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Spillovers in State Capacity Building: Evidence from the Digitization of Land Records in Pakistan[†]

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Digitization reforms have been hailed as an effective way of strengthening state capacity. However, digitization can also fundamentally reshape the organization of bureaucracies. Using a unique administrative dataset on agricultural taxation and surveys of local bureaucrats from Punjab, Pakistan, we show that digitization reforms can have unintended consequences for state capacity. We exploit the staggered rollout of the digitization of land records in Punjab to show that digitization had a negative effect on tax collection. The fall in taxes was not due to a decrease in the tax base. Instead, digitization affected the bureaucrats' capacity to collect taxes. (*JEL* D73, H11, H71, O12, O17)

Strong state capacity is essential for economic development. An effective approach to strengthening it is to introduce technology in bureaucracies. In addition to easing market frictions (Beg 2022a), technology can improve the productivity of bureaucrats and address a range of asymmetric information issues. It has helped to reduce agency problems between bureaucrats and their principals (Duflo, Hanna, and Ryan 2012; Lewis-Faupel et al. 2016; Callen et al. 2020; Dal Bó et al. 2021; Debnath, Nilayamgode, and Sekhri 2023), to improve the reliability of information on taxpayers (Ali et al. 2021; Okunogbe and Pouliquen 2022; Brockmeyer and Sáenz Somarriba 2025; Dzansi et al. 2022), and to identify welfare recipients (Muralidharan et al. 2016).

However, the introduction of technology also forces a restructuring of bureaucracies. As public administration scholars have noted, digitization "reconfigures

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public sector organizations in fundamental, although uneven, ways" (Plesner, Justesen, and Glerup 2018, p. 1177). Digitization reforms can change the relationships between different bureaucratic agencies (Di Giulio and Vecchi 2023) and increase specialization (Gundhus, Talberg, and Wathne 2022a). These changes can affect functions not directly targeted by the introduction of technology. The reorganization of these functions can impact bureaucrats' sense of autonomy and their relationship with the public (Pors and Pallesen 2021) or result in the displacement of corruption onto other activities (Yang 2008; Muralidharan, Niehaus, and Sukhtankar 2025). Whether these changes can limit the benefits of technological reforms remains an open question.

In this paper, we seek to understand whether the organizational changes brought about by the introduction of technology in bureaucracies can weaken state capacity. We study this question in the context of the digitization of land records in Punjab, Pakistan, and show that the reform had a negative impact on the ability of the state to collect taxes. This negative relationship is not due to the direct effect of the digitization reform on the tax base but to its indirect effect on the behavior of bureaucrats collecting taxes.

Digitizing land records is a popular way of leveraging technology to strengthen state capacity. From 2010 to 2019, 52 economies computerized their land registries both in developing and developed countries, using significant resources in the process (World Bank 2019). In most countries, these reforms have also resulted in important bureaucratic reorganizations.[1]

To study the impact of the bureaucratic reorganization induced by digitization reforms, we exploit the staggered rollout of the digitization of land records across districts of Punjab. Since this reform was carried out in three phases between 2011 and 2014, we use a difference-in-differences design to identify the causal effect of the digitization reform on the amount of tax collected by the state. We digitized a novel administrative dataset of rural agricultural taxes, which we combine with data on the rollout of the reform, to test this effect. We complement these data with satellite data on vegetation cover, survey data from local farmers, and unique data on the career trajectory of individual bureaucrats to separate the effect of the reform on the tax base from its effect on the bureaucrats' performance.

We begin by documenting how the digitization reform affected the bureaucracy. First, bureaucrats who were in charge of tax assessment, tax collection, and land records management before the reform were no longer responsible for land records after it. Second, a large portion of bureaucrats (46 percent) reported that digitization negatively impacted tax collection. Of those, 64 percent reported that this was due to lower influence on taxpayers. Finally, bureaucrats lost a lucrative source of bribes: The proportion of bureaucrats who agreed that citizens bribed officials for land titles dropped from 48 percent to 33 percent after digitization.

We then show our main result: The digitization reform had a significant impact on the state's ability to collect taxes. The digitization of land records led to a 47 percent decrease in tax collection in districts in the first two phases of the

[1] For example, the computerization of Denmark's land registry in 2011 was accompanied by the centralization of 82 separate registration offices in charge of registering not only land records but also other legal services such as marriage contracts (Nielsen and Kristiansen 2008). Similarly, the Digital India Land Records Modernization Program both computerized land records and integrated land record services with registration services. See https://dolr.gov.in/programmes-schemes/dilrmp-2/.

program relative to those in the third phase, which were not yet digitized. The modernization of state capacity therefore did not translate into higher tax revenues for the state, but actually reduced them. These results remain robust when using different definitions of the timing of digitization and when using a "stacked regression" approach to avoid biases arising from treatment effect heterogeneity in staggered difference-in-differences designs (Cengiz et al. 2019; Deshpande and Li 2019; Borusyak, Jaravel, and Spiess 2024).

A decrease in tax revenue does not necessarily indicate a decrease in fiscal capacity. It is possible that the tax base decreased while the ability to collect taxes remained unchanged. We show that this was not the case. The tax we study is levied on farmers based on cultivated area or profits, and we find that the reform had a positive but not statistically significant effect on farm profits and a small insignificant effect on cultivated area. Existing studies of this reform (Beg 2022a; Ullah and Hussain 2023) have found positive effects of the reform on farmer productivity and land disputes resolution, in line with studies showing that digitization is a positive force for development (Muralidharan, Niehaus, and Sukhtankar 2016; Dzansi et al. 2022). The direct effect of digitization therefore cannot explain the fall in fiscal revenues.

Instead, we show that the decrease in tax revenues is driven by a change in the bureaucrats' performance. The reform created two main opposing forces that affected their performance. On the one hand, the introduction of technology freed up some time for bureaucrats to focus on tax collection. On the other hand, it changed their relationship with taxpayers, as reported in the survey. First, bureaucrats lost a lucrative source of bribes from land record management. If some of these bribes were displaced towards their tax assessment activities after the reform, then collusion between bureaucrats and taxpayers should increase, leading to lower tax demands and lower tax revenues. Second, by losing responsibility over land record management, bureaucrats lost leverage over taxpayers. Before the reform, tax collectors could offer to process land permits or resolve land disputes in exchange for tax payments. After the reform, this was no longer possible. This loss of influence could therefore lead to lower tax collection as a fraction of tax demands.

We find results consistent with both of these negative forces. First, bureaucrats in digitized districts reported lower cultivated areas in their tax assessments and issued lower tax demands after digitization relative to nondigitized districts. This is despite the fact that we find no significant decrease in the tax base using satellite and household survey data. Second, bureaucrats in the digitized districts collected 35.4 percentage points lower taxes as a percentage of tax demands after digitization. This corresponds to about 66 percent of the average tax collection performance before digitization. The proportion of bureaucrats collecting at least 50 percent of their target and the proportion collecting at least 75 percent both fell, and the share of months during which no tax was collected increased. In sum, the digitization reform both led bureaucrats to issue lower tax demands and to collect a smaller portion of these lower demands.

While the timing of the reform only allows us to estimate causal effects of the reform on tax collection for up to two years after the beginning of the reform, we can compare tax collection across lower geographical units (where the timing of digitization varies by up to five years) to understand the persistence of the effect. The point estimates suggest that the negative effect on tax collection might have persisted for up to five years, though the effects are noisier in later years as we

have less power. We also find that agricultural tax collection in Punjab was 33 percent lower five years after the start of the reform compared to the neighboring province of Sindh that did not digitize its land records and saw a 4.5 times increase in agricultural tax.

Our results highlight a novel channel through which digitization reforms can affect state revenues. While technology did have a positive impact on the tax base and improved service delivery, in line with the existing literature (Muralidharan, Niehaus, and Sukhtankar 2016; Beg 2022a; Dzansi et al. 2022), it also reshaped the relationship between the bureaucracy and its users, which reduced its ability to collect taxes. We find that this second effect, often overlooked in the literature, can be sufficiently strong to generate an overall decline in tax collection. Two features of the context we study might be important to explain these results. One is the influence that bureaucrats exerted over the population and that allowed them to use informal arrangements to enforce taxes. Another is the multiplicity of tasks, which implies that partial digitization of their activities can have spillover effects on other tasks. Digitization reforms are therefore less likely to have unintended consequences in settings where enforcement is formalized (e.g., through audits and courts), where bureaucrats have little leverage over the population (e.g., if most state functions are already digitized), or where bureaucrats have a narrow scope of activities.

These findings have important implications for the design of state capacity reforms. First, reforms to different dimensions of state capacity cannot be evaluated in isolation as they can remove existing complementarities between tasks. Second, investments in technology alone may not be sufficient to improve overall state capacity since the human dimension of the bureaucracy can be affected by these investments. Digitization reforms should therefore consider alternative means for bureaucrats to maintain social connections, or consider changes to human resources policies such as corruption monitoring or incentive schemes.

Our results contribute to three strands of literature: the literature on digitization and development, the literature on state capacity and bureaucracies, and the literature on public finance in developing countries.

We contribute to the rapidly growing literature that examines the effects of technology on economic development (Aker and Mbiti 2010; Fujiwara 2015; Suri 2017) by showing that digitization can have unintended consequences on state capacity. A strand of that literature has focused on the direct effect of technology on the productivity or accountability of bureaucrats (Duflo, Hanna, and Ryan 2012; Lewis-Faupel et al. 2016; Callen et al. 2020; Dal Bó et al. 2021, 2021; Muralidharan et al. 2021; Callen et al. 2023; Debnath, Nilayamgode, and Sekhri 2023; Barnwal 2024; Muralidharan, Niehaus, and Sukhtankar 2025; Dodge et al. 2025). Other studies have found beneficial effects of introducing technology on tax collection. The technology studied either helped improve corporate tax filing (Okunogbe and Pouliquen 2022), value-added tax records (Ali et al. 2021; Brockmeyer and Sáenz Somarriba 2025; Fan et al. 2024), or customs tax (Chalendard et al. 2023); helped identify taxpayers and welfare recipients (Muralidharan et al. 2016); or helped tax collectors geolocate taxpayers (Dzansi et al. 2022). Unlike studies documenting negative consequences of technology on government transfers (Banerjee et al. 2020; Muralidharan, Niehaus, and Sukhtankar 2025) or tax collection (Okunogbe and Pouliquen 2022; Chalendard et al. 2023), the reform we study

was not primarily aimed at improving public finances. Instead, it had an indirect negative effect on tax collection through the reorganization of the bureaucracy that it induced. Our work is therefore most closely related to studies that highlight the importance of organizational or management practices in the success of technological reforms (Milgrom and Roberts 1990; Banerjee, Duflo, and Glennerster 2008; Atkin et al, 2017). Garicano and Heaton (2010) show that the introduction of IT systems in police stations only resulted in higher productivity when coupled with other organizational changes such as resource allocation and management practices. Our results are consistent with a similar "complementarity" hypothesis: Fiscal capacity can suffer from digitization reforms if no further organizational changes are introduced.

We contribute to the literature on state capacity building (Besley and Persson 2009, 2010; Bardhan 2016; Page and Pande 2018; Besley et al. 2022; Muralidharan 2024) by presenting microevidence on the negative spillover effects of an improvement in property rights on tax collection. Because the reform we study reduced the scope of the bureaucrats' work, our paper is most closely related to studies focusing on task design, particularly multitasking in public organizations (Holmstrom and Milgrom 1991; Dewatripont, Jewitt, and Tirole 1999a; Rasul and Rogger 2018; Chen, Li, and Lu 2018; Angelucci and Orzach 2023). We contribute to that literature by showing that reducing the number of tasks can reduce the performance of bureaucrats. Contrary to the existing literature, we also show that changes in the scope of tasks do not just affect the relationship between bureaucrats and their supervisor (Dewatripont, Jewitt, and Tirole 1999b), but also between bureaucrats and the population. Our paper therefore also contributes to understanding how the "embeddedness of the bureaucrat"—the social connections between bureaucrats and the local population—affects the functions of the state.[2] Together, these results contribute to a growing literature on the organizational economics of the state that highlights organization design as a determinant of state capacity (Vannutelli 2022; Mastrorocco and Teso 2023; Garfias and Sellars 2026),[3] and emphasizes the importance of informal authority in organizational performance (Baker, Gibbons, and Murphy 1999; Gibbons and Henderson 2012; Fenske, Haseeb, and Kala 2023; Aman-Rana, Minaudier, and Sukhtankar 2026).

Finally, we also contribute to the large literature on public finance in developing countries that seeks to identify the obstacles that these countries face in collecting taxes (Besley and Persson 2014b; Gadenne and Singhal 2014). These obstacles can include the lack of formal records (Pomeranz 2015; Okunogbe et al. 2021; Jensen 2022), the design of the tax code (Best et al. 2015; Brockmeyer et al. 2021; Bergeron, Tourek, and Weigel, forthcoming; Basri et al. 2021), corruption (Besley

[2] See, for example, McDonnell (2025); Evans (1995); Tsai (2007); Pepinsky, Pierskalla, and Sacks (2017); Bhavnani and Lee (2018); or Overbeck and Lungu (2024).

