Through the Mill:

Tax Technology and Compliance with a New State♔

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Abstract

Technologies can strengthen fiscal capacity but undermine compliance with other state demands. We study this trade-off through Italy's nineteenth-century milling tax, which mandated the introduction of mechanical counters to measure milled grain. Using newly digitized data, we establish that proximity to counters' distribution depots, not local economic conditions or deliberate state targeting, drove counter diffusion. Leveraging this spatial variation, we find that the technology raised tax compliance: provinces with wider diffusion collected higher tax revenues. Yet the same technology eroded civic compliance: in a difference-in-differences, counters increased evasion of military service, a response persisting beyond the tax's repeal. Evasion increased most where little state investment preceded the tax, consistent with negative reciprocity toward the state. We embed both responses in a model of tax administration. Counters were fiscally efficient, but their welfare cost rises with the social value the state places on military service and falls where state investment had come first.

Keywords: state capacity, tax compliance, military conscription, technology diffusion, milling tax, Italian unification.

JEL Codes: D73, N43, H11, H26, O33, Z13.

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I. INTRODUCTION

State capacity is a fundamental prerequisite for economic prosperity (Besley and Persson, 2011; Dincecco and Katz, 2016; Johnson and Koyama, 2017). Building it requires citizens' compliance across a range of state functions, from taxation to law enforcement and military conscription (Levi, 1988; Besley and Dray, 2024). However, policies aimed at strengthening compliance in one domain may inadvertently affect compliance in others. We study this trade-off in the context of nineteenth-century Italy, a newborn nation-state confronting the dual challenge of building fiscal capacity while earning its citizens' cooperation.

Among state functions, taxation bears a double burden: it is not only the principal source of public revenues, essential to sustain state activity; it is also the most direct and recurrent channel through which the state interacts with citizens. As a result, how taxes are collected informs citizens' beliefs about the state (Weigel, 2020; Chiovelli et al., 2025; Krause, 2025). New technologies can enable the state to measure and tax economic activity more effectively, expanding its coercive reach (*tax compliance*). Yet, depending on how they are perceived, tax technologies may also affect citizens' willingness to voluntarily meet their non-fiscal obligations (which we define as *civic compliance*).

Examining how technologies for tax collection shape state capacity poses two empirical challenges. First, one must observe the evolution of citizens' compliance across different policy domains, as a new tax technology is introduced. Second, identification requires plausibly exogenous variation in the diffusion of such technology, uncorrelated with contemporaneous policies or other determinants of state building.

To address these challenges, we consider the historical case of Italy's milling tax (*tassa sul macinato*, 1869–1884), which introduced mechanical counters to measure, and therefore tax, milled grain. We assemble a novel dataset on both tax and civic compliance spanning the first thirty years after Italian unification. Our identification strategy leverages spatial variation in the distribution of mechanical counters, driven by production and logistical constraints, rather than by the local economy or strategic state targeting. Our central finding is that the tax technology induced a trade-off in compliance with the new Italian state: it raised tax compliance but eroded civic compliance.

Nineteenth-century Italy provides a unique historical case: unlike much of Europe, the Italic peninsula had remained deeply fragmented since the fall of the Roman Empire. When one state emerged almost ex nihilo in 1861, its rulers faced a foundational problem: how to devise an effective tax system and build fiscal capacity essentially from scratch. A defining moment in this effort was the introduction of the milling tax. Among the most controversial and unpopular taxes in Italian history, this levy had three distinctive features: (i) it relied on a novel technology—mechanical counters to be installed on millstones to

record their rotations and so estimate milled quantities and taxes due; (ii) it taxed the main ingredient of Italian staples (bread, polenta, and pasta), making it highly regressive and salient for the citizenry; (iii) it was originally conceived to alleviate the public debt inherited from the unification process, rather than to fund an expansion of public services across the kingdom.

Main Findings. Using newly digitized data on the implementation of the milling tax, we establish that proximity to counters' distribution depots was a key driver of technology diffusion (our "first stage"). Provinces that are one walking-day closer to the distribution depots had 6–11 percent more millstones equipped with counters. Importantly for our identification strategy, this relationship is not driven by general market access or exposure to state institutions, local milling potential or cereal demand, broader economic development, or counters' damage in transit. Nor is it explained by deliberate state targeting of counters to provinces with higher tax potential. We further exploit the existence of multiple counter models and show that provinces closer to the distribution depot *of a given model* installed more of that specific model, but not of other models.

The tax technology succeeded in raising tax compliance. At the national level, milling tax revenues steeply rose as counters spread. Across provinces, those one walking-day closer to counters' distribution depots collected 7–9 percent more revenues per capita. The results are robust to controlling for an extensive set of geographic, demographic, and economic controls. Contemporaries and historians alike credit the milling tax with a leading part in bringing Italy to a balanced budget by 1876.

We provide additional evidence consistent with the new technology, not geography or simultaneous economic effects, driving compliance with the milling tax. First, increases in tax revenues arise across pre-unification states with disparate fiscal and political institutions. Second, effects are larger for wheat—the grain with the higher tax rate—suggesting that the technology raised revenues precisely where the returns to under-reporting taxable quantities were greatest. This is consistent with the technology making an otherwise difficult-to-observe tax base more legible to the state. Third, we rule out that effects on tax revenues are driven by alternative mechanisms, such as changes in the milling industry or in trade of grain products,[1] or in monitoring through state inspections.

Greater compliance with the milling tax translated into greater fiscal capacity. We show this by using additional data we digitized on the income tax, which was outside the technology's reach. Near counters' distribution depots, we detect no differential trends in

[1] These are *relative* effects in provinces with higher versus lower diffusion of the tax technology. We cannot rule out a nationwide impact of the tax itself on milling production and economic growth (e.g., Giommoni and Loumeau, 2026).

income tax revenues prior to the policy and no differential changes thereafter: the milling tax did not shift evasion toward a less legible tax. The post-policy null further assuages concerns that the increase in milling tax revenues stemmed from contemporaneous policies, generally tighter tax enforcement, or differential growth in economic activity near depots.

While successful at strengthening fiscal capacity, the technology came at the cost of eroding civic compliance. Since the counters were conceived, provided, and administered by the state, dissatisfaction with the fiscal experience they brought was directed at the state itself. The milling tax fell disproportionately on poorer and disenfranchised households—whose limited political voice left them few margins of response other than the draft, the main non-fiscal obligation of citizens in post-unification Italy. Leveraging a difference-in-differences (DiD) design, we show that draft evasion increased by 1.1 percentage points (pp) in districts with proximity to counters' distribution depots above the sample median, i.e., 42 percent over the counterfactual mean in the post-tax period. These effects are not driven by pre-tax trends nor by any specific pre-unification state. In line with the technology shifting beliefs about the state, we find that effects outlasted the repeal of the tax.

We propose reciprocity (Sobel, 2005; Besley, 2020) as a general mechanism behind the trade-off between tax and civic compliance: taxpayers tolerated a heavier burden from the technology where the state had given before taking. To test this, we estimate heterogeneous effects on draft evasion by railway construction, the largest public investment of the newborn nation-state. The decline in civic compliance concentrates in districts that had received low railway investment by the eve of the milling tax. Districts with high railway investment, instead, show no decline.

The state did not reverse this pattern after the levy: taxpayers most heavily burdened by the technology were not compensated with greater public investment. We show this using event studies of railway density on proximity to counters' distribution depots. Estimates are flat before the milling tax, so policy exposure did not follow prior railway expansion, and remain flat after, so districts bearing higher taxes received no compensating investment in railways.[2]

Welfare. We study the welfare impact of the tax technology by pricing the responses it induced in tax and civic compliance. To do so, we build on a theoretical literature on optimal tax administration (e.g., Kaplow, 1990; Mayshar, 1991) and extend the model of Keen and Slemrod (2017). The introduction of a tax technology, through the lower compliance with non-fiscal obligations it generates, imposes two externalities: the state

[2] This mechanism echoes the experimental evidence of Krause (2025) in Haiti, where tax enforcement paired with the provision of local public goods increases tax compliance, while enforcement alone provokes backlash via political unrest and violence.

spends more pursuing and punishing draft evaders (a *fiscal* externality), and citizens bear the uninternalized cost of draft evasion itself (a *civic* externality). We then map these two externalities into the marginal value of public funds (Hendren and Sprung-Keyser, 2020; Boning et al., 2025) to quantify their welfare implications.

