cutoff (Figure 8).

3.8 Bureaucracy

Finally, to connect corruption to the local bureaucracy and to evaluate the cost implications of the reform, I use administrative records on agricultural land taxation at the district-by-year level, drawn from Aman-Rana and Minaudier (2026), who find that digitization can have unintended consequences for state capacity—specifically, a decline in agricultural tax collection. I argue that this decline arises because the reform broke the monopoly of the local land staff. The dataset includes the assessed cultivated area, official tax demand (accuracy indicators), and the share of demand collected, together with indicators for collecting at least 50 and 75 percent of demand and the share of months with zero collection (performance metrics). This illustrates the transparency-and-information mechanism that empowered citizens and, I argue, raised the reporting of corruption incidents.

3.9 Development

To test whether the rollout was accompanied by differential public investment, I use the Punjab Annual Development Programme (ADP), the province's development budget, recorded scheme by scheme for every fiscal year from 2008–09 through 2024–25.[11] The raw data comprise 115,748 scheme-year records covering more than 76,000 distinct development schemes across 77 sectors and 17 fiscal years. I harmonize these records—whose format changes across years—into a balanced district-by-year panel of the 36 Punjab districts, mapping 95 percent of schemes to a district; schemes coded as province-wide are excluded, and the small number spanning several districts have their funds split equally across them. For each district-year I observe the number of schemes and total development allocation, releases, and utilization (in billions of Pakistani rupees), overall and by sector—agriculture, education, health, irrigation, roads, and other sectors.

I use these data in two ways. First, development spending is not an outcome of the reform but a *placebo*: because provincial development budgets are allocated centrally rather than by the local land bureaucracy, digitization should not shift a district's development resources, and confirming this rules out concurrent public investment as a confounder for the credit, corruption, and fiscal results (Section 5.4). Second, the ADP records the reform's own cost: the land-records digitization project had a final approved cost of roughly PKR 12.3 billion, which I use in the cost–benefit assessment (Section 6).

3.10 Elections

A further concern is that the rollout was politically targeted: if the provincial ruling party prioritized its own strongholds or electorally pivotal districts, treatment timing would be endogenous to local politics and could confound the results. To assess this, I use candidate- and constituency-level returns for Pakistan's National and Provincial Assembly general elections, sourced from the Election Commission of Pakistan. Party wings and characteristics are identified from party manifestos and verified against the Global Party Survey. Because

[11] Planning and Development Board, Government of the Punjab, `https://pnd.punjab.gov.pk`. Each scheme lists its sector, cost, annual allocation, funds released, and utilization, together with the district(s) it serves.

constituencies do not map cleanly onto districts and their boundaries shift with delimitation, I harmonize constituencies to districts using their assigned numbers and aggregate all outcomes to the district level.[12]

I use the pre-reform general elections of 2002 and 2008 to construct baseline political characteristics for each district, supplemented with district-level records on political violence (terrorist attacks and casualties) from the Global Terrorism Database[13]. These variables enter the baseline-balance tests in Figure 2 and Figure D2: phase assignment is uncorrelated with pre-reform political conditions, so the rollout does not appear to have followed electoral or security lines.

4 Empirical Strategy

4.1 Identification

There are multiple challenges in measuring the effect of digitization on the outcomes I study. The reform could have been introduced precisely when local land administration was deteriorating, so that districts adopting it earlier were already on different trajectories for credit access or corruption enforcement. Alternatively, districts could have been prioritized for digitization because their land bureaucracies were performing poorly and were seen to need technological support.

My estimation strategy addresses these concerns. Because the village-level rollout within a district could depend on time-varying local characteristics that correlate with these outcomes, I assign treatment at the coarser phase (cohort) level, dating each cohort by the year its districts collectively cross the 5 percent threshold (Section 3.1), and exploit the administratively fixed order in which cohorts entered the program. Throughout the paper I present an intent-to-treat analysis, which estimates the average return to "as-is" implementation of digitization following the government's "intent" to introduce the new system. These estimates reflect the impact of the decision to digitize land records, net of the logistical and

[12] The constituency-to-district crosswalk is described in Section A.5.

[13] Global Terrorism Database (GTD) is an open-source database including incident information on terrorist events around the world from 1970 through 2020. For each GTD incident, information is available on the date and location of the incident, the weapons used and nature of the target, the number of casualties, and–when identifiable–the group or individual responsible. I used district boundaries from the OCHA Field Information Services Section (FISS) and aggregated the incidents at the district year level. For the balance figures Figure 2 and Figure D2, I restrict the sample from 2002 to 2008.

political-economy challenges of implementation.

I compare the change in each outcome before and after digitization between districts whose cohort was treated earlier and those whose cohort was treated later. The identifying assumption is that early- and late-digitized districts follow parallel trends: districts in Phases 1 and 2 (treated in 2012 and 2013) would have experienced, on average, the same changes in outcomes over time as those in Phase 3 (treated in 2014), had their land records not been digitized. In the credit panel, this comparison is further anchored by never-treated districts outside Punjab. I discuss the validity of this assumption in Section 4.3.

The timing of a cohort's entry generates variation in digitization that, after conditioning on unit and time fixed effects, is plausibly unrelated to its outcome trends. I report two difference-in-differences estimators—a stacked estimator (my preferred specification) and a conventional two-way fixed-effects (unstacked) estimator—together with an instrumental-variables version of each and an event-study specification. Throughout, i indexes the unit of observation (a *district* in the credit, court, tax, crime, and development panels; a *mouza* in the corruption panel) and t indexes time (a year, a year-month, or a week).

4.2 Estimation

Let $digitization_{it}$ equal one in and after unit i's assigned phase-rollout year, and zero otherwise. The baseline specification is

$$y_{it} = \psi \, digitization_{it} + \iota_i + \iota_t + \varepsilon_{it}, \tag{1}$$

where ι_i and ι_t are unit and time fixed effects, and standard errors are clustered at the unit level (district or *mouza*). The coefficient ψ measures the average effect of digitization on outcome y_{it}. In the credit panel, identification is sharpened by the presence of never-treated districts outside Punjab; in the Punjab-only panels, ψ is identified from differences in the *timing* of digitization across cohorts. Because the reform was rolled out in waves, I also estimate an event study relative to each unit's own rollout year, which traces the dynamic path of the effect and tests for pre-trends:

$$y_{it} \;=\; \sum_{j=-q}^{-2} \eta_j \, d_{i,t}^{j} \;+\; \sum_{j=0}^{m} \delta_j \, d_{i,t}^{j} \;+\; \iota_i \;+\; \iota_t \;+\; \varepsilon_{it}, \tag{2}$$

where $d_{i,t}^{j}$ denotes a set of event-time indicators equal to one if unit i at time t is exactly j years relative to its assigned rollout year. I normalize the coefficient for $j = -1$ (the year prior to treatment) to zero, so that all coefficients η_j and δ_j represent the effect of digitization relative to the immediate pre-reform period. The leads (η_j for $j < -1$) test for the presence of pre-trends, while the lags (δ_j for $j \geq 0$) capture the dynamic impacts of the policy.

With staggered adoption and heterogeneous treatment effects, the two-way fixed-effects estimator in Equation (1) can be biased by "forbidden" comparisons that use already-treated units as controls (Goodman-Bacon, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2021). To address this, I implement the stacked DiD estimator of Cengiz et al. (2019). For each phase cohort c I construct a clean sub-experiment that retains only cohort-c units and not-yet-treated (or never-treated) controls within a symmetric event window, and I stack these sub-experiments[14] into a single dataset. I then estimate

$$y_{ict} \;=\; \varphi \, \textit{digitization}_{ict} \;+\; \chi_{ic} \;+\; \chi_{ct} \;+\; \varepsilon_{ict}, \tag{3}$$

where χ_{ic} and χ_{ct} are cohort-interacted unit and time fixed effects, so that treated units are compared only to clean controls within the same sub-experiment. Standard errors are clustered at the unit level. The stacked estimator is my preferred specification; I detail its construction in the footnote above, and Section 5.4 compares it to a suite of recently proposed estimators.

The difference-in-differences estimates identify the effect of the phase-level rollout, and can

[14]Following the stacked DID approach, I construct a separate event-specific dataset for each treatment cohort and then stack them into a single estimation sample. In my setting, the first sub-experiment treats Phase 1 districts (treated in 2012) as the treated group and uses Phase 2 and Phase 3 districts as not-yet-treated controls, retaining each control only in periods before its own digitization begins (2013 for Phase 2, 2014 for Phase 3). The second sub-experiment treats Phase 2 districts (2013) as the treated group and uses Phase 3 districts as not-yet-treated controls; Phase 1 districts, already treated in 2012, are excluded as already-treated units. More generally, each sub-experiment retains a treated cohort together with a clean control group within a symmetric event window around the cohort's rollout year. The resulting stacked dataset absorbs cohort-specific unit fixed effects (χ_{ic}) and cohort-specific time fixed effects (χ_{ct}), ensuring that all treated–control comparisons occur within a single sub-experiment and that no already-treated unit serves as a control. This design eliminates the contamination and negative-weighting problems that can bias conventional two-way fixed-effects estimators under staggered treatment adoption (Goodman-Bacon, 2021).

understate the effect of *realized* digitization: villages are converted gradually, so a unit assigned to an early phase is not fully digitized when its phase begins. To recover the effect of realized digitization, I instrument it with the phase-level treatment, whose cohort assignment was fixed administratively in advance and is therefore unrelated to a unit's contemporaneous outcomes conditional on fixed effects. Let $villages_{it}$ denote realized digitization—the share of a unit's villages digitized by t—and let $digitization_{it}$ be the phase indicator from Equations (1) and (3). The first stage is

$$villages_{it} \;=\; \varrho\, digitization_{it} \;+\; \iota_i \;+\; \iota_t \;+\; u_{it}, \tag{4}$$

so that the same phase indicator that serves as the treatment in Equations (1) and (3) is the excluded instrument here. In the second stage, I replace $digitization_{it}$ in the pooled specification in Equation (1) and the stacked specification in Equation (3) with the fitted share $\widehat{villages}_{it}$[15], yielding instrumented analogues of ψ and φ. I estimate the system by two-stage least squares and report the Kleibergen–Paap first-stage F statistic. The exclusion restriction requires that the rollout schedule affect outcomes only through realized digitization, conditional on unit and time fixed effects—plausible because both the phase calendar and the baseline distribution of estates were fixed before the program began (World Bank, 2019). The first-stage estimates are strong and stable across specifications, and are reported in Table E9 for the credit panel and Table E10 for the corruption panel.

4.3 Robustness

The identifying assumption is that, absent digitization, earlier- and later-digitized units would have followed parallel outcome paths. I assess it in four ways. First, and most directly, the event-study specification in Equation (2) tests for differential pre-trends: in every panel the leads η_j for $j < -1$ are small and statistically indistinguishable from zero, so treated and control units are on common trajectories in the years before digitization (Figure 3 and Figure 5); the dynamic effects emerge only after a unit's own rollout year. Second, the phase schedule is plausibly unrelated to outcome trends by construction: the order in which districts entered the program was fixed administratively in advance for logistical reasons—the scale of digitizing tens of thousands of estates and of establishing service

[15]Since the corruption panel data is structured at the mouza level, realized digitization is recorded as a binary indicator of whether a mouza was digitized according to plan rather than the percentage of villages digitized. Therefore, I use this binary indicator as the main explanatory variable in the IV specification.

centers—rather than in response to local credit or corruption conditions. Third, Figure 2 and Figure D2 show that earlier- and later-digitized districts are balanced on a wide range of pre-digitization characteristics—institutional, socioeconomic, and political covariates—with the very few imbalances small in magnitude and absorbed by the unit fixed effects; in the credit panel, the comparison is anchored further by never-treated districts outside Punjab, so identification does not rest on the early-versus-late contrast alone. Finally, the estimates are robust to the heterogeneity-robust estimators discussed, and three placebos point the same way. First, the corruption effect is absent in non-land departments (Table F22), second, crimes with no land connection do not respond to digitization (Table 10), and third, development spending shows no differential change in digitized districts (Table F23)—indicating that the results reflect the digitized function rather than contemporaneous regional trends or concurrent policy.

Standard errors are clustered at the unit level throughout (district or *mouza*; cohort×unit in the stacked design). I assess robustness along several dimensions, reported in Section 5.4: (i) the $\log(1 + y)$ and inverse hyperbolic sine transformations of the outcomes, which accommodate zeros while preserving a semi-elasticity interpretation (Chen and Roth, 2024); (ii) four modern heterogeneity-robust estimators—Callaway and Sant'Anna (2021), Sun and Abraham (2021), Gardner (2022), and Borusyak et al. (2024); and (iii) leave-one-cohort-out re-estimation.

5 Results

By dismantling the monopoly that a single local official—the *patwari*—held over the only legally recognized record of ownership, digitization replaced discretionary, informal authority with a transparent and formal system. Where formalization complemented an activity, outcomes expanded: formal agricultural credit grew, the detection and prosecution of corruption in the land bureaucracy rose, and land cases moved into and were resolved through the formal courts, and the violence through which disputes had been settled declined. Where an activity had instead relied on the official's *informal* leverage over citizens, formalization eroded it: the collection of agricultural taxes fell. The two main outcomes—credit (Section 5.1) and anti-corruption enforcement (Section 5.2)—and the household, land-case, violence, and fiscal margins that connect them (Section 5.3) all trace to this common source. Section 5.4 reports robustness and a battery of alternative estimators. Throughout, my preferred specification is the stacked difference-in-differences estimator; the instrumental-variables estimates rescale the intent-to-treat effect by the realized share of villages digitized and therefore recover a larger local average treatment effect.

5.1 Credit

Digitization expands formal credit. Table 1 and Figure 3 report the average treatment effect and the dynamic effect of digitization on agricultural credit, respectively. The reform increases the number of agricultural borrowers by approximately 19,000 (columns 1 and 3; both significant at the one percent level)—in the stacked specification, more than twice the dependent variable mean. The magnitude is large, but consistent with an extensive-margin expansion to previously unbanked borrowers rather than inflated lending to incumbents, as the disaggregated and mechanism results below confirm. The instrumental-variables estimates, which rescale the intent-to-treat effects by realized digitization, are about sixty percent larger (roughly $31,000$; columns 2 and 4)—as expected, since not every estate in a treated district is digitized on impact, so the intent-to-treat estimate understates the effect of actual digitization. The first stage is strong, with Kleibergen–Paap F-statistics of 56 to 60, above conventional weak-instrument thresholds. Disbursed credit moves in parallel, rising by PKR 5.3 billion in the stacked specification (column 1) and PKR 7.1 billion in the unstacked panel (column 3), in each case roughly three and two times the respective sample means (PKR 1.8 and 3.65 billion). The event study shows parallel pre-trends followed by a gradual post-rollout increase that builds over several years—the dynamic signature of a reform that operates through the slow accumulation of verifiable titles rather than a one-time shock.

The expansion is concentrated at the bottom of the distribution. Table 2 first decomposes the borrower response by sector. The number of borrowers rises in both the farm sector (5,484; $p < 0.05$) and the non-farm sector (14,596; $p < 0.01$), and disbursed amounts rise in both—so the credit expansion is broad-based rather than confined to one part of the rural economy, even though the non-farm sector accounts for the larger share of the new borrowers. Figure 4 shows the corresponding dynamic effects by sector. Table 3 then disaggregates within each sector by landholding and enterprise class, with the number of borrowers in Panel A and the amount disbursed in Panel B. The new lending reaches precisely the borrowers most likely to have been collateral-constrained: the number of borrowers rises among *subsistence* farm holdings (5,737; $p < 0.05$) and *small* non-farm borrowers (13,887; $p < 0.01$), but is flat—economically and statistically—among economic and above-economic farm holdings[16], which

[16]Rises only marginally among large non-farm borrowers (709; economically small relative to the 14,000

were already banked. Disbursed amounts, by contrast, rise across *every* class (Panel B), indicating that established borrowers take larger loans even where no new borrowers enter. The corresponding dynamic effects are given in Figure G5 and Figure G6. The reform thus operates on the extensive margin for the smallest borrowers and on the intensive margin for larger ones.

Why credit expands. Table 4 adjudicates between competing explanations for the expansion. If digitization works by making ownership cheaply verifiable, the new credit should arrive through wider *reach* and better *screening*, not through larger loans to incumbent borrowers. The estimates bear this out. The number of borrowers per village rises by 0.03 ($p < 0.01$), nearly three times its sample mean (0.0095), while average loan size does not change (-0.39; $p = 0.15$). The recovery rate *improves* by 0.31 ($p < 0.05$), and the debt burden per outstanding borrower *falls* (-0.05; $p < 0.05$). Taken together, these results rule out a deterioration in credit quality: the reform screens in new, creditworthy borrowers against newly verifiable collateral, and repayment improves rather than worsens. This is the pattern predicted by the collateral channel, under which computerized registries expand lending by lowering the cost and risk of secured credit (Deininger and Goyal, 2012; Field, 2007; Besley and Ghatak, 2010). The same outcomes by farm and non-farm sector are in Table H24; the effect is driven mainly by non-farm borrowers and loans. The further disaggregation by farmer and landholding size within each sector is in Table H25.

5.2 Corruption

Digitization raises the detection of corruption in the land bureaucracy. Table 5 reports the effect of digitization on anti-corruption activity in the *mouza*-by-month panel, identified off the staggered phase rollout across the region's districts—which span all three cohorts—with realized *mouza*-level digitization instrumented by the phase indicator, as in Section 4. Digitization *increases* recorded corruption incidents involving the land bureaucracy. Incidents involving the land departments (the Revenue Department, the PLRA, and Local Government) rise by 0.112 in the stacked specification (column 1; $p < 0.05$), and incidents in which the accused is land-administration personnel rise by 0.061 ($p < 0.05$). Relative to the low baseline incidence of recorded corruption (0.038 and 0.022 incidents per *mouza*-month), the effects are proportionally large—on the order of a tripling—and they are estimated with very strong first stages (Kleibergen–Paap F of 149 to 215). The dynamic effects are reported in

increase among small borrowers).

the event study (Figure 5), which shows parallel pre-trends followed by a post-rollout increase.

The increase runs through the entire enforcement funnel, including convictions. Table 6 decomposes the effect by stage of the enforcement process. Complaints, enquiries, and cases all rise, for both land departments (complaints 0.0006, enquiries 0.084, cases 0.028) and land personnel (complaints 0.0105, enquiries 0.040, cases 0.010) (all significant at the five percent level). The corresponding event studies are shown in Figure 6. Crucially, Table 7 shows that the escalation does not stop at the filing stage: the rate at which complaints convert into enquiries (0.102), enquiries into prosecuted cases (0.013), and cases into *convictions* (0.008) all rise (each $p < 0.05$), with the corresponding event study in Figure G9. That the effect propagates all the way to adjudicated convictions is central to interpretation: a mere increase in unfounded complaints would dissipate along the funnel, whereas an increase that survives investigation and judicial scrutiny reflects malfeasance that is genuinely surfaced and substantiated. Table H27 attributes the bulk of the effect to the Revenue Department, Table H28 shows that it is driven by currently employed—not retired—officials, and Table H29 shows that it spans the supporting, administrative, and officer grades of the land hierarchy.

Interpretation: accountability, not displacement. The anti-corruption records measure enforcement actions—complaints, enquiries, and cases—rather than the underlying incidence of bribery, and the natural reading of the results is that digitization made malfeasance in the land bureaucracy detectable and prosecutable. The digital audit trail renders each transaction time-stamped and attributable, so that conduct that was previously unverifiable can be documented, escalated, and punished. Three features of the evidence favor this accountability interpretation over the alternative that the reform *generated* more corruption. First, the effect is concentrated in the very function that was digitized and in the staff whose discretion was curtailed, not displaced onto unrelated departments (Table F22) — nor mirrored in crimes with no land connection (Table 10), ruling out a broader shift in law and order or reporting. Second, the effect runs through to convictions, indicating substantiated rather than spurious cases. Third, independent survey evidence on the same reform finds that routine bribery for land services *fell* (Figure F3). The most consistent account is therefore that digitization curtailed the everyday extraction of rents for routine services while simultaneously surfacing and punishing the malfeasance that remained: a transparency spillover from the registry onto the accountability of the bureaucracy that administered it.

5.3 Mechanisms

The credit and corruption results describe two consequences of the same shock—records that are clearer, centrally held, and harder to manipulate. Four further margins connect them and probe the mechanism: households confirm the credit expansion and reveal how land is reallocated; disputes move into the formal courts; the violence through which disputes were once settled declines; and the local state's tax collection—the activity that had relied on informal leverage—contracts.

Household land and labor markets. Household microdata support the lender-side credit expansion and locate the reallocation channel (Table 8). Digitization raises the probability of obtaining a formal loan by 7.3 percentage points ($p < 0.01$), about a quarter of the baseline rate (0.31)—an independent confirmation, in a separate dataset, of the lender-side expansion in Table 1. Land transactions adjust on the *rental* rather than the *ownership* margin. The area of land rented in rises by 0.60 acres ($p < 0.05$), the probability of renting out land rises by 6.7 percentage points ($p < 0.01$), and cultivated area increases by 0.86 acres ($p < 0.05$), while the probabilities of buying or selling land remain unchanged. This is the footprint of allocative reallocation through the rental market, consistent with Beg (2022), who shows that the same reform redistributed land toward more productive farmers and raised allocative efficiency.

Court cases. If clearer records reduce the ambiguity that breeds litigation, they should also channel cases into the formal system. Table 9 shows that digitization increases total land-related court cases by 19.3 per district-week ($p < 0.01$; about 6 percent of the mean of 336) and, in particular, the number of cases *resolved* through successful mediation (+12.8; $p < 0.01$; also about 6 percent of its mean), while the number of *failed* mediations is statistically unchanged. The increase spans criminal, civil, and family cases (Figure 7), with the disaggregated case-type trends in Figure G10. Rather than a rise in conflict, this pattern indicates the *formalization* of dispute resolution. With verifiable titles, parties bring claims into the formal system and resolve them there, complementing Ullah and Hussain (2023). The same loss of discretionary control that surfaces corruption also moves contested claims out of informal channels and into adjudication.