[3] Several studies show that the incentives of bureaucrats matter for public service delivery. These can be in the form of explicit incentive schemes (Khan, Khwaja, and Olken 2016, 2019), career concerns (Bertrand et al. 2020; Bazzi et al. 2025), reputation concerns (Mattsson 2025), monitoring (Callen et al. 2025), or autonomy in decision making (Rasul and Rogger 2018; Duflo et al. 2018; Bandiera et al. 2021; Aman-Rana, Wantchekon, and Kovo 2025). Others show that the selection of bureaucrats is an important determinant of state effectiveness (Callen et al. 2020b; Barteska and Lee 2023), where selection can be affected either at the recruitment stage (Dal Bó, Finan, and Rossi 2013; Bai and Jia 2016; Deserranno 2019a; Ashraf et al. 2020; Colonnelli, Prem, and Teso 2020a; Moreira and Pérez 2024), or through the assignment of bureaucrats across jobs or promotions (Iyer and Mani 2012; Jia, Kudamatsu, and Seim 2015; Bergeron et al. 2022; Best, Hjort, and Szakonyi 2023; Aman-Rana 2025).

and McLaren 1993; Flatters and MacLeod 1995; Le, Malesky, and Pham 2020), or taxpayers' misreporting (Carrillo, Pomeranz, and Singhal 2017; Naritomi 2019). Within this literature, our work is most closely related to papers that highlight the incentives and the ability of tax collectors as important determinants of fiscal capacity (Khan, Khwaja, and Olken 2016, 2019; Bergeron, Tourek, and Weigel, forthcoming; Bergeron et al. 2022). We contribute to this literature by showing that introducing technology through piecemeal state capacity building can have unintended consequences for fiscal capacity because of its effect on tax collectors.

I. Background and Data

A. *Background*

Agricultural Income Tax.—We focus on the collection of a tax, the Agricultural Income Tax (AIT), which is levied on landowners in rural areas of the province of Punjab. This tax is one of the main sources of revenue for the government from agriculture. The amount of tax due is based on either the area of cultivated land or the profits of the farm. Specifically, farmers owe whichever of the cultivated area-based tax and the profit-based tax is largest (Punjab Agricultural Income Tax Act 1997, 3.4). When land is rented out by landowners to farmers, the landowner is liable for the tax (Punjab Agricultural Income Tax Act 1997, 2.1 and 3.1).

The cultivated area-based tax is progressive and ranges from Rs 300 to Rs 600 per acre, with irrigated areas and orchards subject to a higher tax rate (Punjab Agricultural Income Tax Act 1997, 3.1). The profit-based tax is also progressive and starts with a flat amount of Rs 1,000 for the first tranche (profits between Rs 400,000 and Rs 800,000), progressively increasing to Rs 300,000 plus 15 percent of the amount of profits exceeding Rs 4.8 million (Punjab Agricultural Income Tax Act 1997, 3.3). In practice, due to the difficulty of measuring income, the profit-based tax is restricted to large landowners who own more than 50 acres of land, which only applies to 12 percent of farms (Agricultural Census 2010).

The tax is collected by a team of local bureaucrats called *revenue officers*. Each team of revenue officers covers a jurisdiction comprising 20 to 30 villages called a *revenue circle*. In total, there are 596 bureaucrats, called *Qanungos*, who directly manage these revenue circles.[4] We study bureaucrats at this level of the hierarchy. The taxable amount in a fiscal year, which runs from the first of July to the thirtieth of June the following year, is assessed by the same bureaucrats who collect the tax. At the start of a fiscal year, bureaucrats assess whether a land parcel has been cultivated and note its characteristics (irrigated or not, type of crops) during crop inspections (*Girdawari*) to calculate the cultivated area-based tax. Once tax is assessed, the bureaucrats issue tax demands around November and collect taxes over the remaining course of the fiscal year. Income-based tax is calculated using self-reported profits.

Bureaucrats do not receive any performance-based compensation. Senior officials in the revenue department are expected to conduct random checks of crop inspections conducted by junior officials on a minimum of 25 percent of the land under

[4] The total revenue bureaucracy spans multiple tiers and includes approximately 6,000 officials.

their jurisdiction. If a junior official is found to be underperforming, they may face a suspension. These managers also monitor the progress of the team on tax collection. Similar disciplinary action can be taken if the official systematically fails to collect enough taxes. The bureaucrats' promotions are based on tenure in the bureaucracy according to a predetermined schedule. However, senior officials and politicians can informally influence the timing of promotions to fast-track high-performing bureaucrats. Transfers of bureaucrats across different revenue circles also serve as an additional means of incentivizing performance. These mechanisms introduce some career incentives for bureaucrats to achieve a high performance.

Digitization of Land Records.—In 2005, the government of Punjab began a reform of the land record management system to digitize the records with the support of the World Bank. The reform's objective was to increase the reliability and the transparency of a system that was prone to errors and corruption. The reform had two components. First, land records previously maintained manually by local bureaucrats were digitized. Figure B.1 and Figure B.2 in Supplemental Appendix B show a manual land record and its digitized version. The new system centralized these records in an online database. Second, the reform established service centers staffed by new agents recruited from an external pool of candidates, trained specifically for managing the centers, and available throughout the working day (Board of Revenue, Government of the Punjab 2011). Supplemental Appendix Figure B.3 shows the new "Arazi Record Centers" set up to deliver services using digitized land records. Landholders could visit these centers to obtain certified copies of land titles or to register ownership changes, allowing them to access these services within minutes, without relying on the local bureaucrats that we study.

The government planned to roll out the digitization program in three phases, each covering 10–12 districts with a comparable number of revenue circles (200 in phase 1, 342 in phase 2, and 275 in phase 3). This staggered design was driven by the financial difficulty of rolling out a reform of this size across the whole province at once. Figure 1 shows the geographic distribution of the districts in each phase. While phase 1 districts were somewhat concentrated in the north of the province, phase 2 and phase 3 districts are distributed uniformly around the province.[5] Figure 2 shows that there were no statistically significant differences in baseline characteristics between districts digitized in the first two phases of the rollout and those digitized in the last phase.[6] The initial schedule was to roll out the digitized system in 2009 for phase 1, 2010 for phase 2, and 2011 for phase 3. The actual rollout was delayed and Figure 3 shows the proportion of villages that were digitized in each phase over time.

The reform had two effects. It secured property rights (Beg 2022a; Ullah and Hussain 2023) and it changed the type of tasks carried out by the local bureaucrats that we

[5] Supplemental Appendix Figure B.4 presents balance tests comparing phase 1 and phase 2 districts with phase 3 districts. Phase 1 districts have statistically significantly higher literacy rates and lower rural employment, fertilizer consumption, and agricultural production. Given that our empirical strategy mainly exploits differences between digitized districts (phases 1 and 2) with manual districts (phase 3), the pooled balance plot (Figure 2) remains the most relevant for evaluating baseline comparability.

[6] The estimated differences are relatively small in magnitude: All coefficients are below 1 standard deviation, the largest (agricultural production) corresponds to approximately 0.66 standard deviations, and 7 out of 12 coefficients are below 0.3 standard deviations. Since our outcome variable is normalized by cultivated area at baseline, it takes into account some of the underlying differences in agricultural production.

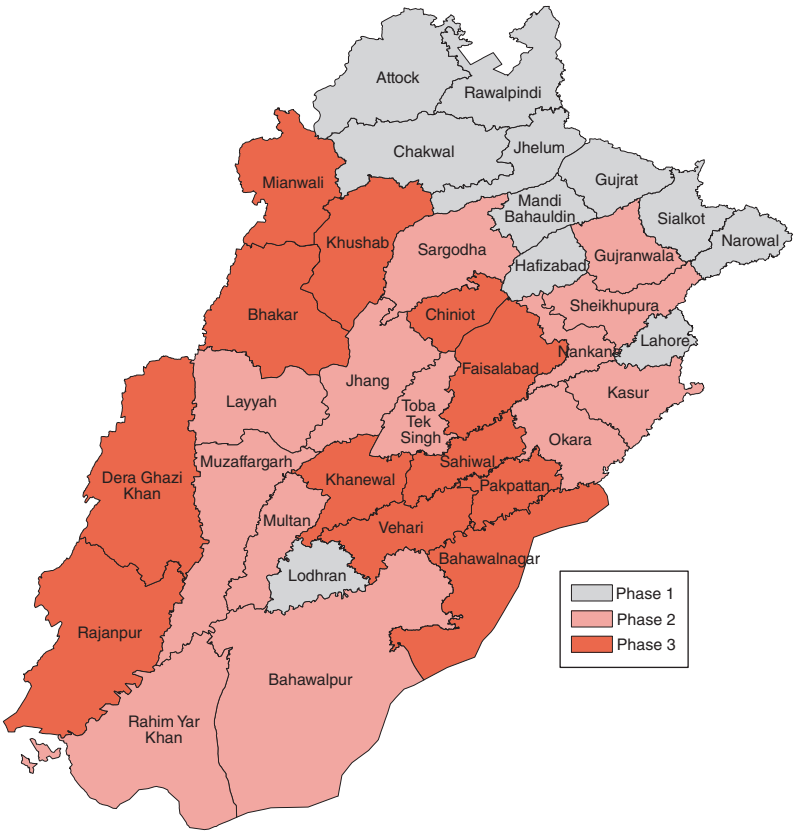

FIGURE 1. GEOGRAPHICAL DISTRIBUTION OF DISTRICTS BY DIGITIZATION PHASE

Note: Geographical distribution of districts across the three phases of land records digitization.

study. Prior to the digitization reform, bureaucrats were responsible for recording sales or exchanges of land and properties and for issuing land titles, as well as for assessing and collecting taxes. The provision of land services was a regular activity for local officials and was frequently needed by landowners. The main types of services provided were issuing land titles (*Fard*), recording land transfers (*Intiqal*), and resolving land disputes. Land titles are required by landowners for many activities, including setting up a water or electricity connection, obtaining a mortgage, gifting land, obtaining official documents, and selling or letting the land. It is an attestation of their right to the land and a new copy is required every time they need to assert their rights. A survey of landowners conducted before the reform showed that 71 percent of respondents contacted the land record department 1–5 times per year, 18 percent more than 5 times per year, and only 9 percent never contacted them (Gallup Pakistan 2009). On average, in 2016, a bureaucrat issued around two documents per day.[7]

[7] We use data on the universe of services issued in digitized centers from 2016. Assuming that the number of requests is stable over time, this number gives an indication of how frequent requests were prior to digitization. The total number of documents (including land records and land transfers) was 3.8 million in 2016. Given a total number of 5,723 bureaucrats (including both the bureaucrats we study and their team members), this corresponds to 666 service requests per bureaucrat per year, or around two per day.

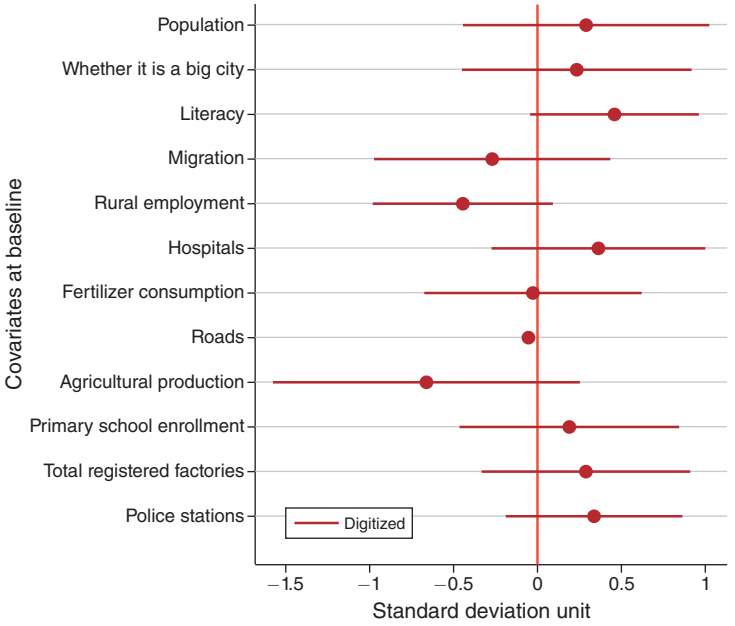

Figure 2: Balance Test on Baseline Characteristics between Digitized and Nondigitized Districts

Notes: Data on baseline characteristics are from the Development Statistics of the Pakistan Bureau of Statistics (1997–2010). The figure is based on 34 districts (the total number of districts is 36 but the baseline data for two districts, Chiniot and Nankana, is unavailable prior to 2011). The point estimates are from a regression of the respective covariates on a dummy that takes value one if the district is in phase 1 or 2 of the digitization reform, and zero otherwise. The reference category are phase 3 districts. Intervals are 95 percent confidence intervals.

