Spillovers in State Capacity Building: Evidence from the Digitization of Land Records in Pakistan*

Shan Aman-Rana[†], Clement Minaudier[‡]

March 2026

Abstract

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. The paper thus sheds light on the importance of understanding technological reforms from an organizational perspective.

JEL codes: D73, H11, H71, O12, O17

Keywords: Digitization, property rights, taxation, bureaucratic agency, bureaucracy, civil service, corruption, Punjab, Pakistan

*We are grateful to Patrick Agte, Charles Angelucci, Gaurab Aryal, Sam Asher, Emily A. Sellars, Oriana Bandiera, Sabrin Beg, Lauren Falcao Bergquist, Michael Best, Christina Brown, Pascaline Dupas, Claudio Ferraz, Raymond Fisman, Anne Fitzpatrick, Thomas Fujiwara, Bob Gibbons, Jessica Goldberg, Daniel A. N. Goldstein, Ali Hasanain, Ardina Hasanbasri, Seema Jayachandran, Anders Jensen, Namrata Kala, Andrew Kloosterman, David Lagakos, Dominic Leggett, Caroline Le Pennec-Çaldichoury, Gilat Levy, Jin Li, Meera Mahadevan, Brendon McConnell, Virginia Minni, John McLaren, Dilip Mookherjee, Mushfiq Mobarak, Anant Nyshadham, Paulina Oliva, Gerard Padro i Miquel, W. Bentley MacLeod, Ben Olken, Rohini Pande, Andrea Prat, Simon Quinn, Imran Rasul, Nic Ryan, Mark Rosenzweig, Carlos Scartascini, Joao Santos-Silva, Sheetal Seekhri, Kartini Shastry, Sandip Sukhtankar, Sebastian Tello-Trillo, Silvana Tenreyro, Eric Verhoogen, Stephanos Vlachos, Leonard Wantchekon, Jonathan Weigel, Stephane Wolton, Pinar Yildirim and seminar participants at workshops for comments and suggestions. Joe Basford, Yuanzhan Gao, Dylan Kaplan, Ryan Keller, Fuzail Khan, Yanwen Wang, Moon Kim, and Shine Yang provided excellent research assistance. The bureaucrat survey was approved by the University of Virginia IRB (Protocol #3840) in July 2020. Financial support from the IGC and UVA is gratefully acknowledged. All mistakes are our own.

[†]Department of Economics, University of Virginia. Email: sa8ey@virginia.edu

[‡]Department of Economics, City St George's, University of London. Email: clement.minaudier@city.ac.uk

1 Introduction

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 et al., 2012; Lewis-Faupel et al., 2016; Callen et al., 2020a; Dal Bó et al., 2021; Debnath et al., 2023), to improve the reliability of information on taxpayers (Ali et al., 2021; Okunogbe and Pouliquen, 2022; Brockmeyer and Sáenz Somarriba, 2022; Dzansi et al., 2022), and to identify welfare recipients (Muralidharan et al., 2016a).

However, the introduction of technology also forces a restructuring of bureaucracies. As public administration scholars have noted, digitization "reconfigures public sector organizations in fundamental, although uneven, ways" (Plesner et al., 2018). Digitization reforms can change the relationships between different bureaucratic agencies (Di Giulio and Vecchi, 2023) and increase specialization (Gundhus et al., 2022). 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 et al., 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, fifty-two 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 data set of rural agricultural taxes which we combine with data on the rollout of the reform to test this effect. We complement this 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%) reported that digitization negatively impacted tax collection. Of those, 64% 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% to 33% 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% decrease in tax collection in districts in the first two phases of the 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"

[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 (DILRMP) both computerized land records and integrated land record services with registration services. See https://dolr.gov.in/programmes-schemes/dilrmp-2/.

approach to avoid biases arising from treatment effect heterogeneity in staggered difference-in-differences designs (Cengiz et al., 2019; Deshpande and Li, 2019; Borusyak et al., 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 et al., 2016b; 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 which could have 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 non-digitized 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% of the average tax collection performance before digitization. The proportion of bureaucrats collecting at least 50% of their target and the proportion collecting at least 75% 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% lower five years after the start of the reform compared to the neighboring province of Sindh which 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 et al., 2016b; 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 which 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 et al., 2012; Lewis-Faupel et al., 2016; Callen et al., 2020a; Dal Bó et al., 2021; Dal Bó et al., 2021; Muralidharan et al., 2021; Callen et al., 2023; Debnath et al., 2023; Barnwal, Forthcoming; Muralidharan et al., 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), VAT records (Ali et al., 2021; Brockmeyer and Sáenz Somarriba, 2022; Fan et al., 2024), or customs tax (Chalendard et al., 2023), helped identify taxpayers and welfare recipients (Muralidharan et al., 2016a), 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 et al., 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 et al., 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 micro evidence 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 et al., 1999a; Rasul and Rogger, 2018; Chen et al., 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 et al., 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 (Garfias and Sellars, 2021; Vannutelli, 2022; Mastrorocco and Teso, 2023),[3] and emphasizes the importance of

[2]See e.g., McDonnell (2025), Evans (1995), Tsai (2007), Pepinsky et al. (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 et al., 2016, 2019), career concerns (Bertrand et al., 2020; Bazzi et al., 2025), reputation concerns (Mattsson, Forthcoming), monitoring (Callen et al., 2013), or autonomy in decision making (Rasul and Rogger, 2018; Duflo et al., 2018; Bandiera et al., 2021; Aman-Rana et al., 2025). Others show that the selection of bureaucrats is an important

informal authority in organizational performance (Baker et al., 1999; Gibbons and Henderson, 2012; Fenske et al., 2023; Aman-Rana et al., 2023).

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, 2014; 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 et al., Forthcoming; Basri et al., 2021), corruption (Besley and McLaren, 1993; Flatters and MacLeod, 1995; Le et al., 2020), or taxpayers' misreporting (Carrillo et al., 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 et al., 2016, 2019; Bergeron et al., Forthcoming, 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.

2 Background and data

2.1 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).

determinant of state effectiveness (Callen et al., 2020b; Barteska and Lee, 2023), where selection can be affected either at the recruitment stage (Dal Bó et al., 2013; Bai and Jia, 2016; Deserranno, 2019; Ashraf et al., 2020; Colonnelli et al., 2020; Moreira and Pérez, 2022), or through the assignment of bureaucrats across jobs or promotions (Iyer and Mani, 2012; Jia et al., 2015; Bergeron et al., 2022; Best et al., 2023; Aman-Rana, 2025).

The cultivated area-based tax is progressive and ranges from Rs. 300 to 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% of the amount of profits exceeding Rs. 4,800,000 (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% of farms (The 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 1st of July to 30th 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% of the land under 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

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

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 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). 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

[5] 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.

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 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% of respondents contacted the land record department 1-5 times per year, 18% more than 5 times per year, and only 9% never contacted them (Gallup, 2009). On average, in 2016, a bureaucrat issued around two documents per day.[7]

The reform relieved the bureaucrats of their land record-related duties, which also affected their interactions with the local population. Overall, 69% of bureaucrats reported that the reform changed their tasks, of which 75% said that some tasks were removed but 59% indicated that some tasks were also added (see Appendix Figure B.5 and Appendix Figure B.6). The tasks added were mostly about

[6] The estimated differences are relatively small in magnitude: all coefficients are below one 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.

[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.

record correction and additional paperwork, which was part of the transition from manual to digitized land records (see 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% of bureaucrats reported no change in hours worked, 4% reported a decrease, and 24% 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 confirms that the net reported decrease in hours is not significantly different from the net reported increase (see 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% of respondents indicated that the way to "remedy the problems faced in accessing land records" was to give a bribe, and 65% reported that they could not access land record services without unofficial payments (Gallup, 2009). Because the bureaucrats no longer had control over the land record process, they lost this source of bribe. Only 2% of households report paying a bribe for land records once those have been digitized and a majority of households had a good or very good experience with the newly digitized services (Apex Consulting, 2016, see Appendix Figure B.9 and Appendix Figure B.10). The bureaucrats reported a similar decline in bribes: Figure 6 shows that 48% of bureaucrats agreed that citizens want to tip to get land titles before digitization compared to 33% 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 reporting bribes after digitization (33%). 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.

2.2 Data sources and key variables

Digitization rollout. The data on digitization includes 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. This data indicates 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 (PLRA) in February 2018. This data describes 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% 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% 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.

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 Appendix Table A.1 and Appendix Table A.2, we show that our results are robust to using alternative thresholds than 5% of villages to define a phase as digitized.

Agricultural tax collection. We hand-collected the agricultural tax collection records of the Board of Revenue (BOR), 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 Appendix C for the record room and the proforma on which this information is collected). The data contains 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 is 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 includes monthly records at the revenue circle level for this period, covering 28,572 revenue circle–months. This data is 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 is 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 is therefore noisier. The resulting data is 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 Appendix Table A.3 and show in 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

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

[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). Appendix Figure B.12 shows that districts with at least some zero values are comparable to those without any zero values on baseline characteristics.

cultivated area is calculated as the average number of cultivated acres in a district between 2007-2011 but excluding 2009 which was missing.[11] The data on cultivated areas was 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 3.

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 Appendix D for details), a commonly used proxy for crop yield in developing countries (Rasmussen, 1992; Vrieling et al., 2011; Beg, 2022a) which allows comparisons of year-on-year changes in vegetation growth (Huete 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 represen-

[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.

tative 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. This data collects 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 (Agriculture Marketing Information Service, 2007–2014) who use the cultivated area reported by the bureaucrats we study to construct average cultivation measures across districts from 2007 to 2013.[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

[13] There is no data available for the years 2006 and 2009.

[14] In 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).

affected their interactions with superiors and with the population. We found 118 respondents who worked as *Qanungos* (revenue circle managers) between 2006-2013. 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 data set includes 78 respondents whose tax performance is observed between 2006-2013. 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 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] This data therefore allows us to study the effects of the reform on bureaucrats' performance.

3 Did the digitization reform affect tax collection?

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

3.1 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 bureaucracies are underperforming due to structural issues. Alternatively, some

[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 5 observations for which both positions had the same duration and 1 observation in which the position was only held for 14 days.

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 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 subsection 3.2.1.

3.2 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 years t between 2006-2013:

$$y_{dt} = \eta_d + \eta_t + \beta \text{Digitization}_{dt} + \varepsilon_{dt} \tag{1}$$

Our outcome variable, y_{dt} is the tax collected per acre in district d, during fiscal year t. Our treatment variable, Digitization_{dt}, 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 (2004)'s 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 weighting problems (Goodman-Bacon, 2021; Roth et al., 2023; De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Sun and Abraham, 2021). We therefore also report results from a "stacked regression" (Gormley and Matsa, 2011; Cengiz et al., 2019; Deshpande and Li, 2019; Baker et al., 2022). This approach constructs event-specific 2×2 datasets which 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:

$$y_{dth} = \mu_{dh} + \mu_{th} + \gamma \text{Digitization}_{dth} + \epsilon_{dth} \tag{2}$$

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

[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.

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% reduction relative to the control mean. The median estimates in Column (4) show that tax collection declined by Rs. 5.60 per acre at the median (p-value < 0.01), representing a 39% reduction relative to the control mean. Due to the skewness of the tax data, the median estimates are 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 (BISP)).[19]

Endogenous rollout and LATE estimates. Our intention to treat 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 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)-(2), and the associated Kleibergen-Paap

[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 × 1,063,250) and of Rs. 5,954,200 using the median estimate (5.60 × 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] Appendix Table A.4 presents the cumulative proportion of villages digitized in each phase for each year, which shows that there is two-sided non-compliance.

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 (FY2017), which implies a decrease of 15.46 in tax collected per acre. The estimated effect corresponds to between 82% and 109% 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 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% of villages digitized would therefore face a reduction in tax collection of Rs. 7.90 per acre, or 42% to 56% of the control mean.

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

[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 FY2006 to FY2011, 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.

3.2.1 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 one if district d in fiscal year t and event h 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% 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.

[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 Table A.3. In addition, since phase 1 districts were treated in FY2012 and our data begin in FY2006, the pre-period 7 years before treatment does not include any phase 1 districts. We therefore dropped the pre-periods 6 and 7 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 post-period coefficients become more imprecisely estimated.

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% of villages in that phase have been digitized by that year. We test the robustness of this result using both less conservative thresholds — defining a phase as *digitized* in a given year if at least 50% of villages have been digitized, and more conservative thresholds — defining a phase as *digitized* in a given year if at least 1% or 2% of villages in the corresponding districts have been digitized by that year.[24] The results are presented in 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 non-digitized 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 non-digitized ones. While such transfers are not allowed by law, we also use our data on the bureaucrats' careers to verify that transfers were rare. This data confirms 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 Appendix Figure B.13). We also rule out that changes at higher levels of the hierarchy could have driven the results. We show in 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

[24] Since the proportion of digitized villages changes throughout a year, there is a range of possible cutoffs which correspond to a given start year. In particular, any cutoff between 4% and 15% corresponds to the same starting years as in our main analysis. Any cutoff between 16% and 59% corresponds to phases 1 and 2 starting to be digitized from FY2013 and phase 3 from FY2014. With a 1% threshold, the treatment years are FY2011 for phase 1, 2012 for phase 2, and 2013 for phase 3. With a 2% threshold, phases 1 and 2 follow the same timeline, but phase 3 is digitized in 2014.

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. 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 tests in this subsection. We first show in 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% of the control mean (vs. 47% for the OLS estimate using the stacked data), and is more precisely estimated. Second, 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

[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.

be digitized in phase 1 and 2 relative to phase 3. 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.

4 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.

4.1 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 2. 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]

[26] As described in subsection 2.2, the farm-level profit data comes from a survey of farmers which is only available for 4 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

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. 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 re-allocation 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 re-allocating land to more productive farmers. While she finds a positive and significant effect on cultivated area per farm, she also finds 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.

4.2 Effect on performance of bureaucrats

If tax collection decreased, as shown in section 3, but the tax base did not, as shown in subsection 4.1, then the digitization reform might have reduced the bureaucrats' effectiveness in collecting taxes. That is, the reform reduced fiscal capacity. In this

phase 2 districts and define them as digitized 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 is 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.

section, we provide evidence that the reform did decrease the tax assessment and collection by bureaucrats.

4.2.1 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 records 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% lower reported cultivated areas (Column 1), as well as 45% lower tax demands (which includes 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. 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 under-report the tax base and reduced the tax demands issued to farmers.

Table 4 shows that, while reported cultivated areas decreased by 10%, tax demands decreased by 45%. There can be several explanations behind this dif-

[27] We observe this data at the revenue circle level in the tax records and not at the taxpayer level. For consistency, we also aggregate this data at the district level. 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.

ference. 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 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% drop in tax demand (column (2)), it cannot explain the 10% 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 2 (see Figure 6 and 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 bureaucrats

[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 non-zero amount. The estimated bribe

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% of their expenditure, their travel expenses remained at 14%, and other expenses only changed from 11.7% to 13.4% 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.

4.2.2 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% of their targets, and whether they achieved at least 75% of their targets. Finally, we also analyze whether the reform affected the

loss is based on the average 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).

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, relative to non-digitized districts (66% of control mean, p-value<0.01). 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%, 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% of the tax demands in their area, and 42 percentage points less likely to collect at least 75% of these tax demands (p-values<0.01). Finally, column (4) shows that digitization also affected the bottom of the perfor-

[30] In 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 (IHS) 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% vs. 45%) and less precisely estimated.

mance 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 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 Appendix Figure B.15 and 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 4.1, 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 under-reporting of the tax base and lower tax collection relative to tax demands.

4.2.3 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% of bureaucrats reported that some tasks were added as a result of the reform (see Appendix Figure B.5), most of which involved correcting records for digitized centers (see 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 Appendix Figure B.8). Second, because, of the 46% of bureaucrats who reported that digitization made tax collection worse, only 2% 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; Balan 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 et al., 2023). These interactions allow them to easily obtain information about the farmers' ability to pay. Second, we show in 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 (FY2006–FY2011) 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 et al. (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, 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 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% of farmers reporting a good or very good experience with the new bureaucracy (see Appendix Figure B.10).

5 Discussion

Transitory vs. 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% of villages in their phase have been digitized, there are many revenue circles that are not digitized immediately within a district which 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 5 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 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% to 43% of the control mean (compared to 37% to 47% 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.

[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 which 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. 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] 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.

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% per year, on 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% 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 Agricultural Income Tax, which has long been viewed as an under-exploited 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 agricultural income tax. 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 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

[33] Using our data, the amount of tax collected across all districts in fiscal year 2017, re-weighted 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.

still in existence, under various names, in India (Shah et al., 2017) and Bangladesh (The 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 re-allocation 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 (Dawn, 2013; Tariq, 2019), in exchange for tax payment. Digitization itself removed several of these levers of influence because the digitized process is fast, 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 et al. (2016b) 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

[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, 2009; Apex Consulting Pakistan, 2016).

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 Agricultural Income Tax accounts for only about 3% 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 by a total of 38.3 million cultivated acres across Punjab) per year. This annual tax loss represents 17% 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% (for three years of loss) and 9% (for five years of loss) of the reform' 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% 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 Appendix G.

[35]The MVPF is particularly well-suited for our case. It is calculated as $MVPF = \frac{\text{Benefits}}{\text{Net Govt Cost}} = \frac{\Delta W}{\Delta E - \Delta C}$, 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.

6 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 et al., 2016b; Dal Bó et al., 2021; Dodge et al., 2023).

Our data allows 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 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.

References

Abadie, Alberto, Susan Athey, Guido W. Imbens, and Jeffrey M. Wooldridge, "Should You Adjust Standard Errors for Clustering?," *The Quarterly Journal of Economics*, September 2023, *138* (1), 1–35.

Adeel, Muhammad, "Evaluating the Role of Cadastre Maps in Pakistan, Land Administration: GIS Perspective," in "Proceedings of the FIG Congress" 2010.

Agriculture Marketing Information Service, "District Wise Data of Pakistan's Agricultural Produce," Agriculture Marketing Information Service (AMIS), Directorate of Agriculture (Economics & Marketing) Punjab, http://www.amis.pk/Agristatistics/DistrictWise/DistrictWiseData.aspx, 2007–2014. Accessed: 2023-02-24.

Aker, Jenny C and Isaac M Mbiti, "Mobile phones and economic development in Africa," *Journal of Economic Perspectives*, 2010, *24* (3), 207–232.

Ali, Merima, Abdulaziz B Shifa, Abebe Shimeles, and Firew Woldeyes, "Building fiscal capacity in developing countries: Evidence on the role of information technology," *National Tax Journal*, 2021, *74* (3), 591–620.

Aman-Rana, Shan, "Meritocracy in a Bureaucracy," *Journal of Development Economics*, 2025, *175*, 103428.

_ and Clement Minaudier, "Data and Code for: Spillovers in State Capacity Building: Evidence from the Digitization of Land Records in Pakistan," 2026. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 2026-02-04. https://doi.org/10.3886/E238822V1.

_ , _ , and Sandip Sukhtankar, "Informal Fiscal Systems in Developing Countries," Technical Report 31793, National Bureau of Economic Research October 2023.

_ , Leonard Wantchekon, and Lazare Kovo, "Bureaucratic Deliberation and Performance: Evidence from a Field Experiment in Benin," 2025. Working paper.