Dispute violence. If verifiable records channel contested claims into the courts, they should also displace the violent resolution of disputes, which in rural Punjab often escalates into the most severe offense categories. Table 10 confirms this. Digitization reduces recorded murders by 0.69 per 100,000 ($p < 0.05$; about 11 percent of the sample mean) and attempted murders by 1.29 per 100,000 ($p < 0.01$; about 18 percent), with the aggregate of dispute-sensitive violent crime falling by roughly 7 percent. Two features tie the decline to the land-records channel rather than to policing or reporting. First, crimes with no land connection—motor-vehicle theft, burglary, and ordinary theft—are unchanged, individually and in aggregate, ruling out both a general law-and-order improvement and a fall in crime reporting. Second, the broad "hurt" category, most of which is unrelated to land, shows no effect, consistent with dilution rather than a uniform decline in violence. Together with the court-case results, the pattern describes a single substitution. Once an authoritative record exists, adjudication replaces violence as the technology for resolving land disputes.

The registry as program infrastructure. A decade after the reform, the digitized registry has become reusable infrastructure for state programs. Punjab's flagship farm-support scheme, the Kissan Card, uses digitized records as its backbone for eligibility and collateral. Applicants are "verified digitally" through an interface with PLRA records, with "on-line charge creation and verification of the land records," while farmers in the few remaining non-digitized *mouzas* must apply manually. Eligibility is means-tested at the 12.5-acre subsistence threshold—precisely the landholding class in which the credit expansion of Table 3 is concentrated. Administrative records on 37,095 recipients in four districts show that 99.6 percent hold at most 12.5 acres, and the distribution of recorded landholding displays no excess mass below the threshold (Figure 8). Landholding records are no longer shaded to manufacture eligibility, a form of manipulation that was routine when a single official controlled the register. The small residual of over-threshold cards (0.42 percent), attributed by program officials to discretionary exceptions, illustrates the same lesson as the corruption results.

The cost of digitization. The reform's spillovers are not uniformly favorable. Table H26 shows that digitization sharply reduced agricultural tax collection. The share of demand collected falls by 35.4 percentage points ($p < 0.01$)—roughly sixty percent of the pre-reform collection rate of 58 percent—the shares of districts collecting at least half and at least three-quarters of demand both fall, and the share of months with zero collection rises. The decline operates through both assessment and collection. Assessed cultivated area falls by about 10 percent and issued tax demand by about 45 percent (-0.10 and -0.60 in logs; both $p < 0.01$). This is the cost side of the same mechanism. Before the reform, the local staff used their control over land records as informal leverage to enforce tax payment—offering to process titles or resolve disputes in exchange for compliance. Once that control was centralized and formalized, the leverage disappeared and collection fell. The reform that expanded credit, strengthened accountability, formalized disputes, and reduced dispute violence thus also eroded a form of fiscal capacity that had rested on informal authority (Aman-Rana and Minaudier, 2026), and a full evaluation of the reform must weigh these opposing spillovers against one another.

5.4 Robustness

Outcome transformation. All headline effects survive transforming the outcomes by $\log(1 + y)$ and the inverse hyperbolic sine, which accommodate the many zeros in the count and household outcomes while preserving a semi-elasticity interpretation (Chen and Roth, 2024). This holds for the main credit and corruption outcomes (Table F11 and Table F16), for the disaggregated credit and corruption outcomes (Table F12 and Table F17), and for the court-case outcomes (Table F18).

Alternative specifications. Although the stacked estimator is my preferred design, the unstacked panel shows quantitatively similar disaggregated results, for both credit (Table F13) and corruption (Table F19).

Poisson. Given the many zeros in the corruption panel, I aggregate the data to the tehsil level and estimate Poisson regressions on both the stacked and unstacked panels (Table F21); the results are consistent in direction and significance with the linear estimates.

Heterogeneity-robust estimators. Because the two-way fixed-effects estimator can be biased under staggered adoption with heterogeneous treatment effects, I re-estimate the headline credit and corruption effects using estimators designed for this setting—Callaway and Sant'Anna (2021), Gardner (2022), and Borusyak et al. (2024) (Table F14 and Table F20). The estimates are consistent with my preferred stacked specification, confirming that the results are not artifacts of negative weighting. The corresponding Callaway and Sant'Anna (2021) event studies are shown for credit in Figure G4 and for corruption in Figure G8. Further, leaving-one-cohort-out and alternative-sample re-estimation show that no single phase cohort drives the findings (Table F15).

Placebo. Three falsification tests confirm that the results reflect the digitized function rather than contemporaneous shocks. First, the corruption effect is confined to the land bureaucracy. A placebo on non-land departments shows no effect (Table F22). Second, crimes with no land connection—motor-vehicle theft, burglary, and ordinary theft—do not respond to digitization (Table 10), ruling out a broader shift in law and order or in reporting behavior.

Third, digitization is not confounded by concurrent public investment. Because provincial development budgets are allocated centrally rather than by the local land bureaucracy, digitization should not move a district's development resources, and Table F23 confirms that it has no effect on the number of schemes, on total allocations or releases, or on spending in any individual sector (agriculture, education, health, irrigation, roads, and other sectors).

OLS. Finally, I report ordinary-least-squares regressions of the main credit and corruption outcomes on the share of villages digitized, using both the stacked and unstacked panels (Section I). For agricultural credit, the stacked and unstacked estimates by farm and non-farm sector are in Table I30 and Table I32, and by farmer and landholding size in Table I31 and Table I33; for corruption, the stacked and unstacked estimates are in Table I34 and Table I35.

6 Discussion

Weighing the spillovers. Digitization expanded formal credit and strengthened the detection of corruption while eroding the local state's capacity to collect agricultural tax, and a complete evaluation must net these against one another. On the cost side, the Punjab Annual Development Programme records a final approved project cost of about PKR 12.3 billion (roughly US$117 million, or PKR 342 million per district), a figure that closely matches the World Bank's project completion report.[17] To this one-time cost the reform adds the recurring operation of the 151 service centers and a province-wide fall in agricultural tax collection of about PKR 0.26 billion per year, roughly PKR 7.2 million per district—enough to fund cash transfers for several thousand families under the country's main social-protection program. Against these, the benefits are large. Digitization raised agricultural credit disbursement by roughly PKR 5.3 billion per treated district (Table 1), reaching precisely the subsistence and small borrowers whose marginal value of credit is plausibly highest and who were previously rationed (Deininger and Goyal, 2012); since the credit has to be paid back, even a conservative social return on relaxing these constraints recovers the one-time program cost within a few years. The share of households paying bribes for land records fell

[17] The project spanned 2009–2016. At exchange rates around its completion (approximately 105 PKR per USD), the ADP figure of PKR 12.3 billion corresponds to roughly US$117 million, in line with the completion report's total final cost of US$114.7 million. Major components include US$34.80 million for system development, US$9.20 million for service delivery, US$5.40 million for project management, and US$1.90 million for business-process improvement, with the remainder covering the establishment of service centers and associated infrastructure. Available at https://documents.worldbank.org/en/publication/documents-reports/documentdetail/488611499886532325.

from a large majority before the reform (Gallup Pakistan, 2009) to about 2 percent (Apex Consulting Pakistan, 2016), a saving that is substantial once applied to the millions of transactions processed annually. Further, the precision in providing loans in future (as evident by Kissan Cards data) add value. Adding the unmonetized gains from faster dispute resolution, an eleven percent decline in murders associated with land disputes, stronger corruption detection, and a more active land rental market (Beg, 2022), the credit and transaction-cost benefits alone plausibly exceed both the program cost and the fiscal loss—the latter an order of magnitude smaller and partly a transfer rather than a deadweight loss.

What the corruption result does and does not establish. Because the anti-corruption data record enforcement actions rather than the underlying incidence of bribery, my estimates identify a change in the *detectability and prosecution* of malfeasance, not necessarily in its level (Erlich et al., 2025). Three features indicate that this reflects accountability rather than a genuine rise in corruption. The effect is specific to the digitized function—placebos on non-land departments and on crimes with no land connection are both null; it survives all the way to convictions rather than dissipating as unfounded complaints; and independent survey evidence on the same reform finds that routine bribery for land services *fell*. The most parsimonious reading is that digitization simultaneously curtailed everyday extraction and surfaced the malfeasance that remained. I cannot, however, fully rule out that some recorded incidents reflect conduct displaced onto adjacent tasks; classifying case narratives by whether the alleged conduct predates or postdates digitization would sharpen this distinction.

External validity. The spillovers I document follow from a particular institutional configuration rather than from digitization *per se*. A single record served many functions at once—collateral, legal evidence, and tax base—and control over it was concentrated in one local official whose discretion was the common source of the frictions the reform relieved and the leverage it removed. Where these conditions hold—as they do for land administration across much of South Asia and Sub-Saharan Africa—digitization should generate spillovers of the kind I find; where the functions are already separated across agencies or enforcement is already formalized, the same reform should have muted cross-domain effects. This scope condition reconciles my findings with the mixed results of the broader digitization literature (Addo and Senyo, 2020).

Policy implications. The results caution against evaluating—or designing—digitization reforms one function at a time. Two complementarities stand out. First, digitization raised the *detection* of corruption, but converting detection into deterrence requires investigative and judicial capacity to act on the audit trail; technology and monitoring are complements, not substitutes. Second, because the reform stripped the local staff of the informal leverage they had used to enforce tax payment, sustaining fiscal capacity requires building formal enforcement to replace what was lost. More broadly, the returns to digitizing the state depend not only on the function digitized but on the web of private and public decisions the records support, and reforms that ignore these linkages may capture only part of the available gains—or pay an unanticipated cost.

Limitations. Several caveats bound these conclusions. The cost–benefit comparison above is deliberately coarse. Credit *disbursed* is a gross flow rather than a welfare gain and so anchors only an upper bound. Several first-order benefits (reduced corruption, dispute resolution, fewer violent deaths, allocative efficiency) and some costs (service-center operations, transitional disruption) are unmonetized. The foregone tax is partly redistributive, and the cost and benefit figures come from different panels and identification strategies rather than a single structural model. Empirically, the corruption panel covers nine districts,[18] unlike the nationwide credit results, it should be read as internally valid for that region rather than as a country-wide estimate. The court-case panel begins after the rollout, so its effects are identified from cross-district variation in digitization intensity and are best read as suggestive. And because police crime categories are not tagged as land-related, the dispute-violence estimates are attenuated by unrelated incidents and understate the effect on land-related violence. However, most crimes are caused by land disputes in rural Pakistan. Assigning explicit welfare weights to credit access, corruption deterrence, and public revenue—and distinguishing the detection of corruption from its incidence with case-level evidence—are left to future work.

7 Conclusion

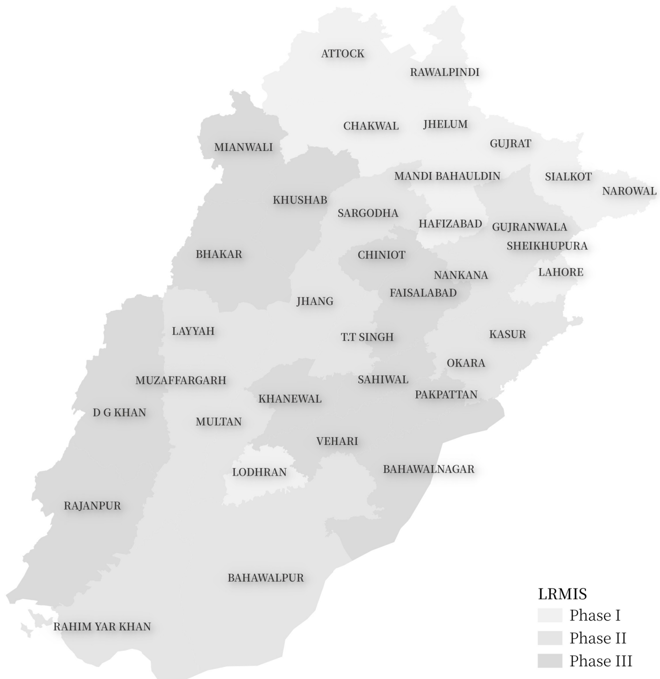
This paper traces the spillovers of a property-rights reform across the markets and institutions that rely on those records. Exploiting the staggered rollout of digitization across the districts, I show that the reform expanded formal agricultural credit—disproportionately for the smallest, previously collateral-constrained borrowers—and increased the detection and prosecution of corruption within the land bureaucracy. These outcomes, together with the household, dispute-resolution, and violence evidence—land cases moved into the formal courts and were resolved there, and the murders through which disputes had once been

[18]The estimation nonetheless has 1,287 clusters and variation across all three phases, so it is internally valid and powered.

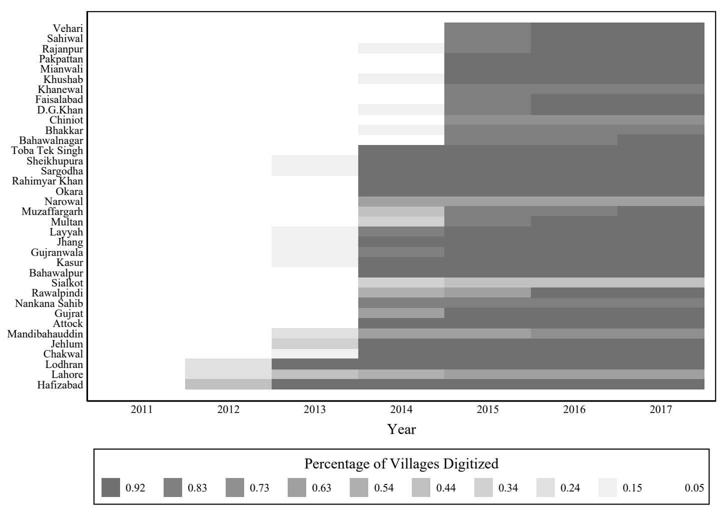
settled declined—stem from a common mechanism. That is, digitization replaced the discretionary, informal authority that a single official had exercised over a multipurpose record with a transparent and formal system. Where formalization complemented the activity, outcomes expanded; where the activity had depended on the official's informal leverage, it contracted.

The central lesson is that the consequences of digitizing the state cannot be read off the function being digitized. A reform confined, on paper, to a record-keeping system reshaped credit access, bureaucratic accountability, the resolution of land disputes and the violence surrounding them, and fiscal capacity at once, and its net value turns on the full set of spillovers it sets in motion. In this setting, a rough accounting suggests the balance is favorable. The credit and transaction-cost gains alone plausibly exceed the program's cost and the fiscal loss it induced. Quantifying that net value with explicit welfare weights, distinguishing the detection of corruption from its incidence through case-level evidence, and tracing the credit expansion through to investment and productivity are the natural extensions of this work.

Figure 1: Rollout of the Digitization



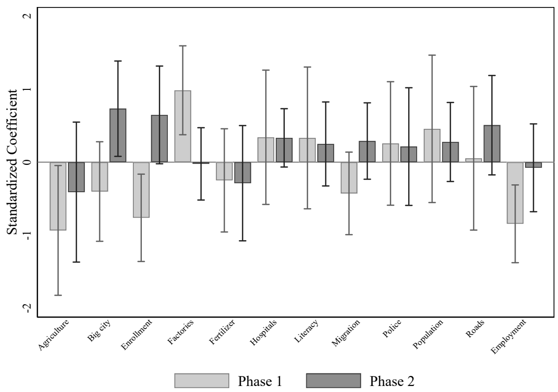
(a) Phase-in Implementation of Digitization



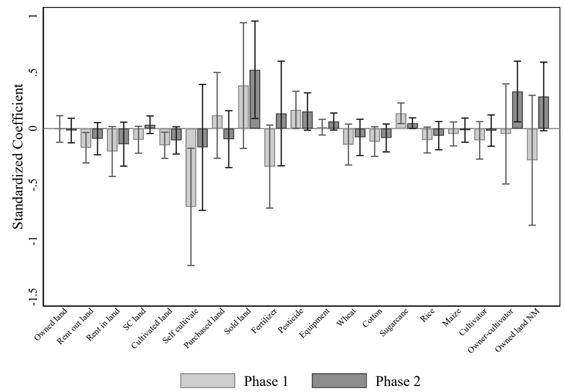
(b) Staggered Rollout of Digitization

Notes: This figure represents the rollout of the digitization reform. Panel (a) of shows the geographical distribution of districts across the three phases of land records digitization. Panel (b) shows the share of villages digitized across districts and years. Districts scheduled for digitization in Phase 1 include Lahore, Lodhran, Hafizabad, Mandi Bahauddin, Nankana Sahib, Jhelum, Gujrat, Sialkot, Chakwal, Attock, and Rawalpindi. Phase 2 districts comprise Bahawalpur, Gujranwala, Jhang, Layyah, Kasur, Multan, Muzaffargarh, Narowal, Okara, Rahim Yar Khan, Sargodha, Sheikhupura, and Toba Tek Singh. Phase 3 districts consist of Bahawalnagar, Bhakkar, Chiniot, Dera Ghazi Khan, Faisalabad, Mianwali, Khanewal, Khushab, Pakpattan, Rajanpur, Sahiwal, and Vehari.

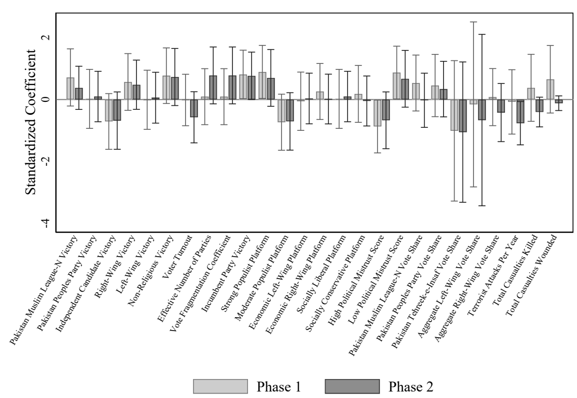
Figure 2: Baseline Balance, Phases



(a) Institutional Characteristics



(b) Socioeconomic Characteristics



(c) Political Characteristics

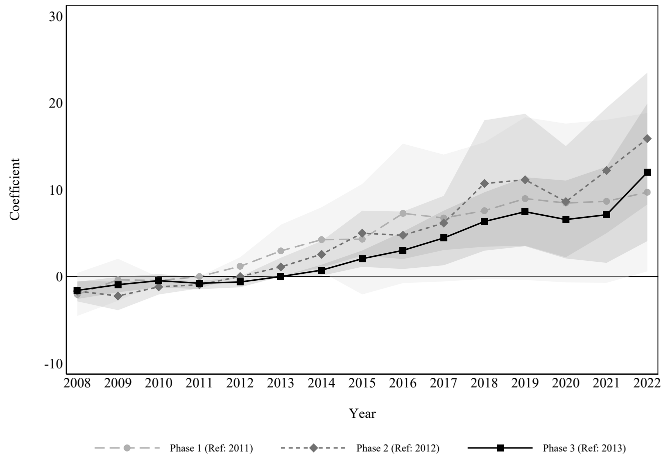
Notes: This figure breaks down the baseline characteristics balance by plotting separate treatment phase effects relative to the Phase 3 baseline group using pre-digitization data. For each covariate, two distinct regressions are estimated. Phase 1 vs. Phase 3 (omitting Phase 2) represented by the light gray bars, and Phase 2 vs. Phase 3 (omitting Phase 1) represented by the dark gray bars. All dependent variables are standardized to have a mean of zero and unit variance. Vertical caps denote 95% confidence intervals based on standard errors clustered at the district level.

Table 1: Impact of Digitization on Agricultural Credit

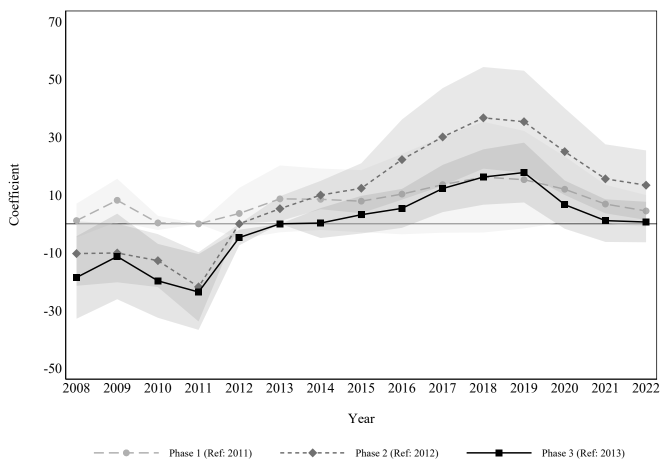
	Stacked		Unstacked	
	DID	IV	DID	IV
	(1)	(2)	(3)	(4)
Panel A: Borrowers				
Digitization	19.3017	30.8522	19.6915	31.0402
	(3.6543)	(5.0459)	(3.6681)	(5.5883)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Mean of dep. var.	8.54	8.54	13.28	13.28
SD of dep. var.	19.49	19.49	26.03	26.03
District FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
District-Cohort FE	Yes	Yes	No	No
Year-Cohort FE	Yes	Yes	No	No
Kleibergen-Paap first-stage F		60.22		55.91
Observations	4,110	4,110	2,235	2,235
Panel B: Disbursement				
Digitization	5.3256	7.6959	7.1461	10.1523
	(1.0172)	(1.2463)	(1.4494)	(1.8239)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Mean of dep. var.	1.80	1.80	3.65	3.65
SD of dep. var.	5.60	5.60	9.60	9.60
District FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
District-Cohort FE	Yes	Yes	No	No
Year-Cohort FE	Yes	Yes	No	No
Kleibergen-Paap first-stage F		60.22		55.91
Observations	4,110	4,110	2,235	2,235

Notes: This table reports estimated effects of LRMIS digitization on agricultural credit, with both outcomes measured in levels. Panel A reports results for the total number of borrowers and Panel B reports results for total disbursed credit (PKR billions). Each cell reports either the reduced-form difference-in-differences coefficient or the instrumented share of villages digitized. Digitization begins in Phase 1 (2012), Phase 2 (2013), or Phase 3 (2014) for Punjab's 36 districts; all non-Punjab districts serve as never-treated controls. Columns (1) and (2) report results from the stacked data, incorporating cohort-interacted district and year fixed effects. Columns (3) and (4) report results from a standard two-way fixed-effects estimator on the full unstacked panel. Columns (1) and (3) are estimated by difference-in-differences, and columns (2) and (4) are two-stage least squares estimates. The Kleibergen-Paap row reports the first-stage Wald statistic. The Mean and SD of the dependent variable are computed on the relevant regression sample, in levels. Standard errors clustered at the unit level (district in unstacked, cohort-district in stacked) are in parentheses; p-values are in square brackets.