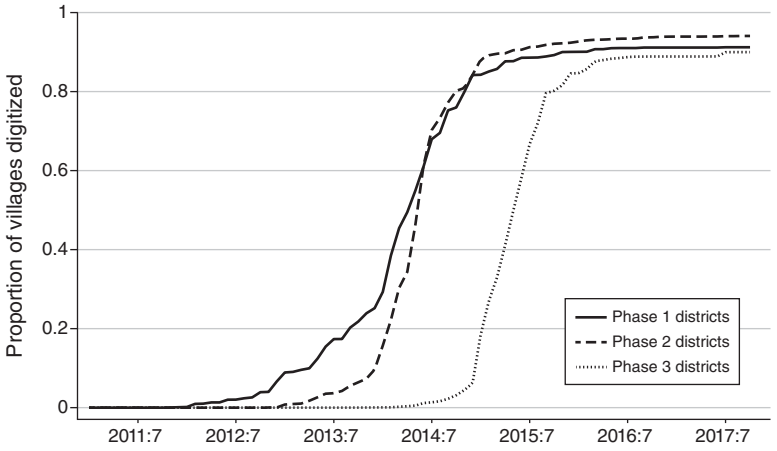

Figure 3. Phasewise Rollout of the Digitization Reform over Time

Notes: Districts that were planned to be digitized in phase 1 are Lahore, Lodhran, Hafizabad, Mandi Bahauddin, Nankana Sahib, Jhelum, Gujrat, Sialkot, Chakwal, Attock, and Rawalpindi. Districts that were planned to be digitized in phase 2 are Bahawalpur, Gujranwala, Jhang, Layyah, Kasur, Multan, Muzaffargarh, Narowal, Okara, Rahim Yar Khan, Sargodha, Sheikhupura, and Toba Tek Singh. Districts that were planned to be digitized in phase 3 were Bahawalnagar, Bhakkar, Chiniot, Dera Ghazi Khan, Faisalabad, Mianwali, Khanewal, Khushab, Pakpattan, Rajanpur, Sahiwal, and Vehari.

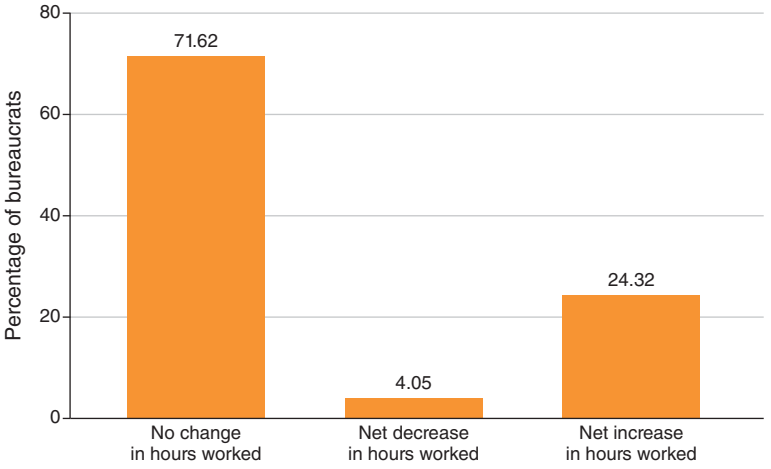

FIGURE 4. CHANGES IN HOURS WORKED BY BUREAUCRATS AFTER THE DIGITIZATION REFORM

Notes: The figure is based on the bureaucrat survey restricted to the 118 bureaucrats who served as *Qanungo* between 2006 and 2013. The figure is based on responses to the follwing questions: "Do you think LRMIS (the digitization reform) has changed the official tasks that you are supposed to do? If so, what is the number of hours per day that were added/reduced because of these changes?" Based on these answers, we calculate the difference between hours added and hours removed. The first bar is the proportion that either responded "No" to the first question or whose net difference was zero. The second (third) bar is the proportion of respondent for whom that difference was negative (positive).

The reform relieved the bureaucrats of their land record-related duties, which also affected their interactions with the local population. Overall, 69 percent of bureaucrats reported that the reform changed their tasks, of which 75 percent said that some tasks were removed but 59 percent indicated that some tasks were also added (see Supplemental Appendix Figure B.5 and Supplemental Appendix Figure B.6). The tasks added were mostly about record correction and additional paperwork, which was part of the transition from manual to digitized land records (see Supplemental Appendix Figure B.7). Therefore, the new tasks were mostly relevant in the short term. On net, the number of hours worked reported by the bureaucrats did not increase significantly. Figure 4 shows that 72 percent of bureaucrats reported no change in hours worked, 4 percent reported a decrease, and 24 percent reported an increase. This suggests that the majority of bureaucrats either used the time freed up from land records to work on other tasks or simply worked less after the reform. Our survey data further confirm that the net reported decrease in hours is not significantly different from the net reported increase (see Supplemental Appendix Figure B.8).

The bureaucrats also reported two interesting changes following the reform. First, they indicated that digitization negatively affected their ability to collect taxes. The main reason cited was a loss of influence over taxpayers as shown in Figure 5. Second, the bureaucrats lost an important source of bribe income. Obtaining bribes or "tips" in exchange for a speedy processing of land records was common before the reform. 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). Because the

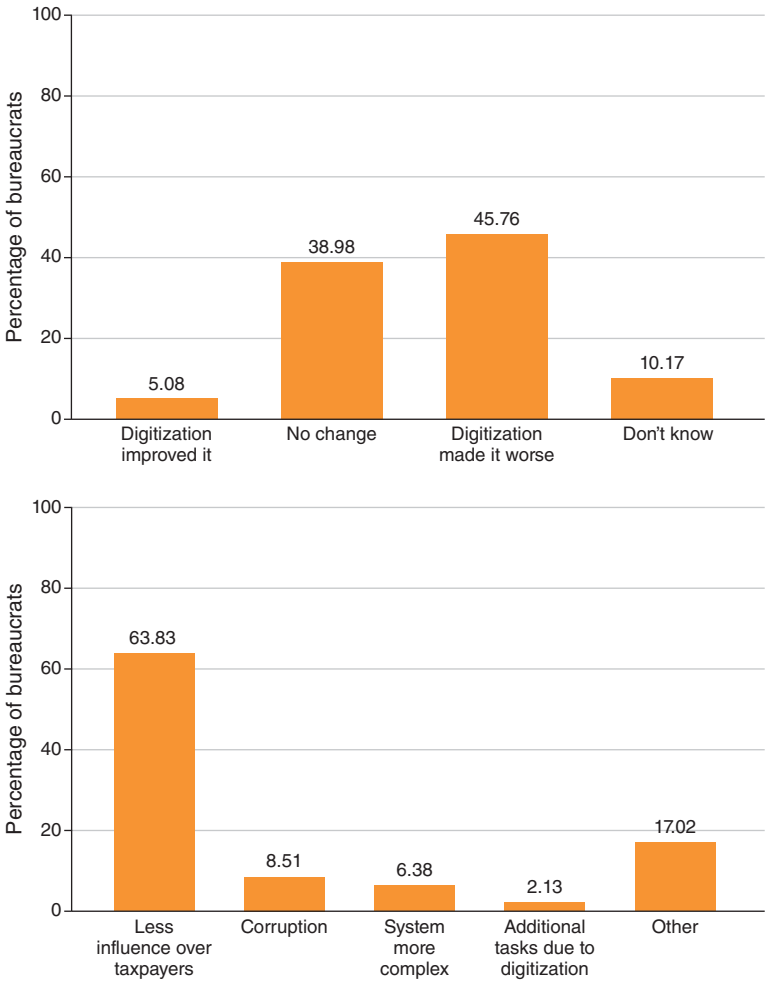

FIGURE 5. BUREAUCRATS' VIEWS ON THE EFFECT OF DIGITIZATION ON TAX COLLECTION

Notes: The figure is based on the bureaucrat survey restricted to the 118 bureaucrats who served as *Qanungo* between 2006 and 2013. The survey questions were "Do you think digitization has improved overall tax collection?" followed by "Please explain how?" The bottom figure is restricted to the 54 bureaucrats who responded "digitization made tax collection worse" in the first question.

bureaucrats no longer had control over the land record process, they lost this source of bribe. 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 2016, see Supplemental Appendix Figure B.9 and Supplemental Appendix Figure B.10). The bureaucrats reported a similar decline in bribes: Figure 6 shows that 48 percent of bureaucrats agreed that citizens want to tip to get land titles before digitization compared to 33 percent after digitization.[8]

[8]The question asked about willingness to tip in a revenue circle that has been digitized which could include both villages that have been digitized and villages that have not. This can explain the positive share of respondents

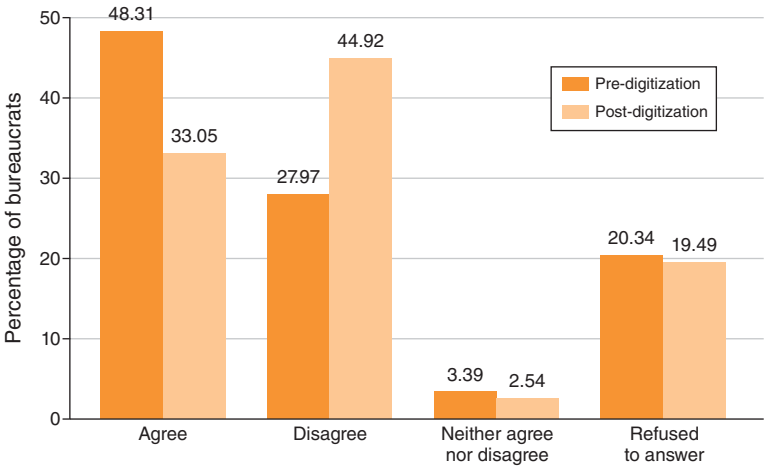

FIGURE 6. DO BUREAUCRATS IN CHARGE OF LAND TITLES RECEIVE BRIBES OR "TIPS" FOR ISSUING THEM?

Notes: The figure is based on the bureaucrat survey. The figure shows the percentage of respondents that responded to the question, "People over there (in a revenue circle) would tip or want to tip a Patwari (bureaucrat's subordinates) for issuing Fard (land title)" measured on a Likert scale. "Agree" and "completely agree" were grouped into "agree," while "disagree" and "completely disagree" were grouped into "disagree."

B. *Data Sources and Key Variables*

Digitization Rollout.—The data on digitization include both the planned and actual rollout of the digitization reform. We obtained the planned rollout of the program from the Land Record Management Information System (LRMIS) project office in Lahore. These data indicate which districts were intended to be digitized in phase 1, 2, or 3 of the program. We obtained the actual progress of the digitization program from the Punjab Land Records Authority in February 2018. These data describe whether and on which date the land records of each village were digitized.

We define a *phase* as the set of districts that were intended to be digitized at the same time as each other in the rollout plan. We consider a phase as being *digitized* in a given year if at least 5 percent of villages in that set of districts have been digitized by that year. We use the actual rollout of digitization to determine the start of digitization rather than the planned rollout, because the actual rollout was significantly delayed relative to the plan, so no districts were actually digitized in the years planned. However, we define the beginning of the digitization at a phase level, rather than at an individual district level, to use variation in the rollout that is not driven by unobserved characteristics of the districts, which could be correlated with both the pace of digitization and tax collection. Considering the entire phase to be digitized if just 5 percent of the villages in a phase were digitized allows us to retain the intention-to-treat (ITT) interpretation of the estimates that we aim to capture.

reporting bribes after digitization (33 percent). Given that admitting to this behavior reflects badly on the bureaucracy, these responses likely underestimate the true magnitude of bribery. We expect the under-reporting to be similar before or after the reform. Figure 6 supports this interpretation since the proportions of respondents that refused to answer the question on tips before and after the reform are similar.

We define our treatment variable, *digitization of land records*, as a dummy variable that takes value 1 in a district and year if the district belongs to a phase that has been digitized by that year. Based on this definition, phase 1 is treated in fiscal year 2012, phase 2 in fiscal year 2013, and phase 3 in fiscal year 2014. In Supplemental Appendix Table A.1 and Supplemental Appendix Table A.2, we show that our results are robust to using thresholds other than 5 percent of villages to define a phase as digitized.

Agricultural Tax Collection.—We hand collected the agricultural tax collection records of the Board of Revenue, the agency in charge of tax collection, and carried out a large-scale digitization exercise to build a unique dataset of agricultural taxation in Punjab (see Supplemental Appendix C for the record room and the proforma on which this information is collected). The data contain both the total amount of taxes collected (combining cultivated area-based tax and income-based tax) and the total tax demands issued to taxpayers, at the revenue circle level. The tax demand is based on the assessment carried out by the bureaucrats and serves as the target amount of taxes for them to collect.