Counters were an efficient instrument from a fiscal standpoint: each lira raised by the state cost citizens between 1.05 and 1.19 lire of welfare. The fiscal externality from draft evasion increases this cost by 4 percent. The civic externality, valued at the state's reservation price for a conscript, adds an additional 70 percent. Prior state investment weakens both externalities and, therefore, mitigates the welfare cost of the tax technology.

Related Literature. Our paper contributes to a long-standing body of research in political economy and economic history on fiscal and state capacity. This body of research has focused on two questions: (i) how states build capacity,[3] and (ii) how capacity shapes economic and political development.[4] We identify technology as a further driver of fiscal capacity, working through *fiscal legibility* (Scott, 1998; Lee and Zhang, 2017), i.e., the state's ability to observe the tax base. In the hands of a new state, an instrument of legibility raised revenue even where it lacked political legitimacy and accountability—precisely because technology does not presuppose them.

We also show that building fiscal capacity can come at the cost of non-compliance with other state demands. Close to our paper, Garfias and Sellars (2026) study a technological innovation that *inadvertently* improved the ability of the Spanish Crown to observe output across colonial Mexico. Following the discovery of the patio process, the Crown could infer local silver output from state-provided mercury input (*accidental legibility*); this fostered political centralization and state development. By contrast, we examine a case in which the technology was *deliberately* designed and introduced by the state to measure millstone rotations and thereby infer milling output (*engineered legibility*).[5] The resulting dissatisfaction was therefore directed at the state: citizens withheld compliance with military service, the principal obligation the state imposed on them beyond taxation.[6]

[3] e.g., through warfare and external threats (Tilly, 1992; Besley and Persson, 2008, 2009; Dincecco et al., 2011; Dincecco and Prado, 2012; Gennaioli and Voth, 2015; Becker et al., 2025; Cox et al., 2025) as well as bureaucratic professionalization (Evans and Rauch, 1999; Rauch and Evans, 2000; Aneja and Xu, 2024).

[4] e.g., Acemoglu et al. (2015); Acemoglu et al. (2016); Dell et al. (2018); Cantoni et al. (2024); Chambru et al. (2024); Besley et al. (2025).

[5] Technological change may also transform the state along other margins. Transport technology, such as the railway, made the rapid movement of mass armies and their supplies possible (Onorato et al., 2014) and, by reducing monitoring costs, drove the expansion and bureaucratization of the federal government in the US (Mastrorocco and Teso, forthcoming). Okunogbe and Tourek (2024) and Hjort and Tian (2025) review the contemporary evidence from developing countries.

[6] Davoine et al. (2026) and Giommoni et al. (2026) examine different forms of backlash against taxation: social conflict and popular unrest that fed the French Revolution, respectively. Chiovelli et al. (2025) show how

By documenting the tension between tax and civic compliance, we contribute to the literature on tax morale and reciprocity toward the state (reviewed by, e.g., Luttmer and Singhal, 2014; Slemrod, 2019). While recent work shows that inequity can depress tax compliance (Ajzenman et al., 2025; Best et al., 2026; Nathan et al., 2026), our analysis traces its spillover into other policy domains. Effective states must perform several functions at once, many of which hinge on citizens' voluntary compliance.[7] The mechanism we propose makes these functions interdependent: perceived unfairness in taxation and the lack of compensating public investment shift citizens' beliefs about the state and prompt negative reciprocity toward it.[8] Ignoring citizens' endogenous response can therefore understate the welfare cost of fiscal and technology policies.

Finally, we add to the literature on the economic history of Liberal Italy and, more specifically, on the ambiguous consequences of policies implemented by its first governments.[9] We focus on a unique and defining moment of Italy's unification process: the introduction of the milling tax and the expansion of the fiscal state. The policy was highly controversial at the time, and the historiography on it remains divided. On the one hand, supporters stress the tax's necessity for—and eventual success in—securing fiscal sustainability (e.g., Marongiu, 1995, 2009). On the other hand, Marxist scholars have interpreted the tax as a regressive instrument of the ruling elite, loading the fiscal burden onto the voteless poor—and so stoking hostility toward the new state (e.g., Sereni, 1947; Bulferetti, 1949; Romano, 1966; Del Carria, 1977). Our quantitative exploration is consistent with both views. As Federico (2023, p.10) puts it, "The *Destra Storica* had to build a new state out of a motley collection of very different polities without much preparation. They managed the task rather successfully, without paying much attention to the political and social consequences of its policies."

reforms that successfully expanded the Spanish Empire's capacity to tax and administer its colonies fueled anti-Spanish sentiment and Latin America's independence movements.

[7] Building on the historical account of Levi (1988), who stresses that coercion alone cannot sustain fiscal capacity, Besley (2020) casts voluntary compliance as a reciprocal bargain between citizens and the state.

[8] This mirrors evidence of positive reciprocity, expressed in military volunteering following government spending (Caprettini and Voth, 2023) and in tax compliance following the provision of local public goods (Fernández et al., 2025). Bagnato (2023) shows that historical fiscal capacity can reduce draft evasion, when effective tax collection signals state capacity and raises perceived returns to participation. In a contemporary field experiment, Weigel (2020) identifies this "participation dividend:" taxation encourages citizens to engage with local governments and demand public goods. Our results indicate that such a dividend is not automatic; it depends on the way in which fiscal capacity is built—and, consequently, the direction in which higher taxes change beliefs about the state.

[9] Existing work has looked at, e.g., the repression of brigandage (Accetturo et al., 2017; Lecce et al., 2022; Ruzzante and Pizzimenti, 2025), the removal of tariffs (A'Hearn and Rueda, 2023), disaster response (Alfonzo, 2026), and administrative centralization reforms (Cainelli et al., 2026).

II. HISTORICAL BACKGROUND: THE MAKING OF A NEW STATE

A. From Italian Unification to Building Fiscal Capacity

From the fall of the Roman Empire to the first half of the nineteenth century, the Italic peninsula remained fragmented into several independent polities, including city-states, duchies, republics, and mid-sized kingdoms.[10] Political fragmentation, coupled with military weakness, turned the *Belpaese* into an easy land of conquest for neighboring powers, therefore delaying the process of institutional reform and economic development that other European nation-states experienced during this period. In the words of Federico (2023), "Italy in 1861 was a poor country with a glorious past:" after a prosperous period during the Middle Ages, the pre-unification states were not able to keep up with the industrializing neighbors and mostly depended on low-intensity agricultural production.[11] Natural resources (namely, coal and minerals) were scarce, and only a handful of modern factories were concentrated in the North-Western regions of Liguria, Lombardy, and Piedmont.

The level of under-development of nearly all pre-unification states was reflected in that of their tax system. Small armies and bureaucracies, as well as negligible spending on public works and other developmental initiatives, left these states with remarkably low revenue requirements, allowing them to maintain mostly balanced budgets. A notable exception to these backward fiscal policies was the Kingdom of Sardinia–Piedmont. Since the 1850s, the northern hegemonic kingdom had pursued economic modernization—driven by the development of a railway network and the construction of other infrastructures—and trade liberalization (Federico, 2010). This process ran on deficit spending, mostly financed with foreign loans: although Piedmont taxed more heavily than any other pre-unification state,[12] its receipts could not keep pace with expenditures.

A further surge in expenditures supported the expansion of the Piedmontese armed forces, which were subsequently deployed in the three Italian Wars of Independence between 1848 and 1866. King Victor Emanuel II and his astute Prime Minister Cavour

[10] The number of independent polities shrank from around one hundred (e.g., the *Comuni*) to eight after the Congress of Vienna of 1814–1815: the Savoyard Kingdom of Sardinia–Piedmont and the Habsburg-controlled Kingdom of Lombardy–Venetia in the North; the small independent duchies of Lucca, Parma, and Modena, the larger Grand Duchy of Tuscany, and the Papal States in the Center; the Bourbon Kingdom of Two Sicilies in the South.