Angelucci, Charles and Roi Orzach, "Job Scope and Motivation under Informal Incentives," *Available at SSRN 4362222*, 2023.

Apex Consulting, "Land Record Management Information System (LRMIS) End of Project Survey," 2016. Available on request from the Punjab Land Records Authority.

Apex Consulting Pakistan, "Land Record Management Information System (LR-MIS): End of Project Survey," Technical Report, Apex Consulting Pakistan 2016. Unpublished internal survey report.

Aron, Arthur, Tracy McLaughlin-Volpe, Debra Mashek, Gary Lewandowski, Stephen C Wright, and Elaine N Aron, "Including others in the self," *European Review of Social Psychology*, 2004, *15* (1), 101–132.

Ashraf, Nava, Oriana Bandiera, Edward Davenport, Scott Lee et al., "Losing Prosociality in the Quest for Talent? Sorting, Selection, and Productivity in the Delivery of Public Services," *American Economic Review*, 2020, *110* (5), 1355–94.

Athey, Susan and Guido W Imbens, "The econometrics of randomized experiments," in "Handbook of economic field experiments," Vol. 1, Elsevier, 2017, pp. 73–140.

Atkin, David, Azam Chaudhry, Shamyla Chaudry, Amit K Khandelwal, and Eric Verhoogen, "Organizational barriers to technology adoption: Evidence from soccer-ball producers in Pakistan," *The Quarterly Journal of Economics*, 2017, *132* (3), 1101–1164.

Bai, Ying and Ruixue Jia, "Elite recruitment and political stability: the impact of the abolition of China's civil service exam," *Econometrica*, March 2016, *84* (2), 677–733.

Baker, Andrew C, David F Larcker, and Charles CY Wang, "How much should we trust staggered difference-in-differences estimates?," *Journal of Financial Economics*, 2022, *144* (2), 370–395.

Baker, George, Robert Gibbons, and Kevin J Murphy, "Informal authority in organizations," *Journal of Law, Economics, and organization*, 1999, *15* (1), 56–73.

Balan, Pablo, Augustin Bergeron, Gabriel Tourek, and Jonathan L Weigel, "Local Elites as State Capacity: How City Chiefs Use Local Information to Increase Tax Compliance in the Democratic Republic of the Congo," *American Economic Review*, March 2022, *112* (3), 762–97.

Bandiera, Oriana, Michael Carlos Best, Adnan Qadir Khan, and Andrea Prat, "The Allocation of Authority in Organizations: A Field Experiment with Bureaucrats," *The Quarterly Journal of Economics*, August 2021, *136* (4), 2195–2242.

Banerjee, Abhijit, Esther Duflo, Clement Imbert, Santhosh Mathew, and Rohini Pande, "E-governance, accountability, and leakage in public programs: Experimental evidence from a financial management reform in India," *American Economic Journal: Applied Economics*, 2020, *12* (4), 39–72.

Banerjee, Abhijit V, Esther Duflo, and Rachel Glennerster, "Putting a band-aid on a corpse: incentives for nurses in the Indian public health care system," *Journal of the European Economic Association*, May 2008, *6* (2-3), 487–500.

Bardhan, Pranab, "State and development: The need for a reappraisal of the current literature," *Journal of Economic Literature*, 2016, *54* (3), 862–892.

Barnwal, Prabhat, "Curbing leakage in public programs: Evidence from India's direct benefit transfer policy," *American Economic Review*, Forthcoming.

Barteska, Philipp and Jay Euijung Lee, "Bureaucrats and the Korean Export Miracle," *Unpublished Manuscript*, 2023.

Basri, M. Chatib, Mayara Felix, Rema Hanna, and Benjamin A. Olken, "Tax Administration versus Tax Rates: Evidence from Corporate Taxation in Indonesia," *American Economic Review*, December 2021, *111* (12), 3827–71.

Bazzi, Samuel, Masyhur Hilmy, Benjamin Marx, Mahvish Shaukat, and Andreas Stegmann, "It Takes a Village Election: Turnover and Performance in

Local Bureaucracies," Technical Report, National Bureau of Economic Research 2025.

Beg, Sabrin, "Digitization and development: Property rights security, and land and labor markets," *Journal of the European Economic Association*, 2022, *20* (1), 395–429.

— , "Digitization and Development: Property Rights Security, and Land and Labor Markets [dataset]," 2022. Retrieved 2022-09-05.

Bergeron, Augustin, Gabriel Tourek, and Jonathan Weigel, "The State Capacity Ceiling on Tax Rates: Evidence from Randomized Tax Abatements in the DRC," *Econometrica*, Forthcoming.

— , Pedro Bessone, John Kabeya Kabeya, Gabriel Z Tourek, and Jonathan L Weigel, "Optimal assignment of bureaucrats: Evidence from randomly assigned tax collectors in the DRC," Technical Report 30413, National Bureau of Economic Research September 2022.

Bertrand, Marianne, Robin Burgess, Arunish Chawla, and Guo Xu, "The Glittering Prizes: Career Incentives and Bureaucrat Performance," *The Review of Economic Studies*, May 2020, *87* (2), 626–655.

Besley, Timothy and John McLaren, "Taxes and bribery: the role of wage incentives," *The Economic Journal*, 1993, *103* (416), 119–141.

— and Torsten Persson, "The origins of state capacity: Property rights, taxation, and politics," *American Economic Review*, September 2009, *99* (4), 1218–44.

— and — , "State capacity, conflict, and development," *Econometrica*, 2010, *78* (1), 1–34.

— and — , "Why do developing countries tax so little?," *Journal of economic perspectives*, November 2014, *28* (4), 99–120.

— , Robin Burgess, Adnan Khan, and Guo Xu, "Bureaucracy and development," *Annual Review of Economics*, 2022, *14*, 397–424.

Best, Michael Carlos, Anne Brockmeyer, Henrik Jacobsen Kleven, Johannes Spinnewijn, and Mazhar Waseem, "Production versus revenue efficiency with limited tax capacity: theory and evidence from Pakistan," *Journal of political Economy*, 2015, *123* (6), 1311–1355.

__ , Jonas Hjort, and David Szakonyi, "Individuals and organizations as sources of state effectiveness," *American Economic Review*, August 2023, *113* (8), 2121–67.

Bhavnani, Rikhil R and Alexander Lee, "Local embeddedness and bureaucratic performance: evidence from India," *The Journal of Politics*, 2018, *80* (1), 71–87.

Bó, Ernesto Dal, Frederico Finan, and Martín A Rossi, "Strengthening state capabilities: The role of financial incentives in the call to public service," *The Quarterly Journal of Economics*, April 2013, *128* (3), 1169–1218.

__ , __ , Nicholas Y Li, and Laura Schechter, "Information technology and government decentralization: Experimental evidence from paraguay," *Econometrica*, 2021, *89* (2), 677–701.

Board of Revenue, "Revised PC-I: Land Records Management and Information Systems Project (Phase-I)," Project Concept I (PC-I) Document, Board of Revenue, Government of the Punjab, Lahore, Pakistan 2011. 2nd Revision.

Board of Revenue, Government of the Punjab, "Land Records Management and Information Systems Project (Phase-I)," Technical Report, Board of Revenue, Government of the Punjab, Punjab, Pakistan 2011.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess, "Revisiting event study designs: Robust and efficient estimation," *Review of Economic Studies*, 2024, p. rdae007.

Brockmeyer, Anne, Alejandro Estefan, Karina Ramírez Arras, and Juan Carlos Suárez Serrato, "Taxing property in developing countries: theory and evidence from Mexico," Technical Report 28637, National Bureau of Economic Research April 2021.

__ and Magaly Sáenz Somarriba, "Electronic Payment Technology and Tax Compliance: Evidence from Uruguay's Financial Inclusion Reform," March 2022.

Business Recorder, "Punjab government changes name of 'patwari' post to 'village officer'," *Business Recorder*, 2019.

Butt, Waseem Ashraf, "Punjab revives old patwar system by replacing ARCs," *Dawn*, 2021.

Bó, Ernesto Dal, Frederico Finan, Nicholas Y. Li, and Laura Schechter, "Information Technology and Government Decentralization: Experimental Evidence From Paraguay," *Econometrica*, 2021, *89* (2), 677–701.

Callaway, Brantly and Pedro HC Sant'Anna, "Difference-in-differences with multiple time periods," *Journal of Econometrics*, 2021, *225* (2), 200–230.

Callen, Michael Joseph, Saad Gulzar, Syed Ali Hasanain, and Muhammad Yasir Yasir Khan, "The political economy of public employee absence: Experimental evidence from Pakistan," August 2013.

Callen, Michael, Saad Gulzar, Ali Hasanain, Muhammad Khan, and Arman Rezaee, "Data and policy decisions: Experimental evidence from Pakistan," *Journal of Development Economics*, 2020, *146* (C), 102523.

_ , _ , _ , Muhammad Yasir Khan, and Arman Rezaee, "Personalities and public sector performance: Evidence from a health experiment in Pakistan," Technical Report 21180, National Bureau of Economic Research August 2020.

_ , _ , _ , _ , and _ , "The political economy of public sector absence," *Journal of Public Economics*, 2023, *218*, 104787.

Carrillo, Paul, Dina Pomeranz, and Monica Singhal, "Dodging the Taxman: Firm Misreporting and Limits to Tax Enforcement," *American Economic Journal: Applied Economics*, April 2017, *9* (2), 144–64.

Cengiz, Doruk, Arindrajit Dube, Attila Lindner, and Ben Zipperer, "The effect of minimum wages on low-wage jobs," *The Quarterly Journal of Economics*, 2019, *134* (3), 1405–1454.

Census, 2010 The Agricultural, "The Agricultural Census 2010," 2010. [Online; accessed 30-July-2023].

Cevik, Serhan, "Unlocking Pakistan's revenue potential," *South Asian Journal of Macroeconomics and Public Finance*, 2018, *7* (1), 17–36.

Chaisemartin, Clément De and Xavier d'Haultfoeuille, "Two-way fixed effects estimators with heterogeneous treatment effects," *American Economic Review*, September 2020, *110* (9), 2964–2996.

Chalendard, Cyril, Ana M Fernandes, Gael Raballand, and Bob Rijkers, "Corruption in customs," *The Quarterly Journal of Economics*, 2023, *138* (1), 575–636.

Cheema, Iftikhar, Simon Hunt, Sarah Javeed, Tanya Lone, and Sean O'Leary, "Benazir income support programme: Final impact evaluation report," *UK: Oxford Policy Management*, 2016.

Chen, Jiafeng and Jonathan Roth, "Logs with zeros? Some problems and solutions," *The Quarterly Journal of Economics*, 2024, *139* (2), 891–936.

Chen, Yvonne Jie, Pei Li, and Yi Lu, "Career concerns and multitasking local bureaucrats: Evidence of a target-based performance evaluation system in China," *Journal of Development Economics*, 2018, *133*, 84–101.

Colonnelli, Emanuele, Mounu Prem, and Edoardo Teso, "Patronage and selection in public sector organizations," *American Economic Review*, October 2020, *110* (10), 3071–3099.

Dawn, "Fake mutations in land records," *Dawn*, 2013.

Dawn Editorial, "Agriculture tax," *Dawn*, 2024.

Debnath, Sisir, Mrithyunjayan Nilayamgode, and Sheetal Sekhri, "Information Bypass: Using Low-cost technological innovations to curb leakages in welfare programs," *Journal of Development Economics*, 2023, *164*, 103137.

Deserranno, Erika, "Financial incentives as signals: experimental evidence from the recruitment of village promoters in Uganda," *American Economic Journal: Applied Economics*, January 2019, *11* (1), 277–317.

Deshpande, Manasi and Yue Li, "Who is screened out? Application costs and the targeting of disability programs," *American Economic Journal: Economic Policy*, 2019, *11* (4), 213–248.

Dewatripont, Mathias, Ian Jewitt, and Jean Tirole, "The economics of career concerns, part I: Comparing information structures," *The Review of Economic Studies*, January 1999, *66* (1), 183–198.

_ , _ , and _ , "The economics of career concerns, part II: Application to missions and accountability of government agencies," *The Review of Economic Studies*, January 1999, *66* (1), 199–217.

Didan, K., "MYD13Q1 MODIS/Aqua Vegetation Indices 16-Day L3 Global 250m SIN Grid V006," https://doi.org/10.5067/MODIS/MYD13Q1.006 2015. Accessed: 2023-03-09.

Dodge, Eric, Yusuf Neggers, Rohini Pande, and Charity M Troyer Moore, "From Delay to PayDay: Easing Bureaucrat Access to Implementation Information Strengthens Social Protection Delivery," Technical Report, National Bureau of Economic Research 2025.

_ , _ , _ , and Charity Troyer Moore, "Updating the State: Information Acquisition Costs and Social Protection Delivery," Technical Report, Yale University Economic Growth Center September 2023. Working Paper.

Duflo, Esther, Michael Greenstone, Rohini Pande, and Nicholas Ryan, "The value of regulatory discretion: Estimates from environmental inspections in India," *Econometrica*, 2018, *86* (6), 2123–2160.

_ , Rema Hanna, and Stephen P Ryan, "Incentives work: Getting teachers to come to school," *American Economic Review*, June 2012, *102* (4), 1241–78.

Dzansi, James, Anders Jensen, David Lagakos, and Henry Telli, *Technology and Tax Capacity: Evidence from Local Governments in Ghana* number 29923, National Bureau of Economic Research, September 2022.

Dávid-Barrett, Elizabeth and Mihály Fazekas, "Anti-corruption in aid-funded procurement: Is corruption reduced or merely displaced?," *World Development*, 2020, *132*, 105000.

Evans, Peter B, *Embedded autonomy: States and industrial transformation*, Princeton University Press, 1995.

Fan, Haichao, Yu Liu, Nancy Qian, and Jaya Wen, "Technological Adoption and Taxation: The Case of China's Golden Tax Reform," *Tax Policy and the Economy*, 2024, *38* (1), 101–122.

Fenske, James, Muhammad Haseeb, and Namrata Kala, "How rules and compliance impact organizational outcomes: Evidence from delegation in environmental regulation," Technical Report, National Bureau of Economic Research 2023.

Flatters, Frank and W Bentley MacLeod, "Administrative corruption and taxation," *International Tax and Public Finance*, 1995, *2*, 397–417.

Fujiwara, Thomas, "Voting technology, political responsiveness, and infant health: Evidence from Brazil," *Econometrica*, 2015, *83* (2), 423–464.

Gadenne, Lucie and Monica Singhal, "Decentralization in developing economies," *Annual Review of Economics*, 2014, *6* (1), 581–604.

Gallup, "Baseline Survey for Land Records Management and Information Systems," Technical Report, Gallup Pakistan 2009.

Garfias, Francisco and Emily A Sellars, "Fiscal legibility and state development: Theory and evidence from colonial Mexico," *American Journal of Political Science*, 2021.

Garicano, Luis and Paul Heaton, "Information technology, organization, and productivity in the public sector: Evidence from police departments," *Journal of Labor Economics*, 2010, *28* (1), 167–201.

Gibbons, Robert and Rebecca Henderson, "Relational contracts and organizational capabilities," *Organization science*, 2012, *23* (5), 1350–1364.

Giulio, Marco Di and Giancarlo Vecchi, "Implementing digitalization in the public sector. Technologies, agency, and governance," *Public Policy and Administration*, 2023, *38* (2), 133–158.

Goodman-Bacon, Andrew, "Difference-in-differences with variation in treatment timing," *Journal of Econometrics*, 2021, *225* (2), 254–277.

Gormley, Todd A and David A Matsa, "Growing out of trouble? Corporate responses to liability risk," *The Review of Financial Studies*, 2011, *24* (8), 2781–2821.

Gundhus, Helene OI, Niri Talberg, and Christin T Wathne, "From discretion to standardization: Digitalization of the police organization," *International journal of police science & management*, 2022, *24* (1), 27–41.

Hanna, Rema and Shing-Yi Wang, "Dishonesty and selection into public service: Evidence from India," *American Economic Journal: Economic Policy*, August 2017, *9* (3), 262–90.

Hendren, Nathaniel and Ben Sprung-Keyser, "A unified welfare analysis of government policies," *The Quarterly Journal of Economics*, 2020, *135* (3), 1209–1318.

__ **and** __ , "The case for using the MVPF in empirical welfare analysis," Technical Report, National Bureau of Economic Research 2022.

Holmstrom, Bengt and Paul Milgrom, "Multitask principal–agent analyses: Incentive contracts, asset ownership, and job design," *The Journal of Law, Economics, and Organization*, January 1991, *7* (special_issue), 24–52.

Huete, Alfredo, Kamel Didan, Tomoaki Miura, E Patricia Rodriguez, Xiang Gao, and Laerte G Ferreira, "Overview of the radiometric and biophysical performance of the MODIS vegetation indices," *Remote sensing of environment*, 2002, *83* (1-2), 195–213.

IMF, "Pakistan: 2024 Article IV Consultation and Request for an Extended Arrangement under the Extended Fund Facility," IMF Staff Country Reports 2024/310, International Monetary Fund October 2024.

Iyer, Lakshmi and Anandi Mani, "Traveling agents: political change and bureaucratic turnover in India," *Review of Economics and Statistics*, 2012, *94* (3), 723–739.

Jamal, Nasir, "Punjab far from exploiting agri income tax potential," *DAWN*, April 4 2021.

Javid, Hassan, "Class, Power, and Patronage: Landowners and Politics in Punjab," *History and Anthropology*, 2011, *22* (3), 337–369.

Jensen, Anders, "Employment structure and the rise of the modern tax system," *American Economic Review*, January 2022, *112* (1), 213–234.

Jia, Ruixue, Masayuki Kudamatsu, and David Seim, "Political selection in China: The complementary roles of connections and performance," *Journal of the European Economic Association*, August 2015, *13* (4), 631–668.

Khan, Adnan Q, Asim I Khwaja, and Benjamin A Olken, "Tax Farming Redux: Experimental Evidence on Performance Pay for Tax Collectors," *The Quarterly Journal of Economics*, October 2016, *131* (1), 219–271.

_, Asim Ijaz Khwaja, and Benjamin A Olken, "Making moves matter: Experimental evidence on incentivizing bureaucrats through performance-based postings," *American Economic Review*, January 2019, *109* (1), 237–270.

Koenker, Roger, "Quantile regression for longitudinal data," *Journal of Multivariate Analysis*, 2004, *91* (1), 74–89.

Le, Duong Trung, Edmund Malesky, and Anh Pham, "The impact of local corruption on business tax registration and compliance: Evidence from Vietnam," *Journal of Economic Behavior & Organization*, 2020, *177*, 762–786.

Lewis-Faupel, Sean, Yusuf Neggers, Benjamin A Olken, and Rohini Pande, "Can electronic procurement improve infrastructure provision? Evidence from public works in India and Indonesia," *American Economic Journal: Economic Policy*, August 2016, *8* (3), 258–283.

Machado, José AF and JMC Santos Silva, "Quantiles via moments," *Journal of Econometrics*, 2019, *213* (1), 145–173.

Mastrorocco, Nicola and Edoardo Teso, "State capacity as an organizational problem. Evidence from the growth of the US state over 100 years," Technical Report, National Bureau of Economic Research 2023.