Although the taxation data are available until 2017, the start of phase 3's digitization in 2014 means we do not have a counterfactual to estimate causal effects after that year. We therefore restrict the dataset to 2006–2013. The data include monthly records at the revenue circle level for this period, covering 28,572 revenue circle–months. These data are an unbalanced panel of revenue circles and months, since some of the tax files were destroyed in flooding and since not all the tax data could be matched to the digitization rollout data. To ensure the data are representative at a district level, we created inverse probability-weighted sums of the revenue circle-level tax. For each time period, the weights are based on the number of revenue circles for which we have data, relative to the total number of revenue circles in a tehsil (subdistrict) and district. We use these weights to aggregate the taxation data at the district level. We also aggregate the monthly data at the year level, since tax assessments are issued annually and the monthly tax collection data are therefore noisier. The resulting data are an unbalanced panel of 212 district-fiscal years.[9] We provide the number of districts for which tax collection is missing for each year and each phase in Supplemental Appendix Table A.3 and show in Supplemental Appendix Figure B.11 that the probability that a district has some missing tax collection data in some years is not correlated with that district's baseline characteristics.[10]

We normalize tax collected (in thousands of Pakistani rupees) by the average district-level cultivated area (in thousands of acres) at baseline. The baseline cultivated area is calculated as the average number of cultivated acres in a district between

[9] Due to the presence of outliers, we dropped a revenue circle-fiscal year if its annual tax demand was more than 2 standard deviations above its average over time. This resulted in a drop of 76 revenue circle-fiscal years out of 3,492 (2.2 percent) and 1 observation at the district-fiscal year level out of 220 (0.5 percent).

[10] Some of the districts have zero tax collection in certain years due to a combination of two factors. In these districts, some revenue circle–month observations are missing, while the revenue circle–months that we do observe collected no taxes (likely due to poor agricultural yields or poor enforcement by bureaucrats). Supplemental Appendix Figure B.12 shows that districts with at least some zero values are comparable to those without any zero values on baseline characteristics.

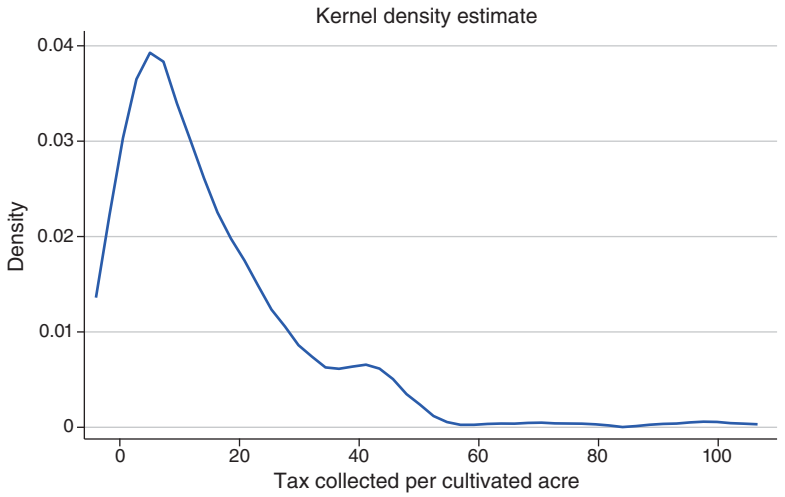

FIGURE 7. KERNEL DENSITY OF TAX COLLECTED PER CULTIVATED ACRE

Notes: Kernel density of tax collection per acre across all districts and all years from fiscal year 2006 to fiscal year 2013. Tax collection per cultivated acre is calculated by dividing the total tax collected (in thousands of Pakistani rupees) by the average district-level cultivated area (in thousands of acres) at baseline.

2007 and 2011 but excluding 2009, which was missing.[11] The data on cultivated areas were obtained from the Directorate of Agriculture (Economics and Marketing) of Punjab. Figure 7 presents the kernel density plot of tax collected per cultivated acre, showing that the distribution of tax per cultivated acre is right-skewed. This pattern informs the choice of estimators we report in Section II.

Figure 8 shows the evolution of the average tax collected per acre across districts within each phase of the digitization reform. The raw trends show that tax collection per acre was on an upward trajectory across all three phases from 2008 to 2011, but that trend reversed for phase 1 and phase 2 districts following digitization. By contrast, the upward trend continued for another two years for phase 3 districts. Once all three phases have been digitized (gray area in the graph), tax collection follows a similar trajectory across the three phases.[12]

Actual Tax Base.—To evaluate the effect of the reform on the tax base (cultivated area or farm income), we rely on three sources of data. First, we compiled satellite data on vegetation cover to measure cultivated area. We use the Normalized Difference Vegetation Index (NDVI) (Didan 2015; see Supplemental Appendix D for details), a commonly used proxy for crop yield in developing countries (Rasmussen 1992; Vrieling, de Beurs, and Brown 2011; Beg 2022a), which allows comparisons of year-on-year changes in vegetation growth (Huete

[11] We use baseline value of cultivated areas as cultivated areas are reported by the bureaucrats and could therefore be affected by the reform. In fact, we show later on that this was the case.

[12] In some of the years prior to digitization, phase 1 and 2 districts appear to be on a different trajectory than phase 3 districts. For instance, in 2011, tax collected per acre falls for phase 3 districts but increases for phase 1 and 2 districts, while in 2012, tax collection falls in phase 2 districts but increases in phase 3 districts. However, these differences are not statistically different from zero and disappear once we control for district and year fixed effects, as shown in the event-study plot in Figure 9.

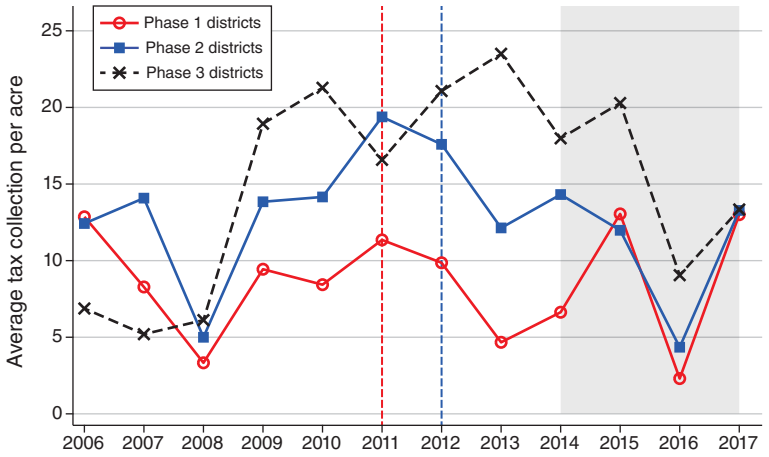

FIGURE 8. MEAN TAX COLLECTION PER CULTIVATED ACRE BY DIGITIZATION PHASE

Notes: This figure illustrates the mean tax collection per cultivated acre across the three phases of land records digitization. Each point represents the average tax collection per cultivated acre, calculated by dividing tax collected in thousands of Pakistani rupees by the average district-level cultivated acres in thousands at baseline. The red and blue vertical dotted lines at 2011 and 2012 indicate the year preceding the start of phases 1 (red) and 2 (blue), respectively. The main analysis in the paper focuses on the period 2006 to 2013, since by 2014, phase 3 is also digitized and there is no counterfactual to estimate the effects. However, since our data extend to 2017, we plot the raw data from 2006 to 2017 to provide a complete picture. The shaded area marks the years excluded from the main analysis, when all three phases are digitized.

et al. 2002). We complement the satellite data with survey data from the Pakistan Living Standards Measurement Survey (PSLM), which includes questions on agricultural land owned (in acres) and agricultural land irrigated from a repeated cross section of rural households across Punjab (Pakistan Bureau of Statistics 2006–2015). We analyze data from the 2006, 2008, 2010, and 2012 waves of this survey, each wave representing approximately 40,000 land-owning citizens in rural households across Punjab. The dataset is representative at the district level. Finally, we use Household Income and Expenditure Surveys (HIES) data from Beg (2022b) to investigate the effects of digitization on agricultural profits, the other possible element of the tax base. These data collect demographic, employment, expenditure, and saving information from a repeated cross section of households from districts of Punjab. We use data from the 2005, 2007, 2011, and 2013 waves of the survey. Beg (2022b) focuses on farm-level data provided by cultivating households (approximately 5,986 out of 15,767 rural households) and calculates profits per acre as the difference between the value of output per acre and the total expenses per acre, with values winsorized as profits are expected to be measured with error.

Tax Base Reported by Bureaucrats.—While we cannot directly observe tax demands issued by bureaucrats to each taxpayer, we can observe two aggregate measures of the tax base assessed by bureaucrats. First, we use data compiled by the Directorate of Agriculture (Economics and Marketing) of Punjab—who use the cultivated area reported by the bureaucrats we study to construct average cultivation measures across districts—from 2007 to 2013 (Agriculture Marketing Information

Service 2007–2014).[13] Second, we use the administrative data on the assessment of tax made by bureaucrats at the revenue circle-fiscal year level, which we aggregate at the district-fiscal year level to ensure comparability with the first measure.[14]

Bureaucrat Career History and Performance.—For the last part of our analysis, we complement the administrative tax collection data with a retrospective survey of 750 bureaucrats working in tax collection around the time of the reform.[15] This survey gives us the career history of the bureaucrats across different revenue circles and their perception of the reform, its effects on tax collection, and how the reform affected their interactions with superiors and with the population. We found 118 respondents who worked as *Qanungos* (revenue circle managers) between 2006 to 2013. We carried out a string matching exercise to merge the revenue circles in the tax collection data with those in the bureaucrats' careers data, since there were no unique revenue circle identifiers in either dataset. We could string-match the revenue circle names for 105 of those 118 respondents. Of those, 27 respondents had missing tax data, so our final dataset includes 78 respondents whose tax performance is observed between 2006 and 2013. Supplemental Appendix E describes the procedure used to match the tax and digitization data and to link these, via string matching, with the bureaucrats' survey data in order to construct a panel of bureaucrats-revenue circles-fiscal years. Details of the bureaucrat survey sampling are presented in Supplemental Appendix F, together with a balance plot showing that there are no systematic differences between bureaucrats matched with the tax and digitization data and those that remain unmatched, other than their age. Merging the tax and digitization data with the bureaucrats' career data allows us to identify the tax performance of a bureaucrat and whether they worked in a revenue circle that was digitized at any given point in time.[16] These data therefore allow us to study the effects of the reform on bureaucrats' performance.

II. Did the Digitization Reform Affect Tax Collection?

We now turn to testing our main question: How did the digitization reform affect tax collection?

A. *Identification Strategy*

There are several difficulties in measuring the effect of digitization reforms on fiscal capacity. Policy makers could introduce digitization reforms at times when

[13] Data available at http://www.amis.pk/Agristatistics/DistrictWise/DistrictWiseData.aspx. There are no data available for the years 2006 and 2009.

[14] In Supplemental Appendix Table A.12, we show that results are robust to using the disaggregated data at the bureaucrat level.

[15] The survey was first carried out in person in September 2020. We carried out a separate telephonic survey focusing on the bureaucrats' career histories in November 2020. For a random subset of the data, we confirmed the accuracy of the responses by comparing them to official records of bureaucratic transfers. To access the data, see Aman-Rana and Minaudier (2026).

[16] If any bureaucrat held two positions in a time period, we used the position with the longer time span and dropped the other from the sample. We dropped five observations for which both positions had the same duration and one observation in which the position was only held for 14 days.

bureaucracies are underperforming due to structural issues. Alternatively, some districts might be targeted for the implementation of the reform because bureaucrats in these districts face difficulties collecting taxes and need technological support in other tasks. Our difference-in-differences strategy helps us address these concerns.

Since the actual rollout of the reform across districts could depend on time-varying district characteristics which correlate with tax collection, we exploit the *planned* rollout of the digitization reform. Throughout the paper, we present an intent-to-treat analysis, which estimates the average return to "as-is" implementation of the digitization reform following the "intent" to implement the new digitized system. These estimates reflect the impact of the government's decision to digitize land records net of the logistical and political economy challenges of implementing this project in practice.

Our strategy compares the difference in tax collection before and after digitization between districts where digitization was planned to be introduced earlier and those where it was intended to be introduced later. The identification assumption motivating this estimation strategy is that early digitized districts and later digitized districts have parallel trends: Districts in phases 1 and 2 of the reform would have experienced, on average, the same changes in tax collection over time as those in phase 3, were it not for the digitization of their land records. We discuss the validity of this assumption below in the Robustness subsection of IIB.

B. *Estimation and Results*

To obtain the causal effect of the digitization reform on tax collection, we estimate the following two-way fixed effects regression for district d and fiscal year t between 2006 and 2013:

$$y_{dt} = \eta_d + \eta_t + \beta Digitization_{dt} + \varepsilon_{dt}. \tag{1}$$

Our outcome variable, y_{dt}, is the tax collected per acre in district d, during fiscal year t. Our treatment variable, *Digitization*, is a dummy that takes the value of one if a district d belongs to a phase that has been digitized in year t. Finally, η_d and η_t are district and fiscal year fixed effects, respectively. The error term is clustered at the district level as that is the level of the treatment (Abadie et al. 2023). To account for the low number of clusters (36 districts), we also report bootstrapped standard errors clustered at the district level, based on 1,000 replications.