Figure 3: Dynamic Impact of Digitization on Agricultural Credit, Event Study



(a) Disbursement



(b) Borrowers

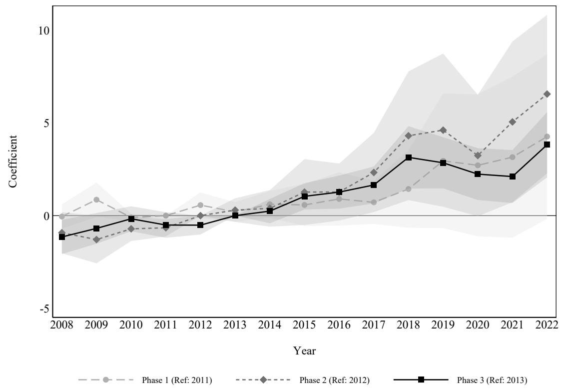
Notes: This figure plots the dynamic treatment effect estimates and corresponding 95% confidence intervals for the impact of digitization by every phase. The model includes district and year fixed effects with standard errors clustered at the district level.

Table 2: Impact of Digitization on Agricultural Credit by Sector

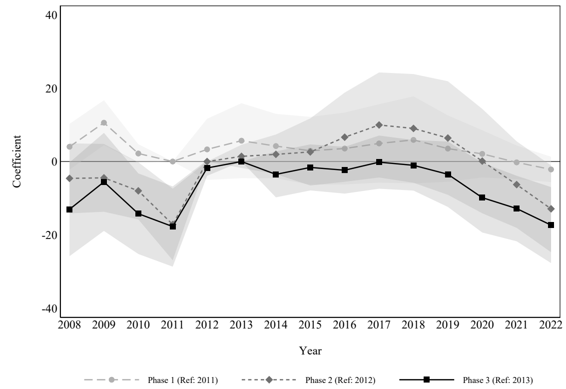
	Number of Borrowers		Disbursed Amount	
	Farm Sector (1)	Non-Farm Sector (2)	Farm Sector (3)	Non-Farm Sector (4)
Digitization	5.4840	14.5956	1.7508	3.3278
	(2.6432)	(1.9271)	(0.3673)	(0.7345)
	[0.0386]	[0.0000]	[0.0000]	[0.0000]
Mean of dep. var.	6.47	2.04	1.13	0.62
SD of dep. var.	15.06	5.89	2.72	3.04
District-Cohort FE	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes
Observations	4,110	4,110	4,110	4,110

Notes: This table reports stacked difference-in-differences estimates evaluating the effect of land record digitization on formal credit markets across sectors. Columns (1) and (2) measure formal credit market participation through the total number of borrowers in thousands in the agricultural (farm) and agricultural (non-farm) sectors, respectively. Columns (3) and (4) report credit volumes through total disbursed loan values in PKR billion. All outcomes are evaluated in level units. All specifications include district-cohort and year-cohort fixed effects. The Mean and SD of the dependent variable are computed on the active regression sample. Standard errors clustered at the cohort-district level are reported in parentheses; p-values are in square brackets.

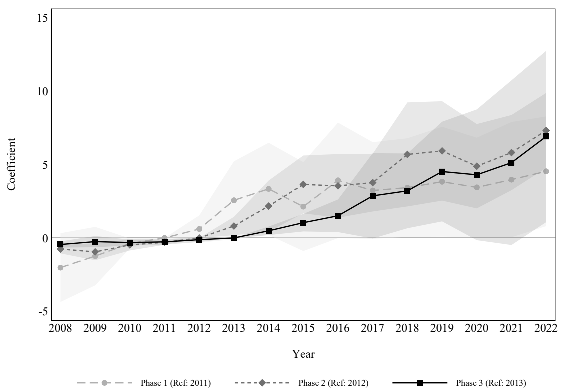
Figure 4: Dynamic Impact of Digitization on Agricultural Credit by Sector, Event Study



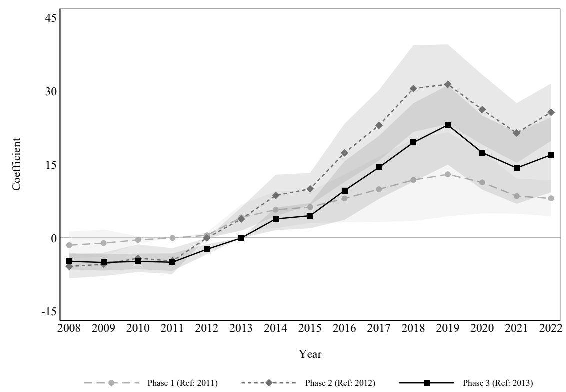
(a) Disbursement, Farm Sector



(b) Borrowers, Farm Sector



(c) Disbursement, Non-Farm Sector



(d) Borrowers, Non-Farm Sector

Notes: This figure plots the dynamic treatment effect estimates and corresponding 95% confidence intervals for the impact of digitization by every phase. The model includes district and year fixed effects with standard errors clustered at the district level.

Table 3: Impact of Digitization on Agricultural Credit by Farmer Size

	Farm Loans			Non-Farm Loans	
	(1)	(2)	(3)	(4)	(5)
	Subsistence Holding	Economic Holding	Above Economic Holding	Small Farmer	Large Farmer
Panel A. Borrowers					
Digitization	5.7372	-0.1744	-0.0788	13.8871	0.7086
	(2.6931)	(0.1729)	(0.0548)	(1.8614)	(0.1359)
	[0.0337]	[0.3136]	[0.1513]	[0.0000]	[0.0000]
Dep. var. mean	5.752	0.610	0.109	1.970	0.071
Dep. var. SD	13.922	1.219	0.267	5.678	0.322
District-Cohort FE	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes
Observations	4,110	4,110	4,110	4,110	4,110
Panel B. Disbursement					
Digitization	0.7169	0.3780	0.6559	1.6095	1.7182
	(0.2286)	(0.0546)	(0.2169)	(0.2004)	(0.6381)
	[0.0018]	[0.0000]	[0.0026]	[0.0000]	[0.0074]
Dep. var. mean	0.594	0.275	0.262	0.219	0.402
Dep. var. SD	1.319	0.561	1.353	0.652	2.704
District-Cohort FE	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes
Observations	4,110	4,110	4,110	4,110	4,110

Notes: This table reports stacked difference-in-differences estimates evaluating the effect of digitization on formal credit markets across disaggregated landholding classes and farmer types. Columns (1) to (3) restrict the sample to agricultural (farm) loans, subdivided into subsistence, economic, and above-economic landholding classes. Columns (4) and (5) isolate agricultural (non-farm) loans, partitioned into small and large borrowers. All dependent variables are evaluated in level units. All models are estimated using the stacked difference-in-differences estimator, incorporating cohort-interacted district and year fixed effects. Standard errors clustered at the cohort-district level are reported in parentheses; p-values are in square brackets.

Table 4: How Digitization Increases Agricultural Credit

	Intensity (Average Loan Size) (1)	Reach (Borrowers Per Village) (2)	Efficiency (Recovery Rate) (3)	Debt Burden (Outstanding Borrowers) (4)
Digitization	-0.3928	0.0300	0.3072	-0.0516
	(0.2731)	(0.0057)	(0.1000)	(0.0220)
	[0.1512]	[0.0000]	[0.0023]	[0.0197]
Mean of dep. var.	0.4935	0.0095	0.9384	0.0484
SD of dep. var.	4.5354	0.0280	2.4598	0.3550
District-Cohort FE	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes
Observations	4,110	4,110	4,110	4,110

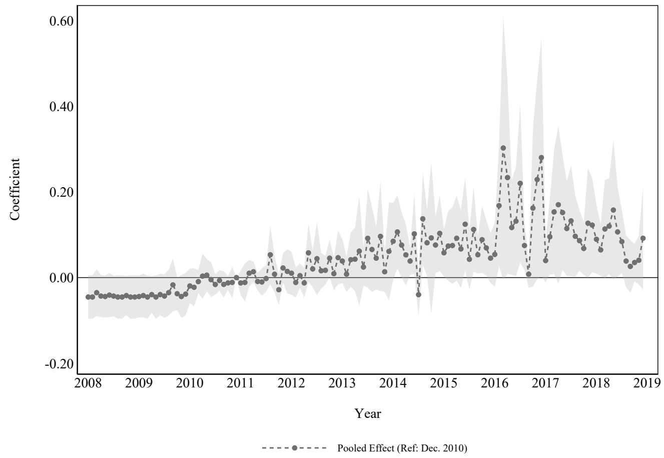
Notes: The table reports stacked difference-in-differences estimates evaluating the effect of digitization across overall credit lines. Column (1) reports credit intensity (average loan size), Column (2) reports extensive market reach (total active unique borrowers divided by local village counts), Column (3) captures banking operational efficiency (repayment recovery rate), and Column (4) isolates borrower portfolio depth (outstanding values per borrower balance). All models utilize the stacked difference-in-differences estimator with cohort-interacted district and year fixed effects. Standard errors clustered at the cohort-district level are reported in parentheses; p-values are in square brackets.

Table 5: Impact of Digitization on Land Related Corruption

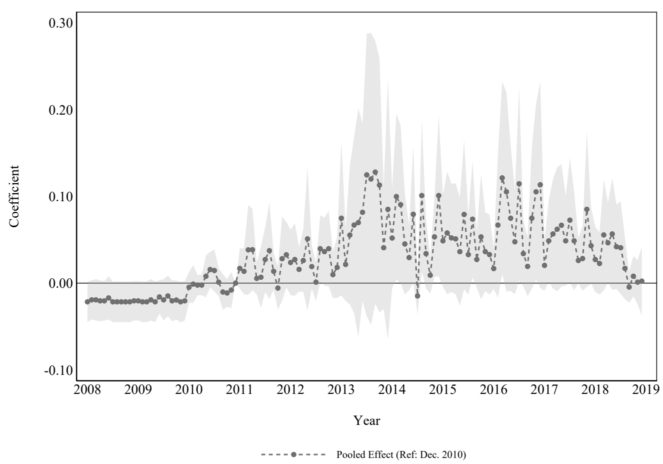
	Stacked		Unstacked	
	DID	IV	DID	IV
	(1)	(2)	(3)	(4)
Panel A. Land Departments				
Digitization	0.1121	0.7897	0.1042	0.3267
	(0.0491)	(0.3531)	(0.0521)	(0.1654)
	[0.0225]	[0.0254]	[0.0458]	[0.0485]
Mean of dep. var.	0.0382	0.0382	0.0411	0.0411
SD of dep. var.	1.0764	1.0764	1.0883	1.0883
Mouza-Cohort FE	Yes	Yes	No	No
Year-Month-Cohort FE	Yes	Yes	No	No
Mouza FE	No	No	Yes	Yes
Year-Month FE	No	No	Yes	Yes
Kleibergen-Paap first-stage F		148.61		214.58
Observations	724,320	724,320	370,656	370,656
Panel B. Land Personnel				
Digitization	0.0609	0.4292	0.0814	0.2552
	(0.0263)	(0.1895)	(0.0423)	(0.1342)
	[0.0207]	[0.0236]	[0.0547]	[0.0575]
Mean of dep. var.	0.0217	0.0217	0.0234	0.0234
SD of dep. var.	0.6089	0.6089	0.6154	0.6154
Mouza-Cohort FE	Yes	Yes	No	No
Year-Month-Cohort FE	Yes	Yes	No	No
Mouza FE	No	No	Yes	Yes
Year-Month FE	No	No	Yes	Yes
Kleibergen-Paap first-stage F		148.61		214.58
Observations	724,320	724,320	370,656	370,656

Notes: This table reports the effect of digitization on anti-corruption activity recorded by the Punjab Anti-Corruption Establishment. Panel A reports level specifications for any land-related incident, including complaints, enquiries, and formal cases involving the land-related departments, while Panel B isolates incidents where the primary accused individual is a land-related or equivalent designation. Columns (1) and (2) report results from a stacked dataset. Columns (3) and (4) report standard two-way fixed effects estimates on the full unstacked panel. For stacked data, cohort interacted with mouza and year-month fixed effects are used, whereas for unstacked data, mouza and year-month fixed effects are applied. The Kleibergen-Paap is the first-stage Wald F-statistic. Means and standard deviations are calculated on the active regression sample in levels. Standard errors reported in parentheses are clustered at the cohort-mouza for stacked and mouza for unstacked, with corresponding p-values in square brackets.

Figure 5: Dynamic Impact of Digitization on Land Related Corruption, Event Study



(a) Incidents Land Departments



(b) Incidents Land Personnel

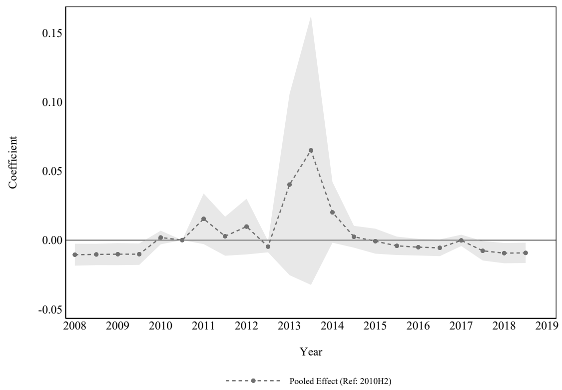
Notes: This figure plots the dynamic treatment effect estimates and corresponding 95% confidence intervals for the impact of digitization. The model includes mouza and month-year fixed effects with standard errors clustered at the mouza level.

Table 6: Impact of Digitization on Land Related Corruption by Categories

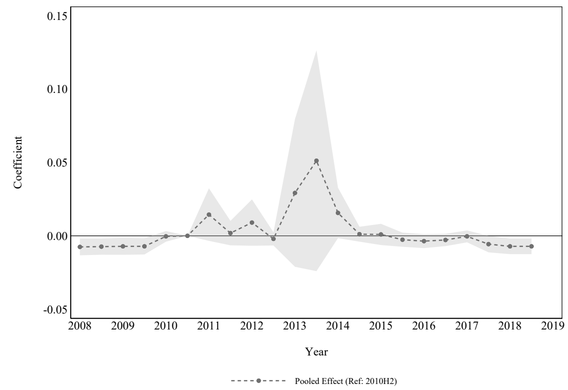
	Land Departments			Land Personnel		
	Complaints (1)	Enquiries (2)	Cases (3)	Complaints (4)	Enquiries (5)	Cases (6)
Digitization	0.0006	0.0838	0.0277	0.0105	0.0400	0.0104
	(0.0003)	(0.0379)	(0.0115)	(0.0053)	(0.0177)	(0.0043)
	[0.0355]	[0.0271]	[0.0159]	[0.0459]	[0.0241]	[0.0146]
Mean of dep. var.	0.0002	0.0273	0.0107	0.0051	0.0127	0.0039
SD of dep. var.	0.0212	0.8287	0.3844	0.2413	0.3898	0.1422
Mouza-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	724,320	724,320	724,320	724,320	724,320	724,320

Notes: This table shows the estimated effects of digitization on anti-corruption activity recorded by the Punjab Anti-Corruption Establishment. Columns (1) through (3) track administrative processing volumes for land departments, split into total complaints filed, formal enquiries initiated, and cases registered. Columns (4) through (6) evaluate the same structural progression tracking incidents where the primary target is land-related personnel. All outcomes are evaluated in original level metrics. Estimation is a stacked difference-in-differences with cohort-interacted mouza and year-month fixed effects. Means and standard deviations correspond to active regression samples. Standard errors in parentheses are clustered at the cohort-mouza level, with p-values provided in square brackets.

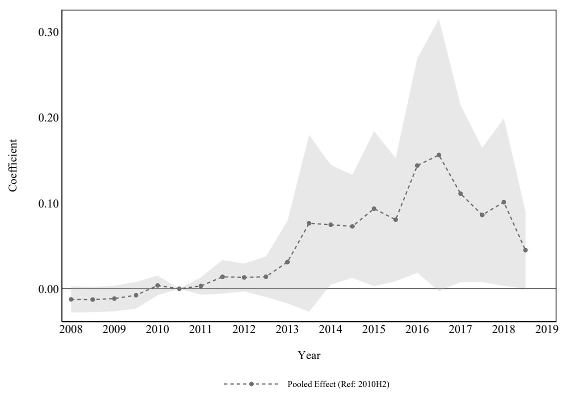
Figure 6: Dynamic Impact of Digitization on Land Related Enquiries and Cases, Event Study



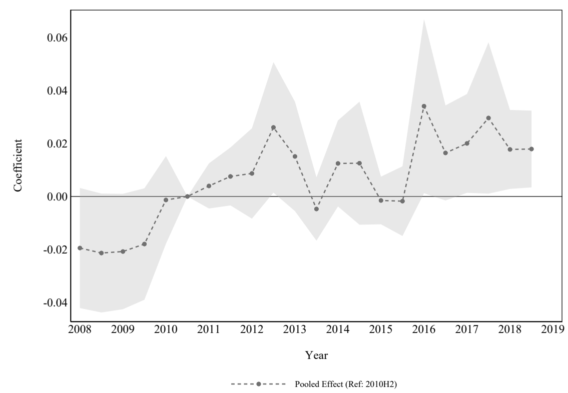
(a) Enquiries Land Departments



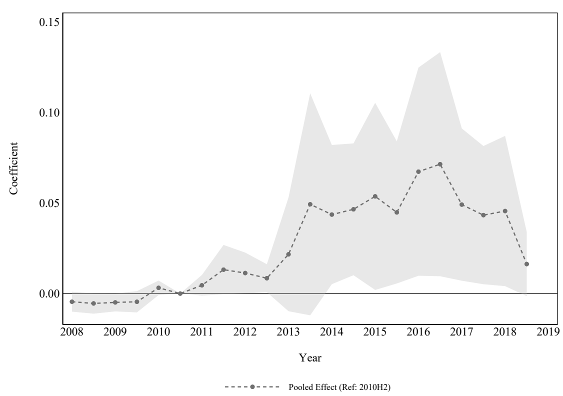
(b) Enquiries Land Departments



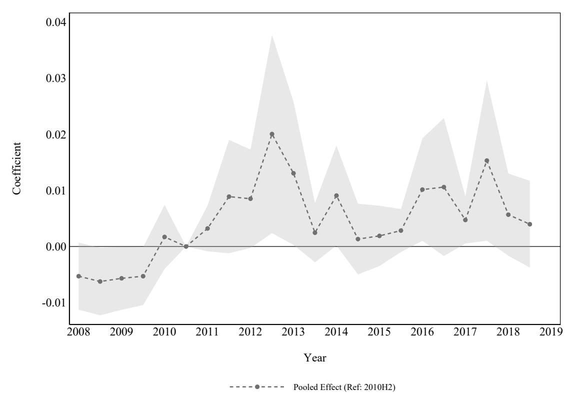
(c) Enquiries Land Departments



(d) Cases Land Personnel



(e) Enquiries Land Personnel



(f) Cases Land Personnel

Notes: This figure plots the dynamic treatment effect estimates and corresponding 95% confidence intervals for the impact of digitization. The model includes mouza and semi-annual fixed effects with standard errors clustered at the mouza level.

Table 7: Impact of Digitization on Corruption Incident Conversion

	Complaints to Enquiries (1)	Enquiries to Cases (2)	Cases Convicted (3)
Digitization	0.1023	0.0130	0.0083
	(0.0502)	(0.0062)	(0.0038)
	[0.0416]	[0.0370]	[0.0283]
Mean of dep. var.	0.0341	0.0060	0.0040
SD of dep. var.	1.1290	0.1844	0.1183
Mouza-Cohort FE	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes
Observations	724,320	724,320	724,320

Notes: This table shows the estimated effects of digitization on anti-corruption activity recorded by the Punjab Anti-Corruption Establishment. Columns (1) and (2) measure the number of complaints converted into enquiries and the number of enquiries converted into cases, respectively. Column (3) tracks the number of cases convicted. All outcomes are evaluated in original level metrics. Estimation is a stacked difference-in-differences with cohort-interacted mouza and year-month fixed effects. Means and standard deviations correspond directly to active regression samples. Standard errors in parentheses are clustered at the cohort-mouza level, with p-values provided in square brackets.

Table 8: Impact of Digitization on Farmers

	Loans (1)	Sold Land (2)	Purchased Land (3)	Land Rent Out (4)	Land Rent In (5)	Cultivated Area (6)
Digitization	0.0729	-0.0022	-0.0013	0.0671	0.5963	0.8623
	(0.0201)	(0.0058)	(0.0022)	(0.0229)	(0.2720)	(0.4110)
	[0.0010]	[0.7012]	[0.5575]	[0.0061]	[0.0355]	[0.0436]
Mean of dep. var.	0.3056	0.0100	0.0038	0.2075	1.5794	6.3482
SD of dep. var.	0.4607	0.0995	0.0613	0.4055	5.8401	8.2577
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	34	34	34	34	34	34
Observations	19,067	7,702	7,702	7,597	7,256	7,256
Sample	Full	Full	Full	Landowners	Cultivators	Cultivators

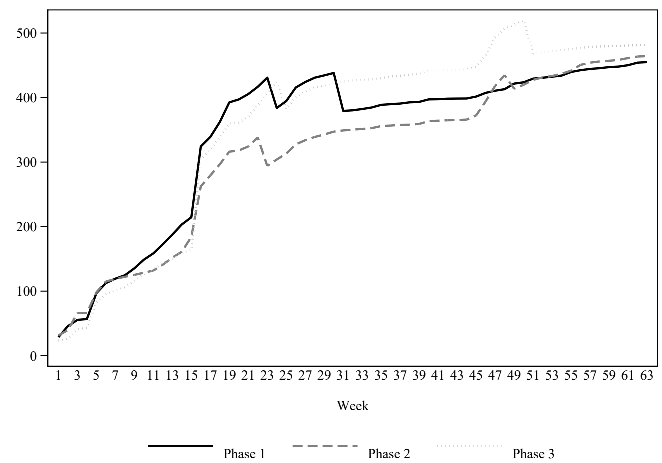
Notes: This table reports difference-in-differences estimates evaluating the effect of land record digitization on credit access, land transaction dynamics, and farm configurations using individual-level data from Punjab. Columns (1) through (4) use binary dependent variables. Columns (5) and (6) measure continuous land sizes in acres. All specifications control for household head characteristics (age, gender, and completed education categories). Standard errors clustered at the district level are reported in parentheses; p-values are in square brackets.