Mattsson, Martin, "Information Systems, Service Delivery, and Corruption: Evidence from the Bangladesh Civil Service (Ungated version)," *American Economic Journal: Applied Economics*, Forthcoming. Forthcoming.

McDonnell, Erin Metz, "Bureaucracy in Action: The Sociology of Public Administration," *Annual Review of Sociology*, 2025, *51.*

Milgrom, Paul and John Roberts, "The economics of modern manufacturing: Technology, strategy, and organization," *American Economic Review*, 1990, *80* (3), 511–528.

Moreira, Diana and Santiago Pérez, "Civil service exams and organizational performance: Evidence from the Pendleton act," Technical Report 28665, National Bureau of Economic Research January 2022.

Muralidharan, Karthik, *Accelerating India's Development: A State-Led Roadmap for Effective Governance*, Penguin Random House India Private Limited, 2024.

_ , Jishnu Das, Alaka Holla, and Aakash Mohpal, *The fiscal cost of weak governance: Evidence from teacher absence in India*, The World Bank, 2016.

_ , Paul Niehaus, and Sandip Sukhtankar, "Building state capacity: Evidence from biometric smartcards in India," *American Economic Review*, October 2016, *106* (10), 2895–2929.

_ , _ , and _ , "Identity verification standards in welfare programs: Experimental evidence from India," *Review of Economics and Statistics*, 2025, *107* (2), 372–392.

_ , _ , _ , and Jeffrey Weaver, "Improving last-mile service delivery using phone-based monitoring," *American Economic Journal: Applied Economics*, April 2021, *13* (2), 52–82.

Naritomi, Joana, "Consumers as Tax Auditors," *American Economic Review*, September 2019, *109* (9), 3031–72.

Nasim, Anjum, "Agricultural income taxation: estimation of the revenue potential in Punjab," *The Pakistan Development Review*, 2012, pp. 321–335.

Nielsen, Soren Reeberg and Birgit Kristiansen, Technical Report, International Federation of Surveyors (FIG) 2008.

Okunogbe, Oyebola and Victor Pouliquen, "Technology, taxation, and corruption: evidence from the introduction of electronic tax filing," *American Economic Journal: Economic Policy*, February 2022, *14* (1), 341–72.

_ **et al.**, *Becoming legible to the state: the role of detection and enforcement capacity in tax compliance*, World Bank, 2021.

Omer, Shahab, "The war against the patwaris," *Profit by Pakistan Today*, 2021.

Overbeck, Daniel and Eliya Lungu, "Bargaining Over Taxes," *Working Paper*, 2024.

Page, Lucy and Rohini Pande, "Ending global poverty: Why money isn't enough," *Journal of Economic Perspectives*, 2018, *32* (4), 173–200.

Pakistan Bureau of Statistics, "Pakistan Social and Living Standards Measurement Survey (District Level) 2006–2015," 2006–2015.

Pepinsky, Thomas B, Jan H Pierskalla, and Audrey Sacks, "Bureaucracy and service delivery," *Annual Review of Political Science*, 2017, *20*, 249–268.

Plesner, Ursula, Lise Justesen, and Cecilie Glerup, "The transformation of work in digitized public sector organizations," *Journal of Organizational Change Management*, 2018, *31* (5), 1176–1190.

Pomeranz, Dina, "No taxation without information: Deterrence and self-enforcement in the value added tax," *American Economic Review*, August 2015, *105* (8), 2539–2569.

Pors, Anja Svejgaard and Eva Pallesen, "The reorganization of the bureaucratic encounter in a digitized public administration.," *Ephemera: Theory & Politics in Organization*, 2021, *21* (3).

Punjab Agricultural Income Tax Act, http://punjablaws.gov.pk/laws/398.html (Accessed: 2023-07-12) 1997.

Rana, Abdul Wajid, "Creating fiscal space for enhancing public investment in Sindh agriculture sector: A qualitative study of provincial spending in Pakistan," 2019.

Rasheed, Khalid, "Patwari culture continues to undermine bureaucracy," *The Express Tribune*, 2024.

Rasmussen, M. S., "Assessment of millet yields and production in northern Burkina Faso using integrated NDVI from the AVHRR.," *International Journal of Remote Sensing*, 1992, *13* (18), 3431–3442.

Rasul, Imran and Daniel Rogger, "Management of bureaucrats and public service delivery: Evidence from the nigerian civil service," *The Economic Journal*, 2018, *128* (608), 413–446.

Roth, Jonathan, Pedro HC Sant'Anna, Alyssa Bilinski, and John Poe, "What's trending in difference-in-differences? A synthesis of the recent econometrics literature," *Journal of Econometrics*, 2023, *235* (2), 2218–2244.

Sequeira, Sandra, "Displacing corruption," Technical Report, Mimeo. London School of Economics 2011.

__ , "Corruption, trade costs, and gains from tariff liberalization: Evidence from Southern Africa," *American Economic Review*, October 2016, *106* (10), 3029–3063.

Shah, Ajay, Anirudh Burman, Devendra Damle, Itishree Rana, and Suyash Rai, "DI-LRMP Implementation in Rajasthan: A Study of the Digital India Land Records Modernisation Programme," Technical Report, National Institute of Public Finance and Policy, New Delhi, India November 2017. Prepared for the Digital India Land Records Modernisation Programme study.

Sun, Liyang and Sarah Abraham, "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects," *Journal of Econometrics*, 2021, *225* (2), 175–199.

Suri, Tavneet, "Mobile money," *Annual Review of Economics*, 2017, *9*, 497–520.

Tariq, Waleed, "Understanding the dying Patwari system," *The Express Tribune*, 2019.

The World Bank, "Bangladesh: Land Acquisition Diagnostic Review," Technical Report, World Bank, Washington, DC August 2022. August 2022.

Tsai, Lily L, "Solidary groups, informal accountability, and local public goods provision in rural China," *American Political Science Review*, 2007, *101* (2), 355–372.

Ullah, Inayat and Saqib Hussain, "Impact of early access to land record information through digitization: Evidence from Alternate Dispute Resolution Data in Punjab, Pakistan," *Land Use Policy*, 2023, *134*, 106917.

Vannutelli, Silvia, "From lapdogs to watchdogs: Random auditor assignment and municipal fiscal performance," Technical Report, National Bureau of Economic Research 2022.

Vrieling, Anton, Kirsten M de Beurs, and Molly E Brown, "Variability of African farming systems from phenological analysis of NDVI time series," *Climatic change*, December 2011, *109*, 455–477.

Waleed, Hamid, "BoR Punjab starts digitized Girdawari of Rabi crops," *Business Recorder*, 2022.

World Bank, "Implementation completion and results report," Technical Report, World Bank 2017.

_ , "Registering Property Good Practices (World Bank Doing Business Database)," https://subnational.doingbusiness.org/en/data/exploretopics/registering-property/good-practices 2019. Accessed on August 18, 2025.

Yang, Dean, "Can Enforcement Backfire? Crime Displacement in the Context of Customs Reform in the Philippines," *The Review of Economics and Statistics*, 02 2008, *90* (1), 1–14.

Young, Alwyn, "Channeling fisher: Randomization tests and the statistical insignificance of seemingly significant experimental results," *The Quarterly Journal of Economics*, November 2019, *134* (2), 557–598.

Tables

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]
Dep. var. 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 FY2012 and FY2013 respectively, and remains zero 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 FY2006 to FY2011, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1000 replications) are in square brackets. Significance levels are denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 2: Two-Stage Least Squares 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	Stacked	Unstacked	Stacked	Unstacked	Stacked
	(1)	(2)	(3)	(4)	(5)	(6)
Digitization of land records	37.42***	39.97***				
	(5.014)	(3.775)				
Percentage of villages digitized			-0.176*	-0.169*	-0.0756	-0.0864
			(0.102)	(0.0968)	(0.0752)	(0.0781)
Kleibergen-Paap Wald F stat	55.7	112.1				
Dep. var. 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 FY2012 and FY2013 respectively, and remains zero 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)-(2)) and the average tax collected per acre (for columns (3)-(6)) across all districts and all years from FY2006 to FY2011, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1000 replications) are in square brackets. Significance levels are denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

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]
Dep. var. 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 dep. var. mean is average profit per acre across all districts and across waves 2005, 2007, and 2011. 'Satellite vegetation cover index' is the Normalized Difference Vegetation Index (NDVI) (ranging from -1 to 1), obtained from NASA's 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 FY2012 and FY2013 respectively, and zero otherwise, and dep. var. mean is the average value of the index across all districts and all years from FY2006 to FY2011, 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. Dep. var. 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 1000 replications) are in square brackets. Significance levels are denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

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]
Dep. var. mean	1069.2	28685.6
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 FY2012 and FY2013 respectively, and remains zero 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 FY2006 to FY2011, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1000 replications) are in square brackets. Significance levels are denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table 5: Did the digitization reform affect the performance of bureaucrats

Dependent Variables:	Performance of bureaucrats			
	$\frac{Tax\ collected}{Tax\ demand}$ (%)	Whether atleast 50% tax demand was collected	Whether atleast 75% 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]
Dep. var. 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 FY2012 and FY2013 respectively, and remains zero 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% (75%) of the annual tax demand was achieved, and remains zero 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 FY2006 to FY2011, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1000 replications) are in square brackets. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

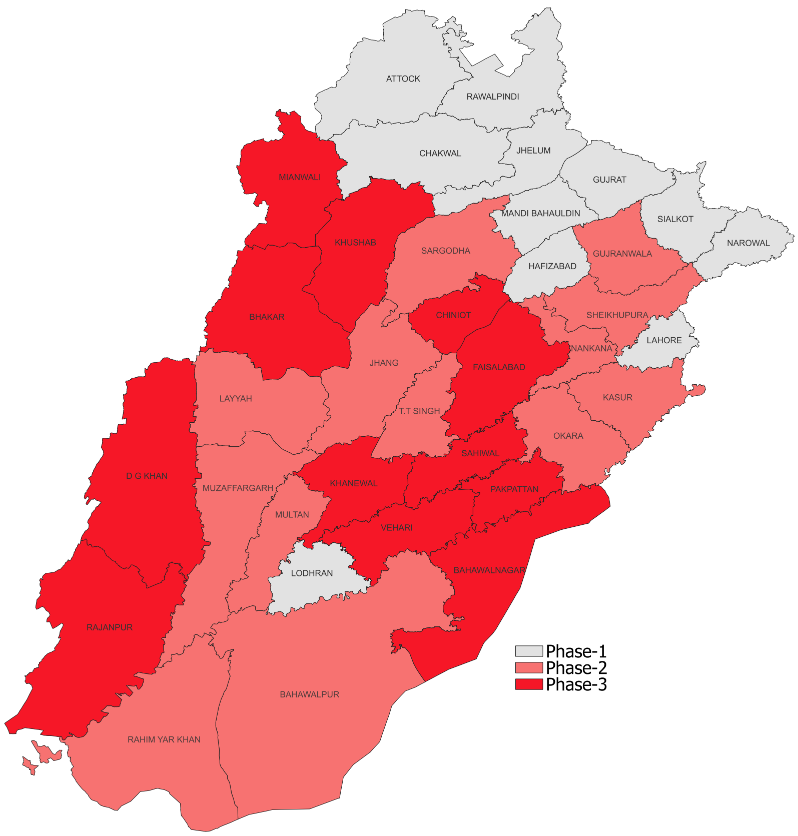
Table 6: Effect of digitization reform on tax collection – Revenue circle-level regression

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]
Dep. var. 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 zero 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 FY2006 to FY2011, prior to any revenue circle's digitization. Standard errors clustered at revenue circle level are in parentheses. Clustered bootstrapped standard errors (with 1000 replications) are in square brackets. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

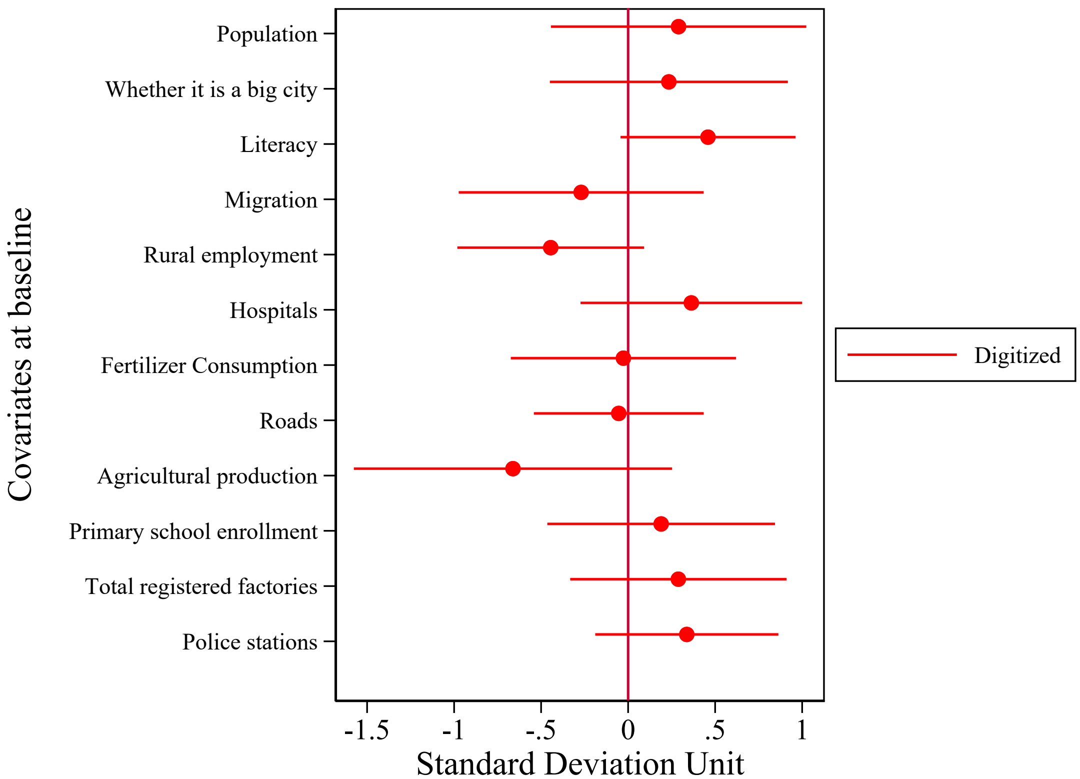
Figures

Figure 1: Geographical Distribution of Districts by Digitization Phase



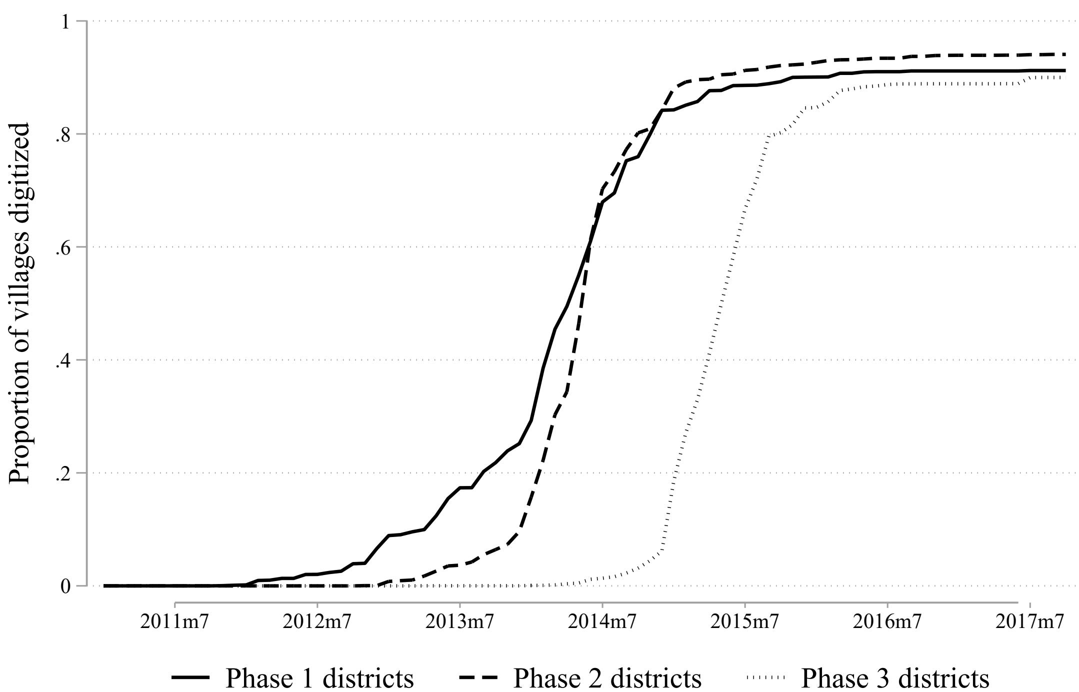
Notes: Geographical distribution of districts across the three phases of land records digitization.

Figure 2: Balance test on baseline characteristics between digitized and non-digitized districts



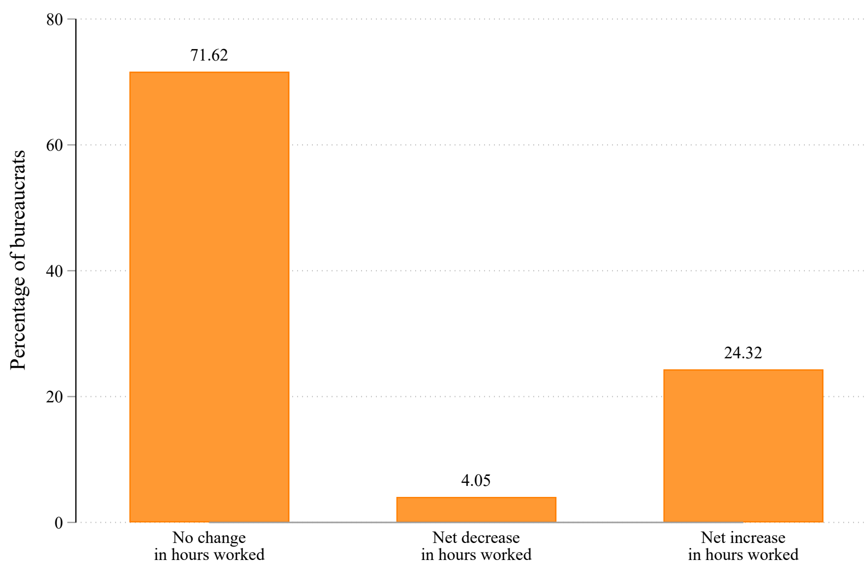
Notes: Data on baseline characteristics are from the Development Statistics of the Pakistan Bureau of Statistics 1997-2010. The figure is based on 34 districts (the total number of districts is 36 but the baseline data for two districts, Chiniot and Nankana, is unavailable prior to 2011). The point estimates 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% confidence intervals.