Since the distribution of tax collected per acre is skewed (Figure 7), the OLS estimate of the effect on tax per acre might be sensitive to outliers. We therefore follow the approach taken by Pomeranz (2015) to study tax data with a right-skewed distribution in a difference-in-differences setting and show a median estimator, using Koenker's (2004) quantile regression framework.[17]

Two-way fixed effects regressions in staggered rollout designs incorporate both valid comparisons between treated and not-yet-treated or never treated units and problematic comparisons between units that are already treated. When treatment effects are heterogeneous, these problematic comparisons can introduce biases due to negative

[17] Specifically, for district d and fiscal year t between 2006 and 2013, we estimate the following median regression: $Q_\tau(y_{dt}|.) = \alpha_d + \alpha_t + \theta Digitization_{dt}$, where $\tau = 0.5$ and all the variables are defined as in equation (1).

weighting problems (Goodman-Bacon 2021; de Chaisemartin and d'Haultfoeuille 2020; Callaway and Sant'Anna 2021; Sun and Abraham 2021; Roth et al. 2023). We therefore also report results from a "stacked regression" (Gormley and Matsa 2011; Cengiz et al. 2019; Deshpande and Li 2019; Baker, Larcker and, Wang 2022). This approach constructs event-specific 2×2 datasets that include the treated districts along with the appropriate *clean* control districts within the treatment window (i.e., not-yet-treated or never treated districts). For each event, this excludes any problematic comparisons between units that are already treated.[18] We assign a unique identifier, h, to each event-specific dataset and estimate the following regression on the stacked dataset, for district d, fiscal year t, and event h:

$$y_{dth} = \mu_{dh} + \mu_{th} + \gamma Digitization_{dth} + \epsilon_{dth}, \tag{2}$$

where μ_{dh}, μ_{th} are district-by-event and fiscal year-by-event fixed effects.

Table 1 shows our main result. Across all specifications, the estimates show that the digitization reform led to a fall in tax collection. In column 3, the decline in tax collected is Rs 6.74 per acre (p-value < 0.1), representing a 47 percent reduction relative to the control mean. The median estimate in column 4 shows that tax collection declined by Rs 5.60 per acre at the median (p-value < 0.01), representing a 39 percent reduction relative to the control mean. Due to the skewness of the tax data, the median estimate is more precise than the OLS estimates.

Rather than increasing fiscal revenues, the modernization of state capacity led to a large and statistically significant decline in tax collection. The magnitude of the effect is substantial, corresponding to an estimated loss of Rs 6 to 7.2 million per district. The decrease in tax collected can have important economic consequences. While the tax that we study is not a large source of revenue for the government, the loss of Rs 7.2 million per district due to the reform still represents a significant shortfall. Extrapolated across all 36 districts, the amount of lost taxes could have funded cash transfers for an additional 13,729 families on the government's main social welfare program (Benazir Income Support Programme—henceforth, BISP).[19]

Endogenous Rollout and LATE Estimates.—Our ITT approach generates conservative estimates of the effect of digitization. Indeed, many districts were not fully digitized within the first year in which we define them as "treated," as shown in Figure 3.[20] To give a sense of how conservative our baseline estimates are, we show

[18] In our setting, we constructed two event-specific datasets that are then stacked together. The first defines phase 1 districts as treated units, with phase 2 (not-yet-treated) and phase 3 (never-treated) districts as controls. Phase 2 districts serve as control districts until their treatment begins in 2013, after which all post-2013 phase 2 observations are excluded from this dataset. The second dataset defines phase 2 districts as treated units, with phase 3 (never-treated) districts as control districts. Observations from phase 1 districts are excluded after the year when their treatment begins (2012). Each event-specific dataset therefore only makes *clean* comparisons, overcoming any biases due to negative weighting.

[19] Given an average of 1,063,250 cultivated acres per district, we calculate an estimated total tax loss of Rs 7,166,305 using the OLS estimate ($6.74 \times 1{,}063{,}250$) and of Rs 5,954,200 using the median estimate ($5.60 \times 1{,}063{,}250$). The annual transfer for families eligible to the BISP was Rs 18,792 in 2015 (Cheema et al. 2016). The loss of Rs 257,986,980 (Rs 7,166,305 multiplied by 36 districts) would therefore cover $\frac{257,986,980}{18,792} = 13{,}729$ families.

[20] Supplemental Appendix Table A.4 presents the cumulative proportion of villages digitized in each phase for each year, which shows that there is two-sided noncompliance.

Table 1—Did the Digitization Reform Affect Tax Collection?

Dependent variable:	Tax collection per cultivated acre			
	TWFE		Stacked DID	
	OLS	Median	OLS	Median
	(1)	(2)	(3)	(4)
Digitization of land records	−6.57	−5.21	−6.74	−5.60
	(3.69)	(2.43)	(3.83)	(1.97)
	[3.67]	[2.01]	[3.80]	[2.03]
Dependent variable mean	14.2	14.2	14.2	14.2
District fixed effects	Yes	Yes	No	No
Fiscal year fixed effects	Yes	Yes	No	No
District-by-event fixed effects	No	No	Yes	Yes
Fiscal year-by-event fixed effects	No	No	Yes	Yes
Observations	212	212	394	394

Notes: The unit of observation is a district-fiscal year. "Digitization of land records" is a dummy variable that takes value 1 for phase 1 and 2 districts in every year from fiscal year 2012 and fiscal year 2013 respectively, and remains 0 otherwise. "Tax collection per cultivated acre" divides tax collected in thousands of Pakistani rupees by average district-level cultivated acres (in thousands) at baseline. Dependent variable mean is the average tax collected per acre across all districts and all years from fiscal year 2006 to fiscal year 2011, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1,000 replications) are in square brackets.

two additional estimations in Table 2. Columns 1–4 show the two-stage least squares (2SLS) estimates from instrumenting the actual rollout across villages within a district with the digitization treatment, as defined in our main specification.

The first stage, shown in columns 1 and 2, and the associated Kleibergen-Paap Wald F-statistic of 112.1 (55.7 for the unstacked regression), suggests that the instrument is predictive of the proportion of villages digitized. The 2SLS results in columns 4 show that the LATE of an additional 1 percent of villages digitized in a district on tax collection is -0.169 (p-value < 0.1) for the stacked regression. To compare this effect to the ITT estimate from our baseline specification, we multiply this coefficient by 91.47, the average percentage of villages ever digitized in a district by the end of the reform (fiscal year 2017), which implies a decrease of 15.46 in tax collected per acre. The estimated effect corresponds to between 82 percent and 109 percent of the average tax per acre in the control group, depending on the benchmark we use.[21] While the LATE is large relative to the ITT estimates, it is not entirely unexpected given the context. Districts which digitized a larger number of villages under the reform (whose treatment effect is captured by the LATE) did so anticipating significant improvements in property rights (World Bank 2017). The reform's benefits were expected to be greatest in areas where property rights were weakest, which are also the areas where bureaucrats exerted the most influence. As we discuss below, losing this influence was a

[21] The control mean of Rs 14.2 per acre, reported in all tables, represents the average tax collected per acre across all districts and all years from fiscal year 2006 to fiscal year 2011, prior to any district's digitization. Alternatively, calculating the control mean as the average tax collected per acre in the year immediately preceding digitization for each of the three phases gives a control mean of Rs 18.9 per acre.

Table 2—2SLS and OLS Estimates of the Effect of the Digitization Reform on Tax Collection

Dependent variable:	Percentage of villages digitized		Tax collected per acre			
	First stage		2SLS		OLS	
	Unstacked (1)	Stacked (2)	Unstacked (3)	Stacked (4)	Unstacked (5)	Stacked (6)
Digitization of land records	37.42 (5.014)	39.97 (3.775)				
Percentage of villages digitized			−0.176 (0.102)	−0.169 (0.0968)	−0.0756 (0.0752)	−0.0864 (0.0781)
Kleibergen-Paap Wald *F*-stat	55.7	112.1				
Dependent variable mean	0.28	0.28	14.2	14.2	14.2	14.2
District fixed effects	Yes	No	Yes	No	Yes	No
Fiscal year fixed effects	Yes	No	Yes	No	Yes	No
District-by-event fixed effects	No	Yes	No	Yes	No	Yes
Fiscal year-by-event fixed effects	No	Yes	No	Yes	No	Yes
Observations	212	394	212	394	212	394

Notes: The unit of observation is a district-fiscal year. "Digitization of land records" is a dummy variable that takes value 1 for phase 1 and 2 districts in every year from fiscal year 2012 and fiscal year 2013 respectively, and remains 0 otherwise. "Percentage of villages digitized" is the percent of villages in a district that are digitized in any given fiscal year. "Tax collected per acre" divides tax collected in thousands of Pakistani rupees by average district-level cultivated acres (in thousands) at baseline. Dependent variable mean is the percentage of villages digitized (for columns 1 and 2) and the average tax collected per acre (for columns 3–6) across all districts and all years from fiscal year 2006 to fiscal year 2011, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1,000 replications) are in square brackets.

driver of the fall in tax collection. We would therefore expect the treatment effect on taxes to be larger in those districts.

The last two columns show the OLS estimates using the proportion of villages digitized within a district as the independent variable. The results show that an additional 1 percent of villages digitized in a district decreases tax collection by Rs 0.0864 per acre (p-value > 0.1). A district with 91.47 percent of villages digitized would therefore face a reduction in tax collection of Rs 7.90 per acre, or 42 percent to 56 percent of the control mean.

Before turning to the mechanisms behind the results, we first present several checks to assess the robustness of our findings.

Robustness.—

Event Study: We assess the evidence in support of the parallel trends assumption using an event-study plot. Specifically, for district d and fiscal year t and event h, we estimate the following regression in the stacked dataset:

$$y_{dth} = \beta_{dh} + \beta_{th} + \sum_{k=-5}^{k=1} \rho_k D_{k(dth)} + \varepsilon_{dth}, \tag{3}$$

where y_{dth} is tax collected per acre in district d, during fiscal year t, and event h. β_{dh} are district-by-event fixed effects and β_{th} are fiscal year-by-event fixed effects. $D_{k(dth)}$ is a set of indicator variables that takes value 1 if district d in fiscal year t and event h

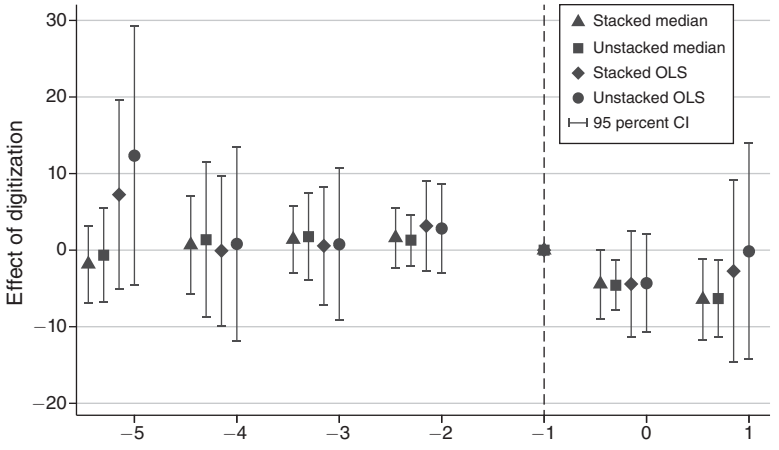

FIGURE 9. EVENT-STUDY PLOT FOR DISTRICT LEVEL TAX COLLECTED PER ACRE

Notes: Data are at the district-fiscal year level. Each coefficient is obtained from a set of indicator variables that take value 1 if, in a given fiscal year, phase 1 or phase 2 districts were k years away from the introduction of digitized land records, as described in equation (3). The reference year is fiscal year 2011 for phase 1 and fiscal year 2012 for phase 2. District-by-event and fiscal year-by-event fixed effects are included for the stacked specification. District and fiscal year fixed effects are included for the unstacked specification. Standard errors were clustered at the district level.

was k years away from being digitized.[22] The error term is clustered at the district level as that is the level of the treatment (Abadie et al. 2023). The coefficients ρ_k estimate the effect of being k years away from being digitized. The omitted time period is the last one before the digitization year.[23] A set of statistically insignificant ρ_k for all the years before treatment lends support to the parallel trends assumption. Figure 9 plots ρ_k for each period k and their corresponding 95 percent confidence intervals for both the OLS and median specifications. The results in Figure 9 support the parallel trends assumption. The coefficients for the years before the digitization reform are close to zero and show no significant pre-trends for both the OLS and median specifications. A joint test of the pre-digitization coefficients yields a p-value of 0.80 and 0.94 for the median stacked and unstacked specification, while it is 0.53 and 0.30 for the stacked and unstacked OLS, respectively. As expected, in the pre-periods, the median estimates are more tightly centered around zero than the OLS estimates.

Alternative Thresholds to Define the Start of Digitization.—In our main analysis, a phase is considered to be *digitized* in a given year if at least 5 percent of villages in that phase have been digitized by that year. We test the robustness of this result using

[22] We also show the results using the unstacked dataset. This specification replaces the district-by-event fixed effects and fiscal year-by-event fixed effects by district and fiscal year fixed effects.