[11] See Federico (2009) for a comparative analysis of productivity and technology adoption in post-unification Italian agriculture.

[12] On the eve of unification, per-capita tax revenues in Piedmont were about 30 percent above the Papal States, close to double the Two Sicilies, Tuscany, and Modena, two and a half times Parma, and four times Lombardy–Venetia (Dincecco et al., 2011, Table 4). These tax gaps did not simply mirror income, as the North–South divide in per-capita GDP started opening only in the mid-1860s (Daniele and Malanima, 2007).

annexed the other regions of Italy through a series of diplomatic maneuvers and military operations. Against all odds, on 17 March 1861, the Kingdom of Italy was proclaimed.

Piedmontese legislation was extended to the rest of the peninsula without any adjustment time, following what the *patriota* Carlo Cattaneo would portray as a "steamrolling unification" approach. Internal customs were removed, the liberal Piedmontese tariff imposed, and the lira replaced the pre-unification currencies. The new state adopted Piedmont's centralized administration of provinces under prefects, as well as its civil, commercial, and maritime codes. It also established a single conscript army (Section II.C).

The public debts of pre-unification states were integrated into a single national liability. As a consequence of the spending spree followed by the Kingdom of Sardinia–Piedmont and the additional costs incurred during the military campaigns for unification, the first Italians inherited a public debt equal to 43 percent of their GDP.[13] Deficit spending did not stop there: the new state invested heavily in infrastructure—especially railways linking North and South, regarded both as essential to kick-starting an integrated national market and as an emblem of modernization (Schram, 1997).[14] This inevitably led to a further spike in the debt-to-GDP ratio, which climbed to a worrying 74% in 1865 (Zamagni, 1998).

More cautious was the government's approach in terms of taxation. The making of a unitary fiscal state began in 1862 with a set of duties on legal and property transactions (*imposte sugli affari*), the field in which pre-unification tax systems were already equipped.[15] The crucial reform came in 1864. On 3 July, the disparate patchwork of consumption taxes (*dazio di consumo*) inherited from pre-unification states was reorganized, with the most important urban duties on foodstuffs and beverages transferred from municipal budgets to the new state. On 14 July, the government introduced a national tax on income not derived from real estate, including income from capital and labor (*imposta di ricchezza mobile*). On the same day, a land tax based on local cadastral assessments (*imposta fondiaria*) was provisionally equalized across the kingdom, pending a uniform land cadastre.[16] The

[13] The unification of public debt was vigorously criticized by the southern political community. Through this measure, the government was distributing among all taxpayers obligations taken in a great extent by (and for) Piedmont. Namely, the debt amounted to around 188 lire per capita in the former Kingdom of Sardinia, 84 in the Two Sicilies, and 55 in Tuscany (Toniolo, 1988). This implied a substantial transfer of resources from the South to the North of the country.

[14] Railway construction alone absorbed 59% of all investments in social overhead capital made over the first five years after unification and nearly 2% of GDP (Fenoaltea, 1988).

[15] Partly modeled on the French tax system, these duties initially comprised registration, stamp, and mortgage; succession and mortmain duties followed over the next years. Moreover, a wholly new tax was imposed on fast railway transport, which passed only over considerable opposition for fear of discouraging trade.

[16] Both of these direct taxes were enacted as repartition taxes. The state fixed a national revenue target to be raised and apportioned it across territorial units and, in turn, taxpayers (*contingente* system): the tax rate followed from the target rather than being set in advance. The *ricchezza mobile* was converted within two years to a fixed rate (*quotità* system): 8 percent in 1866, raised to 12 percent in 1870.

tax on non-rural buildings (*imposta sui fabbricati*) was then detached from land in 1865, allowing assessments to adjust more quickly to rising urban property value.

The fiscal unification did not give Italy the financial stability and international creditworthiness that its first governments had anxiously sought. In April 1866, the value of Italy's perpetual bonds, or consols (the main instrument used to raise debt at the time), collapsed amid an international financial downturn and the eve of the third independence war against Austria. Faced with a potential bank run, the government response was twofold. First, the *Banca Nazionale nel Regno d'Italia*, the de-facto central bank, suspended convertibility into gold and let the currency float. Second, a series of contractionary fiscal measures was weighed in order to quickly shore up the budget.

"Now, gentlemen, it is useless to conceal it from you: to achieve this aim, there was no choice, no possible hesitation. Among the proposals studied by my predecessors," Finance Minister Luigi Cambray-Digny told Parliament, "only one seemed to me capable of meeting the need, only one offered the certainty of giving the treasury a substantial source of revenue, and this is the milling tax" (Camera dei Deputati, 1868a, p. 3580).

After an acrimonious debate, the milling tax was passed by secret ballot, with 59% of the vote at the Chamber on 21 May and 90% at the Senate on 27 June 1868. Starting on 1 January 1869, the law taxed every quintal of milled grains according to the following schedule: 2 lire for wheat, one lira for maize and rye, 1.20 for oat, and 0.50 for barley, dried pulses, and chestnut. Cambray-Digny would later call the tax "the central pivot around which revolves the entire machinery in which Parliament and the Ministry are now engaged, and which must lead to the reorganization of our finances" (Camera dei Deputati, 1870a, p. 13).

B. One Technology to Tax Them All[17]

The milling tax was far from new to Italians: its origins dated back at least to the feudal period. Over the centuries, pre-unification rulers returned to it again and again, in various forms, especially to meet a fiscal emergency.[18] The tax was at times withdrawn in response to the peasant revolts that followed. Yet its memory remained vivid among millers and peasants alike: it evoked a long history of bureaucratic formalities, practical hardships,

[17] The historical account in this subsection is based on Nina (1903), Parravicini (1958), and Marongiu (2009).

[18] The earliest evidence of a milling tax comes from Sicily under Arab rule. By the mid-sixteenth century, the tax had spread to the whole Italic peninsula. It was largely abolished by the early nineteenth century, save in Marche, Sicily, and Umbria, where it survived until unification. There, the milling tax was abolished in 1860, in the immediate aftermath of the Expedition of the Thousand and the Piedmontese occupation of central Italy, as the new authorities sought the favor of local peasants for the cause of unification. For a rich historical overview of the milling tax before unification, see the well-known report prepared by Quintino Sella in support of his 1865 bill (Camera dei Deputati, 1866), described below, and Zangheri (1957).

and administrative abuses.[19]

Aware of these historical precedents, Quintino Sella—the first to propose the reintroduction of the milling tax, "not without hesitation and with great regret" (Camera dei Deputati, 1866, p. 344)—put forward a novel collection method designed to minimize the abuses of the older systems, while still guaranteeing adequate tax revenues. A hydraulic engineer, Sella conceived the idea of a mechanical counter (*contatore*) on millstones to register the number of their revolutions. From this, the authorities were to infer the quantity milled and the tax due. Sella's faith in technology is on full display in his words to Parliament (Camera dei Deputati, 1866, p. 38):

> *The taxman is no longer in contact with the taxpayer; he does not have to monitor his movements, ask him for declarations, impose slips and permits, weigh grain and flour, settle accounts with him. [...] His fidelity is compelled, it does not depend on his morality; a silent and inexorable instrument keeps exact record of the revolutions he imparts to the millstone and of the sums he has received on deposit.*

The law adopted Sella's counter, to be provided at the state's expense and installed on every millstone across the kingdom. The miller served as the tax's collector: he recovered the charge from his customers at the per-quintal rates above and owed the state a payment (*quota*) for every hundred revolutions the counter recorded.[20] Heavy penalties, ranging from monetary fines to the revocation of the milling license and closure of the mill, were stipulated for those who did not comply with the tax.