Figure 3: Phase wise rollout of the digitization reform over time



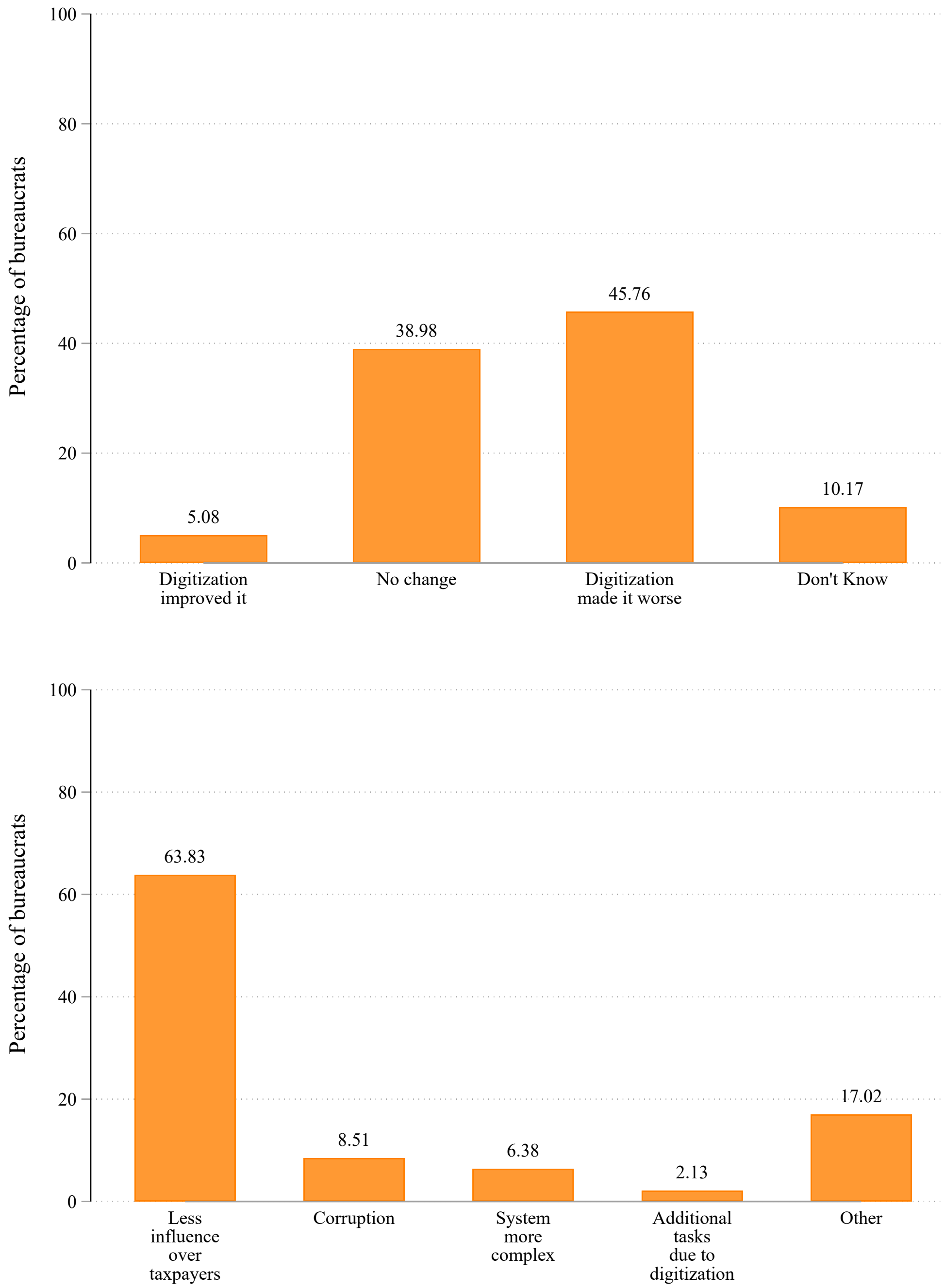
Notes: Districts that were planned to be digitized in phase 1 are Lahore, Lodhran, Hafizabad, Mandi Bahauddin, Nankana Sahib, Jhelum, Gujrat, Sialkot, Chakwal, Attock, Rawalpindi. Districts that were planned to be digitized in phase 2 are Bahawalpur, Gujranwala, Jhang, Layyah, Kasur, Multan, Muzaffargarh, Narowal, Okara, Rahim Yar Khan, Sargodha, Sheikhupura, 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, Vehari.

Figure 4: Changes in hours worked by bureaucrats after the digitization reform



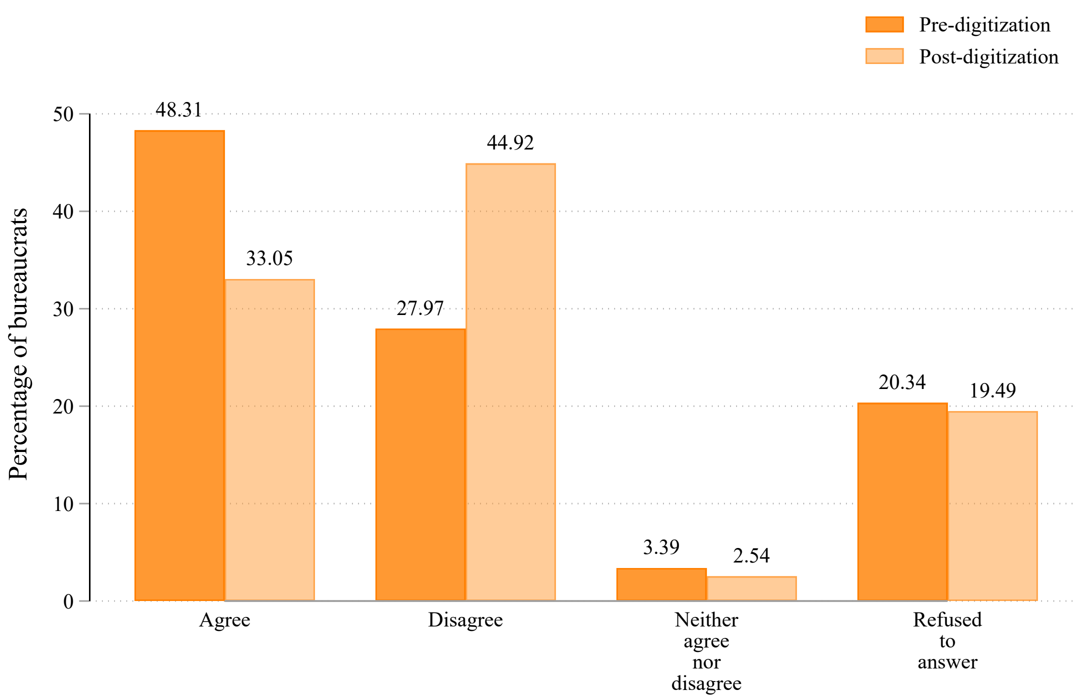
Notes: The figure is based on the bureaucrat survey restricted to the 118 bureaucrats who served as *Qanungo* between 2006-2013. The figure is based on responses to the questions "Do you think LRMIS (the digitization reform) has changed the official tasks that you are supposed to do? If so, what is the 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).

Figure 5: Bureaucrats' views on the effect of digitization on tax collection



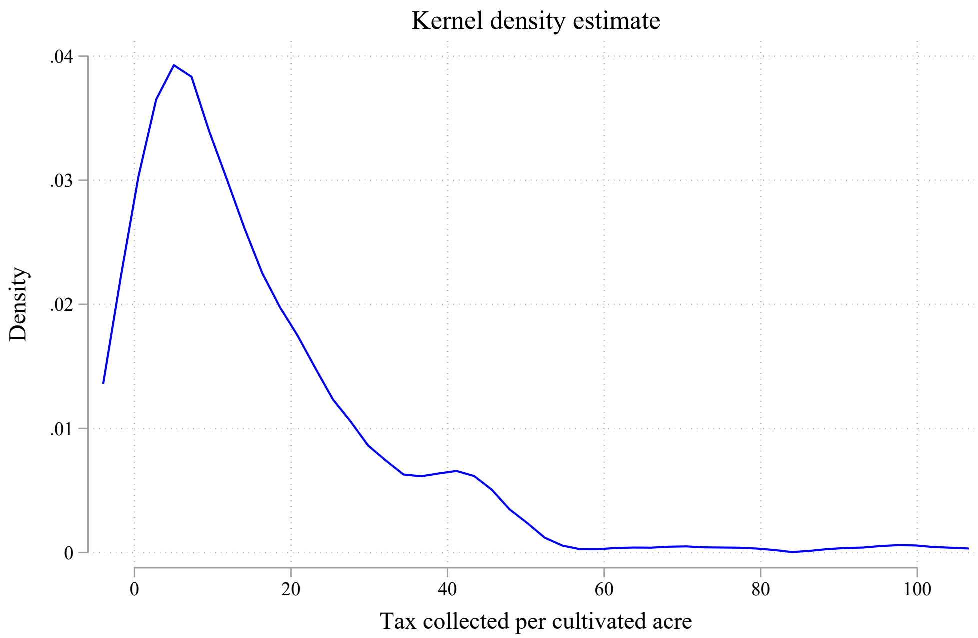
Notes: The figure is based on the bureaucrat survey restricted to the 118 bureaucrats who served as *Qanungo* between 2006-2013. The survey questions were "Do you think digitization has improved overall tax collection?" followed by "Please explain how?" The bottom figure is restricted to the 54 bureaucrats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

Figure 6: Do bureaucrats in charge of land titles receive bribes or "tips" for issuing them?



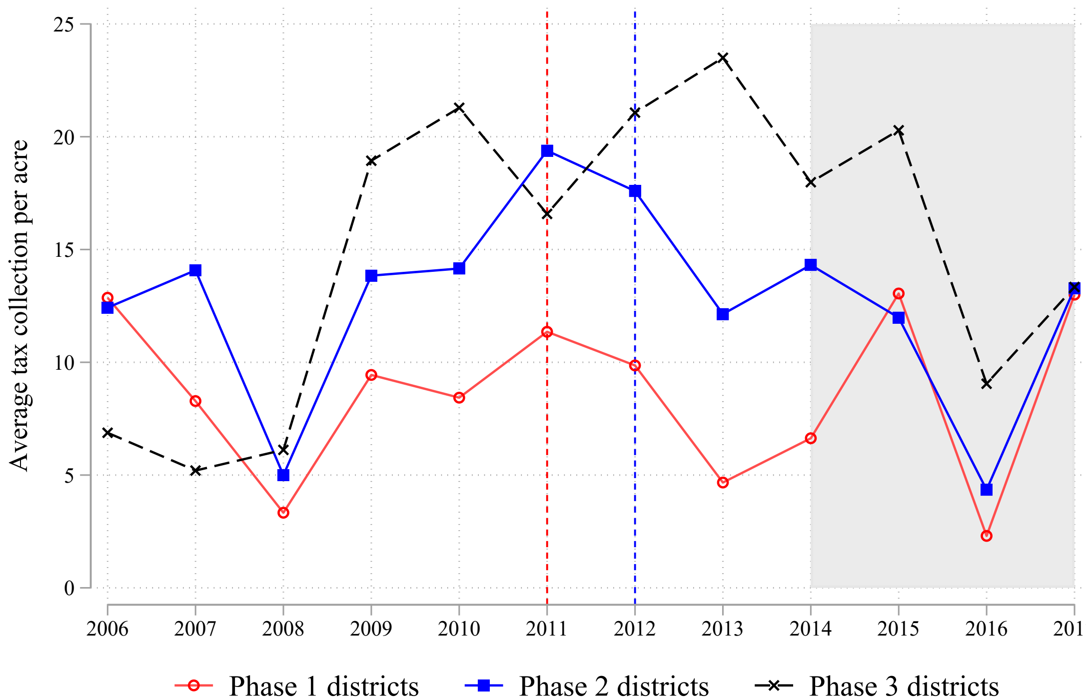
Notes: The figure is based on the bureaucrat survey. The figure shows the percentage of respondents that responded to, "People over there (in a revenue circle) would tip or want to tip a Patwari (bureaucrat's subordinates) for issuing Fard (land title)" measured on a Likert scale. 'Agree', 'completely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

Figure 7: Kernel density of tax collected per cultivated acre



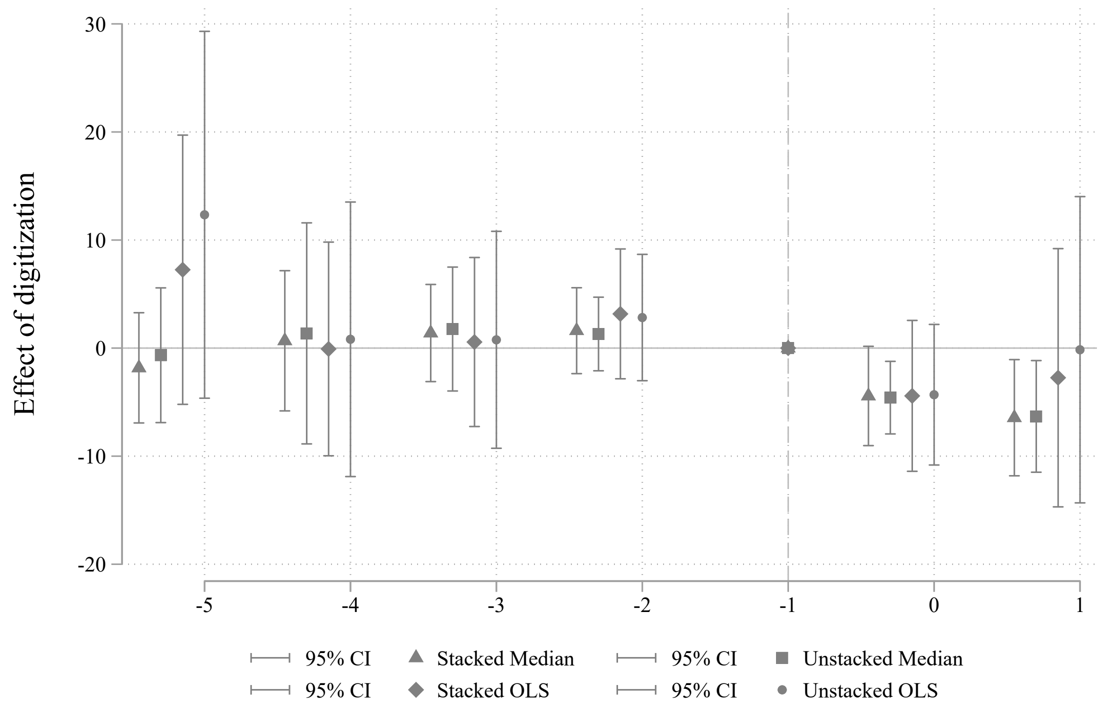
Notes: Kernel density of tax collection per acre across all districts and all years from FY2006 to FY2013. Tax collection per cultivated acre is calculated by dividing the total tax collected (in thousands of Pakistani Rupees) by the average district-level cultivated area (in thousands of acres) at baseline.

Figure 8: Mean Tax Collection per Cultivated Acre by Digitization Phase



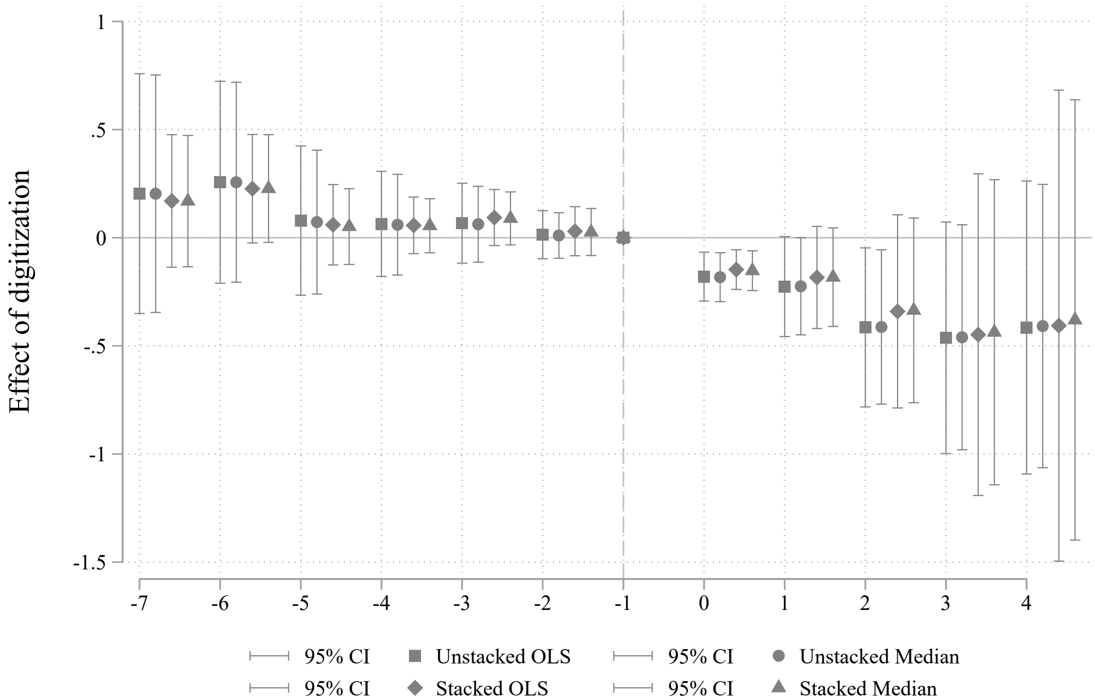
Notes: This figure illustrates the mean tax collection per cultivated acre across the three phases of land records digitization. Each point represents the average tax collection per cultivated acre, calculated by dividing tax collected in thousands of Pakistani Rupees by the average district-level cultivated 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 2006-2013, since by 2014 phase 3 is also digitized and there is no counterfactual to estimate the effects. However, since our data extends to 2017, we plot the raw data from 2006–2017 to provide a complete picture. The shaded area marks the years excluded from the main analysis, when all three phases are digitized.

Figure 9: Event study plot for district level tax collected per acre



Notes: Data is at the district-fiscal year level. Each coefficient is obtained from a set of indicator variables that take values one 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 FY2011 for phase 1 and FY2012 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.

Figure 10: Revenue circle-level event study plot for tax collected per acre



Notes: The data is 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: $y_{rdt} = \tau_r + \tau_t + \sum_{k=-7}^{k=4} \rho_k \text{Actual Digitization}_{k(rdt)} + u_{rdt}$, 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% of the 3,947 total observations used in the regression—leaving insufficient power to estimate coefficients for these periods.

Supplemental Appendix

A Appendix tables

Table A.1: Effect of Digitization Reform on Tax Collection: Robustness to Defining Phase Onset at 50% Village Digitization.

Dependent variable:	Tax collection per cultivated acre	
	TWFE	
	OLS (1)	Median (2)
Digitization of land records	-8.784** (3.986) [3.89]	-6.799*** (2.288) [2.55]
Dep. var. mean	14.7	14.7
District fixed effects	Yes	Yes
Fiscal year fixed effects	Yes	Yes
Observations	212	212

Notes: The unit of observation is a district-fiscal year. 'Digitization of land records' is a dummy equal to one for phase 1 and 2 districts starting in FY2013 and zero otherwise. Unlike Table 1, we do not report stacked DID results, as the simultaneous start of the intervention in treated districts makes them redundant. '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 FY2006 to FY2012, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1000 replications) are in square brackets. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Table A.2: Effect of Digitization Reform on Tax Collection: Robustness to Defining Phase Onset at 1% and 2% of Villages Digitized.