Table 9: Impact of Digitization on Land Related Court Cases

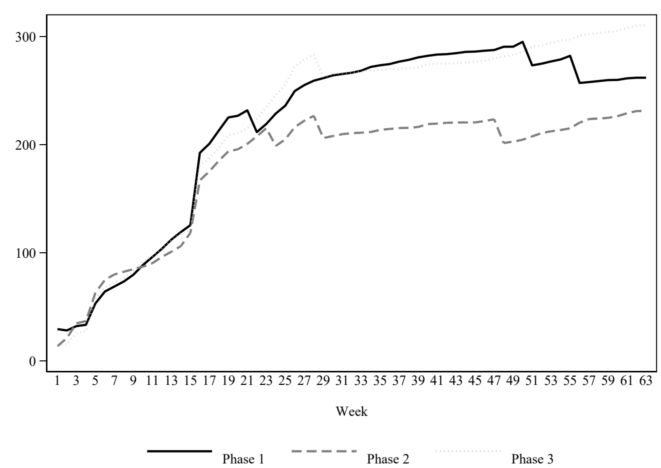
	(1) Total Cases	(2) Criminal Cases	(3) Civil Cases	(4) Family Cases	(5) Guardian Cases	(6) Rent Cases	(7) Other Cases	(8) Cases Failed	(9) Cases Resolved
Digitization	19.3156	12.2033	6.5845	13.6362	0.7811	0.0361	2.0511	1.4622	12.8098
	(3.5268)	(2.3117)	(1.8581)	(3.7604)	(0.3071)	(0.1312)	(0.5321)	(0.8901)	(2.1863)
	[0.0000]	[0.0000]	[0.0011]	[0.0009]	[0.0155]	[0.7847]	[0.0005]	[0.1094]	[0.0000]
Mean of dep. var.	336.4061	106.4815	107.7341	168.1596	10.1750	2.2730	20.6795	56.9599	204.9854
SD of dep. var.	211.7022	108.0144	88.7266	160.2806	16.2837	4.2666	27.0435	44.1432	119.9669
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,268	2,268	2,268	2,268	2,268	2,268	2,268	2,268	2,268

Notes: This table presents the continuous difference-in-differences estimated in level counts. Considering the data limitations (weekly 2017-2018 panel), the independent variable is the interaction between the percentage of villages digitized and the early treatment dummy. Columns (1) through (7) map baseline legal classifications, while Columns (8) and (9) isolate failed and successful mediation resolutions. All specifications include demographic/infrastructure controls, district fixed effects, and weekly time counter fixed effects. Mean and standard deviation of the dependent variable are calculated directly from the regression sample. Standard errors clustered at the district level are reported in parentheses; p-values are in square brackets.

Figure 7: Trend in Land Related Court Cases



(a) Cases Registered



(b) Success Cases

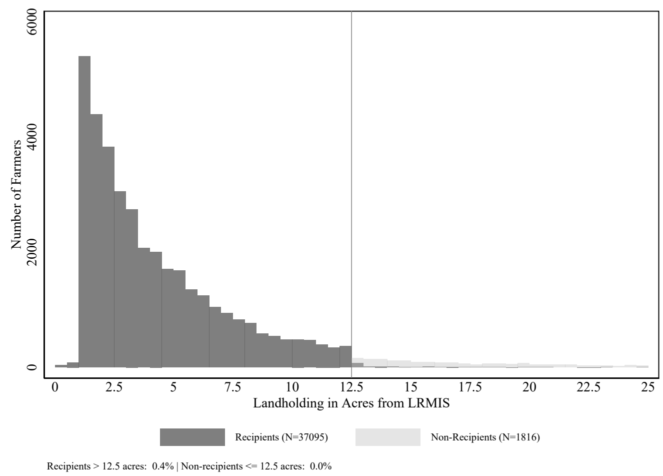
Notes: This figure presents the trend of land-related cases in the high court using Lahore High Court land cases data. Panel (a) shows the total number of land-related cases registered, and Panel (b) shows the number of land-related cases resolved. The x-axis shows the week number, starting from 2017.

Table 10: Impact of Digitization on Recorded Crime

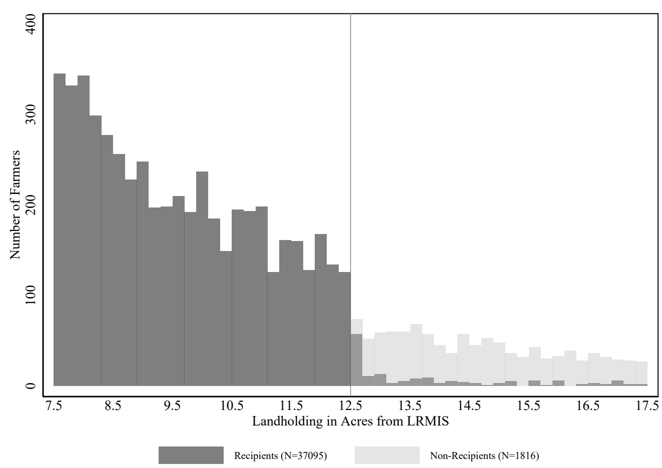
	Dispute Sensitive Crime (1)	Murder (2)	Attempted Murder (3)	Hurt (4)	Rioting (5)	Placebo Crime (6)	Motor Vehicle Theft (7)	Burglary (8)	Theft (9)
Digitization	-2.5062	-0.6872	-1.2943	-0.4580	-0.0666	-0.5071	-0.3605	-0.1884	0.0327
	(1.2178)	(0.2700)	(0.4200)	(1.0153)	(0.0434)	(3.4996)	(1.2737)	(1.2077)	(1.3718)
	[0.0471]	[0.0155]	[0.0040]	[0.6547]	[0.1339]	[0.8856]	[0.7788]	[0.8769]	[0.9811]
Mean of dep. var.	37.02	6.07	7.26	23.55	0.13	48.33	9.91	11.03	27.39
SD of dep. var.	9.60	2.12	2.66	7.09	0.48	24.80	11.76	5.91	11.59
District-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	502	502	502	502	502	502	502	502	502

Notes: This table reports stacked difference-in-differences estimates for the effect of digitization on crime rates per 100,000 using the data from Punjab Policy from Punjab province. Regression includes district-cohort and year-cohort fixed effects. Standard errors clustered at the district-cohort level are in parentheses; p-values are in square brackets.

Figure 8: Distribution of Landholdings by Agricultural Credit



(a) Full Distribution



(b) Zoom Distribution

Notes: This figure plots the density of landholdings (in acres) from LRMIS records for card recipients and non-recipients. The vertical line at 12.5 acres represents the policy eligibility threshold. The statistics reported in the figure correspond to the percentage of recipients exceeding the threshold and non-recipients remaining below it.

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Spillovers of Digitization
(Appendix)

Saqib Hussain[19]

Abstract

Property-rights reforms can reshape institutions far beyond their administrative mandate. I exploit the staggered introduction of land-record digitization to trace its spillovers across rural institutions in Punjab, Pakistan. Drawing on 45,347 confidential anti-corruption incidents, administrative records on agricultural credit, court cases, and taxation, I show that digitization expands agricultural credit, raises corruption detection, improves land-case resolution, and activates rental markets. These gains do not stem from shifts in the landownership patterns. Rather, digitization renders land records public and transparent, breaking local officials' exclusive control over them and reshaping how rural institutions function. (*JEL: D73, G21, H11, K42, O13, Q15; Keywords: Digitization, Land-record Reform, Corruption, Credit, Agriculture*)

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A Data

This appendix describes how I harmonize geographies across sources (A.1), build the anti-corruption *mouza*-month panel (A.2), assign digitization timing (A.3), and define variables (A.4).

Table A1: Data Sources

Dataset	Source	Unit / Period
Agricultural Credit	State Bank of Pakistan	District × year, 2008–2022
Anti-Corruption	Punjab Anti-Corruption	*Mouza* × month, 2000–2023
Record Digitization	Land Records Authority	*Mouza* / district
Development Budget	Planning and Development	District × year, 2008–2025
Court Cases	Lahore High Court	District × week, 2017–2018
Land & Labor Markets	HIES PBS	Individual, Punjab
Crimes	Punjab Police	District × year, 2002–2015
Elections	Election Commission of Pakistan	District/Constituency × year, 2002 & 2008
Farmers Loans	CM Kissan Card Scheme	Cross-section, year, 2024
Tax Administration	Punjab Revenue Authority Aman-Rana and Minaudier (2026)	District × year

Notes: All datasets are merged to the phased LRMIS rollout (Phase 1, 2012; Phase 2, 2013; Phase 3, 2014). The credit panel includes non-Punjab districts as never-treated controls; the remaining Punjab-only panels are identified from variation in the timing of digitization across districts and *mouzas*.

A.1 Geographic Harmonization

The administrative sources record geography inconsistently—districts, tehsils, and *mouza* appear under multiple spellings, transliterations, and abbreviations, and sub-district locations are often embedded in free-text fields. I construct a canonical crosswalk of Punjab districts, tehsils, and revenue estates and map every record to it using a combination of exact matching, keyword rules, and manual review, rolling sub-district strings up to their parent *mouza* and district. Records that cannot be assigned a valid date or location are dropped; retained shares exceed 92 percent in every register.

A.2 From Incidents to a *Mouza*–Month Panel

The anti-corruption registers are recorded at the level of the accused individual, so that a single incident involving several officials generates several rows. I first de-duplicate to unique incidents within each register (complaints, enquiries, cases), then assign each incident to a *mouza* using its location fields and to a calendar month using its filing date. I aggregate to the *mouza*-month level and complete the panel to a balanced grid of 1,287 *mouza* $\times$ 288 months, assigning zeros to cells with no recorded incident. Department is taken from the recorded department field where present and, when missing, inferred from the accused's designation using a word-boundary rule (e.g. *patwari*, *girdawar*, sub-registrar $\rightarrow$ Revenue; constable, sub-inspector $\rightarrow$ Police), which materially improves department coverage of the complaints register. Conviction is coded from the case disposition (charge-sheet submitted or judicial action approved).

A.3 Assigning Digitization Timing

Treatment timing is defined at the district level by the phase to which a district is assigned (Phase 1, 2012; Phase 2, 2013; Phase 3, 2014). For the *mouza*-level corruption panel, I additionally assign within-district timing using the PLRA annual rollout: *mouza* are ordered by their pre-period administrative activity and matched to the district's realized annual digitization counts, so that the share of *mouza* treated by a given year reproduces the district's actual rollout path. The instrument interacts with each unit's baseline phase-specific plan with post-treatment indicators implied by the planned schedule (see Section 4).

A.4 Variable Definitions

Table A2 lists the definition, source, and units of every variable used in the analysis. Monetary credit variables were originally given in millions of pakistani rupees which I deflated and expressed in billions of rupees; corruption and court variables are incident counts or binary indicators; household variables are binary indicators except for land area, measured in acres. Outcomes are studied in levels and, in robustness checks, under the $\log(1 + y)$ and inverse hyperbolic sine transformations (Chen and Roth, 2024).

A.5 Election Data

The election data are recorded at the level of the electoral constituency—National Assembly (*NA-*) and Provincial Assembly (*PP-*) seats—rather than the Administrative district and constituency boundaries have been redrawn twice over the sample period. The elections of 1993 and 1997 used a pre-2002 delimitation; those of 2002, 2008, and 2013 share the 2002 delimitation, under which boundaries are stable, and the 2018 election uses the 2017 delimitation, which redrew the constituency boundaries and renumbered seats following the 2017 census. Within a delimitation era each constituency maps to a single district, but the same constituency number refers to different areas across eras, so any mapping must be constructed era by era.

The district field is populated for most, but not all, constituencies: a subset of seats—concentrated among National Assembly constituencies—carry no district label in any year of an era, so they cannot be filled from the same seat's other elections. I therefore build the constituency-to-district crosswalk in two steps. First, I harmonize the recorded district strings, which appear under multiple spellings and transliterations, to the canonical set of Punjab districts used throughout the paper. Second, I fill the missing labels by exploiting the fact that, under the 2002 delimitation, seats are numbered consecutively within a district: sorting seats by number within each assembly and era, I assign a blank constituency the district of its numeric neighbors, but *only* where the nearest coded seat above and below agree; seats that fall between two different districts—at district boundaries—are left unassigned rather than guessed. I then aggregate all constituency-level outcomes to the district level.

For the baseline-balance analysis, I use the pre-reform general elections of 2002 and 2008. The last election before the rollout and the first with complete provincial coverage in the data. Both fall within the stable 2002 delimitation, so the constituency-to-district mapping is internally consistent, and the 2018 redistricting does not affect any baseline covariate; I note the latter delimitation only because extending the analysis to post-reform elections would require a separate crosswalk.

Table A2: Variable Definitions and Sources

Variable	Definition	Source / Units
Treatment and Instrument		
Digitization	Indicator equal to one for a district in/after its phase-rollout year: Phase 1 (Lahore, Lodhran, Hafizabad, Mandibahauddin, Nankana Sahib, Jehlum, Gujrat, Sialkot, Chakwal, Attock, Rawalpindi; 2012), Phase 2 (Bahawalpur, Gujranwala, Jhang, Layyah, Kasur, Multan, Muzaffargarh, Narowal, Okara, Rahimyar Khan, Sargodha, Sheikhupura, Toba Tek Singh; 2013), or Phase 3 (Bahawalnagar, Bhakkar, Chiniot, D.G. Khan, Faisalabad, Mianwali, Khanewal, Khushab, Pakpattan, Rajanpur, Sahiwal, Vehari; 2014).	PLRA; 0/1
Share digitized	Cumulative number of a district's/mouza's villages digitized by year t, divided by its total villages. Endogenous regressor instrumented by *Digitization* in the IV specifications	PLRA; prop. (0–1)
Agricultural Credit (State Bank of Pakistan; district × year)		
Total borrowers	Number of unique active agricultural borrowers	SBP; thousands

continued on next page

Table A2 continued

Variable	Definition	Source / Units
Amount disbursed	Total credit disbursed	SBP; PKR billions
Amount recovered	Total credit recovered	SBP; PKR billions
Outstanding borrowers	Number of borrowers with outstanding balances	SBP; thousands
Portfolio outstanding	Total outstanding credit	SBP; PKR billions
Average loan size	Amount disbursed divided by number of borrowers	Derived; PKR
Credit reach	Active borrowers divided by number of villages	Derived; ratio
Recovery rate	Amount recovered divided by amount disbursed	Derived; ratio
Debt burden	Outstanding amount divided by outstanding borrowers	Derived; PKR
Anti-Corruption (Punjab ACE; mouza × month)		
Any incident	Count of complaints, enquiries, and cases combined	ACE; count
Land-department	Incident involving the Revenue Department, PLRA, or Local Government	ACE; count
Land-personnel	Incident in which the accused is a *patwari*, *girdawar*, *kanungo*, or *tapedar*	ACE; count
Complaints	Count of incidents at each stage of the enforcement	ACE; count
Enquiries	Count of incidents at each stage of the enforcement	ACE; count
Cases	Count of incidents at each stage of the enforcement	ACE; count
Conviction	Ending in charge-sheet submission or judicial action	ACE; count
Court Cases (Punjab ADR centres; district × week)		
Total cases	Total land-related cases registered	ADR; count
Cases resolved	Cases successfully mediated	ADR; count
Cases failed	Cases where mediation failed	ADR; count
Land and Labor Markets (HIES; individual)		
Formal loan	Household obtained a formal loan	HIES; 0/1
Sold / Purchased land	Household sold (purchased) agricultural land	HIES; 0/1
Area rented in	Agricultural land rented in	HIES; acres
Area rented out	Agricultural land rented out	HIES; 0/1
Cultivated area	Total area cultivated by the household	HIES; acres
Bureaucratic Performance and Taxation (Admin; district × fiscal year)		
Assessed area	Assessed cultivated acreage on the tax rolls	Admin; ln(acres)
Tax demand	Official agricultural land-tax demand	Admin; ln(PKR)
% collected	Share of tax demand collected	Admin; percent

Notes: The phased rollout assigns each of Punjab's 36 districts to one of three cohorts (Phase 1, 2012; Phase 2, 2013; Phase 3, 2014). *Digitization* is used both as the treatment in difference-in-differences and as the instrument for *Share digitized* in the instrumental-variable specifications.

A.6 Agricultural Credit Classifications

In the context of the State Bank of Pakistan (SBP) reporting frameworks, agricultural credit is systematically categorized to evaluate credit distribution, financial inclusion, and capital allocation across the rural economy.

Categorization by Activity: Agricultural financing is divided into two primary operational domains:

- *Farm Credit:* Financing explicitly directed toward crop production and on-farm development. This includes:
  - *Production Loans:* Short-term working capital for agricultural inputs such as seeds, fertilizers, pesticides, and labor.
  - *Development Loans:* Long-term financing for capital improvements, including land leveling, orchard establishment, and the construction of on-farm infrastructure (e.g., warehouses, tube wells).
- *Non-Farm Credit:* Financing for rural economic activities outside the direct scope of crop cultivation. This encompasses livestock management, dairy farming, fisheries, poultry, forestry, and apiculture.

Categorization by Farm Size: Credit requirements are often assessed based on the scale of landholding. These definitions help in applying per-acre credit limits and monitoring outreach to smallholders:

Table A3: Landholding size classifications

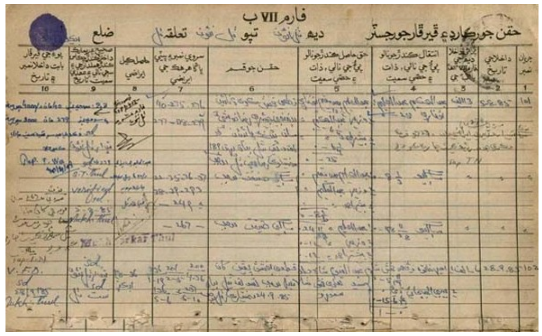
Category	Typical Landholding Size
Subsistence Holding	Up to 12.5 acres
Economic Holding	Above 12.5 to 50 acres
Above Economic Holding	Above 50 acres

Operational Scale: Beyond formal landholding definitions, the SBP utilizes the following conceptual categories to determine credit requirements:

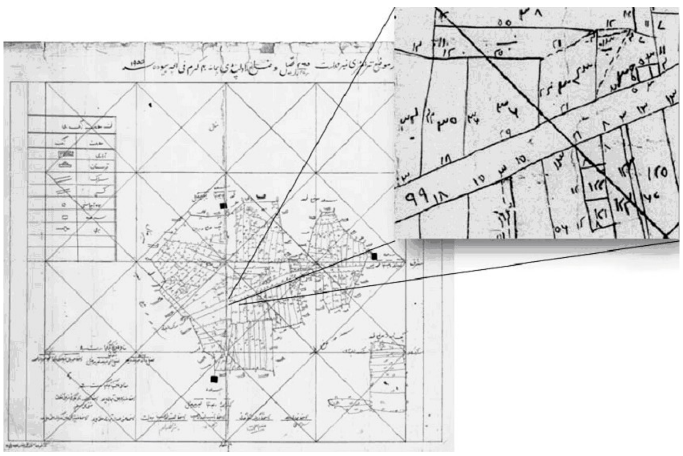
- *Small/Subsistence Farmers:* These operations are characterized by limited land and personal equity. They demonstrate a high dependency on formal credit, as their "own funding" for agricultural inputs is minimal.
- *Medium and Large Farmers:* These operations possess greater land assets and personal financial resources, resulting in a lower relative requirement for formal debt financing per unit of production.

B Digitization

Figure B1: Punjab Land Records



(a) Manual Land Record Register



(b) Cadastral Map

(c) Patwari Office

(d) Land Record Office

Notes: This figure represents the land records of Punjab before and after digitization. Panel (a) shows the manual land record register before digitization, panel (b) shows a related cadastral map, panel (c) shows the Patwari offices before digitization, and panel (d) shows the digitized office after the reform. The sources for these figures include Arab News, Cadastral Maps, Firas Qureshi, New America, and Dawn (Source 1, Source 2)

C Descriptive Statistics

C.1 Agricultural Credit

Table C4: Descriptive Statistics, Agricultural Credit

Variables	(1) Mean	(2) SD	(3) Observations
Panel A: Total Credit Portfolio			
Total Borrowers	13.2814	26.0281	2,235
Total Amount Disbursed	3.6499	9.6044	2,235
Total Amount Recovered	3.3606	8.8029	2,235
Total Outstanding Borrowers	7.5410	23.5151	2,235
Total Portfolio Outstanding	2.3277	4.4339	2,235
Panel B: Farm Credit Portfolio			
Farm: Total Borrowers	8.8615	17.8252	2,235
Farm: Amount Disbursed	1.9501	4.2336	2,235
Farm: Amount Recovered	1.8453	3.9727	2,235
Farm: Outstanding Borrowers	3.8313	12.5134	2,235
Farm: Portfolio Outstanding	1.4209	2.5006	2,235
Panel C: Non-Farm Credit Portfolio			
Non-Farm: Total Borrowers	4.4057	10.1410	2,235
Non-Farm: Amount Disbursed	1.5159	5.2794	2,235
Non-Farm: Amount Recovered	1.3106	4.5823	2,235
Non-Farm: Outstanding Borrowers	3.7120	11.4807	2,235
Non-Farm: Portfolio Outstanding	0.8233	1.8697	2,235
Number of Districts			149

Notes: This table reports descriptive statistics for agricultural credit in Pakistan from 2008 through 2022, using data from the State Bank of Pakistan. Panel A presents the overall credit portfolio, combining farm and non-farm borrowers. Panels B and C report descriptive statistics for farm and non-farm borrowers, respectively. The number of borrowers is measured in thousands, and all monetary values are expressed in billions of Pakistani rupees. Columns (1), (2), and (3) report the sample mean, standard deviation, and number of observations, respectively.