[23] We restrict Figure 9 to a pre-period window of five years. The confidence intervals widen as we move further away from the treatment period. This appears to be driven by the lack of data availability in early years as shown in Supplemental Appendix Table A.3. In addition, since phase 1 districts were treated in fiscal year 2012 and our data begin in fiscal year 2006, the pre-period seven years before treatment does not include any phase 1 districts. We therefore dropped the pre-periods six and seven years away from treatment in Figure 9. Even if we include these two periods, a joint test of the significance of the pre-period coefficients fails to reject the null hypothesis, but the postperiod coefficients become more imprecisely estimated.

both less conservative thresholds—defining a phase as *digitized* in a given year if at least 50 percent of villages have been digitized, and more conservative thresholds—defining a phase as *digitized* in a given year if at least 1 percent or 2 percent of villages in the corresponding districts have been digitized by that year.[24] The results are presented in Supplemental Appendix Table A.1 and Table A.2, respectively. The negative effects of land record digitization on tax collection remain robust, with a clear overall pattern: As the proportion of digitized villages increases, the estimated negative effect on taxes becomes more pronounced.

Bureaucrat Transfers across Districts.—One might worry the digitization reform drove bureaucrats to move out of digitized districts to nondigitized ones or vice versa. These transfers could explain the decrease in tax collection if bureaucrats that were systematically collecting less taxes were relocating to digitized districts or bureaucrats collecting more taxes to nondigitized ones. While such transfers are not allowed by law, we also use our data on the bureaucrats' careers to verify that transfers were rare. These data confirm that only 2 out of the 118 bureaucrats (and only 2 out of their 440 subordinates) have ever been posted outside the districts where they started their careers (see Supplemental Appendix Figure B.13). We also rule out that changes at higher levels of the hierarchy could have driven the results. We show in Supplemental Appendix Table A.5 that our results remain robust when controlling for the proportion of the bureaucrats' managers in each district whose ability was above median. We measured ability using four tests: two incentivized ability tests (a cognitive ability matrix test and a digit span memory test) based on Hanna and Wang (2017), a general knowledge test, and a test of knowledge of rules and laws relevant to their duties as revenue officials. Together, these results indicate that such spillovers do not threaten our identification strategy.

Anticipation Effects.—Another concern is that either the bureaucrats or the citizens could have anticipated the digitization reform and changed their behavior as a result. Anticipation effects could bias our results if they systematically impact tax collection more in phases 1 and 2 districts relative to phase 3 districts. Supplemental Appendix Table A.6 uses two alternative definitions of the timing of digitization as a placebo test. In column 1, the digitization reform is defined as starting in 2006 for phase 1 districts and 2007 for phase 2 districts, while in column 2, these timings are defined as 2009 and 2010, respectively. None of the coefficients are statistically significant and the effects in the median regression are much smaller in magnitude than the main estimates in Table 1. This suggests that anticipation effects are unlikely to bias the results.

Treatment Effect Heterogeneity.—As discussed above, two-way fixed effects regressions can produce inconsistent estimates when treatment effects are heterogeneous. While our stacked regression addresses these concerns, we run additional

[24] Since the proportion of digitized villages changes throughout a year, there is a range of possible cutoffs that correspond to a given start year. In particular, any cutoff between 4 percent and 15 percent corresponds to the same starting years as in our main analysis. Any cutoff between 16 percent and 59 percent corresponds to phases 1 and 2 starting to be digitized from fiscal year 2013 and phase 3 from fiscal year 2014. With a 1 percent threshold, the treatment years are fiscal year 2011 for phase 1, 2012 for phase 2, and 2013 for phase 3. With a 2 percent threshold, phases 1 and 2 follow the same timeline, but phase 3 is digitized in 2014.

tests in this subsection. We first show in Supplemental Appendix Table A.7 that our results are robust to using the estimator proposed by Callaway and Sant'Anna (2021) to account for potential treatment effect heterogeneity in staggered adoption designs. The coefficient's magnitude is close to our baseline results, representing 55 percent of the control mean (versus 47 percent for the OLS estimate using the stacked data), and is more precisely estimated. Second, Supplemental Appendix Table A.8 replicates equation (1) but shows each event's effect separately. The results show that the effects are of similar magnitude to Table 1 when comparing the treatment phases separately.[25]

Randomization-Based Inference Tests.—We replicate Table 1 and compute the *p*-values from permutation tests similar to randomization-based inference tests (Athey and Imbens 2017; Young 2019). This tests whether the effects of digitization are due to chance based on the selection of districts that were assigned to be digitized in phase 1 and 2 relative to phase 3. Supplemental Appendix Table A.9 reports the *p*-value of 0.032 for OLS and 0.001 for the median specification, increasing confidence in our main analysis.

III. Why Did Tax Collection Decline?

We now investigate two possible channels behind the decrease in tax collection: a decrease in the tax base and a decline in the bureaucrats' performance. Our analysis suggests that the bureaucrat's performance is more likely to explain the decrease in tax collection.

A. *Changes in the Tax Base*

Recall that the tax collected by bureaucrats is based on two measures: the area cultivated by farmers and the profits of the farmers, as described in Section I. The amount of tax due is calculated based on the maximum of the tax due on cultivated area and the tax due on profit. The digitization reform could have directly impacted both of these dimensions of the tax base. On the one hand, more secure property rights could lead farmers to start cultivating plots of lands whose ownership was previously disputed or encourage landowners to rent out land to more productive farmers, thus increasing productivity and possibly farm profits (see, e.g., Beg 2022a). On the other hand, more secure property rights can lead to structural change encouraging farmers to move from agriculture to other sectors, thereby reducing cultivated area. We show in this section that digitization had no significant effect on cultivated area or farmers' profits.

To show this, we use four different outcome variables: farm-level profits, the satellite vegetation cover index, a measure of whether land owned was irrigated or not, and the log of agricultural land owned. For each measure, we estimate the effects using the same specification as equation (1).[26]

[25] While the OLS coefficient is larger for event 2 than event 1, the reverse is true for the median. This could be due to the presence of outliers in one of the two treated phases but not the other.

[26] As described in subsection IB, the farm-level profit data come from a survey of farmers that is only available for four waves (2005, 2007, 2011, and 2013) and not yearly. We therefore modify our definition of the treatment year for the estimation based on this outcome: We pool phase 1 and phase 2 districts and define them as digitized

Table 3—Did the Digitization Reform Affect the Agricultural Tax Base?

	Farm profit per acre (1)	Satellite vegetation cover index (2)	Whether agri land irrigated? (3)	Log agricultural land owned (4)
Digitization of land records	4.909	0.00724	−0.0000514	0.0635
	(3.212)	(0.00570)	(0.0490)	(0.0444)
	[3.375]	[0.00563]	[0.0443]	[0.0432]
Dependent variable mean	23.4	0.53	0.12	7.69
District fixed effects	Yes	Yes	Yes	Yes
Fiscal year fixed effects	Yes	Yes	Yes	Yes
Observations	5,986	288	161,796	161,836

Notes: Unit of observation is a household-survey wave in column 1, a district-fiscal year in column 2, and a citizen-survey wave in columns 3 and 4. "Farm profit per acre" is the difference between value of output and total expenses per acre, based on HIES data sourced from Beg (2022a) (restricted to cultivating households), across survey waves 2005, 2007, 2011, and 2013. For this measure, "Digitization of land records" is a dummy variable equal to 1 for phase 1 and 2 districts in the 2013 wave, and 0 otherwise, and dependent variable mean is average profit per acre across all districts and across waves 2005, 2007, and 2011. "Satellite vegetation cover index" is the NDVI (ranging from −1 to 1), obtained from NASA's Moderate Resolution Imaging Spectroradiomete (MODIS) land products. For this measure, "Digitization of land records" is a dummy variable equal to 1 for phase 1 and 2 districts in every year from fiscal year 2012 and fiscal year 2013 respectively, and 0 otherwise, and dependent variable mean is the average value of the index across all districts and all years from fiscal year 2006 to fiscal year 2011, prior to any district's digitization. "Whether agricultural land irrigated" is a dummy variable equal to 1 when the household's agricultural land is irrigated, based on PSLM survey data. "Agricultural land owned" measures the acres of agricultural land owned by households based on PSLM survey data. We use the 2006, 2008, 2010, and 2012 waves of the survey. For these two measures, "Digitization of land records" is a dummy variable equal to 1 for phase 1 districts in the 2012 wave and 0 otherwise. Dependent variable mean are the respective average of each outcome variables (in levels, not in logs for both columns) across all districts and across waves 2006, 2008, and 2010. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1,000 replications) are in square brackets.

Table 3 shows the results. Column 1 estimates the effect of digitization on profits, while columns 2–4 estimate the effect on cultivated land by using the satellite vegetation cover index and the survey data on irrigation and land owned as proxies. The coefficient in column 1 shows that digitization had a positive but not statistically significant effect on profits. Columns 2 to 4 show that digitization had a small and insignificant effect on cultivated land. Supplemental Appendix Table A.10 shows that the results are robust to using the stacked specification.

The positive coefficients are consistent with the findings of Beg (2022a), who exploits the same reform to measure its effects on land and labor markets. Beg (2022a) shows that digitization increased the productivity of farmers due to two mechanisms: a reallocation of land to more productive farmers and an improvement in the use of inputs and investments. Like us, she finds a positive but not statistically significant effect of the reform on farm profits. She shows that this lack of effect can be explained by a decrease in average farm productivity (as farms become larger) offsetting the positive effect of reallocating land to more productive farmers. While she finds a positive and significant effect on cultivated area per farm, she also finds

for the 2013 wave while phase 3 districts remain in the control group. Similarly, data on agricultural land ownership and irrigated land from the PSLM survey are available for the 2006, 2008, 2010, and 2012 waves. In this case, we define phase 1 districts as digitized for the 2012 wave, while phase 2 and 3 districts remain in the control group.

that the number of households operating farms decreases, and that the increase in aggregate cultivated area is not statistically significant, which is consistent with the null effect we find on cultivated area.

Together, these results imply that digitization did not lead to a decrease in the tax base: Cultivated area and profits remained unchanged or weakly increased as a result of it. A change in the tax base is therefore unlikely to explain the decrease in tax.

B. *Effect on Performance of Bureaucrats*

If tax collection decreased, as shown in Section II, but the tax base did not, as shown in subsection IIIA, then the digitization reform might have reduced the bureaucrats' effectiveness in collecting taxes. That is, the reform reduced fiscal capacity. In this section, we provide evidence that the reform did decrease the tax assessment and collection by bureaucrats.

Changes in Tax Assessment.—Bureaucrats determine the size of the cultivated area and its characteristics (irrigation, type of crops) during their crop inspection in fall. This assessment is then used to determine the tax demands that are issued to farmers. If the reform changed the way bureaucrats conducted their tax assessment, it could have led to a fall in tax demand. This fall, in turn, could explain why tax revenue decreased.

To investigate whether this was the case, we use two sets of data: data from the Directorate of Agriculture, which record district-level cultivated areas based on reports provided by the bureaucrats we study, and administrative data on tax demands issued by these bureaucrats to taxpayers, aggregated at the district level.[27] For each measure, we estimate the effects using the same specification as in equation (1).

Table 4 shows the results: After the digitization reform, districts with digitized land records had 10 percent lower reported cultivated areas (column 1), as well as 45 percent lower tax demands (which include both cultivated-area based tax and profit-based tax; see column 2), relative to districts with manual land records.[28] This is despite the fact that the vegetation cover index, the farmers' profits, and the agricultural land irrigated or owned did not decrease significantly, as shown in Table 3. Supplemental Appendix Table A.11 also shows that the results are robust to using the stacked specification. These results indicate that the digitization reform led bureaucrats to underreport the tax base and reduced the tax demands issued to farmers.

Table 4 shows that, while reported cultivated areas decreased by 10 percent, tax demands decreased by 45 percent. There can be several explanations behind this difference. First, since there is a threshold of 12.5 acres below which no tax is due, the fall in *assessed* cultivated areas (shown in column 1) could have translated into a disproportional fall in tax demand. This could happen, for example, if the

[27] We observe these data at the revenue circle level in the tax records and not at the taxpayer level. For consistency, we also aggregate these data at the district level. Supplemental Appendix Table A.12 presents results using the disaggregated data, which are consistent with the findings reported in this section.

[28] These effects are approximated using the transformations $\exp(-0.100) - 1 = -0.10$ and $\exp(-0.600) - 1 = -0.45$, respectively.

Table 4—Bureaucrats' Assessments of the Tax Base (District Level)

	Log assessed cultivated area (1)	Log admin tax demands (2)
Digitization of land records	−0.100	−0.600
	(0.0338)	(0.211)
	[0.0326]	[0.214]
Dependent vararible mean	1,069.2	28,685.6
District fixed effects	Yes	Yes
Fiscal year fixed effects	Yes	Yes
Observations	214	203

Notes: The unit of observation is a district-fiscal year. "Digitization of land records" is a dummy variable that takes value 1 for phase 1 and 2 districts in every year from fiscal year 2012 and fiscal year 2013 respectively, and remains 0 otherwise. The reported cultivated area is measured in thousands of acres, while the administrative tax targets is in thousands of Pakistani rupees. Dependent variable mean is the average assessed cultivated area and tax demand (in levels, not logs) across all districts and all years from fiscal year 2006 to fiscal year 2011, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1,000 replications) are in square brackets.

reform increased collusion between farmers and bureaucrats, leading bureaucrats to under report cultivated areas and to assign more farms to the 0–12.5 acre tax band as a result. Second, the reform could have led to a distributional change in the tax base. As Beg (2022a) shows, the reform led to a decrease in the number of farms but an increase in the size of cultivated area per farm. This increase in farm size could have led farmers to move from the cultivated area-based tax regime to the profit-based tax regime. If it is easier to under report the tax base in the latter (because it is self-declared rather than based on bureaucrat inspections), then the distributional change in the tax base could also explain the decrease in tax assessment. While a distributional change in the tax base can explain the 45 percent drop in tax demand (column 2), it cannot explain the 10 percent decrease in reported cultivated area, as large farms are also included in these reported areas. While it is a possibly important part of the story, this second explanation would therefore not rule out a change in the bureaucrats' behavior.