Timing of phases:	*First 1% villages digitized*				*First 2% villages digitized*			
Dependent variable:	Tax collection per cultivated acre							
	TWFE		Stacked DID		TWFE		Stacked DID	
	OLS	Median	OLS	Median	OLS	Median	OLS	Median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Digitization of land records	-3.565	-2.742	-3.925	-3.388*	-6.173**	-2.932*	-6.376*	-4.674***
	(2.683)	(1.767)	(2.968)	(1.787)	(2.937)	(1.629)	(3.246)	(1.343)
	[2.78]	[1.60]	[3.06]	[1.63]	[2.95]	[1.66]	[3.25]	[1.58]
Dep. var. mean	13.3	13.3	13.3	13.3	13.3	13.3	13.3	13.3
District fixed effects	Yes	Yes	No	No	Yes	Yes	No	No
Fiscal year fixed effects	Yes	Yes	No	No	Yes	Yes	No	No
District-by-event fixed effects	No	No	Yes	Yes	No	No	Yes	Yes
Fiscal year-by-event fixed effects	No	No	Yes	Yes	No	No	Yes	Yes
Observations	178	178	330	330	212	212	376	376

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 starting in years FY2011 and FY2012 respectively, and remains zero otherwise. In columns (1)–(4), phase 3 districts become digitized in FY2013, while in columns (5)–(8), phase 3 districts become digitized in FY2014. Therefore, in Columns (1)–(4), we restrict the data to 2006–2012, as there is no available control group from FY2013 onward. '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 FY2006 to FY2010, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1000 replications) are in square brackets. Significance levels are denoted as: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

Table A.3: Missing District-Year observations by Year and Treatment Phase

	2006	2007	2008	2009	2010	2011	2012	2013
Phase 1 (11 districts in total)	5	3	9	2	2	3	1	0
Phase 2 (13 districts in total)	4	4	7	0	1	0	2	0
Phase 3 (12 districts in total)	7	7	9	0	2	1	0	0

Notes. The table shows the number of districts for which we have no observation within each phase and each fiscal year.

Table A.4: Compliance with the planned rollout of digitization

	2010	2011	2012	2013	2014	2015
Phase 1	0	.02	.155	.611	.886	.91
Phase 2	0	0	.035	.614	.906	.934
Phase 3	0	0	0	.012	.583	.885

Notes: The variable displayed is the cumulative proportion of villages in a district that are digitized in a given fiscal year for each phase.

Table A.5: Effect of Digitization Reform on Tax Collection: Robustness to Controlling for Bureaucrats' Managerial Ability

	District level tax		Performance of bureaucrats			
Dependent Variables:	Tax collection per cultivated acre		$\frac{Tax\ collected}{Tax\ demand}$(%)	Whether atleast 50% tax demand was collected	Whether atleast 75% tax demand was collected	Share of months with zero collection
	OLS	Median	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Digitization of land records	-6.423*	-5.083**	-36.27***	-0.403***	-0.427***	0.274**
	(3.676)	(2.420)	(10.44)	(0.117)	(0.114)	(0.105)
	[3.791]	[1.934]	[11.04]	[0.126]	[0.121]	[0.112]
Avg. ability of managers	-1.355	-0.814	29.50**	0.313**	0.287*	-0.301**
	(6.691)	(3.800)	(12.61)	(0.151)	(0.150)	(0.133)
	[6.925]	[3.929]	[14.58]	[0.173]	[0.179]	[0.154]
Dep. var. mean	14.2	14.2	53.9	0.53	0.43	0.19
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Fiscal year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	210	210	301	301	301	301

Notes: In Columns (1) and (2), the unit of observation is a district-fiscal year, while in Columns (3) through (6) it is a bureaucrat-revenue circles-fiscal year. 'Digitization of land records' is a dummy variable that takes value 1 for phase 1 and 2 districts in years after FY2012 and FY2013 respectively, and remains zero otherwise. 'Tax collection per cultivated acre' divides tax collected in thousands of Pakistani Rupees by average district-level cultivated acres (in thousands) at baseline. The outcome variable in Column (3) is the ratio of tax collected to tax demand issued. The outcome variables in Columns (4) and (5) are dummy variables that take values 1 if at least 50% (75%) of the annual tax demand was collected, and remain zero otherwise. The outcome variable in column (6) is the share of months in the fiscal year in which no tax was collected by bureaucrats. For each column, dependent variable mean is the average of the respective outcome variable across all districts (Columns (1)-(2)) or bureaucrats-revenue circles (Columns (3)-(6)) and all years from FY2006 to FY2011, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1000 replications) are in square brackets. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Table A.6: Effect of Digitization Reform on Tax Collection: Placebo Test Using Alternative Digitization Start Years

Dependent variable:	Tax collection per cultivated acre			
Placebo:	Phase 1 2006 Phase 2 2007		Phase 1 2009 Phase 2 2010	
	OLS (1)	Median (2)	OLS (3)	Median (4)
Digitization of land records	-3.633	-0.230	-6.814	-1.587
	(8.321)	(4.509)	(4.394)	(2.538)
	[8.281]	[6.769]	[4.495]	[2.495]
Dep. var. mean	11.0	11.0	11.6	11.6
District fixed effects	Yes	Yes	Yes	Yes
Fiscal year fixed effects	Yes	Yes	Yes	Yes
Observations	212	212	212	212

Notes: The unit of observation is a district-fiscal year. The placebo is based on defining the 'digitization of land records' variable as a dummy that takes value 1 for phase 1 and 2 districts from years FY2006 (FY2009) and FY2007 (FY2010) respectively (instead of FY2012 and FY2013 in the main specification). '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 in FY2006 for columns (1) and (2) and all districts and all years from to FY2006 to FY2008 for columns (3) and (4). Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1000 replications) are in square brackets. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Table A.7: Robustness to Callaway and Sant'Anna (2021) Estimator

Dependent variable:	Tax collection per cultivated acre	
	TWFE	Callaway & Sant'Anna (2021)
	(1)	(2)
Digitization of land records	-6.57*	-7.78**
	(3.69)	(3.68)
Dep. var. mean	14.2	14.2
District fixed effects	Yes	Yes
Fiscal year fixed effects	Yes	Yes
Observations	212	212

Notes: This table is based on unstacked data. 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 FY2012 and FY2013 respectively, and remains zero 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 FY2006 to FY2011, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Table A.8: Effect of Digitization Reform on Tax Collection: Event-by-event effects

Dependent variable:	Tax collection per cultivated acre			
	Event 1 Phase 1 vs. Control	Event 2 Phase 2 vs. Control	Event 1 Phase 1 vs. Control	Event 2 Phase 2 vs. Control
	OLS	OLS	Median	Median
	(1)	(2)	(3)	(4)
Digitization of land records	-3.365	-11.62**	-6.293***	-4.714
	(5.399)	(4.932)	(2.242)	(2.976)
	[5.662]	[4.824]	[3.345]	[4.610]
Dep. var. mean	14.2	14.2	14.2	14.2
District fixed effects	Yes	Yes	Yes	Yes
Fiscal year fixed effects	Yes	Yes	Yes	Yes
Observations	199	195	199	195

Notes: Data is at the district-fiscal year level. 'Tax collection per cultivated acre' divides tax collected in thousands of Pakistani Rupees by average district-level cultivated acres (in thousands) at baseline. Phase 1 is a dummy that takes the value one for all the districts that were planned to be digitized in phase 1 in any years after FY2012, and remains zero otherwise. Phase 2 is a dummy that takes the value one for all the districts that were planned to be digitized in phase 2 in any years after FY2013, and remains zero otherwise. Phase 3 are districts that are never treated in the sample period 2006-2013. Dependent variable mean is the average tax collected per acre across all districts and all years from FY2006 to FY2011, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1000 replications) are in square brackets. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Table A.9: Effect of Digitization Reform on Tax Collection: Randomization Inference p-value

Dependent variable:	Tax collection per cultivated acre			
	TWFE		Stacked DID	
	OLS	Median	OLS	Median
	(1)	(2)	(3)	(4)
Digitization_d × post_t	-7.146*	-5.768**	-6.746*	-4.940**
	(3.970)	(2.570)	(3.935)	(1.969)
	[4.011]	[1.892]	[3.974]	[1.942]
Randomization inference p-val	0.082	0.004	0.028	0.001
Dep. var. mean	14.2	14.2	14.2	14.2
District fixed effects	Yes	Yes	Yes	Yes
Fiscal year fixed effects	Yes	Yes	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, and remains zero otherwise. Post is a dummy variable that takes the value 1 in years after FY2012, and remains zero otherwise. 'Tax collection per cultivated acre' divides tax collected in thousands of Pakistani Rupees by average district-level cultivated acres (in thousands) at baseline. Randomization inference p-values (at the bottom of the table) are from permutation tests similar to the randomization based inference test (Athey and Imbens, 2017; Young, 2019). We re-assign digitization over districts 1000 times and compute the estimates under the null hypothesis that the treatment has no effect. We then locate the point estimates coming from our real data in the distribution of the 1000 treatment assignment simulations. The p-value is based on the share of estimates from the 1000 reassignments that are higher in absolute value than our point estimates in Table 1. Because we only carry out the reassignment over districts, we create separate dummy variables "Digitization" and "post". As a result, we can only use a single year to capture the start of digitization, unlike the staggered adoption over two years in our main specification. Dependent variable mean is the average tax collected per acre across all districts and all years from FY2006 to FY2011, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1000 replications) are in square brackets. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Table A.10: Effect of Digitization Reform on Agricultural Tax Base: Robustness to using stacked data

	Farm profit per acre	Satellite vegetation cover index	Whether agri land irrigated?	Log agricultural land owned
	(1)	(2)	(3)	(4)
Digitization of land records	4.906	0.00846	-0.0000514	0.0635
	(3.329)	(0.00548)	(0.0490)	(0.0444)
	[3.546]	[0.00538]	[0.0443]	[0.0432]
Dep. var. mean	23.4	0.53	0.12	7.69
District-by-event fixed effects	Yes	Yes	Yes	Yes
Fiscal year-by-event fixed effects	Yes	Yes	Yes	Yes
Observations	10,916	541	314,704	314,767

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 dep. var. mean is average profit per acre across all districts and across waves 2005, 2007, and 2011. 'Satellite vegetation cover index' is the Normalized Difference Vegetation Index (NDVI) (ranging from -1 to 1), obtained from NASA's 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 FY2012 and FY2013 respectively, and zero otherwise, and dep. var. mean is the average value of the index across all districts and all years from FY2006 to FY2011, 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. Dep. var. 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 1000 replications) are in square brackets. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Table A.11: Bureaucrats' assessments of the tax base (district-level): Robustness to using stacked data

	Log assessed cultivated area	Log admin tax demands
	(1)	(2)
Digitization of land records	-0.106***	-0.568***
	(0.0351)	(0.195)
	[0.0339]	[0.198]
Dep. var. mean	1069.2	28685.6
District-by-event fixed effects	Yes	Yes
Fiscal year-by-event fixed effects	Yes	Yes
Observations	393	376

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 FY2012 and FY2013 respectively, and remains zero 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 FY2006 to FY2011, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1000 replications) are in square brackets. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Table A.12: Did the digitization reform affect the performance of bureaucrats: Robustness to using bureaucrat FE

Dependent Variables:	Performance of bureaucrats				Assessment
	$\frac{Tax\ collected}{Tax\ demand}$(%)	Whether atleast 50% tax demand was collected	Whether atleast 75% tax demand was collected	Share of months with zero collection	Admin tax demands (000 PKR)
	(1)	(2)	(3)	(4)	(5)
Digitization of land records	-29.54**	-0.338**	-0.394**	0.225	-259.3
	(14.09)	(0.158)	(0.152)	(0.139)	(1248.5)
	[12.21]	[0.138]	[0.135]	[0.119]	[1195.7]
Dep. var. mean	53.9	0.53	0.43	0.19	875.2
Bureaucrat fixed effects	Yes	Yes	Yes	Yes	Yes
Fiscal year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	304	304	304	304	301

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 FY2012 and FY2013 respectively, and remains zero 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% (75%) of the annual tax demand was achieved, and remains zero otherwise. The fourth measure is the share of months in the fiscal year in which no tax was collected. The final column presents results for tax assessments conducted by bureaucrats. It reports the total tax demands (in 000 Pakistani Rupees) issued by bureaucrats within each revenue circle. For each column, dependent variable mean is the average of the respective outcome variable across all bureaucrats and all years from FY2006 to FY2011, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1000 replications) are in square brackets. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Table A.13: Did the digitization reform affect the performance of bureaucrats? Robustness to using stacked data

Dependent Variables:	Performance of bureaucrats			
	$\frac{Tax\ collected}{Tax\ demand}$ (%)	Whether atleast 50% tax demand was collected	Whether atleast 75% tax demand was collected	Share of months with zero collection
	(1)	(2)	(3)	(4)
Digitization of land records	-36.70***	-0.405***	-0.430***	0.241**
	(9.355)	(0.107)	(0.100)	(0.100)
	[9.749]	[0.115]	[0.107]	[0.103]
Dep. var. mean	53.9	0.53	0.43	0.19
District-by-event fixed effects	Yes	Yes	Yes	Yes
Fiscal year-by-event fixed effects	Yes	Yes	Yes	Yes
Observations	549	549	549	549

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 FY2012 and FY2013 respectively, and remains zero 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% (75%) of the annual tax demand was achieved, and remains zero 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 FY2006 to FY2011, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1000 replications) are in square brackets. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

Table A.14: Effect of Digitization Reform on Tax Collection: Testing the Information Mechanism

Dependent Variables:	Performance of bureaucrats			
	$\frac{Tax\ collected}{Tax\ demand}$(%)	Whether atleast 50% tax demand was collected	Whether atleast 75% tax demand was collected	Share of months with zero collection
	(1)	(2)	(3)	(4)
Digitization	-38.52***	-0.427***	-0.486***	0.300**
	(12.80)	(0.142)	(0.132)	(0.135)
	[13.28]	[0.149]	[0.143]	[0.140]
Digitization × Variance in tax demands	8.706	0.0904	0.194	-0.103
	(11.99)	(0.153)	(0.151)	(0.143)
	[13.62]	[0.168]	[0.174]	[0.152]
Dep. var. 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 FY2012 and FY2013 respectively, and remains zero otherwise. 'Variance in tax demands' is a dummy variable equal to one if a district's baseline variance in tax demands is above the median. The first outcome 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% (75%) of the annual tax demand was achieved, and remains zero 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 FY2006 to FY2011, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1000 replications) are in square brackets. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

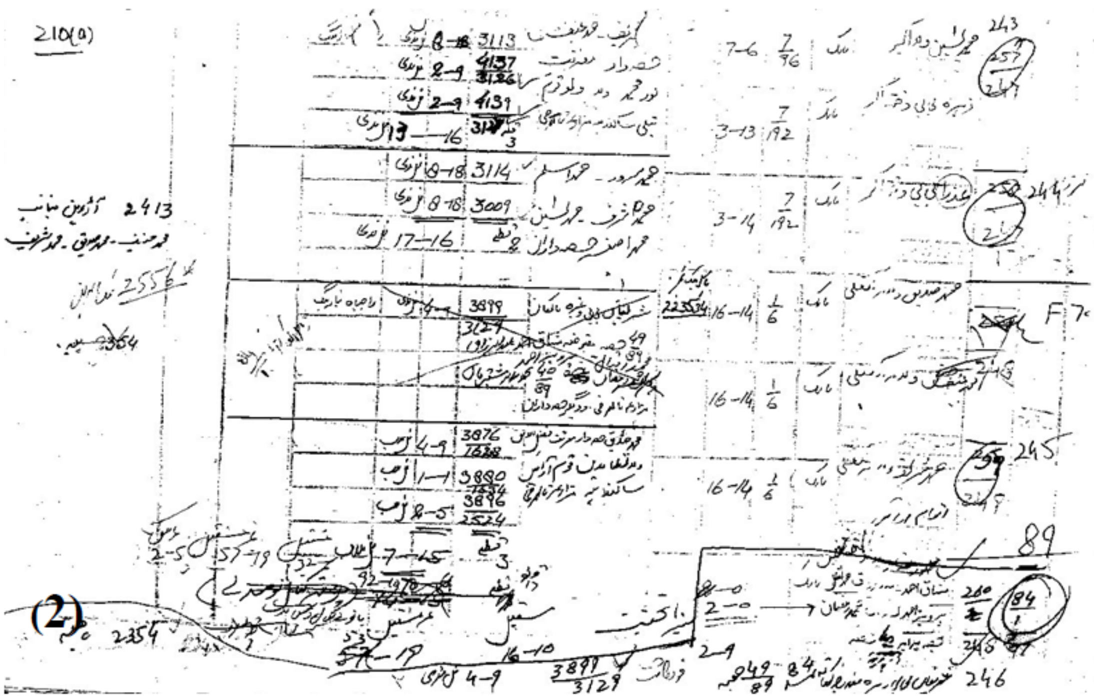
Table A.15: Effect of Digitization Reform on Tax Collection: Robustness to using Machado and Silva (2019)'s Quantile Regression

Dependent variable:	Tax collection per cultivated acre	
	TWFE Median	
	Unstacked	Stacked
	(1)	(2)
Digitization of land records	-6.472*	-7.009**
	(3.477)	(3.535)
	[3.523]	[3.649]
Dep. var. mean	14.2	14.2
District fixed effects	Yes	No
Fiscal year fixed effects	Yes	No
District-by-event fixed effects	No	Yes
Fiscal year-by-event fixed effects	No	Yes
Observations	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 FY2012 and FY2013 respectively, and remains zero 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 FY2006 to FY2011, prior to any district's digitization. Standard errors clustered at district level are in parentheses. Clustered bootstrapped standard errors (with 1000 replications) are in square brackets. Significance levels are denoted as: * p<0.1, ** p<0.05, *** p<0.01.

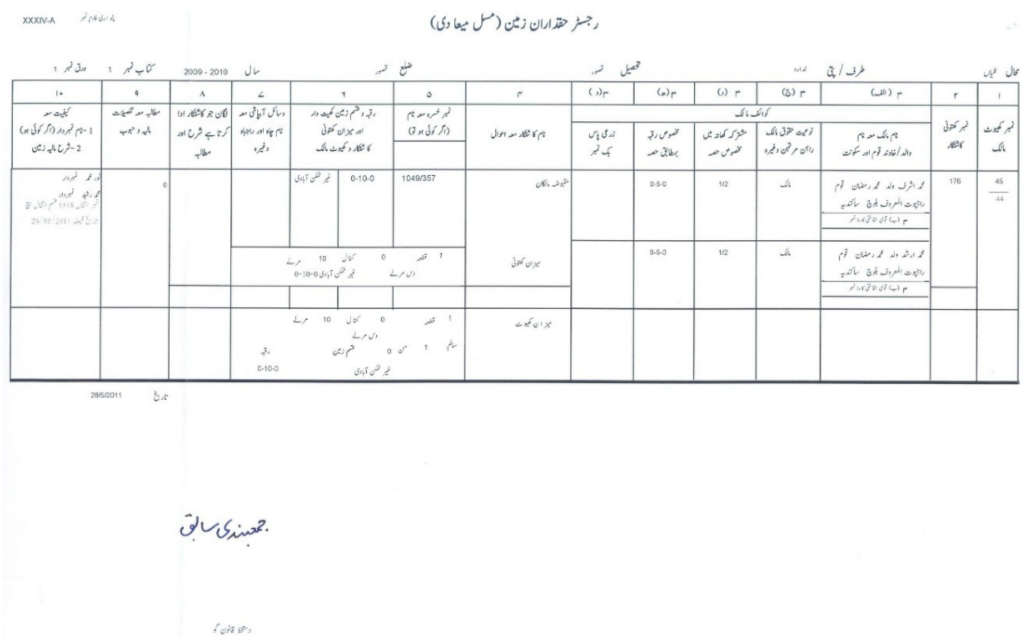
B Appendix: Figures

Figure B.1: An example of a manual land record



Notes: The source of the figure is *Figure 2* in Adeel (2010). Before the digitization reform such land records were maintained by the bureaucrats we study.