The tax drew harsh criticism on three fronts. The first questioned the accuracy of the technological device. Milling was perhaps the only industry which was spread across the whole Italian territory, making it an obvious object of taxation. However, most mills were small, technologically backward, and often remote (Aliberti, 1970), making the application and functioning of counters uncertain and possibly unreliable: "the counter cannot count," as a refrain of the opposition went.[21]

[19] The most infamous of these being that milling was permitted only during certain daytime hours; after sunset it was prohibited, unless the miller agreed to remain locked inside the mill overnight, free to break down the door only in the event of a flood or fire. In addition, the miller was required to reside in the mill, which could be accessed through a single door, one key to which was kept by the weighing inspector (*custode pesatore*). That tax official could search, without judicial authorization, not only the premises used for milling but every room attached to the mill. The miller could not even dispose of his own grist unaided: the inspector held one of the two keys to the chest into which the milled grain had to be poured. Yet for all this panoply of controls, tax evasion proved all but impossible to prevent.

[20] The payment to the state was in cash, though customers could settle their share in kind, typically with part of the grain they brought to the mill. According to the law, each mill had to keep a stamped scale (*bilancia bollata*) and to post the latest official list of prices (*mercuriali*) from the nearest market, signed by the mayor; millers had to weigh and value each customer's share accordingly.

[21] The main alternative to the counter was proposed by Sella's successor at the Ministry of Finance, Antonio

Setting the technology aside, the second criticism targeted the tax's regressivity. Particularly vocal against it was Francesco Crispi, who had abolished the milling tax in Sicily after he was appointed First Secretary of State by Giuseppe Garibaldi (Footnote 18). Crispi, himself the son of a grain merchant, called it a "progressive tax, not in proportion to wealth, but in proportion to misery. It affects bread, the food of life" (Camera dei Deputati, 1868b, p. 5008). Finally, critics doubted the magnitude of the projected revenues and warned of the risk of social unrest. Crispi, again: "With the bill we are discussing you'll have turmoil but you won't have the tithe" (Camera dei Deputati, 1868b, p. 5011).[22]

Despite the critics, the policy proved effective and, by the mid-1870s, delivered a steady flow of government revenues. As Cambray-Digny had argued, "Taxes settle as they age; [...] the best way to make them bearable is the habit of paying them" (Camera dei Deputati, 1870a, p. 19). Looking back, Francesco Saverio Nitti (1905, p. 537) attributed the acceptance of the tax to the fiscal apparatus itself and held that "Once collection and enforcement methods were refined, the tax became part of everyday life without much difficulty." With the counters running throughout the kingdom, the milling tax yielded around 8 percent of government revenues (Figure 1). Partly as a result of this effort, Italy reached a balanced budget in 1876 for the first time in its history—Sella's gamble had paid off.[23]

The balanced budget did not suffice to keep the Historical Right in power. In March 1876, the Right split and ceded office to the Historical Left (*Sinistra Storica*). Opposition to the milling tax had been a central plank of the Left's platform in the November 1876 elections, where it secured a decisive electoral victory. Yet, once in office, the new majority hesitated, wary of unbalancing the budget, and proposed a repeal only in 1878. The Right, now in opposition—Sella foremost among them—warned that abolition would reopen the deficit or else force cuts to defense, housing, and schools. After a long parliamentary fight, which brought down three governments and prompted the dissolution of the Chamber, the tax was gradually eliminated from 1879 onward and fully abolished in 1884.

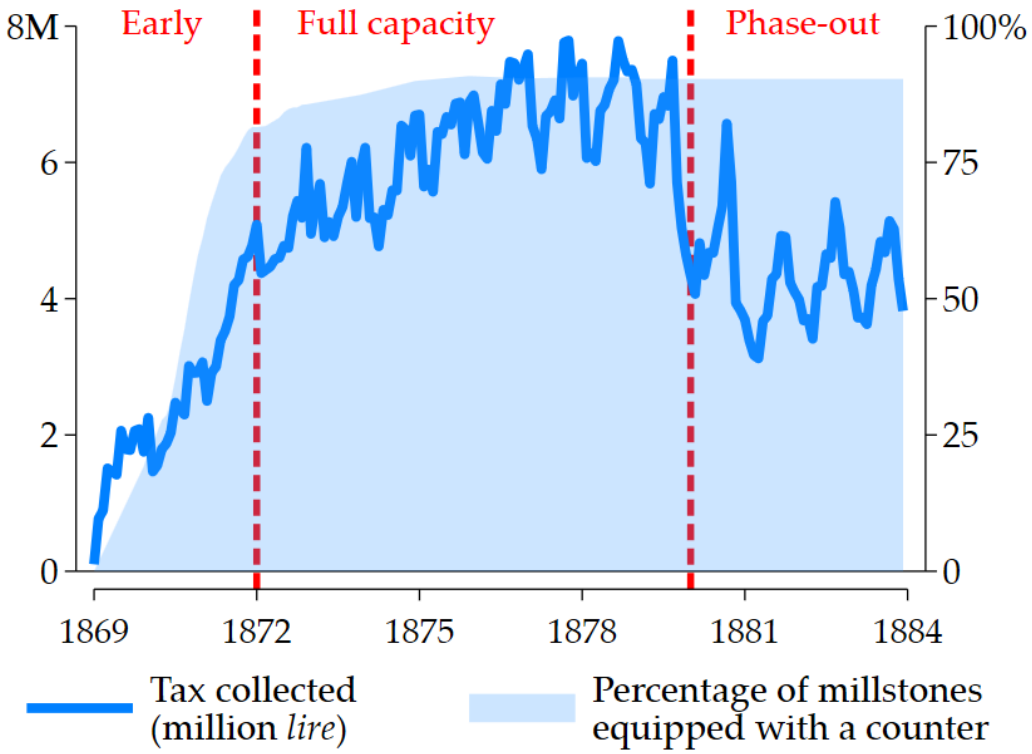
Manufacturing and Distribution of Counters. Once the milling tax passed in 1868, the first question that lawmakers faced was which counter model could best measure

Scialoja (Camera dei Deputati, 1867). His proposal was to rely on the self-declarations of millers, in line with the income tax introduced in 1864. The final version of the bill would allow for self-declaration only in the interim period before the installation of counters. Arguably, this halfway solution lies behind some of the difficulties that beset tax collection in the first two years of policy roll-out (see, e.g., the Parliamentary speech by Federico Seismit Doda, Camera dei Deputati, 1869a).

[22] The democratic press of the day sounded the same alarm: "The tax will ruin Italy and could spark serious unrest"; "Bread or death! [...] The social revolution is on the verge of erupting;" the tax "could very well serve as a pretext for the enemies of our unity, and especially for the deposed princes, to stir up serious unrest and bring the Italian state to ruin" (reported by Rosselli, 1927, pp. 93–94).

[23] This would only happen two more times, namely in 1893–1896 and in 1925. Since then, Italy has always run a budget deficit and, nowadays, maintains the second highest sovereign debt in Europe.

Figure 1. Policy Implementation, Technology Diffusion, and Tax Revenues



Notes: Data are detailed in Section III.

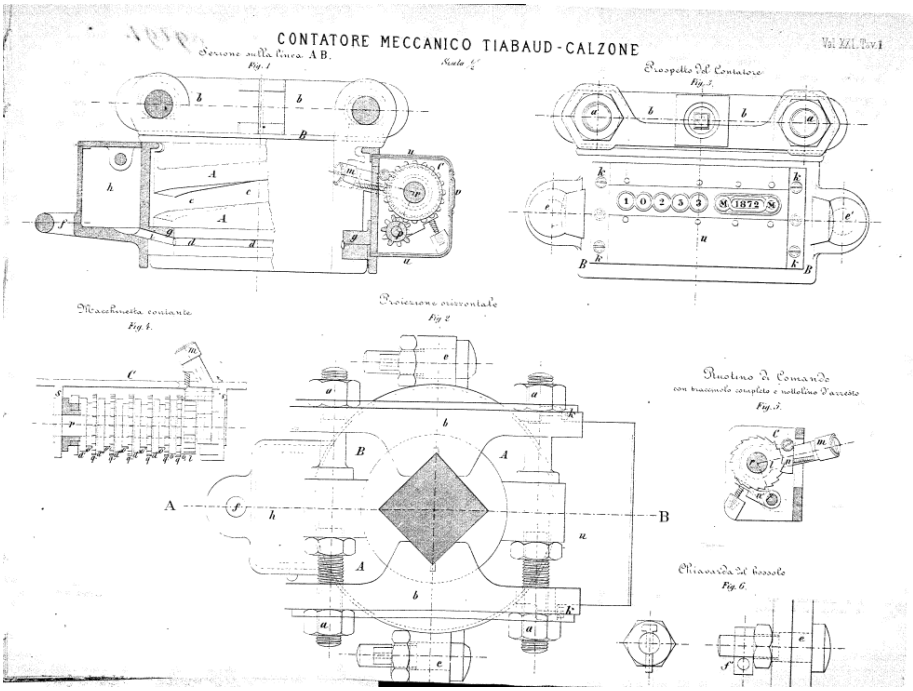
milled quantities, among those that had been experimented with before the vote. A special commission selected four models: the first, invented by two Italian mechanics (Thiabaud and Calzone), became known as the *Italian model* (Figure 2a); a second was known as the *French model*; a third was designed for wooden poles, which were especially prevalent in water mills (Figure 2b); and a fourth was to be placed in the sleeve of the dormant millstone, in mills where the counter could not be fixed to the pole (Figure 2c).