Table C5: Descriptive Statistics, Agricultural Credit (Detailed)

Variables	(1) Mean	(2) SD	(3) Observations
Panel A: Farm Sector: Subsistence Landholdings			
Farm: Subsistence: Total Borrowers	7.9803	16.6162	2,235
Farm: Subsistence: Amount Disbursed	0.9413	1.7977	2,235
Farm: Subsistence: Amount Recovered	0.9041	1.7276	2,235
Farm: Subsistence: Outstanding Borrowers	3.5727	11.8895	2,235
Farm: Subsistence: Portfolio Outstanding	0.9387	1.7009	2,235
Panel B: Farm Sector: Economic Landholdings			
Farm: Economic: Total Borrowers	0.7446	1.3285	2,235
Farm: Economic: Amount Disbursed	0.4106	0.7358	2,235
Farm: Economic: Amount Recovered	0.3963	0.7005	2,235
Farm: Economic: Outstanding Borrowers	0.2274	0.6399	2,235
Farm: Economic: Portfolio Outstanding	0.3057	0.5183	2,235
Panel C: Farm Sector: Above-Economic Landholdings			
Farm: Above Economic: Total Borrowers	0.1365	0.2934	2,235
Farm: Above Economic: Amount Disbursed	0.5982	2.3471	2,235
Farm: Above Economic: Amount Recovered	0.5449	2.1927	2,235
Farm: Above Economic: Outstanding Borrowers	0.0312	0.0919	2,235
Farm: Above Economic: Portfolio Outstanding	0.1765	0.5063	2,235
Panel D: Non-Farm Sector: Small-Scale Enterprises			
Non-Farm: Small Scale: Total Borrowers	4.1831	9.6000	2,235
Non-Farm: Small Scale: Amount Disbursed	0.5114	1.1257	2,235
Non-Farm: Small Scale: Amount Recovered	0.4392	0.9938	2,235
Non-Farm: Small Scale: Outstanding Borrowers	3.4863	10.7148	2,235
Non-Farm: Small Scale: Portfolio Outstanding	0.5540	1.1539	2,235
Panel E: Non-Farm Sector: Large-Scale Enterprises			
Non-Farm: Large Scale: Total Borrowers	0.2226	0.7241	2,235
Non-Farm: Large Scale: Amount Disbursed	1.0046	4.7131	2,235
Non-Farm: Large Scale: Amount Recovered	0.8713	4.0864	2,235
Non-Farm: Large Scale: Outstanding Borrowers	0.2256	0.9437	2,235
Non-Farm: Large Scale: Portfolio Outstanding	0.2694	1.0483	2,235
Number of Districts			149

Notes: This table reports detailed descriptive statistics for agricultural credit in Pakistan across sub-categories from 2008 through 2022, using data from the State Bank of Pakistan. Panels A, B, and C present breakdowns for the farm sector by landholding capacity (Subsistence, Economic, and Above-Economic, respectively). Panels D and E report statistics for the non-farm sector by enterprise scale (Small-Scale and Large-Scale, respectively). The number of borrowers is measured in thousands, and all monetary values are expressed in billions of Pakistani rupees. Columns (1), (2), and (3) report the sample mean, standard deviation, and number of observations, respectively.

C.2 Corruption

Table C6: Descriptive Statistics, Corruption

Variables	(1) Mean	(2) SD	(3) Observations
Panel A: Land Related Departments			
Land Department Incident	0.0411	1.0883	370,656
Land Department Complaints	0.0002	0.0212	370,656
Land Department Enquiries	0.0291	0.8367	370,656
Land Department Cases	0.0118	0.3940	370,656
Panel B: Land Related Personnel			
Land Personnel Incident	0.0234	0.6154	370,656
Land Personnel Complaints	0.0053	0.2413	370,656
Land Personnel Enquiries	0.0137	0.3962	370,656
Land Personnel Cases	0.0044	0.1493	370,656
Panel C: Conversion and Conviction			
Complaints converted to enquiry	0.0367	1.1312	370,656
Enquiries converted to case	0.0065	0.1867	370,656
Cases with chargesheet/judicial action	0.0043	0.1210	370,656
Number of Villages/Mouza			1,287

Notes: This table reports descriptive statistics for corruption data. This data is panel at the mouza (consisting of 1-6 villages) and month-year level. All variables are binary. Panel A shows the number of incidents for the land-related departments, including the Revenue Department, the Punjab Land Record Authority and the Local Government, and Panel B shows the number of incidents reported against land-related staff in any department, such as patwari, girdawar, kanungo, or tapedar. Panel C shows the number of complaints converted to enquiries, enquiries converted to cases, and cases convicted. Columns (1), (2), and (3) report the sample mean, standard deviation, and number of observations, respectively.

Table C7: Descriptive Statistics, Court Cases

Variables	Mean (1)	SD (2)	Observations (3)
Total Number of Cases	336.4061	211.7022	2,268
Criminal Cases	106.4815	108.0144	2,268
Civil Cases	107.7341	88.7266	2,268
Family Cases	168.1596	160.2806	2,268
Guardian Cases	10.1750	16.2837	2,268
Rent Cases	2.2730	4.2666	2,268
Other Cases	20.6795	27.0435	2,268
Mediation Failure Cases	56.9599	44.1432	2,268
Mediation Success Cases	204.9854	119.9669	2,268
Number of Districts			36

Notes: This table reports descriptive statistics for the Alternative Dispute Resolution (ADR) data merged with LRMIS digitization for 36 Punjab districts at the weekly frequency over 2017–2018. Columns (1), (2), and (3) report the mean, standard deviation, and number of observations, respectively.

C.3 Household Integrated Economic Survey

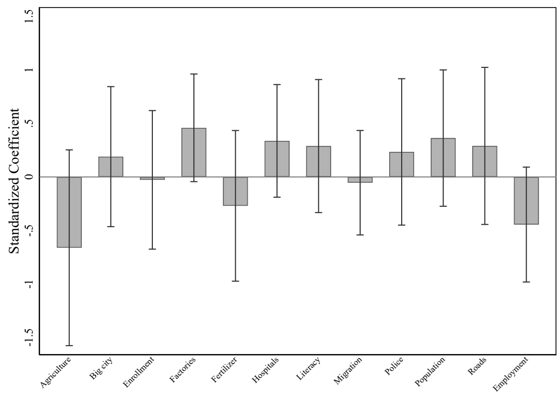
Table C8: Descriptive Statistics, Household Integrated Economic Survey

Variables	(1) Mean	(2) SD	(3) Observations
Panel A: Land and Credit			
Obtained Formal Loan	0.3056	0.4607	19,067
Sold Agricultural Land	0.0100	0.0995	7,702
Purchased Agricultural Land	0.0038	0.0613	7,702
Rented Out Agricultural Land	0.2075	0.4055	7,597
Agricultural Area Rented In (Acres)	1.5726	5.9354	7,461
Total Area Cultivated (Acres)	6.3327	8.2574	7,418
Panel B: Household Characteristics			
Household Head Age	46.5627	14.3402	19,067
Female Household Head	0.1220	0.3273	19,067
Education: None	0.5137	0.4998	19,067
Education: Primary	0.0046	0.0674	19,067
Education: Middle	0.0001	0.0072	19,067
Education: Matriculation	0.0113	0.1058	19,067
Education: Higher	0.0001	0.0072	19,067
Number of Districts			34

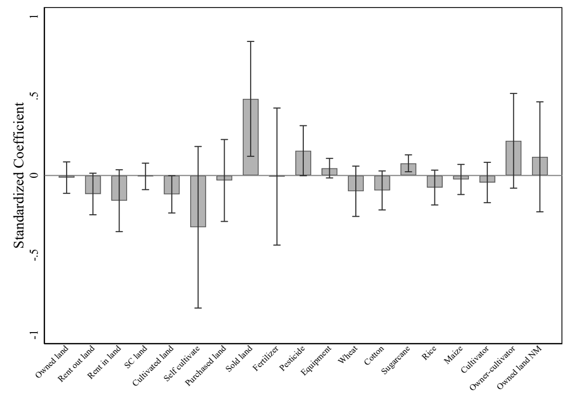
Notes: This table reports descriptive statistics of individual level data. Panel A provides summary statistics for formal credit adoption and land market reconfigurations, where binary indicators take the value of 1 if a transaction occurred and continuous indicators measure land area in acres. Panel B breaks down household-level demographic covariates including the head's age, gender, and level of formal schooling completed. Columns (1), (2), and (3) display the calculated sample mean, standard deviation, and full observation count respectively.

D Balance Test

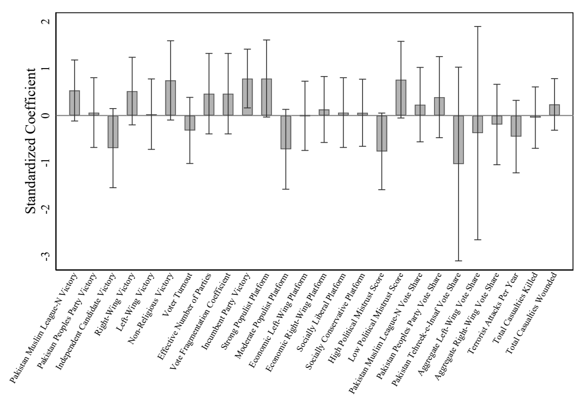
Figure D2: Baseline Balance, Pooled



(a) Institutional Characteristics



(b) Socioeconomic Characteristics



(c) Political Subcategories

Notes: This figure evaluates baseline covariate balance to test for pre-treatment between early and late implementation groups using data from the pre-digitization period. The plotted coefficients represent the standardized difference from pooling Phase 1 and Phase 2 districts contrasted against Phase 3 (omitted baseline) districts. Vertical caps denote 95% confidence intervals based on standard errors clustered at the district level.

E First Stage

E.1 Agricultural Credit

Table E9: Impact of Digitization Plan on Digitization, First-Stage

	(1)	(2)	(3)	(4)	(5)
Panel A: Number of Villages Digitized					
Phase 1 × Post	0.4400			0.5257	
	(0.0838)			(0.0927)	
	[0.0000]			[0.0000]	
Phase 2 × Post		0.6886		0.7484	
		(0.0345)		(0.0337)	
		[0.0000]		[0.0000]	
Phase 3 × Post			0.7602	0.8353	
			(0.0317)	(0.0165)	
			[0.0000]	[0.0000]	
Plan × Post					523.0360
					(34.5614)
					[0.0000]
Year FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Observations	2,235	2,235	2,235	2,235	2,235
Panel B: Percentage of Villages Digitized					
Phase 1 × Post	0.0005			0.0006	
	(0.0002)			(0.0002)	
	[0.0021]			[0.0009]	
Phase 2 × Post		0.0008		0.0008	
		(0.0001)		(0.0001)	
		[0.0000]		[0.0000]	
Phase 3 × Post			0.0011	0.0012	
			(0.0001)	(0.0001)	
			[0.0000]	[0.0000]	
Plan × Post					0.7657
					(0.0217)
					[0.0000]
Year FE	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Observations	2,235	2,235	2,235	2,235	2,235

Notes: This table reports first-stage estimates evaluating the digitization program using district-level credit panel data. Panel A uses the total number of digitized villages as the dependent variable, while Panel B uses the time-varying share of total villages digitized. In both panels, Columns (1) through (3) isolate individual phase-specific shift-share instruments. Column (4) includes all three phase instruments jointly, and Column (5) utilizes the aggregate binary treatment interaction variable. All specifications incorporate district and year fixed effects. Standard errors reported in parentheses are clustered at the district level. The corresponding p-values are displayed in square brackets.

E.2 Corruption

Table E10: Impact of Digitization Plan on Digitization, First-Stage

	(1)	(2)	(3)	(4)
Phase 1 × Post	0.1455		0.2198	
	(0.0059)		(0.0110)	
	[0.0000]		[0.0000]	
Phase 2 × Post		0.1073	0.1310	
		(0.0132)	(0.0136)	
		[0.0000]	[0.0000]	
Plan × Post				0.1098
				(0.0082)
				[0.0000]
Month-Year FE	Yes	Yes	Yes	Yes
Mouza FE	Yes	Yes	Yes	Yes
Observations	370,656	370,656	370,656	370,656

Notes: This table reports first-stage estimates evaluating the digitization using the corruption panel data. The outcome is a binary indicator if a mouza is digitized by the planned time. Columns (1) and (2) isolate phase-specific instruments. Column (3) includes both phases jointly, and Column (4) utilizes the aggregate binary treatment interaction variable. Standard errors reported in parentheses are clustered at the mouza level. The corresponding p-values are displayed in square brackets.

F Robustness

F.1 Agricultural Credit

F.1.1 Outcome Transformations

Table F11: Impact of Digitization on Agricultural Credit, Outcome Transformations

	Stacked		Unstacked	
	DID	IV	DID	IV
	(1)	(2)	(3)	(4)
Panel A: Borrowers				
Digitization: Log	0.2752	0.4404	0.2917	0.4351
	(0.0710)	(0.0767)	(0.0773)	(0.0856)
	[0.0001]	[0.0000]	[0.0002]	[0.0000]
Digitization: IHS	0.2696	0.4337	0.2888	0.4277
	(0.0750)	(0.0810)	(0.0851)	(0.0934)
	[0.0004]	[0.0000]	[0.0009]	[0.0000]
Mean of dep. var. (level)	8.54	8.54	13.28	13.28
SD of dep. var. (level)	19.49	19.49	26.03	26.03
District FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
District-Cohort FE	Yes	Yes	No	No
Year-Cohort FE	Yes	Yes	No	No
Kleibergen-Paap first-stage F		60.22		55.91
Observations	4,110	4,110	2,235	2,235
Panel B: Disbursement				
Digitization: Log	0.4080	0.6086	0.5041	0.7138
	(0.0497)	(0.0564)	(0.0581)	(0.0744)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Digitization: IHS	0.4481	0.6664	0.5515	0.7727
	(0.0577)	(0.0650)	(0.0662)	(0.0847)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Mean of dep. var. (level)	1.80	1.80	3.65	3.65
SD of dep. var. (level)	5.60	5.60	9.60	9.60
District FE	No	No	Yes	Yes
Year FE	No	No	Yes	Yes
District-Cohort FE	Yes	Yes	No	No
Year-Cohort FE	Yes	Yes	No	No
Kleibergen-Paap first-stage F		60.22		55.91
Observations	4,110	4,110	2,235	2,235

Notes: This table re-estimates the Table 1 using two alternative transformations of outcome variables. Within each panel, the first row reports estimates with outcomes transformed by $\log(1 + y_{dt})$, and the second row uses the inverse hyperbolic sine, $\text{asinh}(y_{dt}) = \log(y_{dt} + \sqrt{y_{dt}^2 + 1})$. Each cell reports either the reduced-form difference-in-differences coefficient or the instrumented share of villages digitized. Columns (1) and (2) utilize the stacked data with cohort-interacted district and year fixed effects, while Columns (3) and (4) use a standard two-way fixed-effects estimator using district and year fixed effects. The Kleibergen-Paap row reports the first-stage Wald statistic. Mean and SD parameters are reported in levels. Standard errors clustered at the district level in unstacked and cohort-district in stacked data are given in parentheses; p-values are in square brackets.

Table F12: Impact of Digitization on Agricultural Credit by Sector, Outcome Transformations

	Number of Borrowers		Disbursed Amount	
	Farm Sector (1)	Non-Farm Sector (2)	Farm Sector (3)	Non-Farm Sector (4)
Panel A: Stacked (DID)				
Digitization: Log	0.0720	1.0141	0.2042	0.6737
	(0.0813)	(0.0827)	(0.0483)	(0.0711)
	[0.3761]	[0.0000]	[0.0000]	[0.0000]
Digitization: IHS	0.0720	1.1342	0.2311	0.8765
	(0.0880)	(0.0895)	(0.0605)	(0.0877)
	[0.4137]	[0.0000]	[0.0002]	[0.0000]
Observations	4,110	4,110	4,110	4,110
Panel B: Stacked (IV)				
Digitization: Log	0.1912	1.4575	0.3449	1.0067
	(0.0909)	(0.1017)	(0.0561)	(0.0891)
	[0.0359]	[0.0000]	[0.0000]	[0.0000]
Digitization: IHS	0.2011	1.5997	0.3924	1.3162
	(0.0962)	(0.1124)	(0.0690)	(0.1075)
	[0.0372]	[0.0000]	[0.0000]	[0.0000]
Kleibergen-Paap F	60.22	60.22	60.22	60.22
Observations	4,110	4,110	4,110	4,110
Panel C: Unstacked (DID)				
Digitization: Log	0.0311	1.1499	0.2849	0.7904
	(0.0852)	(0.0854)	(0.0551)	(0.0788)
	[0.7156]	[0.0000]	[0.0000]	[0.0000]
Digitization: IHS	0.0346	1.2826	0.3241	1.0218
	(0.0943)	(0.0957)	(0.0677)	(0.0951)
	[0.7142]	[0.0000]	[0.0000]	[0.0000]
Observations	2,235	2,235	2,235	2,235
Panel D: Unstacked (IV)				
Digitization: Log	0.1260	1.5691	0.4251	1.1366
	(0.1016)	(0.1069)	(0.0733)	(0.0968)
	[0.2165]	[0.0000]	[0.0000]	[0.0000]
Digitization: IHS	0.1367	1.7176	0.4807	1.4720
	(0.1098)	(0.1213)	(0.0894)	(0.1158)
	[0.2150]	[0.0000]	[0.0000]	[0.0000]
Kleibergen-Paap F	55.91	55.91	55.91	55.91
Observations	2,235	2,235	2,235	2,235

Notes: This table replicates the Table 2 and F12 using alternative transformations of the outcome variables. Each coefficient is a separate regression. Within each panel, the first row reports estimates where the dependent variable is transformed by $\log(1 + y_{dt})$, and the second row reports estimates using the inverse hyperbolic sine transformation, $\text{asinh}(y_{dt}) = \log(y_{dt} + \sqrt{y_{dt}^2 + 1})$. Panel A reports stacked DID. Panel B reports stacked 2SLS models instrumenting the time-varying share of digitized villages. Panel C reports unstacked two-way fixed-effects models on the full panel. Panel D reports unstacked 2SLS models using TWFE. All models include relevant fixed effects as Table 2 and F12. Standard errors clustered at the district and district-cohort level are reported in parentheses; p-values are in square brackets.

F.1.2 Alternate Specifications

Table F13: Impact of Digitization on Agricultural Credit by Sector, Alternate Specifications

	Number of Borrowers		Disbursed Amount	
	Farm Sector (1)	Non-Farm Sector (2)	Farm Sector (3)	Non-Farm Sector (4)
Panel A: Stacked (IV)				
Digitization	9.3962	23.4481	3.0480	4.4176
	(3.9407)	(2.9199)	(0.6182)	(0.8485)
	[0.0176]	[0.0000]	[0.0000]	[0.0000]
Kleibergen-Paap F-stat	60.22	60.22	60.22	60.22
Mean of dep. var.	6.4709	2.0404	1.1309	0.6215
SD of dep. var.	15.0561	5.8909	2.7218	3.0442
District-Cohort FE	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes
Observations	4,110	4,110	4,110	4,110
Panel B: Unstacked (DID)				
Digitization	3.1927	16.9829	2.5649	4.0893
	(2.5233)	(2.0818)	(0.5537)	(0.9066)
	[0.2078]	[0.0000]	[0.0000]	[0.0000]
Mean of dep. var.	8.8615	4.4057	1.9501	1.5159
SD of dep. var.	17.8252	10.1410	4.2336	5.2794
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	2,235	2,235	2,235	2,235
Panel C: Unstacked (IV)				
Digitization	6.3470	25.8882	4.0005	5.4772
	(3.9916)	(3.1583)	(0.8607)	(1.0882)
	[0.1140]	[0.0000]	[0.0000]	[0.0000]
Kleibergen-Paap F-stat	55.91	55.91	55.91	55.91
Mean of dep. var.	8.8615	4.4057	1.9501	1.5159
SD of dep. var.	17.8252	10.1410	4.2336	5.2794
District FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	2,235	2,235	2,235	2,235

Notes: This table re-estimates the Table 2 using two alternative specifications. Panel A reports IV estimates using stacked data. Panel B reports standard two-way fixed-effects models estimated on the full unstacked panel. Panel C reports unstacked two-stage least squares (2SLS) models on the unstacked panel, instrumenting the time-varying share of digitized villages. Columns (1) and (2) measure the number of borrowers in thousands, while Columns (3) and (4) measure total disbursed loan values in billion rupees. Mean and SD are evaluated in level units matching the active sample. Standard errors clustered at the district of district-cohort level are in parentheses, and p-values are in square brackets.

F.1.3 Alternate DID Estimator

Table F14: Impact of Digitization on Agricultural Credit, Alternate DID Estimator

	TWFE (1)	Stacked DiD (2)	Callaway Sant'Anna (3)	Gardner Two-Stage (4)	Borusyak Jaravel Spiess (5)
Panel A: Disbursement					
Digitization	7.1461	5.3256	6.6138	7.5156	7.5156
	(1.4494)	(1.0172)	(1.5683)	(1.7461)	(1.7363)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Panel B: Borrowers					
Digitization	19.6915	19.3017	12.6883	18.1879	18.1879
	(3.6681)	(3.6542)	(3.1456)	(4.0247)	(3.5274)
	[0.0000]	[0.0000]	[0.0001]	[0.0000]	[0.0000]
District FE	Yes	No	Yes	Yes	Yes
Year FE	Yes	No	Yes	Yes	Yes
District-Cohort FE	No	Yes	No	No	No
Year-Cohort FE	No	Yes	No	No	No
Observations	2,235	4,110	2,235	2,235	2,235

Notes: This table compares five different-in-differences estimators of the effect of digitization on agricultural credit. The outcome variables are estimated in level units. Column (1) presents the standard pooled TWFE estimator with district and year fixed effects. Column (2) presents the stacked difference-in-differences estimator with cohort-interacted district and year fixed effects. Column (3) reports the Callaway and Sant'Anna (2022) estimator using doubly robust inverse probability weighting (DRIPW); the reported coefficient is the overall ATT aggregated across cohorts. Column (4) reports Gardner's (2022) two-stage imputation estimator, where unit and time fixed effects are identified from the sub-sample of non-treated observations. Column (5) reports the Borusyak, Jaravel, and Spiess (2024) imputation-based estimator. Standard errors clustered at the district or district-cohort level are reported in parentheses; p-values are in square brackets.

F.1.4 Alternate Cohorts

Table F15: Impact of Digitization on Agricultural Credit, Alternate Cohorts

	Main Specification (1)	Drop Phase 1 (2)	Drop Phase 2 (3)	Drop Phase 3 (4)
Panel A: Disbursement				
Digitization	5.3256	5.7167	4.8100	5.4648
	(1.0172)	(1.0992)	(1.3457)	(1.3218)
	[0.0000]	[0.0000]	[0.0004]	[0.0000]
Panel B: Borrowers				
Digitization	19.3017	24.8498	12.7622	19.8629
	(3.6543)	(4.1423)	(3.6680)	(5.2078)
	[0.0000]	[0.0000]	[0.0006]	[0.0002]
District-Cohort FE	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes
Observations	4,110	4,011	3,902	3,810

Notes: This table assesses the sample robustness of the stacked difference-in-differences estimates across alternative rolling sub-samples using a leave-one-out framework. All outcome metrics are evaluated in level units. Column (1) reports the baseline main specification. Columns (2) through (4) systematically drop single rollout cohort phases sequentially to verify that the stability of the estimates is not artifact-driven by specific subsets of districts. All specifications include cohort interacted district, and year fixed effects. Standard errors clustered at the district-cohort level are reported in parentheses; p-values are in square brackets.