Figure B.2: An example of a digitized land record

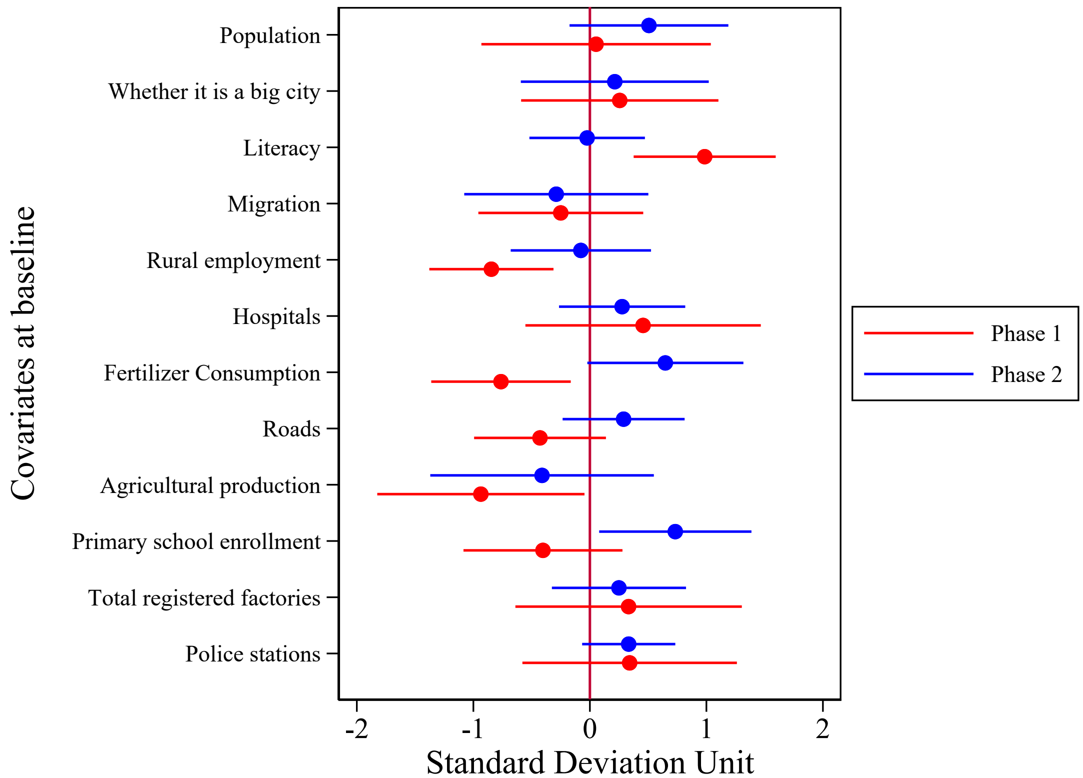


Notes: The source of the figure is: http://cadastraltemplate.org/pakistan.php

Figure B.3: A new bureaucracy set up to handle digitized land records

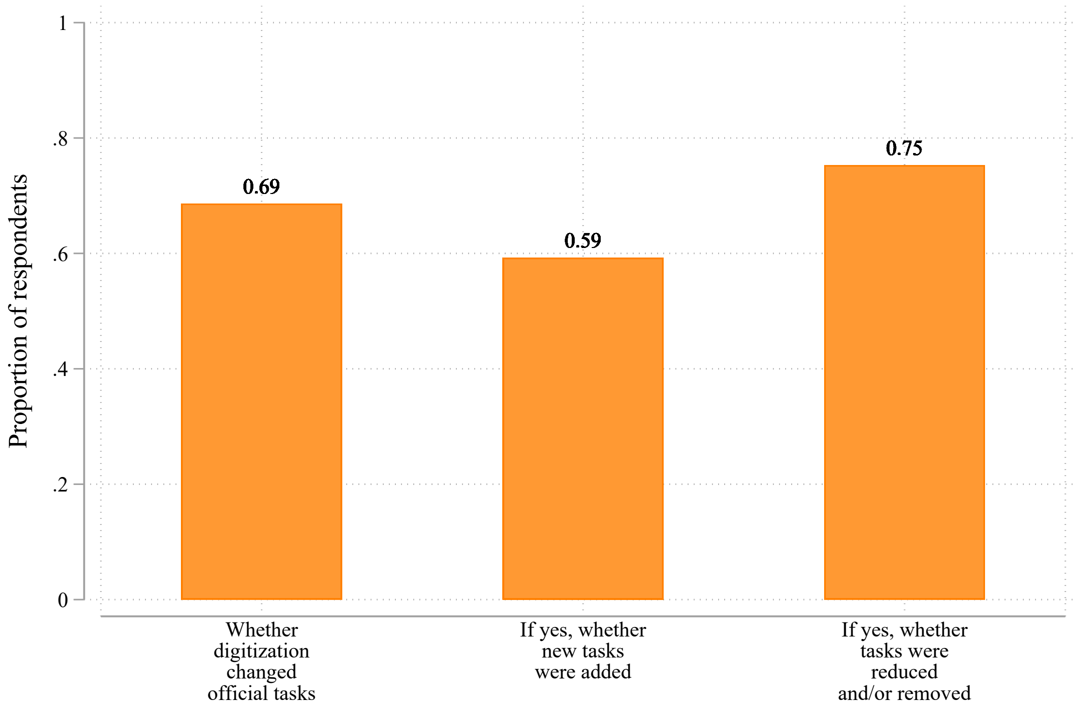
Notes: The source of the image is the World Bank (2017). New centers were set up across Punjab to deliver computerized land record services.

Figure B.4: Balance test on baseline characteristics of districts in phase 1 and 2 of the digitization reform



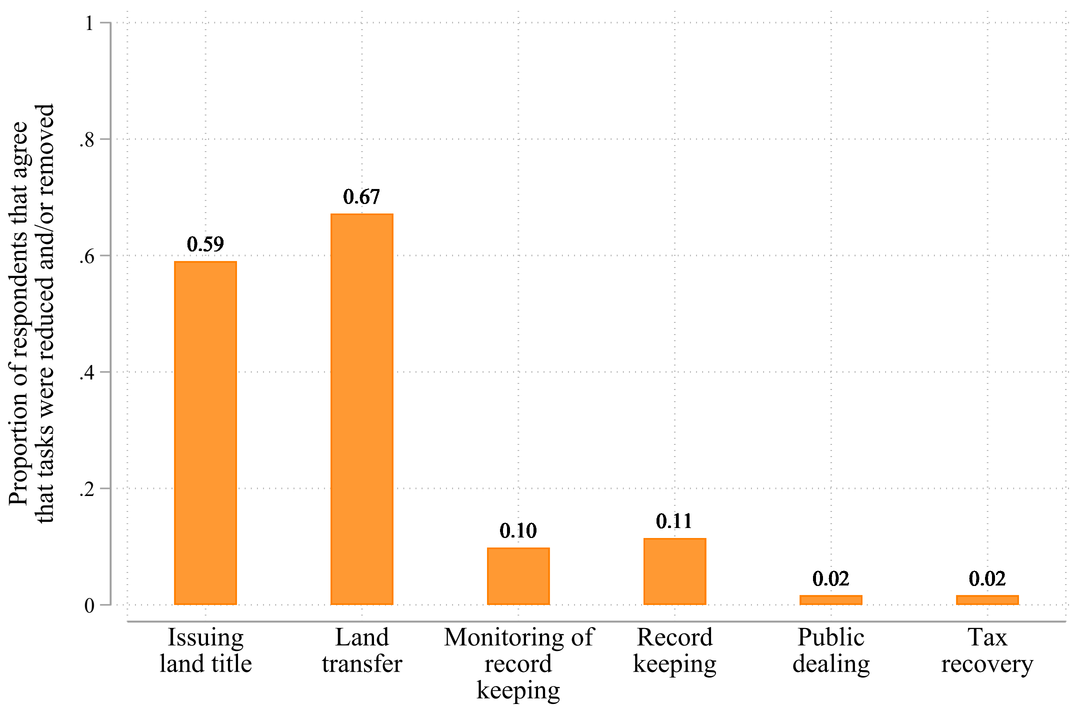
Notes: Data on baseline characteristics are from the Development Statistics of the Pakistan Bureau of Statistics 1997-2010. The point estimates are from a regression of the respective covariates on a dummy that takes value one if the district is in phase 1 or 2 of the digitization reform, and zero otherwise. The reference category are phase 3 districts. Intervals are 95% confidence intervals.

Figure B.5: Changes in the tasks of the bureaucrats after the digitization reform



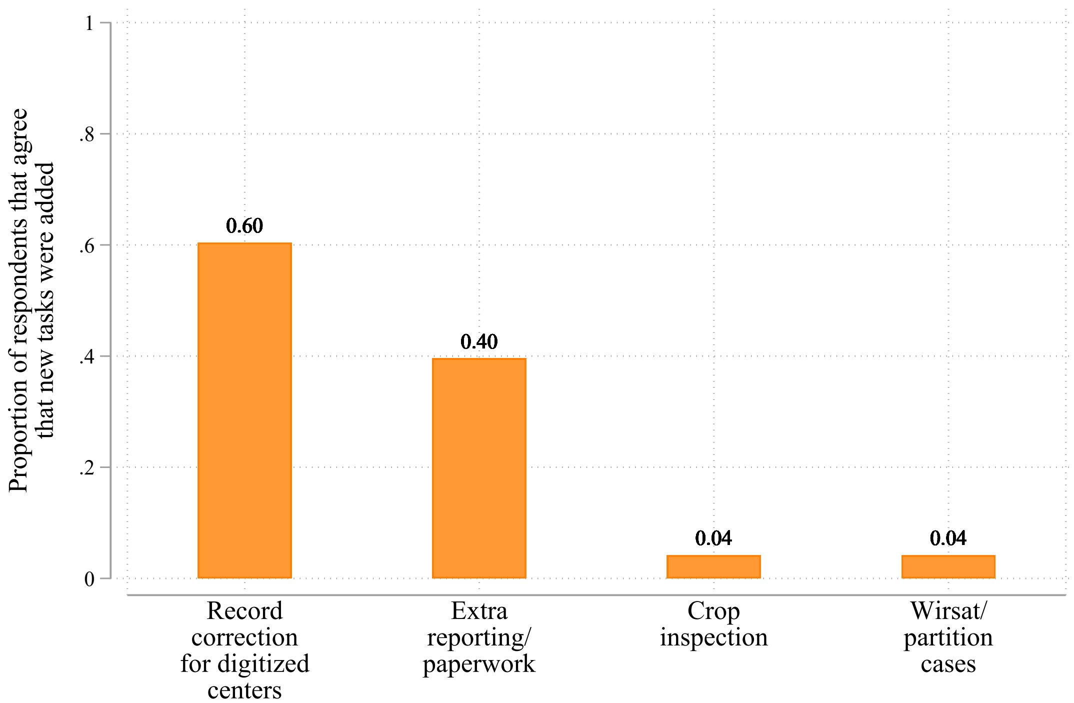
Notes: The figure is based on the bureaucrat survey restricted to the 118 bureaucrats who served as *Qanungo* between 2006-2013. The first bar plots the proportion of bureaucrats that agreed in the question "Do you think LRMIS (the digitization reform) changed the official tasks that you are supposed to do?"

Figure B.6: Bureaucrats' tasks reduced after the digitization reform



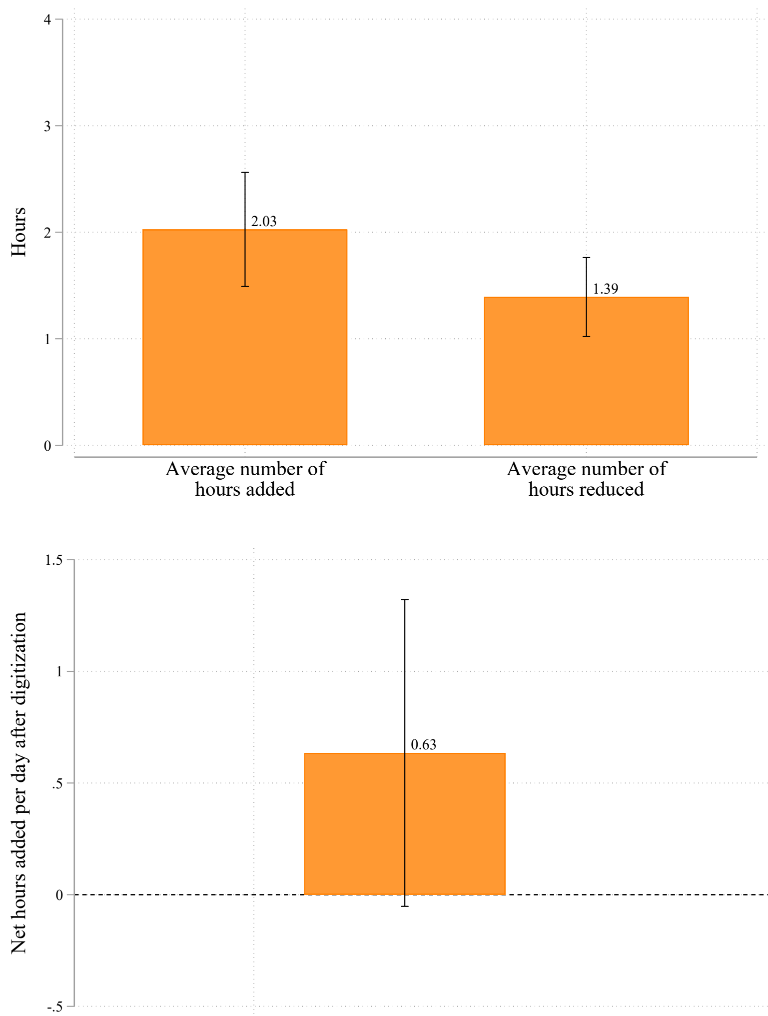
Notes: The figure is based on the bureaucrat survey restricted to the 81 bureaucrats who served as *Qanungo* between 2006-2013 and who agreed with the question "Do you think LRMIS (the digitization reform) changed the official tasks that you are supposed to do?"

Figure B.7: Bureaucrats' tasks added after the digitization reform



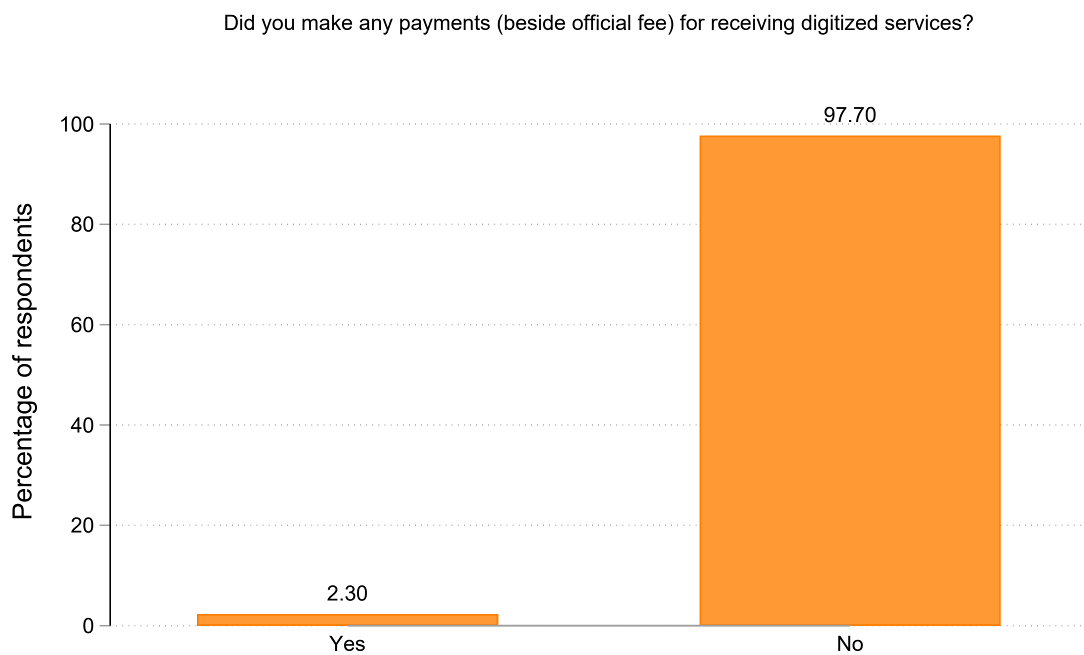
Notes: The figure is based on the bureaucrat survey restricted to the 81 bureaucrats who served as *Qanungo* between 2006-2013 and who agreed with the question "Do you think LRMIS (the digitization reform) changed the official tasks that you are supposed to do?"

Figure B.8: Changes in number of hours worked after the digitization reform



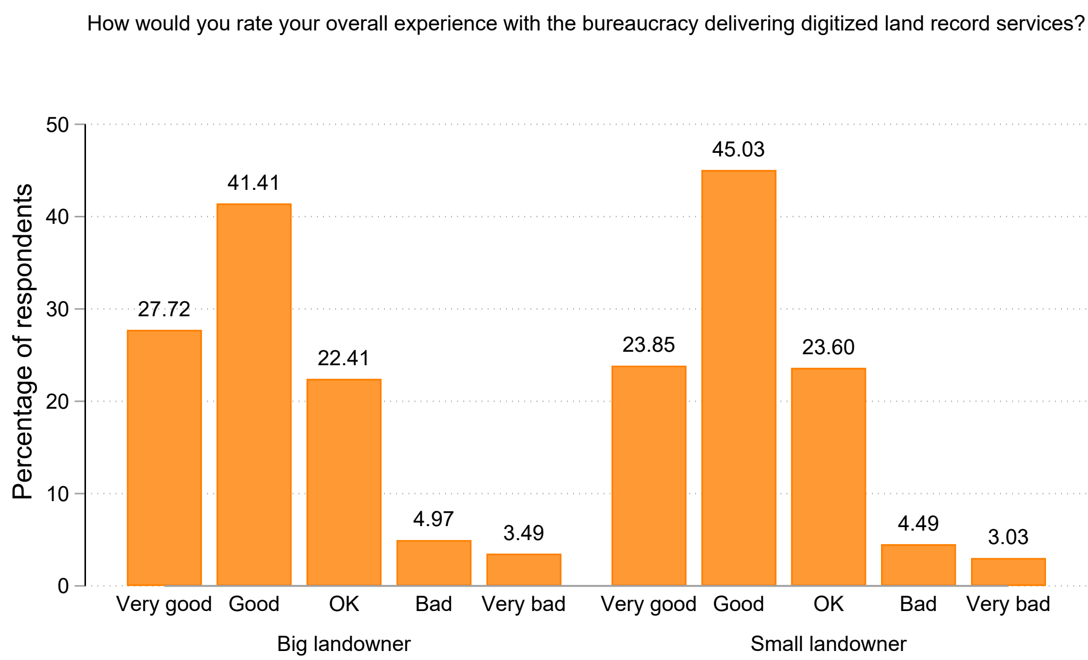
Notes: The figure is based on the bureaucrat survey restricted to the 118 bureaucrats who served as *Qanungo* between 2006-2013. The figure is based on responses to the question "Do you think LRMIS (the digitization reform) has changed the official tasks that you are supposed to do? If so, what is the number of hours per day that were added / reduced because of these changes?" Based on these answers, we calculate the average number of hours added and the average number of hours reduced across respondents. These are reported in the top panel. In the bottom panel we report the average net change in hours with 95% confidence intervals. The number is calculated by subtracting the number of hours reduced per day from the number of hours added per day, as reported by the bureaucrats.