The government then commissioned the production of 14,000 Italian models, 21,000 French, 1,000 for wooden poles, and 100 for sleeves. The 14 manufacturers of the Italian model were all in Northern Italy (9 in Turin, 1 each in Pinerolo, Como, Milan, Brescia, and Udine); the French models came from Mulhouse and Lyon; the wooden-pole counters were manufactured in Florence; the sleeve counters in Turin (Figure 3a, Camera dei Deputati, 1870b, pp. 5–6).

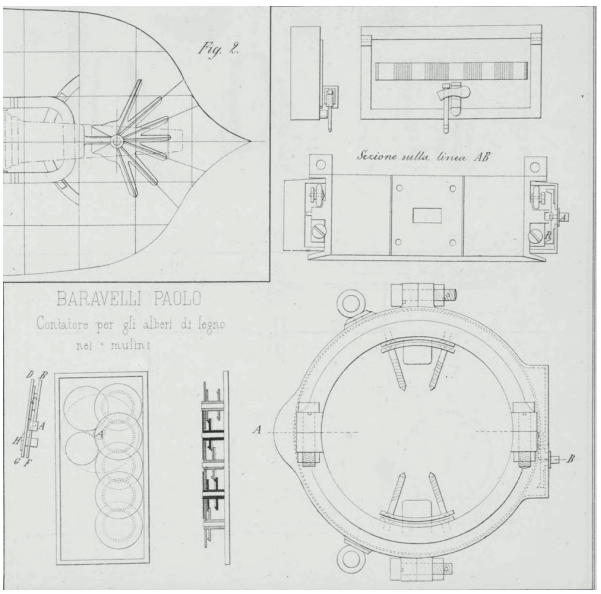
The deployment of the four models was first split between two government depots. The French and wooden-pole models were verified and distributed by the mechanical workshop set up in 1868 at the Ministry of Finance in Florence; the Italian and sleeve models by the Security Printing Works (*Officina delle carte valori*) in Turin.

Figure 2. The Tax Technology: Mechanical Counters

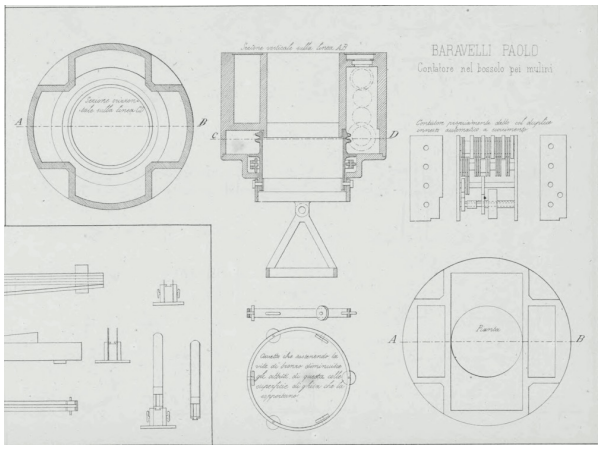
(a) Italian Model



(b) For Wooden Poles



(c) In the Sleeve



Notes: The image of the Italian model is from Camera dei Deputati (1870b, Allegato II). The images of the other two models are from Ministero di Agricoltura, Industria e Commercio (1869, Tavola 110 and 101, respectively).

In 1870, the government commissioned a second batch of counters. Each manufacturer of the Italian model from the first wave was commissioned an additional 1,000. To expand

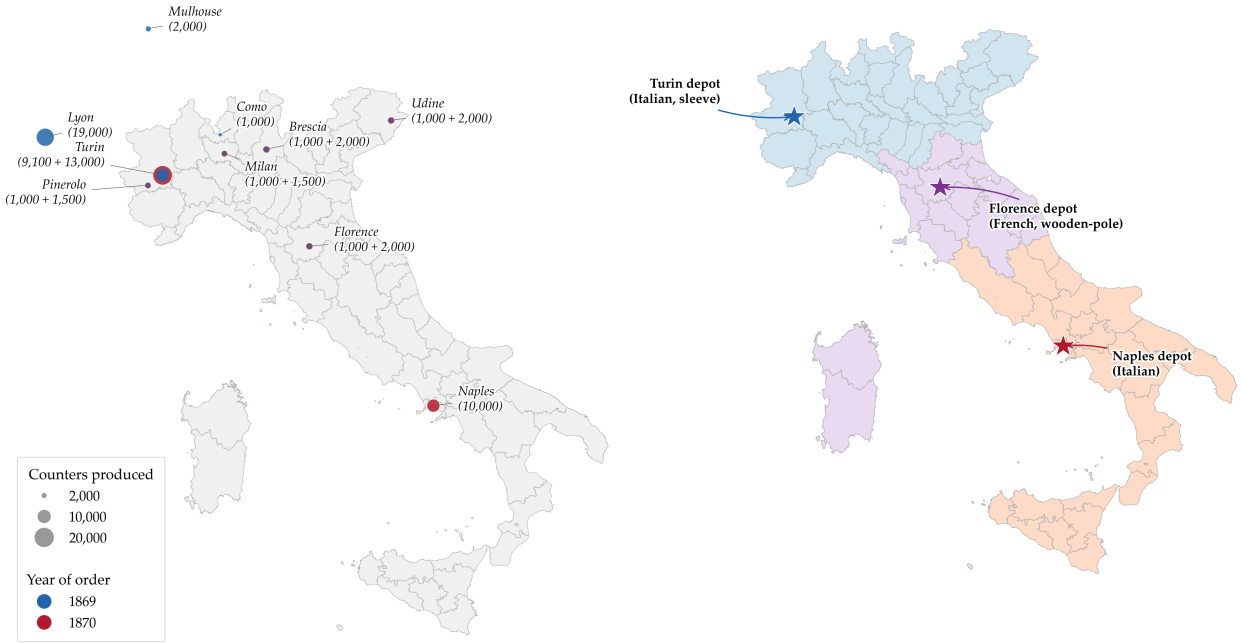
the installation of counters to the South, production was extended to Naples, where a manufacturer in Torre Annunziata was commissioned 6,000 counters and one in Pietrarsa 4,000. Finally, 2,000 more sleeve counters were commissioned in Florence.

With production extended southward, the distribution network was reorganized. A new depot was set up in Naples on 10 April 1870, responsible for installing exclusively Italian-model counters in the Southern provinces: from Lazio and the Abruzzi down to Sicily. The Florence depot retained the central provinces: Tuscany, Umbria, and Marche. The Turin depot served the North: Liguria, Piedmont, Lombardy, and Venetia (Figure 3b).

Figure 3. The Geography of the Tax Technology

(a) Manufacturing Plants of Counters

(b) Distribution Depots



Notes: locations and number of counters manufactured digitized by the authors from Camera dei Deputati (1870b, pp. 7–9); list of provinces assigned to each distribution depot taken from Royal Decree (*Regio Decreto*) no. 5595, 13 March 1870.