F.2 Corruption

F.2.1 Outcome Transformations

Table F16: Impact of Digitization on Land Related Corruption, Outcome Transformations

	Stacked		Unstacked	
	DID	IV	DID	IV
	(1)	(2)	(3)	(4)
Panel A. Land Departments				
Digitization: Log	0.0203	0.1431	0.0162	0.0507
	(0.0080)	(0.0576)	(0.0057)	(0.0182)
	[0.0109]	[0.0131]	[0.0044]	[0.0055]
Digitization: IHS	0.0253	0.1779	0.0200	0.0628
	(0.0099)	(0.0715)	(0.0069)	(0.0223)
	[0.0107]	[0.0129]	[0.0040]	[0.0050]
Mean of dep. var. (level)	0.0382	0.0382	0.0411	0.0411
SD of dep. var. (level)	1.0764	1.0764	1.0883	1.0883
Mouza-Cohort FE	Yes	Yes	No	No
Year-Month-Cohort FE	Yes	Yes	No	No
Mouza FE	No	No	Yes	Yes
Year-Month FE	No	No	Yes	Yes
Kleibergen-Paap first-stage F		148.61		214.58
Observations	724,320	724,320	370,656	370,656
Panel B. Land Personnel				
Digitization: Log	0.0158	0.1115	0.0145	0.0454
	(0.0064)	(0.0461)	(0.0051)	(0.0165)
	[0.0133]	[0.0157]	[0.0049]	[0.0061]
Digitization: IHS	0.0200	0.1412	0.0180	0.0565
	(0.0081)	(0.0584)	(0.0063)	(0.0204)
	[0.0133]	[0.0157]	[0.0045]	[0.0056]
Mean of dep. var. (level)	0.0217	0.0217	0.0234	0.0234
SD of dep. var. (level)	0.6089	0.6089	0.6154	0.6154
Mouza-Cohort FE	Yes	Yes	No	No
Year-Month-Cohort FE	Yes	Yes	No	No
Mouza FE	No	No	Yes	Yes
Year-Month FE	No	No	Yes	Yes
Kleibergen-Paap first-stage F		148.61		214.58
Observations	724,320	724,320	370,656	370,656

Notes: This table re-estimates the main corruption specifications utilizing log and inverse hyperbolic sine transforms to verify functional form consistency. Row 1 of each panel logs outcomes as $\ln(1+y\_vt)$, while Row 2 implements an inverse hyperbolic sine transformation, $\text{asinh}(y\_vt) = \ln(y\_vt + \sqrt{y\_vt^2 + 1})$, accommodating zero-valued variables while yielding direct semi-elasticity evaluations. Panel A uses overall land department incidents (complaints, enquiries, and cases), and Panel B isolates occurrences targeting land-related personnel. Columns (1) and (2) use stacked data incorporating cohort-interacted mouza and year-month fixed effects. Columns (3) and (4) use standard unstacked data incorporating mouza and year-month fixed effects. Means and deviations in original level units. Standard errors in parentheses are clustered at the cohort-mouza for stacked and mouza for unstacked data alongside p-values in square brackets.

Table F17: Impact of Digitization on Land Related Corruption by Categories, Outcome Transformations

	Land-Related Departments			Land-Related Personnel		
	Complaints	Enquiries	Cases	Complaints	Enquiries	Cases
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Stacked (DID)						
Digitization: Log	0.0003	0.0167	0.0081	0.0036	0.0116	0.0044
	(0.0002)	(0.0070)	(0.0032)	(0.0016)	(0.0049)	(0.0018)
	[0.0291]	[0.0178]	[0.0107]	[0.0232]	[0.0188]	[0.0145]
Digitization: IHS	0.0005	0.0209	0.0104	0.0046	0.0147	0.0056
	(0.0002)	(0.0088)	(0.0041)	(0.0020)	(0.0063)	(0.0023)
	[0.0293]	[0.0179]	[0.0105]	[0.0231]	[0.0188]	[0.0144]
Observations	724,320	724,320	724,320	724,320	724,320	724,320
Panel B: Stacked (IV)						
Digitization: Log	0.0025	0.1177	0.0574	0.0251	0.0816	0.0307
	(0.0011)	(0.0508)	(0.0231)	(0.0113)	(0.0355)	(0.0129)
	[0.0326]	[0.0205]	[0.0129]	[0.0261]	[0.0215]	[0.0171]
Kleibergen-Paap F	148.61	148.61	148.61	148.61	148.61	148.61
Digitization: IHS	0.0032	0.1469	0.0731	0.0321	0.1038	0.0396
	(0.0015)	(0.0634)	(0.0293)	(0.0144)	(0.0451)	(0.0166)
	[0.0328]	[0.0206]	[0.0128]	[0.0260]	[0.0216]	[0.0171]
Kleibergen-Paap F	148.61	148.61	148.61	148.61	148.61	148.61
Observations	724,320	724,320	724,320	724,320	724,320	724,320
Panel C: Unstacked (DID)						
Digitization: Log	-0.0000	0.0126	0.0077	0.0047	0.0105	0.0050
	(0.0001)	(0.0050)	(0.0030)	(0.0023)	(0.0042)	(0.0022)
	[0.7653]	[0.0113]	[0.0115]	[0.0411]	[0.0124]	[0.0213]
Digitization: IHS	-0.0000	0.0156	0.0098	0.0059	0.0131	0.0064
	(0.0001)	(0.0061)	(0.0038)	(0.0028)	(0.0052)	(0.0028)
	[0.7653]	[0.0105]	[0.0108]	[0.0370]	[0.0117]	[0.0213]
Observations	370,656	370,656	370,656	370,656	370,656	370,656
Panel D: Unstacked (IV)						
Digitization: Log	-0.0001	0.0395	0.0240	0.0149	0.0328	0.0157
	(0.0004)	(0.0159)	(0.0097)	(0.0074)	(0.0134)	(0.0069)
	[0.7654]	[0.0130]	[0.0132]	[0.0439]	[0.0142]	[0.0235]
Kleibergen-Paap F	214.58	214.58	214.58	214.58	214.58	214.58
Digitization: IHS	-0.0001	0.0489	0.0306	0.0186	0.0412	0.0202
	(0.0005)	(0.0195)	(0.0123)	(0.0090)	(0.0166)	(0.0089)
	[0.7636]	[0.0122]	[0.0125]	[0.0397]	[0.0134]	[0.0235]
Kleibergen-Paap F	214.58	214.58	214.58	214.58	214.58	214.58
Observations	370,656	370,656	370,656	370,656	370,656	370,656

Notes: The table shows the effect of digitization on anti-corruption activity in the Punjab Anti-Corruption Establishment, by stage of investigation (columns) and alternative outcome transformations across four specifications (panels). Within each panel, the first sub-block reports estimates using the $\log(1 + y)$ transformation, and the second sub-block uses the inverse hyperbolic sine ($\text{asinh}(y)$) transformation. Mean and SD metrics correspond to dependent variables evaluated at levels. In the stacked panel, cohort-mouza and cohort-month-year fixed effects are used, whereas in the unstacked panel, mouza and month-year fixed effects are used. Standard errors clustered at the cohort-mouza level for stacked and mouza level for unstacked data, and given in parentheses; p-values are in square brackets.

Table F18: Impact of Digitization on Land Related Court Cases, Outcome Transformations

	(1) Total Cases	(2) Criminal Cases	(3) Civil Cases	(4) Family Cases	(5) Guardian Cases	(6) Rent Cases	(7) Other Cases	(8) Cases Failed	(9) Cases Resolved
Panel A: Outcomes in Log									
Digitization	0.0980	0.1554	0.0829	0.1184	0.0844	0.0387	0.1096	0.0157	0.1078
	(0.0117)	(0.0166)	(0.0181)	(0.0145)	(0.0176)	(0.0170)	(0.0194)	(0.0159)	(0.0124)
	[0.0000]	[0.0000]	[0.0001]	[0.0000]	[0.0000]	[0.0285]	[0.0000]	[0.3299]	[0.0000]
Mean of dep. var. (level)	336.4061	106.4815	107.7341	168.1596	10.1750	2.2730	20.6795	56.9599	204.9854
SD of dep. var. (level)	211.7022	108.0144	88.7266	160.2806	16.2837	4.2666	27.0435	44.1432	119.9669
Panel B: Outcomes in IHS									
Digitization	0.0994	0.1619	0.0885	0.1221	0.1036	0.0509	0.1271	0.0187	0.1102
	(0.0119)	(0.0189)	(0.0190)	(0.0149)	(0.0208)	(0.0208)	(0.0221)	(0.0176)	(0.0127)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0195]	[0.0000]	[0.2962]	[0.0000]
Mean of dep. var. (level)	336.4061	106.4815	107.7341	168.1596	10.1750	2.2730	20.6795	56.9599	204.9854
SD of dep. var. (level)	211.7022	108.0144	88.7266	160.2806	16.2837	4.2666	27.0435	44.1432	119.9669
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,268	2,268	2,268	2,268	2,268	2,268	2,268	2,268	2,268

Notes: This table presents the continuous difference-in-differences estimated in level counts. Considering the data limitations (weekly 2017-2018 panel), the independent variable is the interaction between the percentage of villages digitized and the early treatment districts. Panel A transforms original counts via $\ln(1+y)$, while Panel B uses the inverse hyperbolic sine transformation. All models incorporate demographic/infrastructure controls, district fixed effects, and weekly time counter fixed effects. Descriptive for the dependent variable represent raw, untransformed level metrics computed directly from regression sample. Standard errors clustered at the district level are reported in parentheses; p-values are in square brackets.

F.2.2 Alternate Specifications

Table F19: Impact of Digitization on Land Related Corruption by Categories, Alternate Specifications

	Land-Related Departments			Land-Related Personnel		
	Complaints (1)	Enquiries (2)	Cases (3)	Complaints (4)	Enquiries (5)	Cases (6)
Panel A: Stacked (IV)						
Digitization	0.0040	0.5905	0.1949	0.0740	0.2818	0.0734
	(0.0019)	(0.2723)	(0.0827)	(0.0377)	(0.1275)	(0.0308)
	[0.0400]	[0.0302]	[0.0185]	[0.0494]	[0.0271]	[0.0173]
Kleibergen-Paap F	148.61	148.61	148.61	148.61	148.61	148.61
Mean of dep. var.	0.0002	0.0273	0.0107	0.0051	0.0127	0.0039
SD of dep. var.	0.0206	0.8287	0.3844	0.2413	0.3898	0.1422
Mouza-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Mouza FE	No	No	No	No	No	No
Year-Month FE	No	No	No	No	No	No
Observations	724,320	724,320	724,320	724,320	724,320	724,320
Panel B: Unstacked (DID)						
Digitization	-0.0001	0.0780	0.0262	0.0231	0.0457	0.0126
	(0.0002)	(0.0409)	(0.0125)	(0.0173)	(0.0223)	(0.0057)
	[0.6765]	[0.0563]	[0.0361]	[0.1814]	[0.0411]	[0.0276]
Mean of dep. var.	0.0002	0.0291	0.0118	0.0053	0.0137	0.0044
SD of dep. var.	0.0212	0.8367	0.3940	0.2413	0.3962	0.1493
Mouza-Cohort FE	No	No	No	No	No	No
Year-Month-Cohort FE	No	No	No	No	No	No
Mouza FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	370,656	370,656	370,656	370,656	370,656	370,656
Panel C: Unstacked (IV)						
Digitization	-0.0003	0.2448	0.0821	0.0724	0.1432	0.0396
	(0.0006)	(0.1296)	(0.0397)	(0.0545)	(0.0710)	(0.0182)
	[0.6768]	[0.0591]	[0.0386]	[0.1838]	[0.0438]	[0.0300]
Kleibergen-Paap F	214.58	214.58	214.58	214.58	214.58	214.58
Mean of dep. var.	0.0002	0.0291	0.0118	0.0053	0.0137	0.0044
SD of dep. var.	0.0212	0.8367	0.3940	0.2413	0.3962	0.1493
Mouza-Cohort FE	No	No	No	No	No	No
Year-Month-Cohort FE	No	No	No	No	No	No
Mouza FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	370,656	370,656	370,656	370,656	370,656	370,656

Notes: This table shows the effects of digitization on anti-corruption activity recorded by the Punjab Anti-Corruption Establishment. Columns (1) through (3) track administrative processing volumes for land departments, split into total complaints filed, formal enquiries initiated, and criminal cases registered. Columns (4) through (6) evaluate the same structural progression tracking incidents where the primary target is land-related personnel. All outcomes are evaluated in original level metrics. Estimation is a stacked difference-in-differences with cohort-interacted mouza and year-month fixed effects. Means and standard deviations correspond directly to active regression samples. Standard errors shown in parentheses are clustered at the cohort-mouza level, with p-values provided in square brackets.

F.2.3 Alternate DID Estimator

Table F20: Impact of Digitization on Land Related Corruption, Alternate DID Estimator

	TWFE (1)	Stacked DiD (2)	Callaway Sant'Anna (3)	Gardner Two-Stage (4)	Borusyak Jaravel Spiess (5)
Panel A: Land Departments					
Digitization	1.2499	1.2015	0.6683	1.0991	1.2477
	(0.6252)	(0.5474)	(0.3022)	(0.4766)	(0.5379)
	[0.0458]	[0.0283]	[0.0270]	[0.0211]	[0.0204]
Panel B: Land Personnel					
Digitization	0.9764	0.8710	0.2542	0.5947	0.6723
	(0.5077)	(0.4237)	(0.1271)	(0.2507)	(0.2800)
	[0.0547]	[0.0400]	[0.0455]	[0.0177]	[0.0164]
Mouza FE	Yes	No	Yes	Yes	Yes
Year FE	Yes	No	Yes	Yes	Yes
Mouza-Cohort FE	No	Yes	No	No	No
Year-Cohort FE	No	Yes	No	No	No
Observations	30,888	40,608	30,888	30,888	30,888

Notes: This table compares five different-in-differences estimators of the effect of digitization on anti-corruption activity at the mouza-year level. Outcomes are aggregated counts. Panel A reports results for any land-related incident (Revenue/PLRA/LocalGov departments). Panel B reports results for any land-related personnel incident. Column (1) presents the pooled TWFE estimator. Column (2) presents the stacked difference-in-differences estimator with cohort-interacted mouza and year fixed effects. Column (3) reports the Callaway and Sant'Anna (2021) estimator using doubly robust inverse probability weighting (DRIPW) with the 'notyet' treated group (Phase 3) as control; the coefficient is the simple aggregated ATT. Column (4) reports Gardner's (2021) two-stage imputation estimator. Column (5) reports the Borusyak, Jaravel, and Spiess (2024) imputation-based estimator. Standard errors clustered at the mouza level for unstacked and mouza-cohort level for stacked data, and given in parentheses; p-values in square brackets.

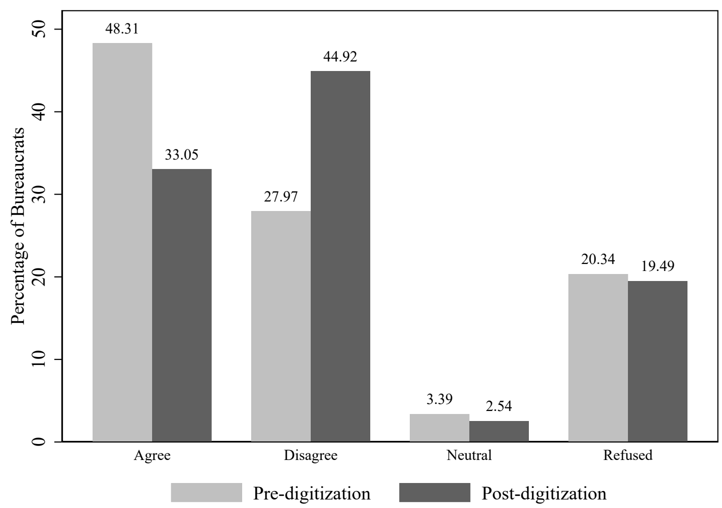
F.2.4 Poisson

Table F21: Impact of Digitization on Land Related Corruption, Poisson

	Stacked			Unstacked		
	Incidents	Enquiries	Cases	Incidents	Enquiries	Cases
Panel A. Land Departments						
Digitization	0.9179	0.8911	0.9965	3.6854	3.9937	3.2526
	(0.2916)	(0.2824)	(0.3426)	(0.6378)	(0.6070)	(0.7399)
	[0.0016]	[0.0016]	[0.0036]	[0.0000]	[0.0000]	[0.0000]
Mean of dep. var.	0.0608	0.0450	0.0190	0.0654	0.0481	0.0208
SD of dep. var.	1.3573	1.0640	0.5109	1.3723	1.0746	0.5235
Tehsil FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	22	20	19	22	20	19
Observations	455,215	439,075	409,887	232,947	224,525	209,817
Panel B. Land Personnel						
Digitization	0.9575	0.9458	1.1823	4.1064	4.2688	3.7343
	(0.3013)	(0.2864)	(0.4150)	(0.6000)	(0.6146)	(0.7991)
	[0.0015]	[0.0010]	[0.0044]	[0.0000]	[0.0000]	[0.0000]
Mean of dep. var.	0.0349	0.0221	0.0073	0.0377	0.0239	0.0082
SD of dep. var.	0.7721	0.5142	0.1949	0.7803	0.5227	0.2046
Tehsil FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	22	16	18	22	16	18
Observations	450,185	415,909	385,273	230,373	212,771	197,225

Notes: This table reports the effect of digitization on anti-corruption activity using Poisson regressions. Panel A presents outcomes for land departments, while Panel B restricts the sample to incidents involving land personnel. Columns (1)-(3) employ a stacked difference-in-differences estimator, and columns (4)-(6) report standard two-way fixed effects estimates on the unstacked panel. All regressions include tehsil and year fixed effects, with standard errors clustered at the tehsil level.

Figure F3: Do Bureaucrats in Charge of Land Titles Receive Bribes or "Tips" for Issuing Them?



Notes: The figure is based on the bureaucrat survey from Aman-Rana and Minaudier (2026). The figure shows the percentage of respondents who responded to the question, "People over there (in a revenue circle/mouza) would tip or want to tip a Patwari (bureaucrat's subordinates) for issuing Fard (land title)" measured on a Likert scale.

F.2.5 Placebo

Table F22: Impact of Digitization on Corruption in Other Departments, Placebo

	Health Department (1)	Public Health (2)	Irrigation (3)
Digitization	0.0020	0.0001	0.0010
	(0.0014)	(0.0001)	(0.0006)
	[0.1720]	[0.3720]	[0.0754]
Mean of dep. var.	0.0007	0.0002	0.0004
SD of dep. var.	0.0606	0.0254	0.0826
Mouza-Cohort FE	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes
Observations	724,320	724,320	724,320

Notes: This table shows the effects of digitization on anti-corruption activity recorded by the Punjab Anti-Corruption Establishment. The outcome variables are the number of incidents in departments unrelated to land record digitization, such as health (clinical and hospital-based patient care), public health (community-wide disease prevention, wellness, and environmental safety), and irrigation (water resources, plans agricultural irrigation systems, and builds flood protection infrastructure). Estimation is a stacked difference-in-differences with cohort-interacted mouza and year-month fixed effects. Means and standard deviations correspond directly to active regression samples. Standard errors in parentheses are clustered at the cohort-mouza level, with p-values provided inside square brackets.

Table F23: Impact of Digitization on Development Spending

	Number of Projects (1)	Development Budget Allocation (2)	Total Releases (3)	Agricultural Releases (4)	Education Releases (5)	Health Releases (6)	Irrigation Releases (7)	Roads Releases (8)	Other Releases (9)
Digitization	-15.9705	-0.1036	0.1939	0.0067	-0.0577	0.0004	-0.1718	0.2483	0.0397
	(12.1911)	(0.6457)	(0.6264)	(0.0149)	(0.0435)	(0.0713)	(0.1398)	(0.2114)	(0.2757)
	[0.1952]	[0.8731]	[0.7580]	[0.6521]	[0.1900]	[0.9951]	[0.2240]	[0.2449]	[0.8858]
Mean of dep. var.	172.24	4.76	3.50	0.03	0.32	0.26	0.45	0.90	1.03
SD of dep. var.	123.45	4.55	3.74	0.08	0.26	0.44	0.60	1.09	1.54
District-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	610	610	610	610	610	610	610	610	610

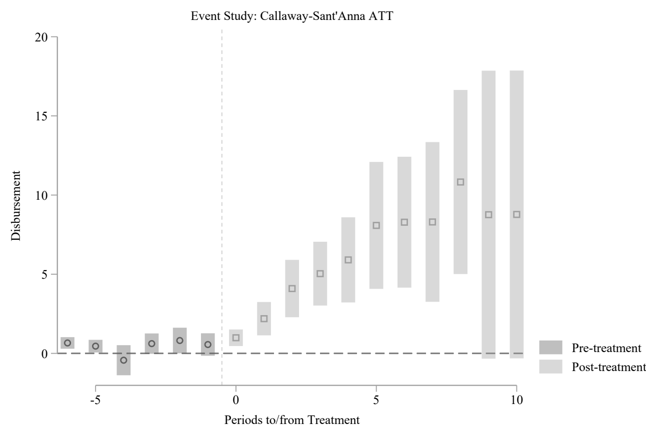
Notes: This table reports placebo regressions for the effect of land record digitization on development spending using the 36 Punjab districts. Models are evaluated using the stacked difference-in-differences estimator with cohort-interacted district and year fixed effects. Standard errors clustered at the cohort-district level are in parentheses; p-values are in square brackets.