Figure B.9: Bribe payments for getting land record services from the new bureaucracy



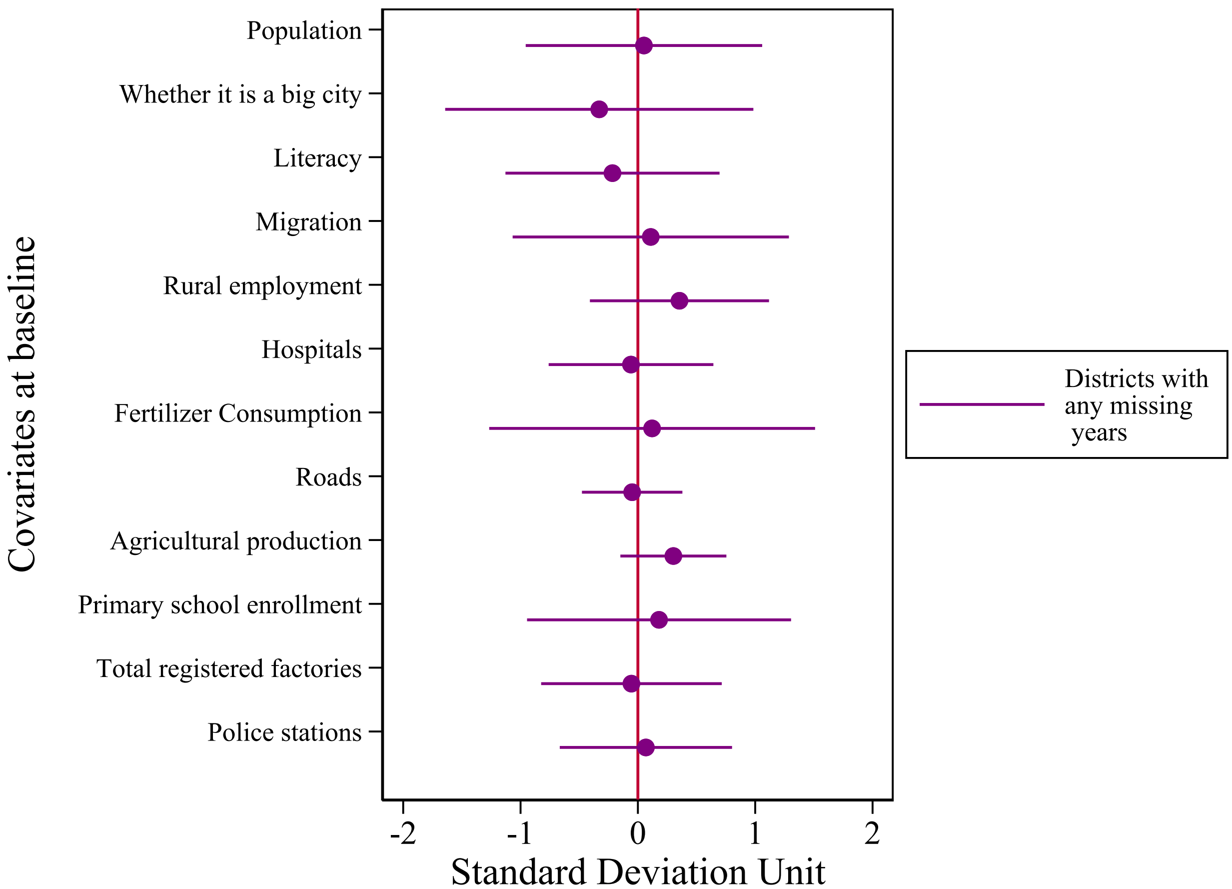
Notes: This data is from an endline citizen exit survey in 2016 on the services provided by the bureaucracy responsible for delivering digitized land record services.

Figure B.10: Citizens' satisfaction with the new bureaucracy delivering digitized land record services



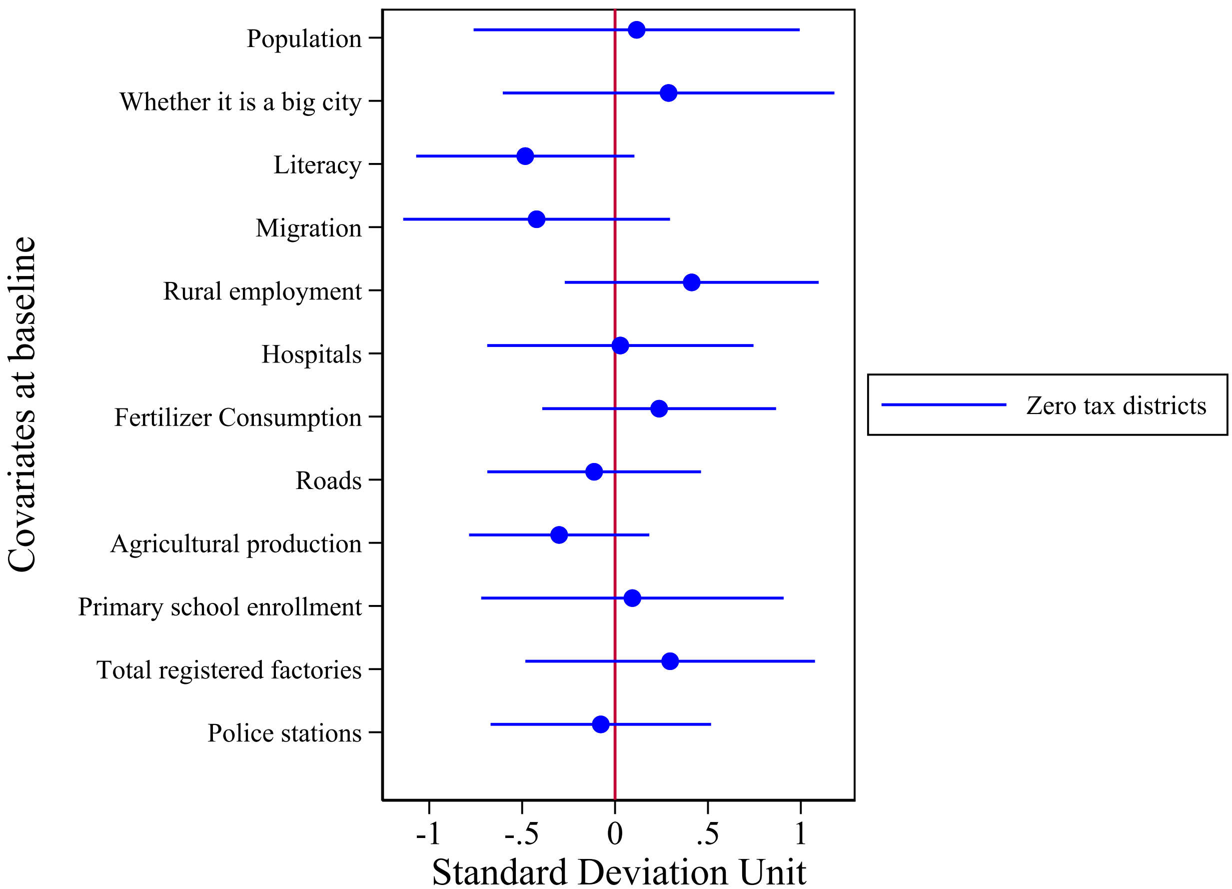
Notes: This data is from an endline citizen exit survey in 2016 on the services provided by the bureaucracy responsible for delivering digitized land record services. Big landlords are defined as those who own land acreage above the median.

Figure B.11: Balance on Baseline Characteristics of Districts with missing years



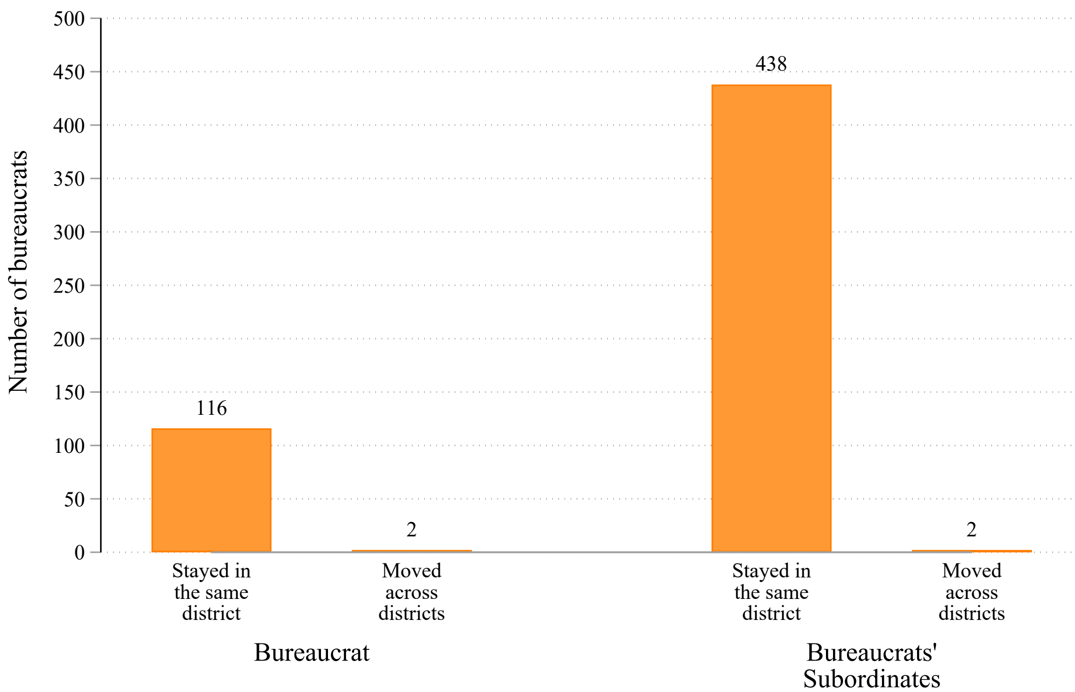
Notes. The point estimates are from a regression of the respective covariates on a dummy equal to one if a district has at least one year with missing tax collection. The districts of Chiniot and Nankana are excluded, as baseline data for these districts are unavailable prior to 2011.

Figure B.12: Balance on Baseline Characteristics of Districts with Zero Tax Collection



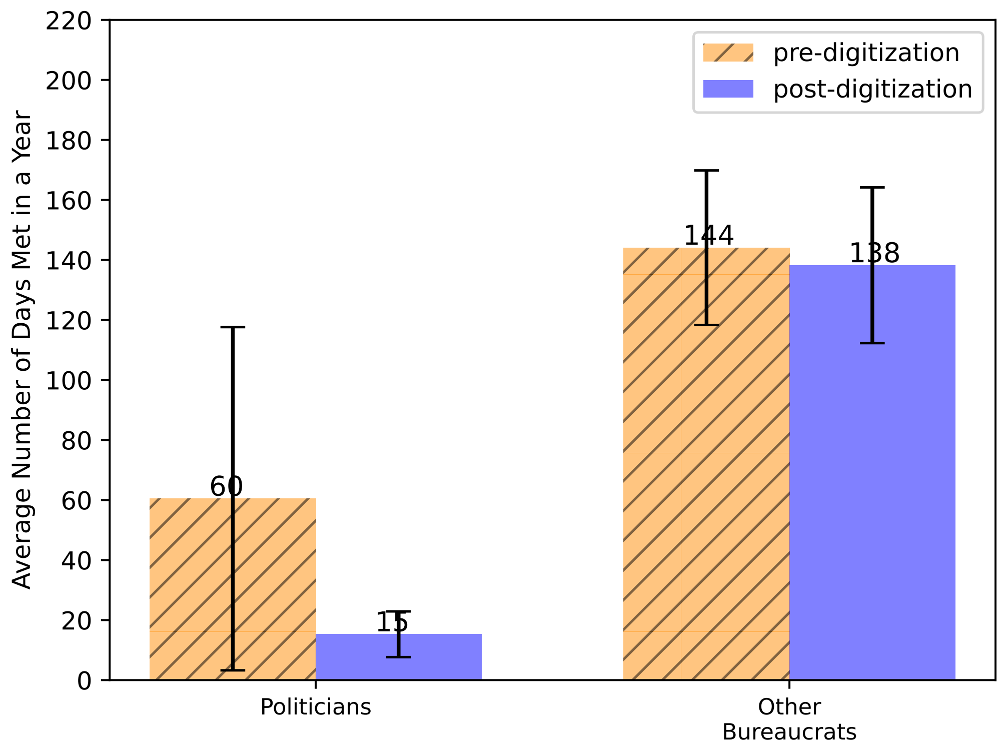
Notes. The point estimates are from a regression of the respective covariates on a dummy equal to one if a district has at least one year with zero tax collection. The districts of Chiniot and Nankana are excluded, as baseline data for these districts are unavailable prior to 2011.

Figure B.13: Movement of bureaucrats across districts



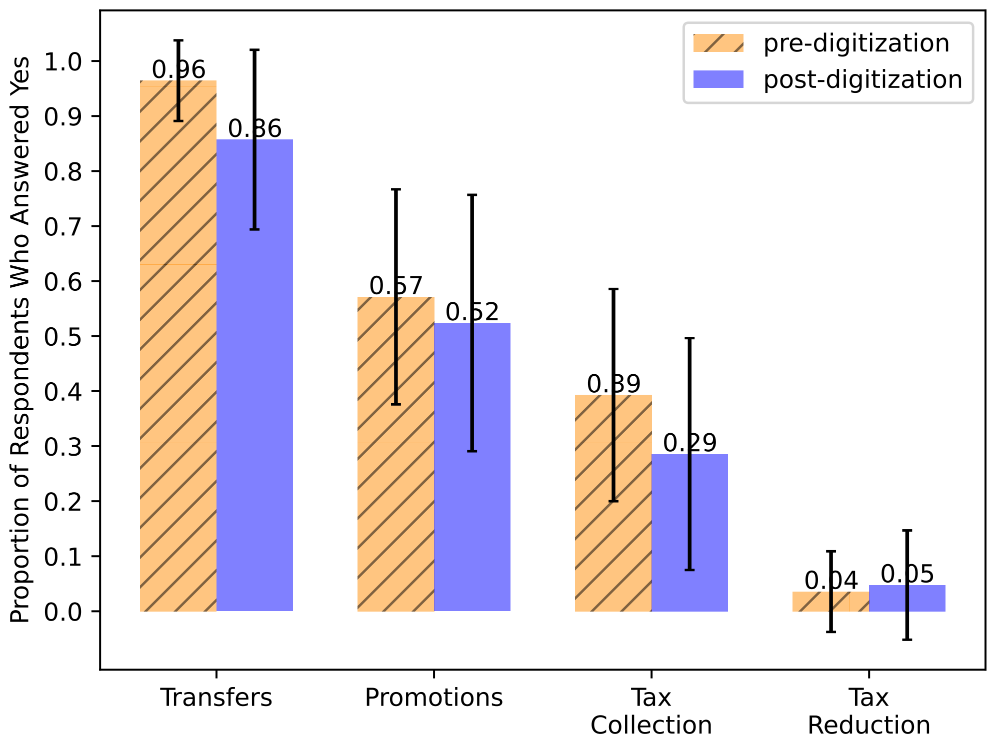
Notes: The figure is based on the bureaucrat survey. The left-hand side shows transfers among the bureaucrats that are the focus of this paper (Qanungos). The right-hand side shows transfers among their subordinates (Patwaris).

Figure B.14: Bureaucrats' social interactions with politicians and other bureaucrats before and after the digitization reform



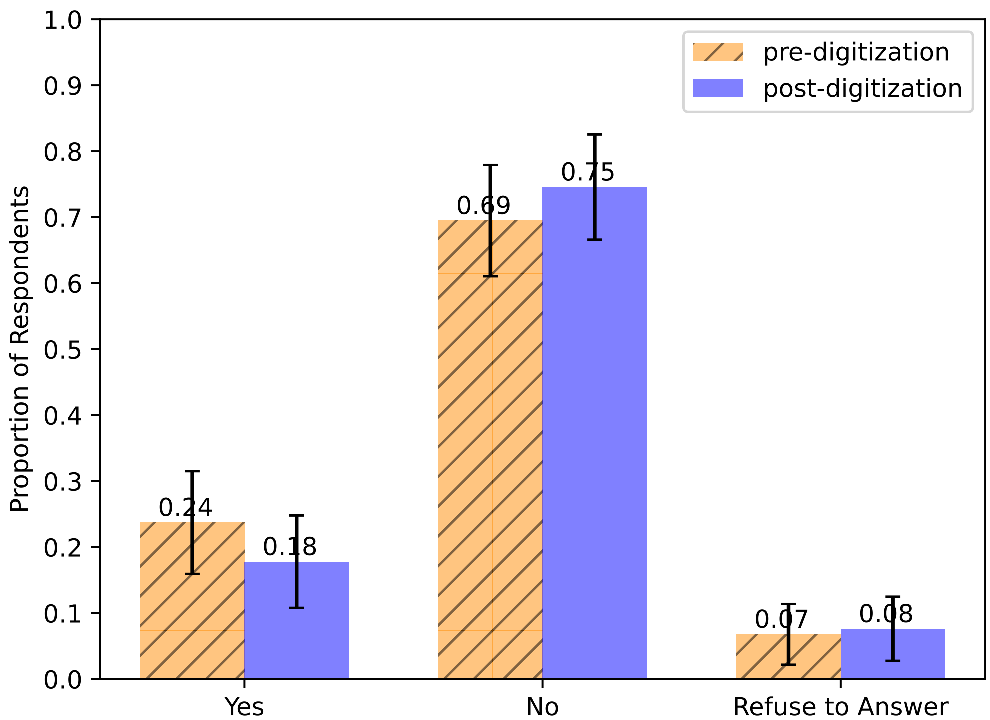
Notes: The figure is based on the bureaucrat survey restricted to the 118 bureaucrats who served as *Qanungo* between 2006-2013. We used a Likert scale to ask about the frequency of interactions between the respondent and politicians or other bureaucrats, before, and after, the reform. The Likert scale options were as follows: daily, twice a week, weekly, bi-monthly, monthly, quarterly, bi-annually, annually, less than once per year and never. We calculated the average number of days of interactions in a year for each bureaucrat based on these responses.