Tax Collection and Resistance. The administration of the tax enjoyed a certain degree of organizational autonomy. It was entrusted to a newly minted corps of tax officials with technical and economic training: the "engineers of the milling tax" (*ingegneri del macinato*).[24] Their recruitment criteria and legal status were themselves innovative for the

[24] Set apart from the ordinary revenue bureaucracy, this special corps was organized in provincial offices, coordinated and supervised by the three distribution depots in Turin, Florence, and Naples. At its peak in 1872, the corps counted 245 salaried engineers and 1,861 daily-wage field staff (Camera dei Deputati, 1872, pp. 202–203).

time: the post required a license to practice as an engineer, completion of the regular course in mathematics and the advanced training program at the kingdom's technical schools (*scuole di applicazione*). For promotions, merit took precedence over seniority (Aliberti, 1970, Part II, Chapter III).[25]

The engineers of the milling tax had three main tasks. First, they were in charge of installing and maintaining the counters throughout the kingdom. Second, they carried out unannounced, non-periodic inspections to verify the lawful use of counters and detect fraud. Third, and most importantly, they had to determine the *quota* a miller owed for every hundred revolutions of his millstones. But the same number of revolutions could yield different quantities of flour depending on many overlapping factors, such as the variety of grain being ground, the fineness sought, the millstone's diameter and condition, and the motive power applied.

The tax met immediate and widespread resistance from both millers and peasants. On the one hand, a large swathe of millers refused to take out their operating licenses and did not open their mills on 1 January 1869.[26] On the other hand, the abrupt lockout brought to the town squares masses of angry peasants, armed with pitchforks, shovels, and sticks, asserting their sacrosanct right to milling. Peasants' grievances fell on millers, saddled with the unwanted role of tax collectors, as well as on the local authorities. They demanded the abolition of the tax and the immediate reopening of local mills.[27] In the provinces of Bologna, Modena, Parma, and Reggio Emilia, the government proclaimed a state of siege on 5 January 1869, and repressed the rebellion through the deployment of military troops

[25] These engineers embodied Sella's plan to create a new type of tax official suited to "the new tax systems based on scientific criteria." Sella eventually intended to entrust them with the administration of all manufacturing taxes. In a letter to his friend Costantino Perazzi, who was also an engineer by training and became the secretary general of finance in 1872, Sella joked: "Here's a real bombshell. I am thinking about entrusting you with the [new] manufacturing tax. What is the milling tax if not a manufacturing tax? And who better than engineers to tax [...] factories? [...] This is a serious bombshell. And yet it's right" (Museo Centrale del Risorgimento Italiano, 1871).

[26] Of the 74,083 existing mills, 11,959 closed, 464 were kept open by the authority as a measure of public order, 3,141 were in operation without millers collecting any part of the tax, and 20,810 paid only a part of the expected tax (Camera dei Deputati, 1870b, p. 3).

[27] The protests and uncoordinated episodes of violence arose spontaneously, without guidance from any political movement. They featured extremely incoherent slogans, expressing support for the new king of Italy, a republic system, the pope, or the Austrian emperor. In some municipalities, rioters stormed municipal offices and archives, where they burned civil registers and other official records, including population rolls used to assess the income tax and draft lists. More sporadically, rioters seized rifles from the National Guard, with the alleged connivance of its local members, and attacked the properties of wealthy landowners. Peasants' demands never involved better working conditions and class struggle. Nor did democratic and republican circles, including Giuseppe Mazzini himself, attempt to connect rural disturbances with emerging urban labor mobilization. Cammelli (1984) provides a detailed description of the events. Manzotti (1956) zooms in on the experience of Emilia, Romito (2021) of Parma, Casellato (2013) and Buonanno (2015) of Venetia.

under General Raffaele Cadorna.[28]

After the restoration of law and order, most mills reopened and resumed milling, but resistance did not end there. Millers contested the quotas assigned to them; many petitioned for scrapping the counter and for recasting the tax on the consumption of flour rather than on the production of the millstone. Being the main interface between the state and the population, the engineers of the milling tax bore the brunt of the millers' resentment, while the counter became the focus of their distrust (Aliberti, 1970, Part II, Chapter IV). A group of millers from Salerno ask:

> *But is it possible that the engineers of the milling tax, however competent and well-intentioned they may be, can keep track of all the calculations of power, speed, and industrial output, which ordinarily occur in a mill due to diversity of grains and processing losses?*

A letter to the Parliament, from Catanzaro, recounts that

> *The millers [...] unanimously replied that they could not entrust their fortunes and their property to the cabalistic workings of a machine that was, to them, uncomprehended and incomprehensible! [...] And when we asked them why they so distrusted the counter, they replied, "How can we trust a machine that records one quintal of flour for a millstone, when this has ground only thirty kilograms? This is a diabolical invention designed to put millers at the mercy of the engineers, who, at their own whim, can save or ruin the millers."*

Where they could, millers tampered with the counter directly, manually forcing the devices and breaking the seals (Aucello, 2017, p. 4). They also manipulated the mill so that the revolutions the counter recorded understated the flour actually milled, slowing the millstones, concentrating the force on them, and grinding coarser flour (Ministero delle finanze, 1877, p. 3). Beyond the machinery, some millers misreported the grain itself and recorded wheat as maize to pay its lower tax rate (Criscuolo, 1873, p. 135).

C. Serving the State

Paying taxes was not the only obligation that the new state imposed on its citizens: serving in the military was another—and it fell on almost all men, regardless of wealth or standing.

[28] Citing statistics published in contemporary newspapers, Rosselli (1927, p. 101) reports that the January 1869 protests resulted in 257 deaths, 1,099 injured individuals, and 3,788 arrests across Italy. The opposition parliamentarian Giuseppe Ferrari condemned the government's response with the following words: "In a long time Italy has not confronted a more serious issue. For the first time in living memory, taxes are being collected at gunpoint" (Camera dei Deputati, 1869b, p. 8837).

As Piedmontese legislation was progressively extended across the peninsula, every able-bodied man became liable for military service upon reaching the prescribed age. By contrast, political participation remained highly restricted. Before universal male suffrage was introduced in 1912, voting was open only to literate men over the age of 25 who met minimum tax requirements, leaving between 2 and 7 percent of the population eligible to vote. Seats in municipal councils and enrollment in the *Guardia Nazionale*, a communal militia responsible for maintaining public order, were subject to similar restrictions.

Military service came to occupy a central place in the state's conception of citizenship, with the figure of the peasant-soldier elevated to one of the founding myths of the Italian nation. Italy, it was said, "can and must, without excessive burden, have two hundred thousand combatants ready to sacrifice themselves to repel any enemy threatening the nation" (Bollati, 1984, p. 57). Similarly, Rigotti Colin (1985, p. 330) argues that "the militia must be, and truly is, the foremost concern of the nation" and that soldiers should become "the priests of our civil religion." Conscription also marked the first direct and sustained interaction between the institutions of the new state and citizens drawn from populations that had long lived under separate and often rival polities.

Compulsory military service was mandated by the Law of 13 July 1862 (no. 696) for all men upon reaching the age of 21.[29] Each year, a cohort was entered on the draft lists (*liste di leva*). We refer to men registered on these lists as 'conscripts', whether or not they subsequently served. Registration could be done in person, by a third party such as a parent, or ex officio. At registration, men eligible for exemption from the draft had to declare the grounds, such as being the sole male in the family or having at least one brother permanently serving in the army. Following registration, all men underwent a medical examination; the annual contingent was drawn by lottery from those suitable for military service.

Failure to appear for the medical examination classified a conscript as a draft evader (*renitente*). Draft evaders were pursued by the police. Avoiding military service meant months, and sometimes years, in hiding before arrest or voluntary surrender. Contemporary accounts describe police surveillance of relatives and, where evasion persisted, guards stationed outside family homes to seize fugitives (Uguccioni, 1982). Upon capture, draft evaders faced imprisonment, with the severity of the sentence increasing with the duration of evasion,[30] and still owed their full term of compulsory military service. Thus, evading

[29] Prior to 1866, cohorts born in a particular year were conscripted at the age of 20. Cohorts born in 1846 faced conscription in two phases: in 1866 for those already residing in the Kingdom of Italy, and in 1867 for those in Venetia and Mantua. From 1868 onward, conscription occurred at the age of 21.