G Event Study

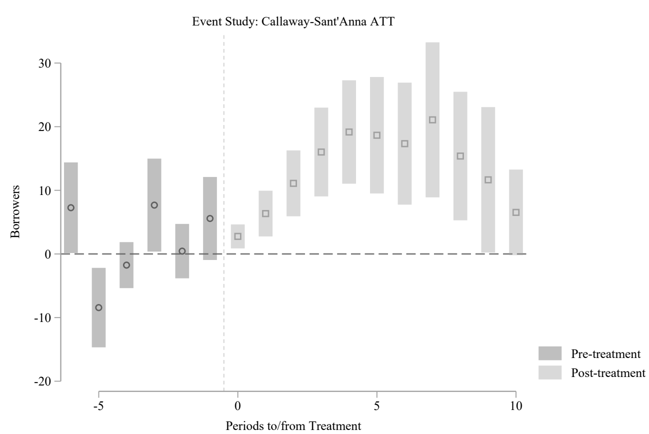
G.1 Agricultural Credit

G.1.1 Callaway-Sant'Anna

Figure G4: Dynamic Impact of Digitization on Agricultural Credit, Event Study (Callaway-Sant'Anna)



(a) Disbursement

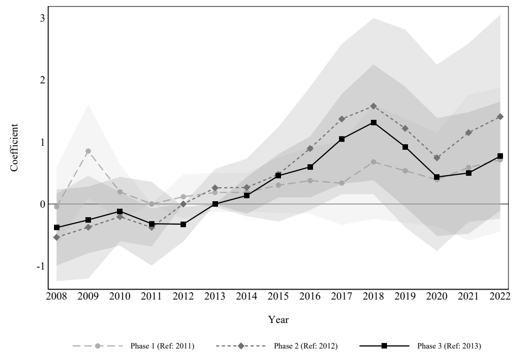


(b) Borrowers

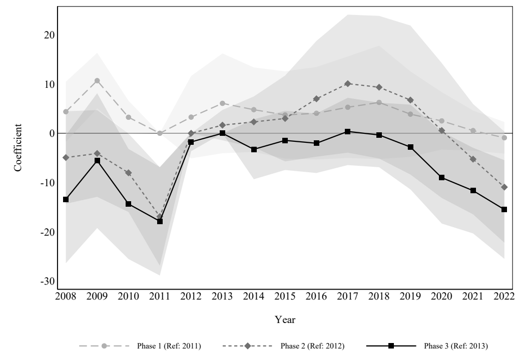
Notes: This figure plots the Callaway-Sant'Anna treatment effect estimates and corresponding 95% confidence intervals for the impact of digitization.

G.1.2 Farm Sector, Farmer Size

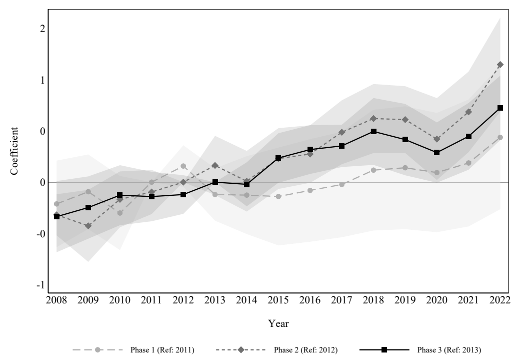
Figure G5: Dynamic Impact of Digitization on Agricultural Credit by Farm Size, Event Study



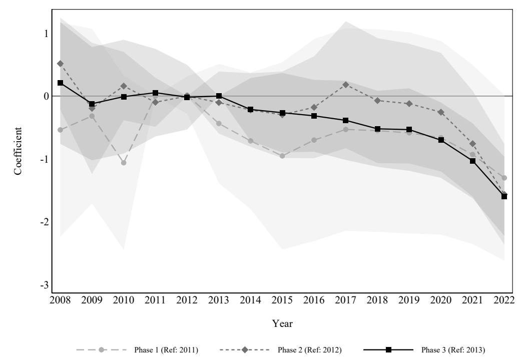
(a) Disbursement, Farm Sector (Subsistence)



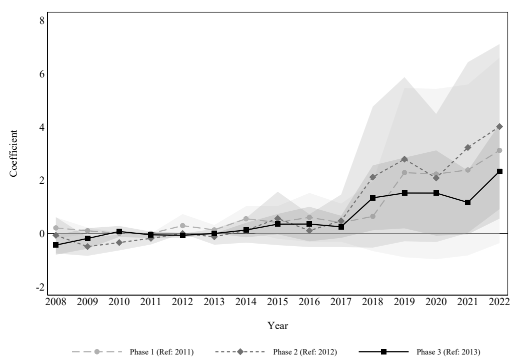
(b) Borrowers, Farm Sector (Subsistence)



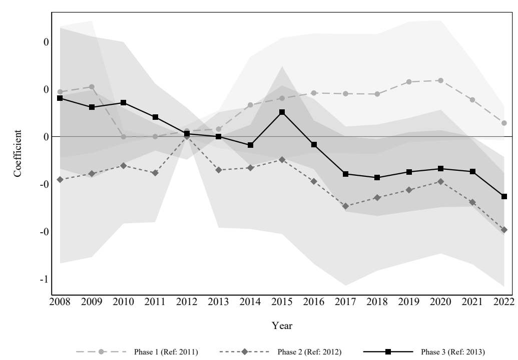
(c) Disbursement, Farm Sector (Economic)



(d) Borrowers, Farm Sector (Economic)



(e) Disbursement, Farm Sector (Above Economic)

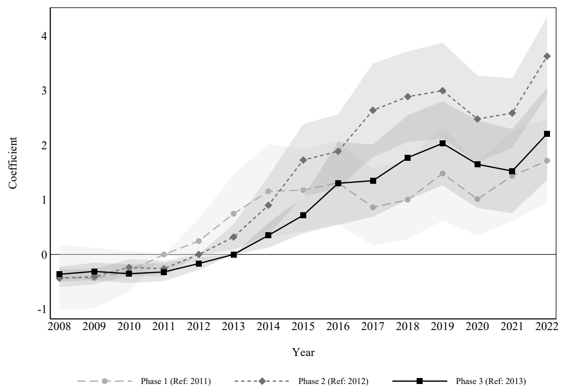


(f) Borrowers, Farm Sector (Above Economic)

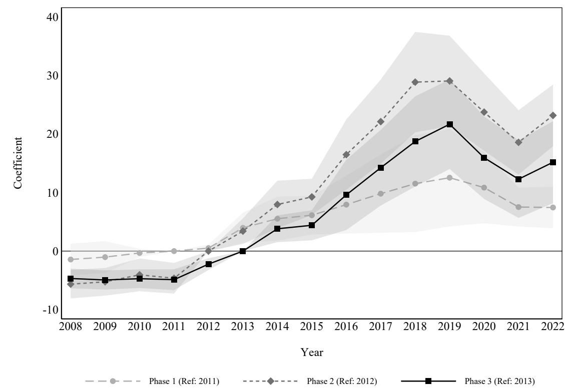
Notes: This figure plots the dynamic treatment effect estimates and corresponding 95% confidence intervals for the impact of digitization by every phase. The model includes district and year fixed effects with standard errors clustered at the district level.

G.1.3 Non-Farm Sector, Landholding Size

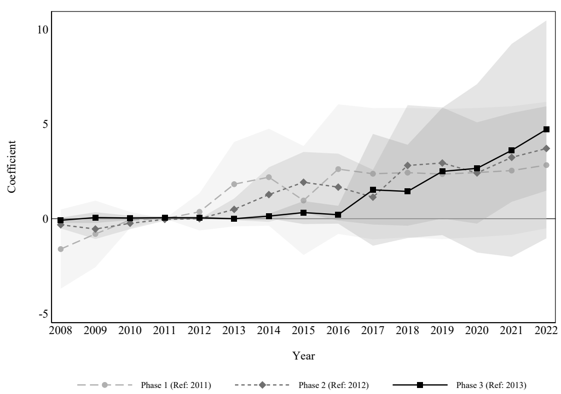
Figure G6: Dynamic Impact of Digitization on Agricultural Credit by Landholding Size, Event Study



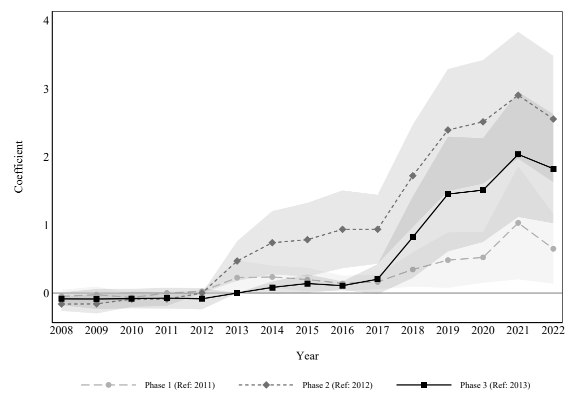
(a) Disbursement, Non-Farm Sector (Small)



(b) Borrowers, Non-Farm Sector (Small)



(c) Disbursement, Non-Farm Sector (Large)

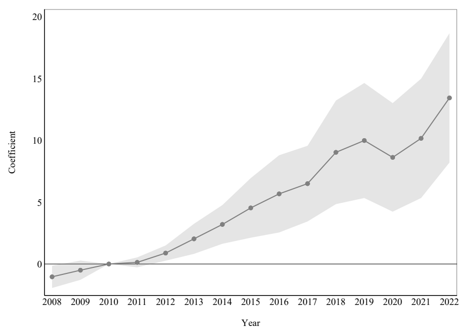


(d) Borrowers, Non-Farm Sector (Large)

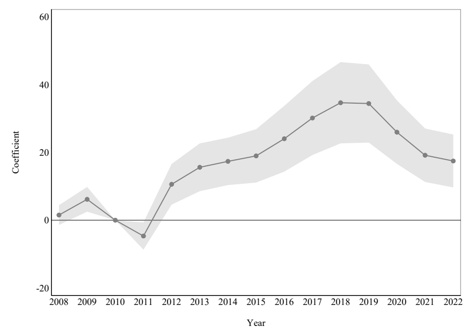
Notes: This figure plots the dynamic treatment effect estimates and corresponding 95% confidence intervals for the impact of digitization by every phase. The model includes district and year fixed effects with standard errors clustered at the district level.

G.1.4 Agricultural Credit, Pooled

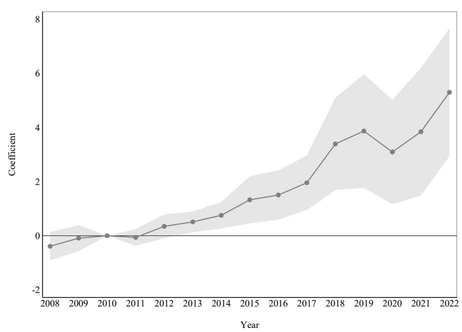
Figure G7: Dynamic Impact of Digitization on Agricultural Credit, Event Study (Pooled)



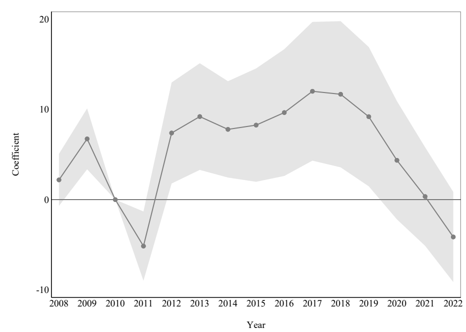
(a) Total Disbursement



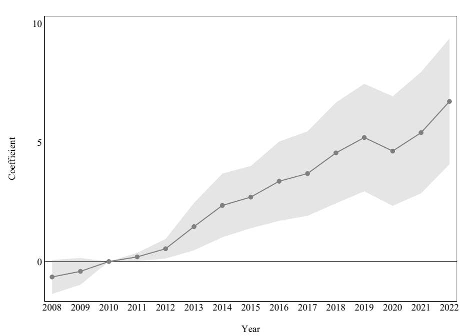
(b) Total Borrowers



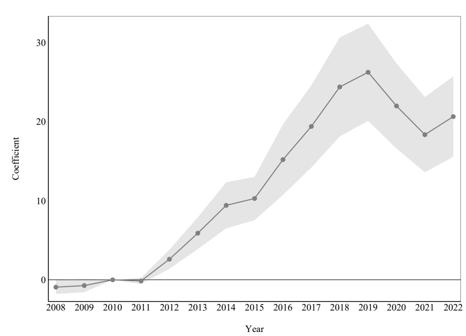
(c) Disbursement, Farm Sector



(d) Borrowers, Farm Sector



(e) Disbursement, Non-Farm Sector



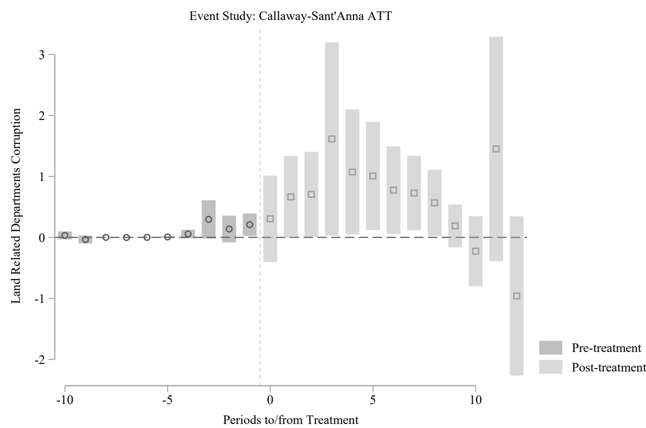
(f) Borrowers, Non-Farm Sector

Notes: This figure plots the dynamic treatment effect estimates and corresponding 95% confidence intervals for the impact of digitization using a pooled model across all phases with 2011 omitted as the baseline reference year. The model includes district and year fixed effects with standard errors clustered at the district level.

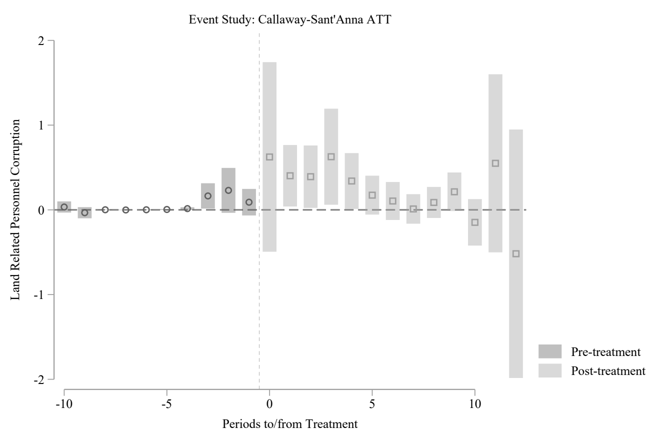
G.2 Corruption

G.2.1 Callaway-Sant'Anna

Figure G8: Dynamic Impact of Digitization on Land Related Corruption, Event Study (Callaway-Sant'Anna)



(a) Incidents Land Departments

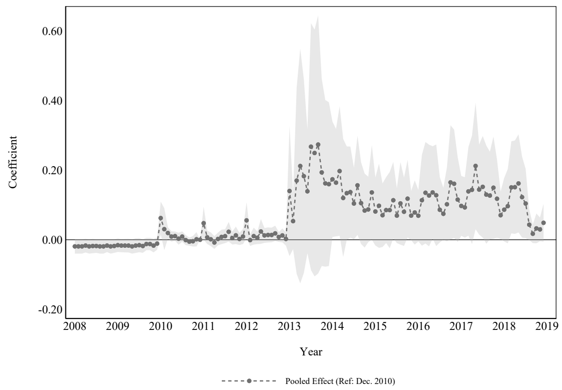


(b) Incidents Land Personnel

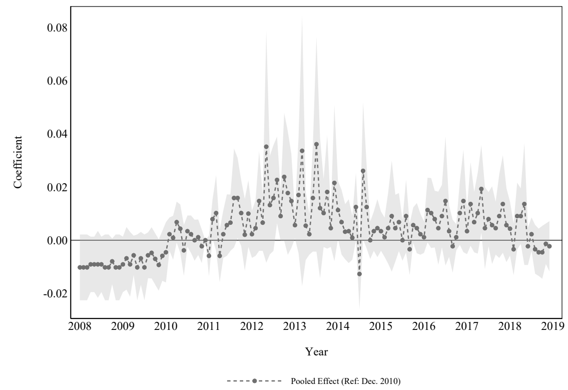
Notes: This figure plots the Callaway-Sant'Anna treatment effect estimates and corresponding 95% confidence intervals for the impact of digitization.

G.2.2 Conversion and Conviction

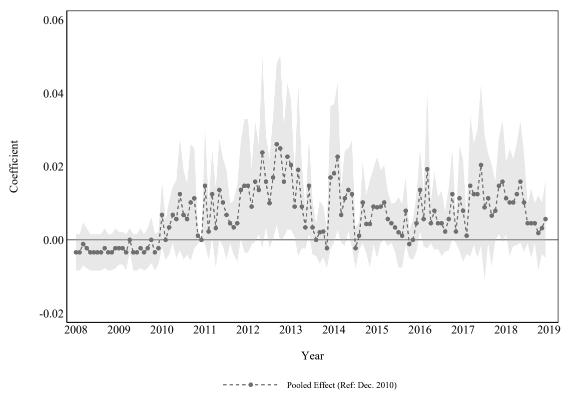
Figure G9: Dynamic Impact of Digitization on Land Related Corruption Incident Conversion, Event Study



(a) Complaints to Enquiries



(b) Enquiries to Cases

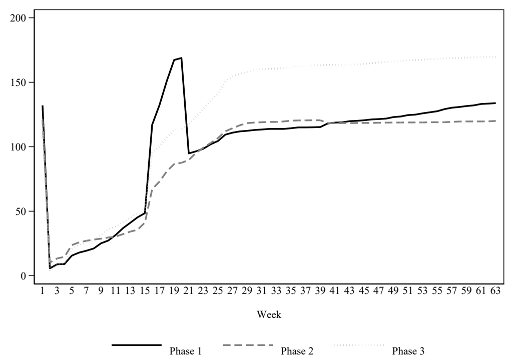


(c) Case Conviction

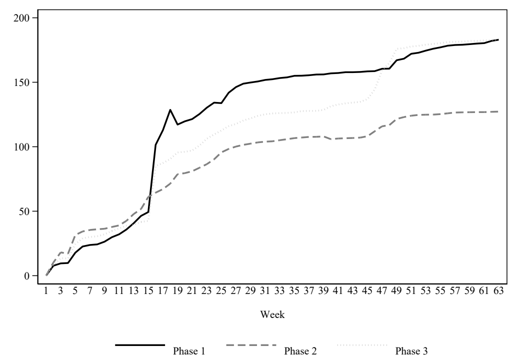
Notes: This figure plots the dynamic treatment effect estimates and corresponding 95% confidence intervals for the impact of digitization. The model includes mouza and month-year fixed effects with standard errors clustered at the mouza level.

G.2.3 Land Cases

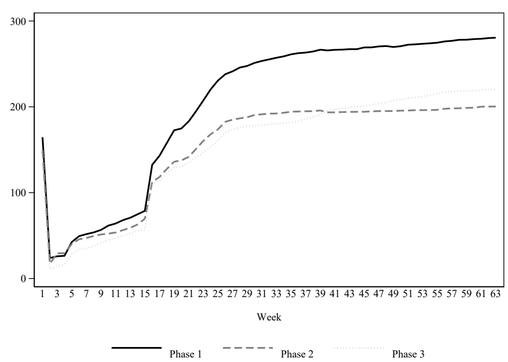
Figure G10: Trend in Land Related Court Cases by Type



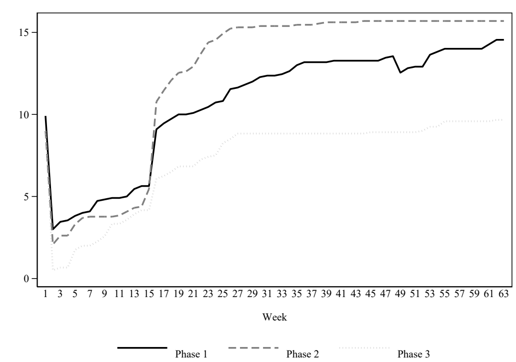
(a) Criminal Cases



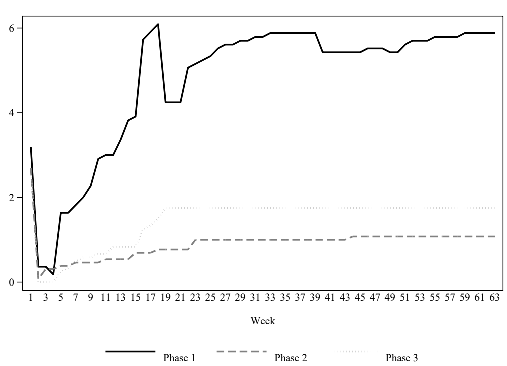
(b) Civil Cases



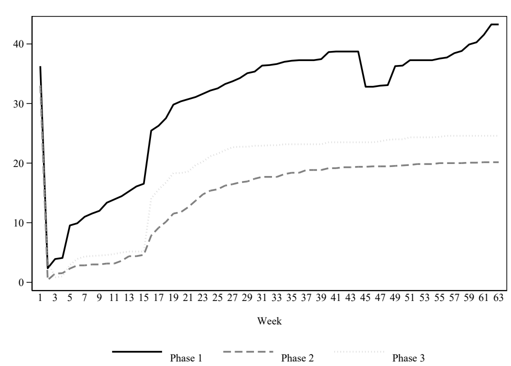
(c) Family Cases



(d) Guardian Cases



(e) Rent Cases



(f) Other Cases

Notes: This figure presents the trend of land-related cases in the high court using Lahore High Court land cases data.

H Mechanisms

H.1 Agricultural Credit

Table H24: How Digitization Increases Agricultural Credit by Sector

	Intensity (Average Loan Size) (1)	Reach (Borrowers Per Village) (2)	Efficiency (Recovery Rate) (3)	Debt Burden (Outstanding Borrowers) (4)
Panel A. Farm Sector				
Digitization Impact	-1.8636	0.0076	0.2124	-0.1860
	(0.9799)	(0.0040)	(0.0931)	(0.1127)
	[0.0579]	[0.0611]	[0.0230]	[0.0996]
Mean of dep. var.	1.2510	0.0074	1.0138	0.1118
SD of dep. var.	16.7865	0.0215	2.2931	1.8062
District-Cohort FE	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes
Observations	4,110	4,110	4,110	4,110
Panel B. Non-Farm Sector				
Digitization Impact	-0.8706	0.0225	0.9887	-0.0237
	(0.3637)	(0.0027)	(0.3588)	(0.0154)
	[0.0171]	[0.0000]	[0.0061]	[0.1239]
Mean of dep. var.	0.4550	0.0021	1.0635	0.0320
SD of dep. var.	3.1643	0.0083	8.7519	0.2583
District-Cohort FE	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes
Observations	4,110	4,110	4,110	4,110

Notes: The table reports stacked difference-in-differences estimates evaluating the effect of digitization across overall credit lines. Panel A uses the outcomes for the farm sector, whereas Panel B uses the outcomes of the non-farm sector. Column (1) reports credit intensity (average loan size), Column (2) reports extensive market reach (total active unique borrowers divided by local village counts), Column (3) captures banking operational efficiency (repayment recovery rate), and Column (4) isolates borrower portfolio depth (outstanding values per borrower balance). All models utilize the stacked difference-in-differences estimator with cohort-interacted district and year fixed effects. Standard errors clustered at the cohort-district level are reported in parentheses; p-values are in square brackets.