Figure B.15: Matters in which politicians interfere



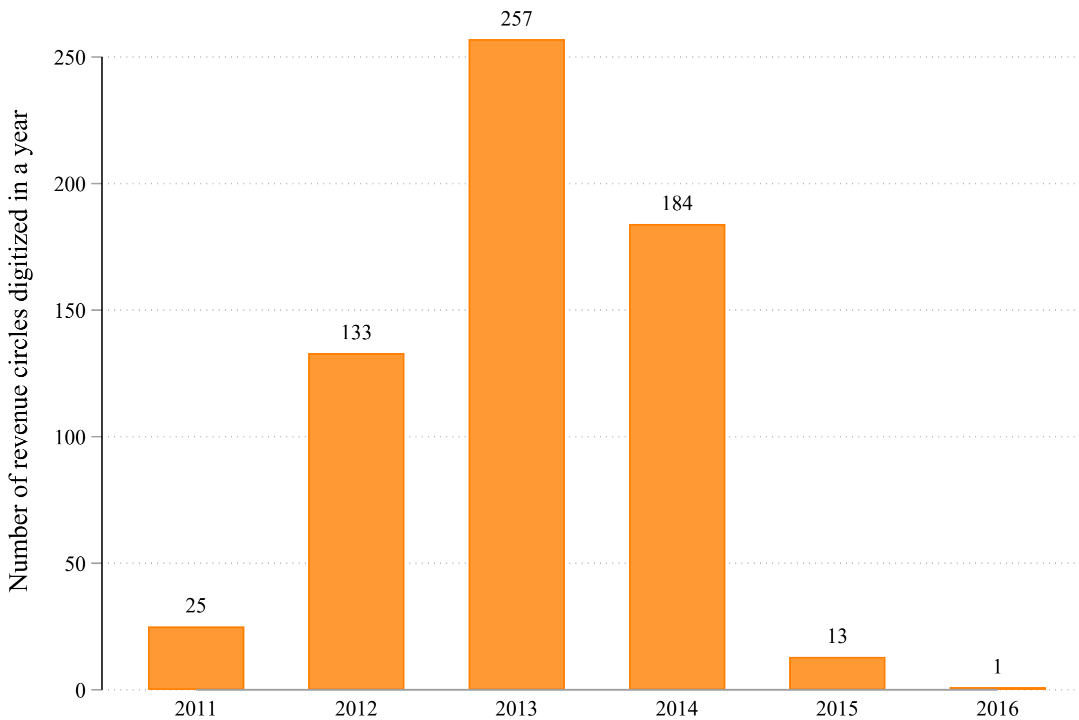
Notes: The figure is based on the bureaucrat survey restricted to the 118 bureaucrats who served as *Qanungo* between 2006-2013. Bureaucrats who responded 'yes' to the question: "In general, would you say that politicians interfere with the work of revenue officials in this revenue circle?" were further asked "On which matters politicians usually interfere with work?" The matters listed above were suggested by the research team along with the category of "others".

Figure B.16: Political interference in the work of bureaucrats



Notes: The figure is based on the bureaucrat survey restricted to the 118 bureaucrats who served as *Qanungo* between 2006-2013. The question was: "In general, would you say that politicians interfere with the work of revenue officials in this revenue circle?"

Figure B.17: Number of revenue circles digitized over the years

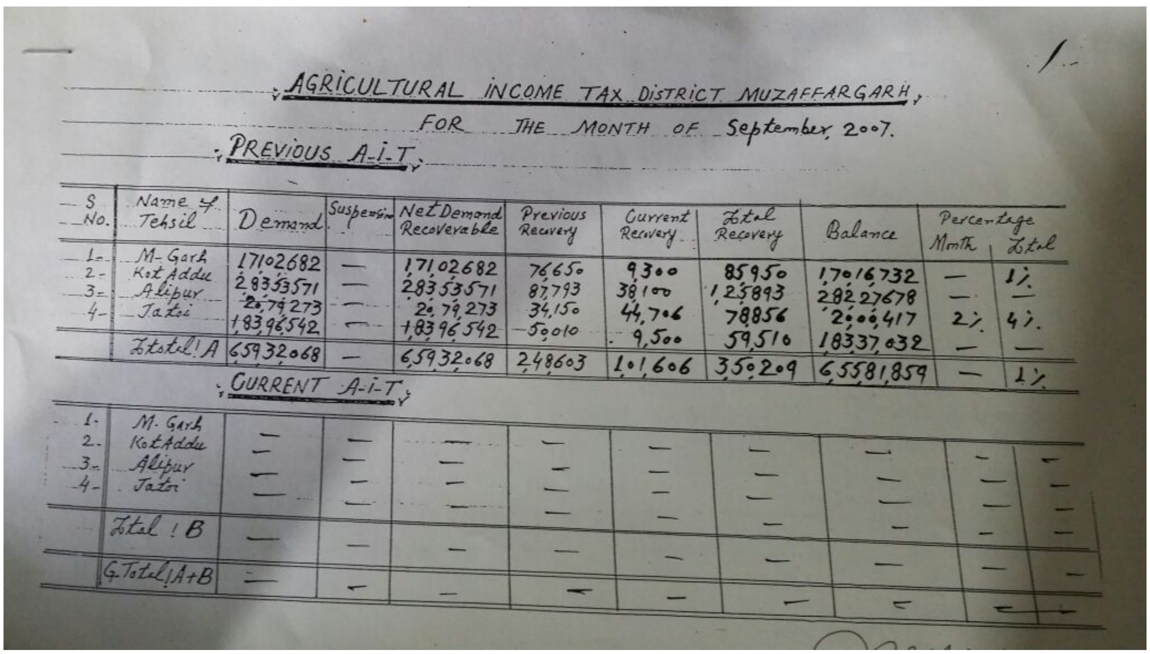


Notes: This figure displays the number of revenue circles digitized in a given year across all districts. A revenue circle is defined as digitized if at least one village in that revenue circle is digitized. In total, phase 1 covered 200 revenue circles, phase 2 covered 342 revenue circles, and phase 3 covered 275 revenue circles.

C Data Sources

Figure C.1: The Board Of Revenues' (BOR) record room

Figure C.2: The BOR tax collection pro forma



AGRICULTURAL INCOME TAX DISTRICT MUZAFFARGARH,

FOR THE MONTH OF September, 2007.

PREVIOUS A-I-T

S No.	Name of Tehsil	Demand	Suspension	Net Demand Recoverable	Previous Recovery	Current Recovery	Total Recovery	Balance	Percentage Month	Percentage Total
1-	M-Garh	17102682	—	1,71,02,682	76,650	9,300	85,950	1,70,16,732	—	1%
2-	Kot Addu	2,83,53,571	—	2,83,53,571	87,793	38,100	1,25,893	2,82,27,678	—	—
3-	Alipur	20,79,273	—	20,79,273	34,150	44,706	78,856	20,00,417	2%	4%
4-	Jatoi	1,83,96,542	—	1,83,96,542	50,010	9,500	59,510	1,83,37,032	—	—
	Total: A	65932068	—	6,59,32,068	2,48,603	1,01,606	3,50,209	6,55,81,859	—	1%

CURRENT A-I-T

S No.	Name of Tehsil	Demand	Suspension	Net Demand Recoverable	Previous Recovery	Current Recovery	Total Recovery	Balance	Percentage Month	Percentage Total
1.	M. Garh	—	—	—	—	—	—	—	—	—
2.	KotAddu	—	—	—	—	—	—	—	—	—
3.	Alipur	—	—	—	—	—	—	—	—	—
4-	Jatoi	—	—	—	—	—	—	—	—	—
	Total: B	—	—	—	—	—	—	—	—	—
	G.Total (A+B)	—	—	—	—	—	—	—	—	—

D Satellite vegetation cover data

We used NASA's MODIS land products to observe a satellite based vegetation cover index. MODIS vegetation indices provide consistent spatial and temporal comparisons of vegetation canopy greenness, a composite property of leaf area, chlorophyll and canopy structure. The normalized difference vegetation index (NDVI) are derived from atmospherically-corrected reflectance in the red, near-infrared, and blue wavebands. NDVI ranges from -1 to +1. If the NDVI values are negative it is highly likely that it's water. On the other hand, values close to +1 suggest that there is a high possibility that there are dense green leaves.[36]

NASA's MODIS land products rely on the Sinusoidal Tile Grid System, which divides earth into 36 x 18 sinusoidal grids to locate a particular area on earth.[37]. Since we are only interested in the Punjab province of Pakistan, our first step was to locate the tiles where Punjab is located. Given the shape file of districts of Punjab, we uniformly sample 10,000 points within each district and calculated their locations on the sinusoidal tile.[38] We find that all 36 $\times$ 10,000 points lie within three tiles, namely (horizontal 24, vertical 5), (horizontal 24, vertical 6), and (horizontal 23, vertical 5). There are a total of 4800 $\times$ 4800 pixels within each of the three tiles mentioned above, with each pixel having a 250 m $\times$ 250 m size. Moreover, data for each year is divided into time intervals of 16 days. This results in 23 different time intervals in a given year. For each 16-day time interval in a year, and for each of the 36 districts of Punjab, we obtained the NDVI values of all pixels belonging to that particular district and took the average to get the NDVI value for that particular district in that particular 16-day interval. Since each year has 23, 16-day intervals, we end up having a list of 23 different NDVI values for a particular district in a particular year. Following the method used in Beg (2022a), we use the maximum value of that list as the NDVI value for that district for that year.

[36] Details accessed at `https://gisgeography.com/ndvi-normalized-difference-vegetation-index/`

[37] Accessed at: `https://modis-land.gsfc.nasa.gov/MODLAND_grid.html`

[38] We decided to rely on this method rather than just consider the district's center and calculate their locations on the sinusoidal tile. This allows us a more holistic view of the vegetation cover of the district.

E Details on string matching revenue circles

We carried out an extensive string matching exercise to merge the tax, digitization, and bureaucrats' careers datasets. We took the following steps to merge the three sets of data:

- As a first step we manually checked each revenue circle, tehsil and district in the tax data against their counterparts recorded in the digitization data from the Punjab Land Records Authority (PLRA) and allocated a unique ID to each.[39] There were 1125 revenue circles in total, out of which 838 were given IDs using this process.
- The district, tehsil and revenue circle names in the bureaucrat survey data was manually cleaned. There were 690 unique revenue circles-tehsil-districts in this data, out of these we were able to give IDs to 458.
- Finally, we merged all three datasets on revenue circle, tehsil and district names.

We next checked the veracity of these data using further records from the government on details of revenue circle, tehsil and district names across Punjab. These were personally obtained from the government in 2020.

[39]Digitization data from PLRA contained details of names of most of the tehsils and revenue circles except the following 19 (out of 141 in total) tehsils: Gujranwala Sadar, Kabirwala, Kharian, Shorkot, Khushab, Quaidabad, Jauharabad, Lahore city, Nishtar Town, Muzaffargarh, Depalpur, Renala khurd, Arifwala, Khanpur, Murree, Rawalpindi city, Rawalpindi Sadar, Rawalpindi Cantt and Daska.

F Sampling for the bureaucrat survey

The retrospective survey was carried out in 2020, with the main aim to rebuild career trajectories of bureaucrats between 2006-2013, the period for which tax data are available and reform effects can be estimated. Our sampling frame therefore, included people who were in charge of revenue circles (*Qanungo*) as well as people who in the recent past had worked as a *Qanungo*. These included bureaucrats that had risen through the ranks via promotions and were in-charge of the tehsils: (*Tehsildars* and *Naib-tehsildars*).

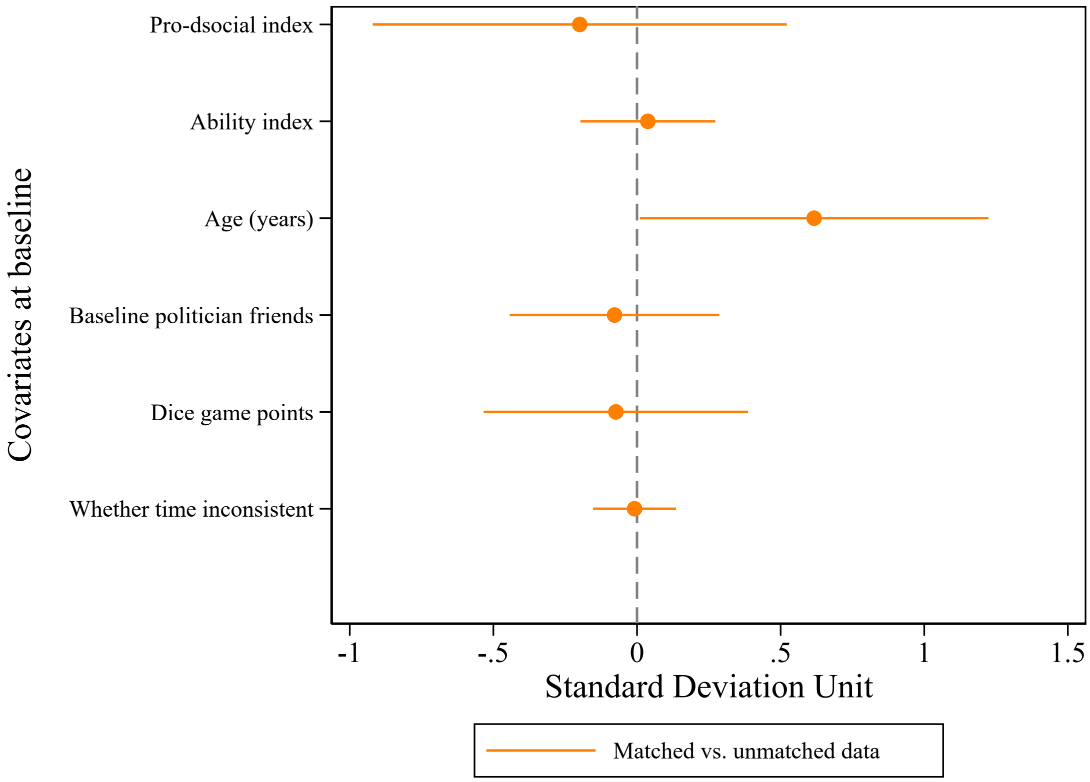
We stratified on districts and randomly sampled tehsils within each district.[40] We next created a sampling frame by contacting the local offices. Using that sampling frame we selected the universe of *Tehsildars* and *Naib-Tehsildars* working in the selected tehsils in Punjab. One *Qanungo* working with each of the *Naib-tehsildars* was randomly selected for the survey. We found 118 respondents who worked as Qanungos between 2006-2013.[41]

We could string-match the revenue circle names for 105 of those 118 respondents to match the survey data with the tax collection data. Of those the tax performance was missing for 27, so our final data set includes 78 respondents whose tax performance is observed between 2006-2013. In Figure F.1 below, we examine the potential systematic differences between these bureaucrats and the broader sample across various characteristics, utilizing data gathered from the bureaucrat survey. The only covariate showing marginal statistical significance is age. The p-value resulting from a joint significance test of all covariates in the figure is 0.5290, providing evidence that the sample is not systematically selected based on characteristic of the bureaucrats.

[40]Out of 141 tehsils in Punjab, we were able to survey bureaucrats from 138 tehsils. We were unable to survey the bureaucrats from the following three tehsils: Nishtar Town (Lahore districts), Shahkot (Nankana Sahib district), Ahmed Pur (Sheikhupura district).

[41]To find these, we started by surveying a total of 610 bureaucrats across different levels of hierarchy. Of those, 488 responded to the second round of telephonic survey about their career trajectory. The telephonic survey was used to recap the career paths of the bureaucrats, while their perceptions of digitization as well as their traits were measured in-person. In the pilot, the field team suggested this approach as the most effective way to achieve the maximum response rate, as the combined length of the two exceeded 1 hour and 30 minutes.

Figure F.1: Balance test on characteristics of the bureaucrats in the sample (that were matched with the tax data) and those who remained unmatched



Note: Data on bureaucrats' characteristics is from the bureaucrat survey. The point estimates are from a regression of the respective covariates on a dummy that takes the value of one if the bureaucrats in the survey data were matched with the tax data, and remains zero otherwise. Pro-sociality index is created from five measures: Inclusion of Others in Self scale (Aron et al., 2004; Ashraf et al., 2020), whether they have donated blood, money donated in public good game, whether they do volunteer work and whether they give charity. Ability index is created from four measures: an incentivized matrix game and a memory game as in Hanna and Wang (2017), response to questions on general knowledge and revenue rules and regulations, respectively. Politician friends are the number of friends of the bureaucrats that are either federal or provincial politicians. Dice game points is a proxy for dishonesty and it is the total in an incentivized dice game as in Hanna and Wang (2017). Intervals are 95% confidence intervals.

G Marginal Value of Public Fund Calculations

The MVPF is calculated as $MVPF = \frac{\text{Benefits}}{\text{Net Govt Cost}} = \frac{\Delta W}{\Delta E - \Delta C}$. We explain the calculation behind each term in turn:

- **Benefits** (ΔW): The benefits included in the World Bank's cost-benefit analysis are "an increase in land value, reduced transaction costs, and revenue generation" (World Bank, 2017, p. 18). The World Bank incorporates this benefit as the Net Present Value (NPV) of the increase in land value from 2014 to 2023. Using the 10% discount rate from the World Bank report, the NPV of this benefit is equal to Rs. 34,560 million (World Bank, 2017, Table 5, Annex 3).[42]

- **Cost of the reform** (ΔE): we calculate the cost of the reform as the net present value of the sum of the total capital costs and the total operating costs, as reported by the World Bank (World Bank, 2017), over the period 2007-2028.[43] The total capital cost of the reform (including software development, construction of the digitized centers, and hardware for the centers) amounts to Rs. 13,852 million (World Bank, 2017, Table 5, Annex 3), and its NPV is Rs. 5,837 million. The total operational cost of the reform (including staffing of the digitized centers, internet connection, and overheads) is Rs. 52,858 million (World Bank, 2017, Table 5, Annex 3), and its NPV is Rs. 13,125 million. The NPV of the total cost is therefore Rs. 18,962 million.

- **Long-run change in government cost** (ΔC): Our analysis suggests an additional effect of the reform on government costs due to lost government

[42]Our analysis suggests an additional source of benefit: an increase in farmer's income. In our calculation, we have assumed that all benefits are reflected in land prices. However, if land markets are imperfect, the increase in income might not be fully reflected in land values. In this case, our MVPF calculation would understate the benefits of the reform. Extrapolating our own estimate of the increase in profits experienced by farmers of Rs. 4.9 per acre (Table 3) over the period 2012-2028 (from the start of the reform being implemented to the end of the World Bank's financial projections) and adding it to the benefit calculated by the World Bank would increase the total benefit of the reform to Rs. 35,587 million. This would increase the MVPFs to 1.88 (excluding the tax loss) and 1.75 (including the tax loss).

[43]The time period for the costs begins from the first outlay of expenses towards the reform (which predates the start of the digitization as the software had to be developed and the centers built) and concludes at the end of the project's economic life, as determined in the World Bank's financial projections.

revenue. We calculate this as the loss in tax from our causal estimate, presented in Table 1. We assume that the tax loss lasts throughout the period 2012-2028 in order to make it comparable to the benefit and direct cost.[44] Using the -6.74 loss in tax per acre estimate (Table 1), multiplied by the number of acres per districts and extrapolated across the time period, we obtain an NPV of Rs. 1,413 million.

- The MVPFs are therefore: MVPF $= \frac{34{,}560}{18{,}962} = 1.82$ excluding the tax loss, and $\frac{34{,}560}{18{,}962+1{,}413} = 1.70$ including the tax loss.

[44]While the government could, in principle, offset tax losses during this period, in the absence of evidence of such interventions, we estimate the effect assuming no policy response.