The first explanation (that collusion increased) is consistent with a bribe displacement effect (Yang 2008; Sequeira 2011, 2016; Dávid-Barrett and Fazekas 2020). Our survey of the bureaucrats and household surveys indicated an important drop in bribes from land record services, as discussed in Section I (see Figure 6 and Supplemental Appendix Figure B.9 and Figure B.10). The digitization reform could have therefore led bureaucrats to try and make up for this lost income by increasing collusion on tax assessment. In this scenario, we should expect not only bribes for land services to fall, but also bribes for tax assessments to increase. While we do not have direct evidence on the change in bribes for tax assessment, our survey of bureaucrats provides some indicative indirect evidence. First, while the respondents reported that the monthly income of a bureaucrat decreased by Rs 7,248 after the reform, we estimate that the loss of bribe income from issuing land records was around Rs 16,775.[29] The gap between the two figures suggests that

[29] The average incomes are based on responses from 45 and 41 respondents, respectively, as most respondents refused to answer that question or provide a nonzero amount. The estimated bribe loss is based on the average

bureaucrats could have obtained around Rs 9,500 from other income sources, possibly from bribes on assessments. Second, the respondents reported little change in how their expenditure was split before and after the reform: Their household consumption remained at 45 percent of their expenditure, their travel expenses remained at 14 percent, and other expenses only changed from 11.7 percent to 13.4 percent after the reform (based on 804 respondents). If the decrease in income associated with the significant loss of bribe income from land services had not been compensated by an increase in other sources of income, we would have expected a shift in the allocation of expenditures from luxury (like travel and other expenses) to necessities (like household consumption).

The decrease in the reported tax base and the corresponding lower tax demand can explain part of the decrease in tax income shown in Table 1. However, we show in the next subsection that tax collection decreased even relative to this reduced tax demand. Collusion between bureaucrats and taxpayers in assessing cultivated areas can therefore not explain all of the decrease in tax collection.

Change in Performance Relative to Tax Demand.—In addition to the decrease in the reported tax base and the associated tax demand, it is possible that the digitization reform led bureaucrats to *collect* less tax. We investigate whether tax collection performance declined by looking at four different measures.

First, we look at the effect of the digitization reform on the tax collected by bureaucrats as a percentage of the tax demand they need to collect. The tax demand issued by bureaucrats is the target that bureaucrats are expected to collect by their superiors. We complement this measure with two alternative variables: whether bureaucrats achieved at least 50 percent of their targets, and whether they achieved at least 75 percent of their targets. Finally, we also analyze whether the reform affected the bottom end of the performance distribution by looking at the share of months per year in which the bureaucrats collected no taxes at all.

Combining the bureaucrat survey data with tax collection records allows us to carry out the analysis at the individual bureaucrat level instead of the district-level analysis in the previous section. For each measure, we estimate the effects using the same specification as in equation (1), but where the unit of analysis is a bureaucrat–fiscal year instead of a district–fiscal year.[30]

Table 5 shows the results. Column 1 shows that the digitization reform led to a substantial decrease in the bureaucrats' performance. Bureaucrats in digitized districts collected 35 percentage points less of their collection target after digitization,

reported "tip" for a land title of Rs 305 (based on 192 responses) multiplied by an average of 55 land services provided per month per bureaucrat. This is likely to underestimate the amount of bribes for two reasons, first because social desirability bias should lead respondents to understate the amount of bribes and second because the question was based on bribes for land titles, while bribes for transactions records are likely to be larger (World Bank 2017).

[30] In Supplemental Appendix Table A.12, we repeat this analysis using bureaucrat fixed effects instead of district fixed effects to exploit within-bureaucrat variation in performance and account for bureaucrat-level unobserved heterogeneity. This table also presents the bureaucrat-level specification for the tax demand regression shown in the second column of Table 4 (the reported cultivated area, shown in column 1 of Table 4, is measured at the district level and therefore cannot be analyzed at the bureaucrat level). For the assessment regression, we do not apply the log transformation used in Table 3, as 21 out of 301 observations in the bureaucrat-level data have zero demand. Given that log-like transformations such as the inverse hyperbolic sine are sensitive to scale (Chen and Roth 2024), we present the results in levels. The last column shows that the estimated effects on assessments are of the same order of magnitude as those in the district-level regression, though smaller (30 percent versus 45 percent) and less precisely estimated.

Table 5—Did the Digitization Reform Affect the Performance of Bureaucrats

Dependent variables:	Performance of bureaucrats			
	$\frac{Tax\_collected}{Tax\_demand}(\%)$	Whether at least 50 percent tax demand was collected	Whether at least 75 percent tax demand was collected	Share of months with zero collection
	(1)	(2)	(3)	(4)
Digitization of land records	−35.42	−0.394	−0.417	0.263
	(11.52)	(0.128)	(0.122)	(0.116)
	[11.59]	[0.131]	[0.127]	[0.115]
Dependent variable mean	53.9	0.53	0.43	0.19
District fixed effects	Yes	Yes	Yes	Yes
Fiscal year fixed effects	Yes	Yes	Yes	Yes
Observations	304	304	304	304

Notes: The unit of observation is a bureaucrat-fiscal year. "Digitization of land records" is a dummy variable that takes value 1 for phase 1 and 2 districts in every year from fiscal years 2012 and 2013 respectively, and remains 0 otherwise. The first measure is the ratio of the tax they collected to the tax demand they issued. The second and third measure are dummy variables that take values 1 if at least 50 percent (75 percent) of the annual tax demand was achieved, and remains 0 otherwise. The final measure is the share of months in the fiscal year in which no tax was collected. For each column, dependent variable mean is the average of the respective outcome variable across all bureaucrats and all years from fiscal years 2006 to 2011, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1,000 replications) are in square brackets.

relative to nondigitized districts (66 percent of control mean, p-value < 0.01). Supplemental Appendix Table A.13 also shows that these results are robust to using the stacked specification. We can exclude the possibility that this decrease is due to the denominator increasing since Table 4 shows that tax demands decreased, if anything, as a result of the digitization reform. In other words, tax collection decreased even more than the tax demands did, implying that the effectiveness of bureaucrats at collecting taxes went down.

One possibility is that this is driven by bureaucrats whose tax collection dropped completely, given that the tax collected was quite a low percentage of tax demand, even before digitization (54 percent, on average). However, columns 2 and 3 show that the digitization reform also affected the ability of bureaucrats to achieve higher levels of tax demands: Bureaucrats were 39 percentage points less likely to collect at least 50 percent of the tax demands in their area, and 42 percentage points less likely to collect at least 75 percent of these tax demands (p-values < 0.01). Finally, column 4 shows that digitization also affected the bottom of the performance distribution. Indeed, the share of months in which no tax was collected at all increased by 26 percentage points (p-values < 0.05) in digitized districts after the reform.

These results can be explained by the digitization reform reducing the leverage that bureaucrats had over taxpayers. Before the reform, bureaucrats had influence over the taxpayers' decision to pay taxes because they could choose to delay the resolution of the taxpayers' land issues (such as issuing a land title, or resolving a dispute) if taxes were not paid in full. After the reform, bureaucrats lost this source of influence and their capacity to collect taxes decreased. A loss of influence is, in fact, the main reason cited by bureaucrats for the negative effect of the reform on tax collection (see Figure 5). Further results from our survey of the bureaucrats also support this mechanism. Bureaucrats reported an important decline in their interactions with politicians (see Supplemental Appendix Figure B.14). In

our context, politicians are often large landowners and would therefore benefit from the bureaucrat's help with resolving land issues (Javid 2011). Following the reform, these politicians no longer needed to interact with bureaucrats as often if these bureaucrats could not help them resolve land issues. Politicians could help bureaucrats collect taxes but bureaucrats reported that they were less likely to do so following the reform (see Supplemental Appendix Figure B.15 and Supplemental Appendix Figure B.16). We interpret this type of exchange of favor as a form of influence that bureaucrats lost as a result of the reform. Before the reform, they could promise to help politicians with their land issues in exchange for help collecting taxes from farmers. After the reform, bureaucrats lost this leverage and no longer received help with their tax collection.

The decline in bureaucrats' performance, together with the analysis of the tax base presented in subsection IIIA, indicates that the responsibility for the decrease in fiscal revenues lies with the bureaucrats' behavior rather than changes in the tax base. This decrease in performance can be attributed to both underreporting of the tax base and lower tax collection relative to tax demands.

Alternative Mechanisms.—There are other possible channels through which the digitization reform could have affected bureaucratic performance in tax collection. We discuss each in turn.

Temporary Disruptions in Bureaucrats' Tasks: The bureaucrats were required to support the reform by helping correct records that had been digitized when necessary. Indeed, 59 percent of bureaucrats reported that some tasks were added as a result of the reform (see Suppplemental Appendix Figure B.5), most of which involved correcting records for digitized centers (see Supplemental Appendix Figure B.7). If correcting records distracted bureaucrats from collecting taxes, this disruption could partly explain the decrease in collection. However, this channel seems unlikely to explain the large fall in tax collection that we observe for two reasons. First, because most bureaucrats did not report that these new tasks added to their hours worked (see Figure 4 and Supplemental Appendix Figure B.8). Second, because, of the 46 percent of bureaucrats who reported that digitization made tax collection worse, only 2 percent indicated that this was due to additional tasks (Figure 5).

Changes to Information Available to Bureaucrats: The reform could have affected the information available to bureaucrats in two ways. First, the reform could have led bureaucrats to lose access to information on land records, which might be necessary to determine the owner of a plot of land. Without this information, bureaucrats might be unable to issue tax demands to the right taxpayer, which in turn could reduce tax demands and tax collection. Government reports (Board of Revenue 2011) and qualitative interviews with the bureaucrats reveal that this was not the case. After the reform, the provincial government ensured that bureaucrats were given hard copies of the records from the digitized record centers. These records helped them continue to carry out crop inspections and subsequent tax-related activities. Second, if the reform reduced interactions between the bureaucrats and taxpayers, bureaucrats could have lost information about the ability of different farmers to pay their tax (Dzansi et al. 2022; Balán et al. 2022). However, bureaucrats still

frequently interacted with the local population after the reform. Besides carrying out two crop inspections per year, the bureaucrats are also active community members (Aman-Rana, Minaudier, and Sukhtankar 2026). These interactions allow them to easily obtain information about the farmers' ability to pay. In addition, we show in Supplemental Appendix Table A.14 that districts where local information was more likely to be important for the bureaucrats did not experience a larger decline in tax collection due to the digitization reform. Specifically, we use the variance in tax demand across years within each district at baseline (fiscal years 2006–2011) as a proxy for the importance of local information. We then test whether districts with higher variance (and therefore where bureaucrats cannot rely as much on their experience from previous years to support their current tax collection) experienced a larger fall in tax collection. We find that this was not the case.

Changes in Monitoring of Bureaucrats: The reform could have affected the way supervisors monitored the bureaucrats, which, as a result, would have affected their incentives. This would be in line with theoretical explanations of multitasking problems such as Dewatripont, Jewitt, and Tirole (1999a). While we cannot rule out that the reform led supervisors to change the type of information they used to assess the bureaucrats' performance, we note that there was no change in the incentive or monitoring structure of the bureaucrats. Moreover, Supplemental Appendix Table A.5 shows that our results remain robust to controlling for the proportion of the bureaucrats' managers in each district whose ability was above median, and Supplemental Appendix Figure B.14 shows that bureaucrats did not report significant changes in their interactions with supervisors following the reform.

Sabotage by Influential Taxpayers: If taxpayers were against the reform, they could have attempted to stop it through active sabotage. Taxpayers may have refused to pay taxes to express their dissatisfaction with the new system, which would explain the fall in fiscal revenues. However, satisfaction was high among both small and large farmers, with 69 percent of farmers reporting a good or very good experience with the new bureaucracy (see Supplemental Appendix Figure B.10).