Table H25: How Digitization Increases Agricultural Credit by Farmer Size

	Farm Loans			Non-Farm Loans	
	(1)	(2)	(3)	(4)	(5)
	Subsistence Holding	Economic Holding	Above Economic Holding	Small Farmer	Large Farmer
Panel A: Average Loan Size					
Digitization	-0.6032	0.0852	-22.2884	-0.0445	-2.9915
	(0.3480)	(0.0395)	(14.0274)	(0.0623)	(3.6973)
	[0.0838]	[0.0314]	[0.1128]	[0.4758]	[0.4189]
Panel B: Credit Market Reach					
Digitization	0.0081	-0.0004	-0.0002	0.0214	0.0010
	(0.0041)	(0.0003)	(0.0001)	(0.0026)	(0.0002)
	[0.0472]	[0.1874]	[0.0289]	[0.0000]	[0.0000]
Panel C: Credit Recovery Rate					
Digitization	0.5074	0.1639	-0.1580	1.0219	-1.5674
	(0.1988)	(0.1311)	(0.1545)	(0.3617)	(0.7355)
	[0.0110]	[0.2118]	[0.3071]	[0.0050]	[0.0337]
Panel D: Outstanding Debt Burden					
Digitization	-0.1233	0.0855	-0.7233	-0.0027	0.0363
	(0.0762)	(0.0133)	(0.6430)	(0.0046)	(0.2145)
	[0.1064]	[0.0000]	[0.2613]	[0.5572]	[0.8658]
District-Cohort FE	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes
Observations	3,877	3,877	3,877	3,877	3,877

Notes: This table reports stacked difference-in-differences estimates evaluating the effect of digitization on structural and operational ratios across credit market segments. Columns (1) to (3) restrict the sample to agricultural (farm) loans, subdivided into subsistence, economic, and above-economic landholding classes. Columns (4) and (5) isolate agricultural (non-farm) loans, partitioned into small and large commercial categories. Panel A evaluates average loan size (total disbursement divided by unique borrower counts). Panel B measures market reach (unique borrower headcounts divided by total villages). Panel C tracks the credit recovery rate (total recovered volume divided by total disbursement). Panel D reports the outstanding debt burden (outstanding credit volume divided by active outstanding borrower counts). All dependent variables are evaluated in level units. Specifications are estimated via a stacked difference-in-differences model, incorporating cohort-interacted district and year fixed effects. The Mean and SD parameters are calculated on the active regression sample. Standard errors clustered at the cohort-district level are reported in parentheses; p-values are in square brackets.

H.2 Corruption

Table H26: Impact of Digitization on Bureaucratic Performance

	Records Accuracy		Bureaucratic Performance Metrics			
	Assessed Area	Tax Demand	% Collected	$\geq$ 50% Collected	$\geq$ 75% Collected	Zero Collection Months
	(1)	(2)	(3)	(4)	(5)	(6)
Digitization	-0.1004	-0.5998	-35.4153	-0.3945	-0.4170	0.2629
	(0.0338)	(0.2106)	(11.5239)	(0.1280)	(0.1224)	(0.1158)
	[0.0053]	[0.0074]	[0.0045]	[0.0044]	[0.0019]	[0.0305]
Mean of dep. var.	1,092.8367	9.6956	58.0957	0.5954	0.4671	0.1612
SD of dep. var.	439.5656	1.2140	38.9209	0.4916	0.4997	0.3683
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Fiscal-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	214	203	304	304	304	304

Notes: This table reports district-year level panel estimates evaluating the institutional mechanisms behind land record digitization. Columns (1) and (2) are the log-transformed variables and look at land administration records accuracy via assessed cultivated acreage and official revenue demands. Columns (3) through (6) trace changes in fiscal revenue recovery metrics. Standard errors clustered at the district level are reported in parentheses; p-values are in square brackets.

Table H27: Impact of Digitization on Land Related Corruption by Departments

	Revenue Department			Local Government		
	Complaints (1)	Enquiries (2)	Cases (3)	Complaints (4)	Enquiries (5)	Cases (6)
Digitization	0.0006	0.0597	0.0186	0.0000	0.0237	0.0090
	(0.0003)	(0.0264)	(0.0076)	(0.0000)	(0.0119)	(0.0044)
	[0.0361]	[0.0235]	[0.0138]	[0.2417]	[0.0457]	[0.0401]
Mean of dep. var.	0.0002	0.0191	0.0067	0.0000	0.0081	0.0040
SD of dep. var.	0.0206	0.5734	0.2324	0.0053	0.3162	0.2357
Mouza-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	724,320	724,320	724,320	724,320	724,320	724,320

Notes: This table shows the effects of digitization on anti-corruption activity recorded by the Punjab Anti-Corruption Establishment. Columns (1) through (3) track administrative processing volumes for the revenue department, split into total complaints filed, formal enquiries initiated, and cases registered. Columns (4) through (6) evaluate the same structural progression tracking incidents where the primary target is a local government. All outcomes are evaluated in original level metrics. Estimation is stacked difference-in-differences with cohort-interacted mouza and year-month fixed effects. Means and standard deviations correspond directly to active regression samples. Standard errors in parentheses are clustered at the cohort-mouza level, with p-values provided inside square brackets.

Table H28: Impact of Digitization on Land Related Corruption by Individuals

	Currently Employed			Retired		
	Complaints (1)	Enquiries (2)	Cases (3)	Complaints (4)	Enquiries (5)	Cases (6)
Digitization	0.2303	0.2239	0.0718	0.0003	0.0009	0.0003
	(0.1083)	(0.1026)	(0.0295)	(0.0001)	(0.0004)	(0.0001)
	[0.0335]	[0.0291]	[0.0151]	[0.0300]	[0.0160]	[0.0489]
Mean of dep. var.	0.0833	0.0731	0.0288	0.0001	0.0003	0.0001
SD of dep. var.	2.2455	2.1850	0.8782	0.0113	0.0193	0.0117
Mouza-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	724,320	724,320	724,320	724,320	724,320	724,320

Notes: This table shows the effects of digitization on anti-corruption activity recorded by the Punjab Anti-Corruption Establishment. Columns (1) through (3) track administrative processing volumes for the number of currently employed individuals included in the reported incident, split into total complaints filed, formal enquiries initiated, and cases registered. Columns (4) through (6) evaluate the same structural progression tracking incidents for number of retired individuals involved. All outcomes are evaluated in original level metrics. Estimation is a stacked difference-in-differences with cohort-interacted mouza and year-month fixed effects. Means and standard deviations correspond directly to active regression samples. Standard errors listed in parentheses are clustered at the cohort-mouza level, with p-values provided inside square brackets.

Table H29: Impact of Digitization on Land Related Corruption by Grade

	Low Grade (1)	Middle Grade (2)	High Grade (3)	Contract Private Temporary (4)	Missing Grade (5)
Panel A: Enquiries					
Digitization	0.0724	0.0758	0.0287	0.0407	0.0063
	(0.0336)	(0.0354)	(0.0131)	(0.0182)	(0.0033)
	[0.0311]	[0.0325]	[0.0285]	[0.0254]	[0.0533]
Mean of dep. var.	0.0238	0.0249	0.0094	0.0128	0.0022
SD of dep. var.	0.7658	0.8202	0.3481	0.4581	0.3413
Mouza-Cohort FE	Yes	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes	Yes	Yes
Observations	724,320	724,320	724,320	724,320	724,320
Panel B: Cases					
Digitization	0.0167	0.0179	0.0056	0.0298	0.0018
	(0.0070)	(0.0077)	(0.0026)	(0.0122)	(0.0010)
	[0.0168]	[0.0200]	[0.0324]	[0.0144]	[0.0785]
Mean of dep. var.	0.0076	0.0072	0.0027	0.0105	0.0008
SD of dep. var.	0.2404	0.2479	0.1559	0.4148	0.0658
Mouza-Cohort FE	Yes	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes	Yes	Yes
Observations	724,320	724,320	724,320	724,320	724,320

Notes: This table shows the effect of digitization on anti-corruption activity recorded by the Punjab Anti-Corruption Establishment. Columns (1) through (3) track the enquiries and cases by individual grades called Basic Pay Scale (BPS). The low-grade employees are the supporting staff, the middle-grade are the admin staff, and the high-grade are the officers. Panel A shows the number of enquiries, while Panel B shows the number of cases. Due to missing information in initial complaints, the grades cannot be identified in the complaints data. All outcomes are evaluated in original level metrics. Estimation is a stacked difference-in-differences with cohort-interacted mouza and year-month fixed effects. Means and standard deviations correspond directly to active regression samples. Standard errors listed in parentheses are clustered at the cohort-mouza level, with p-values provided in square brackets.

I OLS

I.1 Agricultural Credit

I.1.1 Stacked

Table I30: Impact of Digitization on Agricultural Credit, Stacked OLS

	Number of Borrowers			Disbursed Amount		
	Total	Farm Sector	Non-Farm	Total	Farm Sector	Non-Farm
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Outcome in Level						
Digitization	24.9663	7.6212	18.7089	6.6898	2.2808	3.9930
	(4.4439)	(3.2954)	(2.3262)	(1.2985)	(0.4996)	(0.8745)
	[0.0000]	[0.0212]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Mean of dep. var.	8.54	6.47	2.04	1.80	1.13	0.62
SD of dep. var.	19.49	15.06	5.89	5.60	2.72	3.04
Panel B: Outcome in Log						
Digitization	0.3814	0.1257	1.2719	0.5039	0.2681	0.8224
	(0.0765)	(0.0908)	(0.0918)	(0.0572)	(0.0567)	(0.0876)
	[0.0000]	[0.1669]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Mean of dep. var.	1.13	0.94	0.57	0.52	0.43	0.21
SD of dep. var.	1.34	1.27	0.82	0.79	0.67	0.50
Panel C: Outcome in IHS						
Digitization	0.3789	0.1277	1.4208	0.5540	0.3054	1.0701
	(0.0799)	(0.0973)	(0.0997)	(0.0649)	(0.0697)	(0.1073)
	[0.0000]	[0.1901]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Mean of dep. var.	1.38	1.16	0.71	0.66	0.54	0.26
SD of dep. var.	1.61	1.53	1.02	0.99	0.86	0.62
District-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,110	4,110	4,110	4,110	4,110	4,110

Notes: This table reports OLS estimates using stacked panel data. Columns (1) to (3) examine the total number of borrowers, while Columns (4) to (6) capture the amount disbursed. Panel A evaluates dependent variables in level units, i.e., amount in billion PKR and number of borrowers in thousands. Panel B applies a log transformation, $\log(1 + y_{dt})$. Panel C utilizes the inverse hyperbolic sine transformation, $\text{asinh}(y_{dt}) = \log(y_{dt} + \sqrt{y_{dt}^2 + 1})$. All models include cohort-interacted district and year fixed effects. Mean and SD parameters are calculated directly on the active estimation sample for each panel. Standard errors clustered at the cohort-district level are reported in parentheses; p-values are in square brackets.

Table I31: Impact of Digitization on Agricultural Credit by Farmer Size, Stacked OLS

	Farm Loans			Non-Farm Loans	
	(1)	(2)	(3)	(4)	(5)
	Subsistence Holding	Economic Holding	Above Economic Holding	Small Farmer	Large Farmer
Panel A: Total Disbursed Credit					
Digitization	0.9921	0.3887	0.9000	1.9991	1.9939
	(0.2811)	(0.0663)	(0.3030)	(0.2539)	(0.7472)
	[0.0005]	[0.0000]	[0.0031]	[0.0000]	[0.0079]
Mean of dep. var.	0.59	0.27	0.26	0.22	0.40
SD of dep. var.	1.32	0.56	1.35	0.65	2.70
Panel B: Credit Recovery Volumes					
Digitization	0.9090	0.2930	0.5994	1.7777	1.5904
	(0.2472)	(0.0506)	(0.2473)	(0.2193)	(0.6697)
	[0.0003]	[0.0000]	[0.0158]	[0.0000]	[0.0180]
Mean of dep. var.	0.58	0.27	0.24	0.18	0.35
SD of dep. var.	1.28	0.54	1.26	0.56	2.38
Panel C: Total Borrowers					
Digitization	8.1532	-0.4304	-0.1016	17.8330	0.8759
	(3.2953)	(0.1771)	(0.0590)	(2.2576)	(0.1690)
	[0.0138]	[0.0155]	[0.0857]	[0.0000]	[0.0000]
Mean of dep. var.	5.75	0.61	0.11	1.97	0.07
SD of dep. var.	13.92	1.22	0.27	5.68	0.32
Panel D: Outstanding Borrowers					
Digitization	18.7160	1.0216	0.1218	15.5883	0.7601
	(2.5940)	(0.1152)	(0.0165)	(2.1522)	(0.1878)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0001]
Mean of dep. var.	1.01	0.07	0.01	0.92	0.04
SD of dep. var.	6.22	0.36	0.05	5.26	0.35
Panel E: Total Outstanding Amount					
Digitization	1.2152	0.1770	0.1485	1.8477	0.5628
	(0.2510)	(0.0539)	(0.0661)	(0.2412)	(0.1448)
	[0.0000]	[0.0011]	[0.0252]	[0.0000]	[0.0001]
Mean of dep. var.	0.59	0.21	0.09	0.25	0.10
SD of dep. var.	1.24	0.41	0.29	0.64	0.58
District-Cohort FE	Yes	Yes	Yes	Yes	Yes
Year-Cohort FE	Yes	Yes	Yes	Yes	Yes
Observations	4,110	4,110	4,110	4,110	4,110

Notes: This table reports OLS estimated using stacked panel data. Panels A, B, C, D, and E report results for total disbursed credit, credit recovery volumes, total borrowers, outstanding borrower counts, and total outstanding credit volumes, respectively. All dependent variables are evaluated in level units. All models include cohort-interacted district and year fixed effects. Standard errors clustered at the cohort-district level are reported in parentheses; p-values are in square brackets.

I.1.2 Unstacked

Table I32: Impact of Digitization on Agricultural Credit, Unstacked OLS

	Number of Borrowers			Disbursed Amount		
	Total	Farm Sector	Non-Farm	Total	Farm Sector	Non-Farm
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Outcome in Level						
Digitization	23.1348	3.6379	20.3433	8.9859	3.1456	4.8856
	(4.4488)	(3.0240)	(2.4939)	(2.0370)	(0.7721)	(1.1098)
	[0.0000]	[0.2309]	[0.0000]	[0.0000]	[0.0001]	[0.0000]
Mean of dep. var.	13.28	8.86	4.41	3.65	1.95	1.52
SD of dep. var.	26.03	17.83	10.14	9.60	4.23	5.28
Panel B: Outcome in Log						
Digitization	0.3562	0.0166	1.3670	0.5865	0.3454	0.9126
	(0.0849)	(0.0981)	(0.0975)	(0.0734)	(0.0738)	(0.0961)
	[0.0000]	[0.8659]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Mean of dep. var.	1.42	1.17	0.86	0.77	0.61	0.40
SD of dep. var.	1.52	1.40	1.10	1.02	0.84	0.74
Panel C: Outcome in IHS						
Digitization	0.3547	0.0144	1.5275	0.6404	0.3945	1.1782
	(0.0927)	(0.1085)	(0.1093)	(0.0816)	(0.0897)	(0.1170)
	[0.0002]	[0.8947]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Mean of dep. var.	1.72	1.42	1.06	0.96	0.77	0.50
SD of dep. var.	1.79	1.67	1.34	1.26	1.05	0.92
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,235	2,235	2,235	2,235	2,235	2,235

Notes: This table reports OLS estimates using unstacked panel data. Columns (1) to (3) examine the total number of borrowers, while Columns (4) to (6) capture the amount disbursed. Panel A evaluates dependent variables in level units, i.e., amount in billions PKR and number of borrowers in thousands. Panel B applies a log transformation, $\log(1 + y_{dt})$. Panel C utilizes the inverse hyperbolic sine transformation, $\text{asinh}(y_{dt}) = \log(y_{dt} + \sqrt{y_{dt}^2 + 1})$. All models include district and year fixed effects. Mean and SD parameters are calculated directly on the active estimation sample for each panel. Standard errors clustered at the district level are reported in parentheses; p-values are in square brackets.

Table I33: Impact of Digitization on Agricultural Credit by Farmer Size, Unstacked OLS

	Farm Loans			Non-Farm Loans	
	(1)	(2)	(3)	(4)	(5)
	Subsistence Holding	Economic Holding	Above Economic Holding	Small Farmer	Large Farmer
Panel A: Total Disbursed Credit					
Digitization	0.9773	0.5124	1.6558	2.1673	2.7183
	(0.3273)	(0.0814)	(0.5313)	(0.2547)	(1.0175)
	[0.0033]	[0.0000]	[0.0022]	[0.0000]	[0.0084]
Mean of dep. var.	0.94	0.41	0.60	0.51	1.00
SD of dep. var.	1.80	0.74	2.35	1.13	4.71
Panel B: Credit Recovery Volumes					
Digitization	0.8930	0.4217	1.3528	1.9493	2.1558
	(0.2962)	(0.0667)	(0.4595)	(0.2283)	(0.8625)
	[0.0030]	[0.0000]	[0.0038]	[0.0000]	[0.0135]
Mean of dep. var.	0.90	0.40	0.54	0.44	0.87
SD of dep. var.	1.73	0.70	2.19	0.99	4.09
Panel C: Total Borrowers					
Digitization	4.4327	-0.6643	-0.1305	19.0230	1.3203
	(3.0165)	(0.1856)	(0.0562)	(2.3501)	(0.2033)
	[0.1438]	[0.0005]	[0.0216]	[0.0000]	[0.0000]
Mean of dep. var.	7.98	0.74	0.14	4.18	0.22
SD of dep. var.	16.62	1.33	0.29	9.60	0.72
Panel D: Outstanding Borrowers					
Digitization	24.2368	1.2842	0.1728	22.5400	1.6077
	(3.2249)	(0.1383)	(0.0209)	(2.6387)	(0.2887)
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
Mean of dep. var.	3.57	0.23	0.03	3.49	0.23
SD of dep. var.	11.89	0.64	0.09	10.71	0.94
Panel E: Total Outstanding Amount					
Digitization	1.2249	0.2169	0.2865	2.1504	0.9786
	(0.2903)	(0.0601)	(0.0873)	(0.2603)	(0.2269)
	[0.0000]	[0.0004]	[0.0013]	[0.0000]	[0.0000]
Mean of dep. var.	0.94	0.31	0.18	0.55	0.27
SD of dep. var.	1.70	0.52	0.51	1.15	1.05
District FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	2,235	2,235	2,235	2,235	2,235

Notes: This table reports OLS estimated using unstacked panel data. Panels A, B, C, D, and E report results for total disbursed credit, credit recovery volumes, total borrowers, outstanding borrower counts, and total outstanding credit volumes, respectively. All dependent variables are evaluated in level units. All models include district and year fixed effects. Standard errors clustered at the district level are reported in parentheses; p-values are in square brackets.

I.2 Corruption

I.2.1 Stacked

Table I34: Impact of Digitization on Land Related Corruption, Stacked OLS

	Land Departments				Land Personnel			
	Any Incident (1)	Complaints (2)	Enquiries (3)	Cases (4)	Any Incident (5)	Complaints (6)	Enquiries (7)	Cases (8)
Digitization	-0.0418	0.0003	-0.0383	-0.0038	-0.0264	-0.0023	-0.0211	-0.0029
	(0.0688)	(0.0004)	(0.0581)	(0.0113)	(0.0314)	(0.0032)	(0.0287)	(0.0047)
	[0.5433]	[0.3915]	[0.5093]	[0.7370]	[0.4019]	[0.4730]	[0.4617]	[0.5400]
Mean of dep. var.	0.0382	0.0002	0.0273	0.0107	0.0217	0.0051	0.0127	0.0039
SD of dep. var.	1.0764	0.0212	0.8287	0.3844	0.6089	0.2413	0.3898	0.1422
Mouza-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	724,320	724,320	724,320	724,320	724,320	724,320	724,320	724,320

Notes: This table shows the estimated effects of the mouza digitization on anti-corruption activity recorded by the Punjab Anti-Corruption Establishment. Columns (1) through (4) track administrative processing volumes for land departments, split into total complaints filed, formal enquiries initiated, and criminal cases registered. Columns (5) through (8) evaluate the same structural progression tracking incidents where the primary target is land-related personnel. All outcomes are evaluated in original level metrics. Estimations deploy a stacked OLS with cohort-interacted mouza and year-month fixed effects. Means and standard deviations correspond directly to active regression samples. Standard errors in parentheses are clustered at the cohort-mouza level, with p-values provided in square brackets.

I.2.2 Unstacked

Table I35: Impact of Digitization on Land Related Corruption, Unstacked OLS

	Land Departments				Land Personnel			
	Any Incident (1)	Complaints (2)	Enquiries (3)	Cases (4)	Any Incident (5)	Complaints (6)	Enquiries (7)	Cases (8)
Digitization	-0.0358	0.0003	-0.0341	-0.0020	-0.0242	-0.0032	-0.0190	-0.0020
	(0.0951)	(0.0005)	(0.0802)	(0.0158)	(0.0436)	(0.0049)	(0.0397)	(0.0067)
	[0.7069]	[0.5670]	[0.6711]	[0.9004]	[0.5784]	[0.5074]	[0.6324]	[0.7686]
Mean of dep. var.	0.0411	0.0002	0.0291	0.0118	0.0234	0.0053	0.0137	0.0044
SD of dep. var.	1.0883	0.0212	0.8367	0.3940	0.6154	0.2413	0.3962	0.1493
Mouza FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	370,656	370,656	370,656	370,656	370,656	370,656	370,656	370,656

Notes: This table shows the estimated effects of the mouza digitization on anti-corruption activity recorded by the Punjab Anti-Corruption Establishment. Columns (1) through (4) track administrative processing volumes for land departments, split into total complaints filed, formal enquiries initiated, and criminal cases registered. Columns (5) through (8) evaluate the same structural progression tracking incidents where the primary target is land-related personnel. All outcomes are evaluated in original level metrics. Estimations deploy an unstacked OLS with mouza and year-month fixed effects. Means and standard deviations correspond directly to active regression samples. Standard errors in parentheses are clustered at the mouza level, with p-values provided in square brackets.