[30] Arrested evaders faced one to two years of imprisonment. Those who surrendered within one year of being declared evaders faced two to six months; those who surrendered later, six months to one year.

the draft qualifies as a particularly costly act of non-compliance with the new state.

III. DATA: MEASURING COMPLIANCE

The state's effort to enforce and monitor the milling tax generated rich archival documentation: annual ministerial reports chronicling the implementation of the policy from the procurement of counters to the collection of revenues.[31] We draw on these reports to measure the diffusion of the tax technology and compliance with the levy. We complement them with further historical sources to trace compliance with other taxes and with state demands beyond the tax system.

Technology Supply and Proximity. The first ministerial report (Camera dei Deputati, 1870b) portrays the geography of the tax technology, described in Section II.B and mapped in Figure 3. For each counters' manufacturing plant: its location, the model it produced, the volume of counters commissioned and the price paid by the central government, and the year of commission. For each counters' distribution depot: its location, the model(s) it stocked and supplied, and the first year of operation. We calculate the travel time between the centroid of each geographical unit (province or district, depending on the level of the analysis) and depots using the Human Mobility Index (HMI) of Özak (2010, 2018).[32]

At the onset of the policy, counters were verified and distributed by two depots, Turin and Florence, with no provincial assignment: we measure 1870 exposure as proximity to the nearest of the two. With the southward expansion of counter production, and the establishment in April 1870 of a third distribution depot in Naples, each depot served an exclusive set of provinces (Section II.B): we measure post-1870 exposure as proximity to the assigned depot. Formally,

$$\text{Proximity}_{i,t} = \begin{cases} -\min\left\{\text{HMI}_{i\to\text{Turin}},\ \text{HMI}_{i\to\text{Florence}}\right\} & \text{if } t = 1870 \\ -\text{HMI}_{i\to\text{Depot}(i)} & \text{if } t > 1870 \end{cases}$$

where $\text{HMI}_{i\to d}$ is walking time from geographical unit i to depot d, and $\text{Depot}(i) \in$

Individuals found medically unfit for service were nonetheless required to serve at least one month in prison. Appendix D.3 combines this sentencing schedule with the probability of arrest or surrender to price state spending per draft evader.

[31] The Minister of Finance (or his under-secretary) presented the reports to the Chamber of Deputies, and the Ministry of Finance published them in most years between 1870 and 1883. We collected the complete series at the Library of the Chamber of Deputies.

[32] The HMI estimates the minimum time required to travel between two locations relying only on pre-industrial transport technology and accounting for terrain and human biological constraints. We employ the version of the index that incorporates seafaring, so that travel times are defined also for the islands (Sardinia and Sicily). Unit centroids are calculated from the 1871 administrative boundaries of the *Istituto Nazionale di Statistica* (ISTAT).

{Turin, Florence, Naples} is the depot assigned to unit i's province under the March 1870 royal decree.

Two features make the HMI well suited to our setting. First, when counters were introduced, Italy's railway network was still sparse and fragmented, rendering geodesic distance an imperfect proxy for overland transport costs across the peninsula's uneven terrain. The HMI instead weights distance by the difficulty of moving goods and people over it. Second, the index is pre-determined with respect to the tax technology: built exclusively on pre-industrial transport and geography, it is unaffected by contemporaneous infrastructure investments that may correlate with state targeting and enforcement capacity.

Technology Diffusion and Tax Compliance. The ministerial reports on the milling tax track several other dimensions of the policy implementation: the diffusion of the tax technology (counters, by mechanical model, stage of deployment, and condition); enforcement inputs (staff and expenditures on technical and administrative services); inspections (by cause, from ordinary verifications to detected violations); taxed quantities and tax revenues (by cereal); imported and exported grain quantities (by cereal and product) with the corresponding duties. Data are reported in standardized printed tables at the province-year level: 69 provinces, up to 12 years depending on the outcome.[33]

Milling Activity. The first ministerial report (Camera dei Deputati, 1870b) also includes province-level counts of open and closed mills and millstones (by mode of tax assessment) as of the end of February 1870, but no comparable data appear in the subsequent reports. To overcome this limitation, we collected and digitized additional data from two sources. For 1866, we draw on Sella's milling-tax bill (Camera dei Deputati, 1866). For 1882, we use the industrial survey of grain milling by the Ministero di Agricoltura, Industria e Commercio (1889). As the survey is at the district level, we aggregate it to the province level. Together, these yield a three-year panel of mills and millstones.[34]

Income Tax. To assess whether the tax technology affected broader fiscal capacity, we digitized data on the income tax (*imposta sulla ricchezza mobile*). Introduced in 1864 (Section II.A), the tax grew into one of the largest sources of state revenues, roughly 15 percent of the total throughout our sample period (Ragioneria Generale dello Stato,

[33] Appendix Figure A1 reproduces one of the original tables. The province of Rome is absent from the 1870 report, when it was still under papal rule.

[34] In the analysis, millstones in 1870 serve as the denominator for the share of millstones equipped with a counter rather than millstones in 1866, because the latter are observed for only 59 out of 69 provinces: Mantua and Venetia (annexed in 1866) and Rome (annexed in 1870) are missing. The 1870 counts cover all provinces except Rome. In Appendix Table B9, we show that millstones in 1870 are likely not endogenous to proximity to distribution depots.

1909). It remained in force, with several modifications to its rates and schedule, until 1973. Unlike the milling tax, whose base was recorded mechanically, by counting millstone rotations, the income tax relied on taxpayers' self-declarations, with no technology available to verify earned income directly (with the exception of taxes withheld at source on public salaries and pensions).[35] The data are drawn from the annual reports of the Ministry of Finance (*Relazione della Direzione Generale delle Imposte Dirette e del Catasto*), which record taxable income and tax revenues at the province-year level.[36] The resulting dataset covers all Italian provinces for the period 1864–1893: 59 provinces through 1865, 68 once Venetia and Mantua are annexed in 1866, 69 once Rome is annexed in 1871.

Civic Compliance. As discussed in Section II.C, military service was one of the few civic obligations that applied almost universally in post-unification Italy. We therefore use draft evasion as our measure of civic compliance with the new state. Data are collected and digitized from the annual statistics published by the Ministry of War (*Della leva di terra sui giovani nati dell'anno yyyy*), commonly known as the "Torre reports" after General Federico Torre. For each district and birth cohort, the reports record both the number of draft evaders (*renitenti*) and the number of conscripts (*iscritti di leva*). Our main outcome is the share of conscripts who evaded the draft. The data cover the cohorts born between 1843 and 1869 and called to military service between 1863 and 1890.[37]

Public Investments: Railways and Roads. As discussed in Section II.A, the historical period we study was one of state building and administrative centralization. To measure public investments during this period, we focus on infrastructure, among the largest budget items of the first Italian governments (Footnote 14). For railways, we use the network reconstructed by Ciccarelli and Groote (2017, 2018) and overlay it on historical district boundaries to construct a yearly panel of railway length (by line type and gauge) for 1860–1890. For roads, we use the network by Martinez (2024), available for 1863, 1904, and 1910, and construct an analogous panel of road length (by jurisdiction) for those three years. We normalize railway and road length (in kilometers) by the land area of the district (in 100 square kilometers). While roads were mostly locally provided, with provincial authorities responsible for 93 percent of the road network in 1863, railways were planned and financed primarily by the central government (Bonfatti et al., 2021). They capture one

[35] The provincial tables in the annual reports cover income assessed through taxpayers' declarations rather than withholding at source. The revenue figures follow the same definition.

[36] Data for the years 1864–1870 were published jointly in Camera dei Deputati (1871); subsequent reports were issued annually by the Ministry of Finance.

[37] As discussed in Footnote 29, the cohort born in 1846 was conscripted in two separate waves: in 1866 for territories already belonging to the Kingdom of Italy, in 1867 for Venetia and Mantua, which were annexed in 1866. For consistency, we merge these two observations into a single cohort-year covering all provinces.

of the most deliberate and visible investments of the new state.