IV. Discussion

Transitory versus Persistent Effects of the Reform.—We confirm that the disruptions are not simply short-run "teething problems" by using data at the revenue circle-fiscal year level (a lower geographical unit than the district, comprising a few villages). Since we define districts as digitized when at least 5 percent of villages in their phase have been digitized, there are many revenue circles that are not digitized immediately within a district that we count as digitized. Therefore, while each set of districts is considered to be digitized one year apart in our main analysis, the lag between the first digitized revenue circles and the last ones to be digitized is up to five years. Using the rollout across revenue circles—rather than the planned rollout across districts—enables us to compare revenue circle-level tax collection before the reform to tax collection up to five years after the reform, using revenue circles not yet digitized (or never digitized) as a comparison group. We estimate a regression similar to equation (3) but at the revenue circle level. We define a revenue circle as digitized

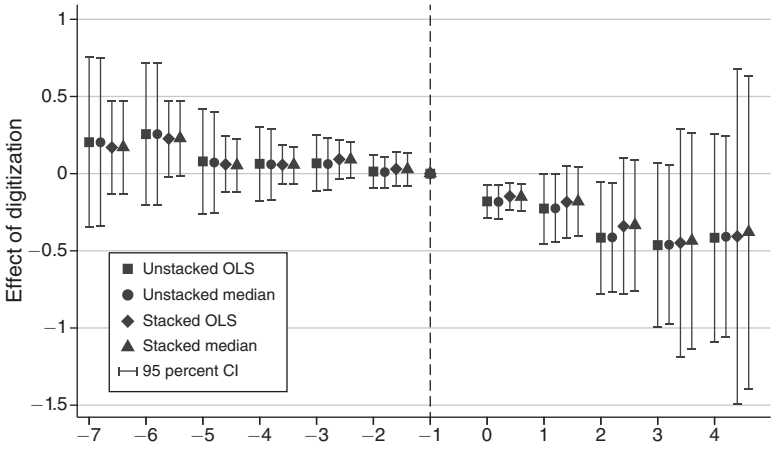

FIGURE 10. REVENUE CIRCLE-LEVEL EVENT-STUDY PLOT FOR TAX COLLECTED PER ACRE

Notes: The data are at the revenue circle-fiscal year level. For the unstacked OLS coefficients, we estimate the following regression for revenue circle r, in district d, and fiscal year t: $y_{rdt} = \tau_r + \tau_t + \sum_{k=-7}^{k=4} \rho_k Actual\_Digitization_{k(rdt)} + u_{rdt}$, where y_{rdt} is tax collected per acre, $Actual\_Digitization_{k(rdt)}$ is a set of indicator variables that takes value one if a revenue circle r in district d and fiscal year t was k years away from being digitized, and τ_r and τ_t are revenue circle and fiscal year fixed effects, respectively. We classify a revenue circle as digitized in year t if at least one village within revenue circle r is digitized in that year. The reference year is the last fiscal year before a revenue circle is digitized. Revenue circle-by-event and fiscal year-by-event fixed effects are included in the stacked version. Standard errors are clustered at the revenue circle level. Results remain robust when standard errors are clustered at the district level. The specifications are similar for the median regressions. Pre-periods 8 and 9 years before treatment were dropped in all specifications, as they included only 27 observations—just 0.007 percent of the 3,947 total observations used in the regression—leaving insufficient power to estimate coefficients for these periods.

in a given year if at least one village in that revenue circle is digitized.[31] The results, presented in Figure 10, indicate that the negative effect of the reform persists over time. The effect remains negative for up to five years after the start of the digitization reform, although the estimates become less precise as we move further from the year of digitization.[32] While the event-study plot highlights the dynamics of the effect, Table 6 shows that the estimated magnitudes are very similar to those from the district-level regression in Table 1, even over a longer time horizon, with treatment effects ranging from 37 percent to 43 percent of the control mean (compared to 37 percent to 47 percent in Table 1). This suggests that, even after tax collectors and taxpayers have had several years to adjust to the new system, tax collection remains depressed.

The long-run decrease in tax collection following the reform is also reflected in aggregate statistics over the period of time we study. From 2006 to 2011, the agricultural tax collection across Punjab was increasing by 8.5 percent per year, on

[31] As in our main analysis, we also report results using a median specification. The median regression in this subsection is based on Machado and Silva (2019), which is less computationally demanding, as the Koenker (2004) estimator that we use for our main estimation does not converge for the revenue circle regressions. The estimator in Machado and Silva (2019), based on conditional means, offers a practical alternative but requires stronger moment existence assumptions. Supplemental Appendix Table A.15 replicates the median regressions from Table 1 using Machado and Silva (2019) and shows that the results are not sensitive to the choice of estimator.

[32] Supplemental Appendix Figure B.17 shows the number of revenue circles digitized over time, indicating that power decreases as we move further from the year of digitization.

Table 6—Effect of Digitization Reform on Tax Collection—Revenue Circle-Level Regression

Dependent variable:	Revenue circle tax/district cultivated acres			
	TWFE		Stacked DID	
	OLS	Median	OLS	Median
	(1)	(2)	(3)	(4)
Digitization of land records	−0.222	−0.223	−0.263	−0.261
	(0.101)	(0.0988)	(0.144)	(0.139)
	[0.102]	[0.0992]	[0.142]	[0.137]
Dependent variable mean	0.60	0.60	0.60	0.60
Revenue circle fixed effects	Yes	Yes	No	No
Fiscal year fixed effects	Yes	Yes	No	No
Rev. circle-by-event fixed effects	No	No	Yes	Yes
Fiscal year-by-event fixed effects	No	No	Yes	Yes
Observations	3,974	3,974	15,470	15,470

Notes: The unit of observation is a revenue circle-fiscal year. "Digitization of land records" is a dummy variable that takes value 1 from the year in which a revenue circle becomes digitized, and remains 0 otherwise. "Revenue circle tax/district cultivated acres" is the tax collected in a revenue circle (in thousands of Pakistani rupees) divided by the average cultivated area (in thousands of acres) in the corresponding district, at baseline. Dependent variable mean is the average of this variable across all revenue circles and all years from fiscal years 2006 to 2011, prior to any revenue circle's digitization. Standard errors clustered at revenue circle level are in parentheses. Clustered bootstrapped standard errors (with 1,000 replications) are in square brackets.

average. Had this annual growth continued over the period 2012–2017, the amount of tax collected would have been Rs 1.07 billion, or 2.4 times higher than the actual tax collection in 2017. Instead, overall agricultural taxation across districts of Punjab fell by 33 percent between 2011 and 2017.[33] By contrast, the neighboring province of Sindh, where the digitization of land record has not been completed (as of 2025), saw a 4.5 times increase in agricultural tax between 2011 and 2017 (Rana 2019). The government is aware of its failure to exploit the full revenue potential of the AIT, which has long been viewed as an underexploited source of revenue for the Pakistani government (Nasim 2012; Jamal 2021). In recent years, it has introduced various additional reforms to improve its collection. For instance, in 2019, the government changed the structure of the bureaucrats' career paths (Business Recorder 2019), and in 2021 it digitized the tax assessment process (*Girdawari*) and provided laptops to tax collectors (Butt 2021; Waleed 2022). Despite these changes, the collection of this tax remains low. In October 2024, the IMF explicitly included an improvement to agricultural tax collection as a condition for its financial support (IMF 2024), and in January 2025, the Government of Punjab passed new legislation to update the AIT. However, the new legislation did not seem designed to take advantage of the land record digitization (*Dawn Editorial* 2024).

Generalizability.—The reform we studied combined two changes: the digitization of the records and the removal of the bureaucrat's responsibility over land records

[33] Using our data, the amount of tax collected across all districts in fiscal year 2017, reweighted to adjust for missing districts, was Rs 439.03 million. The amount of tax collected across all districts in fiscal year 2011 (the last year before the reform started), adjusted for missing districts, was Rs 652.31 million.

as a result of the digitization. The local official's dual role as land record manager and tax collector is not idiosyncratic to Punjab, Pakistan, but widespread across the Indian subcontinent. The "Patwar system" in which local officials are responsible for both land record and agricultural tax collection predates British rule and is still in existence, under various names, in India (Shah et al. 2017) and Bangladesh (World Bank 2022). However, an important question is whether the effect of the reform would have been the same, had it only involved the digitization of land records but not the reallocation of responsibilities.

In this counterfactual world, we would expect the reform to have similar negative effects, but of a potentially different magnitude. The two main effects of the reform were to diminish the influence, or leverage, that the bureaucrats exerted over the population and to reduce the bribes they received from land services. In practice, this leverage took two different forms: artificially delaying the issuance of a land permit (Rasheed 2024) or refusing to resolve a land dispute in someone's favor (Bokhari 2013; Tariq 2019), in exchange for tax payment. Digitization itself removed several of these levers of influence because the digitized process is fast, is harder to tamper with, and creates a paper trail documenting bureaucrats' misconduct (Omer 2021). As a result, even if bureaucrats retained responsibility over digitized land services, it would be harder for them to create the delays or record changes that allowed them to exert influence over the population. With less influence, the bureaucrats' ability to collect taxes would therefore also decrease.[34] However, we would expect the magnitude of the effects to be smaller in a reform in which records were digitized, but bureaucrats retained control of land services than in the one we study since the bureaucrats' leverage over taxpayers would not completely disappear.

The loss of influence that resulted from the reform we study is not unique to our context. Indeed, digitization reforms often reshape interactions between bureaucrats and the population or replace the informal processes that bureaucrats used to enforce the law. For instance, Muralidharan, Niehaus, and Sukhtankar (2016) show that, when biometric smartcards were introduced in India, both the adoption of new digital technology and the reorganization induced by this reform played an important role. Okunogbe and Pouliquen (2022) show that the digitization of corporate tax filing in Tajikistan also replaced direct interactions between tax collectors and firms and, as a consequence, removed an informal lever that tax collectors used to enforce tax collection.

Weighing Costs and Benefits.—Although the AIT accounts for only about 3 percent of total government revenues in Punjab (Nasim 2012), its persistently low collection has long been a concern for the Pakistani government (Nasim 2012; Cevik 2018; IMF 2024), and the digitization reform further strained the state's already limited capacity to enforce tax compliance.

The lost revenues represent a significant share of the reform's cost. Our estimate of the total tax loss due to the reform is Rs 258 million (Rs 6.74 per acre multiplied

[34] By reducing the bureaucrats' ability to create delays or tamper with records, digitization would also reduce the bribes that bureaucrats can extract and thus have negative effects on tax assessment due to bribe displacement. Indeed, comparing the baseline and end-of-project surveys of users of land services showed that corruption was lower among the new bureaucrats processing the digitized services (Gallup Pakistan 2009; Apex Consulting Pakistan 2016).

by a total of 38.3 million cultivated acres across Punjab) per year. This annual tax loss represents 17 percent of the reform's annual average operating cost (including staffing of the digitized centers, internet connection, and overheads) over the period we study (World Bank 2017, table 5, annex 3). Extrapolating the forgone taxes over time, this loss represents between 6 percent (for three years of loss) and 9 percent (for five years of loss) of the reform's total capital cost (including software development, construction of the digitized centers, and hardware for the centers).

However, since the reform also had a range of positive impacts, it is important to compare the tax loss to the benefit that the reform brought. We therefore evaluate its effect on the cost-benefit analysis of the reform using the marginal value of public fund (MVPF) approach proposed by Hendren and Sprung-Keyser (2020).[35] Including the discounted value of the loss in tax revenue over the economic life of the project into the MVPF calculation decreases the MVPF by 6.9 percent from 1.82 to 1.70. This highlights that taking into account the indirect impact of the reform on tax revenue is important to assess the reform's value. We provide the full calculations in Supplemental Appendix G.

V. Conclusion

Building strong state capacity is a prerequisite for sustainable economic development. However, state capacity is not simply the sum of the technologies and processes in which governments invest. The capacity of states to raise taxes and protect property rights also depends on the behavior of state officials.

We show that technological reforms in bureaucracies can have unintended consequences by changing the relationship between bureaucrats and taxpayers. Despite the positive effect of digitization on property rights and agricultural productivity, we find that the reform decreased the collection of agricultural tax.

Our findings highlight two key dimensions of the reform we study. First, bureaucrats relied on their personal influence and informal arrangements to enforce taxes. Second, bureaucrats had a broad scope of responsibilities, meaning that digitization of one function can have spillover effects on other tasks. This suggests digitization reforms are likely to have unintended negative consequences when they reduce bureaucrats' informal levers of compliance or target only a subset of their activities, but less so when enforcement mechanisms are formalized, bureaucratic influence is limited, or the scope of the bureaucrats' tasks is narrow. Finally, these negative consequences are not inevitable, especially in situations where digitization was designed to overcome principal-agent problems (Muralidharan, Niehaus, and Sukhtankar 2016; Dal Bó et al. 2021; Dodge et al. 2023).

Our data allow us to estimate the causal impact of the reform for up to two years after its implementation. In the longer run, bureaucrats may adapt their behavior—for example, by rebuilding relationships with the population—to offset the initial effects of the reform. The state itself may also adjust, by revising the

[35] The MVPF is particularly well-suited for our case. It is calculated as $MVPF = \frac{Benefits}{Net\_Govt\_Cost} = \frac{\Delta W}{\Delta E - \Delta C}$, where W denotes the individual benefits across the population, E is the government's expenditure on the policy, and C denotes the long-run change in government costs due to the policy's causal effect (Hendren and Sprung-Keyser 2022). This last parameter allows us to incorporate the loss in tax revenue due to the reform.

implementation of the reform and incorporating complementary organizational changes. While our results suggest that impacts may have persisted beyond the two-year window, the long-run consequences of digitization reforms for state capacity remain an open question that could be addressed in future research.

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