Controls. Our empirical analysis conditions on a wide set of pre-policy characteristics that could affect both technology diffusion and compliance with state demands. To control for general market access and proximity to institutional hubs, we calculate the travel time (HMI) from the centroid of each geographical unit to its *compartimento* capital (the principal city of its statistical division under the 1871 census) and to Rome (the national capital from 1871 onward). A series of agro-climatic variables capture natural conditions and agronomic potential for cereal cultivation: the historical variability of temperature and precipitation during the growing season between 1500 and 1800 (Luterbacher et al., 2004 and Pauling et al., 2006, respectively),[38] the potential yields for wheat and maize—the two cereals targeted by the milling tax—from the FAO Global Agro-Ecological Zones (GAEZ), and the Caloric Suitability Index (Galor and Özak, 2015, 2016). We proxy for local economic development with population from the 1871 census and with 1871 provincial value-added estimates by sector (agriculture, industry, and services) from Chiaiese (2024).

IV. EMPIRICAL EVIDENCE: THE COMPLIANCE TRADE-OFF

Our empirical analysis proceeds in three steps. First, we study the determinants of technology diffusion. Second, we examine how the technology affected tax revenues (our "second stage" on tax compliance). Third, we investigate whether it also reshaped compliance with non-fiscal obligations (our "second stage" on civic compliance).

A. Technology Diffusion

Because we aim to understand how tax technology—in the form of mechanical counters recording millstone rotations—affects compliance with the state, we need a plausible source of exogenous variation in the diffusion of counters. In this subsection, we show that proximity to counters' distribution depots provides one such source: proximity determines where counters spread, and a battery of tests returns no evidence that this relationship is confounded by other local factors or explained by broader state activity.

We start by estimating the following regression,

$$\text{Technology}_{p,t} = \alpha_t + \beta \cdot \text{Proximity}_{p,t} + \mathbf{X}_p' \gamma + \varepsilon_{p,t} \tag{1}$$

where Technology is the number of counters *installed* in province p in year t, divided by the number of millstones in that province in 1870 (Footnote 34). Proximity is the negative of

[38] The original data are gridded in 0.5° cells, roughly 55 by 40 km at Italian latitudes. For geographical units spanning multiple cells, we calculate a weighted average based on the land area each cell contributes to the unit.

travel time, expressed in walking days, between the province's centroid and the distribution depot serving it in that year (Section III). A one-unit increase in Proximity means that the province lies one walking-day closer to the distribution depot. α_t are year fixed effects; $\mathbf{X}$ collects a set of time-invariant controls and fixed effects, which we introduce incrementally across specifications and discuss below. We cluster standard errors at the province level to account for serial correlation within provinces (Bertrand et al., 2004).

Table 1 reports estimates of the effect of proximity to distribution depots on technology diffusion. Provinces that are one walking-day closer to their depot had, on average, 4 to 8 pp more millstones equipped with counters (6 to 11% of the sample mean). All estimates are statistically significant at the 5 percent level and their magnitude stable across specification. To aid interpretation, Appendix Table B1 weights the regressions by the number of millstones in 1870: millstones one day closer to their depot were, on average, 14–17 pp more likely to carry a counter (29–34%). The technology spread more in provinces holding more of the kingdom's milling capacity.

Table 1. Proximity to distribution drove the diffusion of the tax technology ("first stage")

	(1)	(2)	(3)	(4)	(5)	(6)
Share of millstones equipped with a counter						
Proximity to distribution depot	0.061**	0.044**	0.046**	0.058**	0.080***	0.077***
	(0.026)	(0.021)	(0.022)	(0.023)	(0.021)	(0.017)
	[0.019]	[0.043]	[0.035]	[0.013]	[0.000]	[0.000]
Mean of dependent variable	0.757					
Number of observations	476					
Number of provinces	68					
R-squared	0.048	0.506	0.510	0.612	0.685	0.899
Year fixed effects		✓	✓	✓	✓	✓
Proximity to other institutional hubs			✓	✓	✓	(✓)
Agro-climatic and economic controls				✓	✓	(✓)
Pre-unification-state fixed effects					✓	(✓)
Province fixed effects						✓

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: province × year. All regressions are estimated by least squares of Equation 1 with linear controls and fixed effects as indicated in the last five rows of the table. (✓) marks controls and fixed effects subsumed by finer fixed effects in that column. Standard errors clustered at the province level in parentheses; p-values in brackets. The dependent variable is the province–year number of counters, divided by the province's number of millstones in 1870. The explanatory variable is defined in Section III and measured in days of travel time. 'Proximity to other institutional hubs' includes negative travel time to the province's *compartimento* (statistical-division) capital under the 1871 census and negative travel time to Rome. 'Agro-climatic and economic controls' include the standard deviation of historical temperature and precipitation (1500–1800), FAO-GAEZ attainable yields for wheat and maize, the post-1500 Caloric Suitability Index, log population in 1871, and the agriculture and industry shares of GDP in 1871.

Our identifying assumption is that, conditional on the controls and fixed effects discussed below, proximity to the distribution depot serving a province is unrelated to other determinants of tax and civic compliance. What this requires is that the logistics of distributing counters from a depot, rather than local characteristics or differential state intervention, govern where the technology spread. In Table 1, we assess this assumption by progressively accounting for the main factors that could confound the relationship between proximity to a depot and counter diffusion.

Year fixed effects in Column 2 absorb time-specific shocks that are common to all provinces, such as the national volume of counters manufactured, leaving the relationship largely unaffected.

A concern is that depots were located in large and administratively important cities, so proximity to one may capture broader market access or exposure to state institutions rather than access to the technology itself. Column 3 therefore controls for proximity to other institutional hubs: the capital of each province's statistical division and Rome, the national capital from 1871 onward. The estimate remains virtually unchanged.

Another concern is that provinces near a depot differed in their agricultural and economic conditions, and so in the scope for grain production and milling. Column 4 therefore adds measures of soil suitability for wheat and maize, historical temperature and precipitation, caloric suitability, population, and sectoral GDP shares. These proxy for local differences in milling potential, cereal demand, and economic development. Reassuringly, accounting for these measures leaves the estimate unchanged.

More broadly, proximity to depots could be correlated with persistent differences in infrastructure, administrative capacity, and the fiscal organization of milling. Column 5 addresses this concern by including pre-unification-state fixed effects. Identification comes from comparing provinces within the same former polity. The estimated relationship remains stable: proximity predicts diffusion within Savoyard Sardinia–Piedmont, Habsburg Lombardy–Venetia, and the Bourbon South alike.

Column 6 introduces province fixed effects, absorbing all time-invariant differences across provinces. This is the most demanding specification: identification comes exclusively from changes in proximity generated by the 1870 reorganization of the distribution network. The relationship remains remarkably stable even in this specification.

The effect of proximity on diffusion appears along two dimensions: the expansion in technology access over time and the cross-sectional differences that outlasted it. One year after the introduction of the policy, the share of millstones equipped with a counter averaged 21%, increasing steeply with proximity to the nearest initial depot (Appendix Figure B1a). By 1873, with the distribution network at full capacity, access to technology

improved across the South, and the diffusion of counters approached its ceiling in much of the kingdom. Nonetheless, proximity still mattered: the provinces farthest from their depot remained 15 pp below the nearest in terms of share of equipped millstones (Appendix Figure B1b).

Table 2 shows in a regression framework that diffusion tracked the distribution network precisely as it expanded. The tax technology reached provinces near the initial depots first. Only after April 1870, as the distribution network was extended southward, did proximity to the late depots positively predict counter diffusion. The initial network thus defined exposure for little more than a year, the assigned depots for all that followed. It is this persistent cross-sectional variation that the analysis in the remainder of the paper exploits.

Table 2. Technology diffusion followed the expansion of the distribution network

	(1)	(2)	(3)
Share of millstones equipped with a counter			
Post-1870	0.476***	0.759***	0.599***
	(0.068)	(0.069)	(0.089)
	[0.000]	[0.000]	[0.000]
Proximity to initial distribution depot	0.129***		0.129***
	(0.023)		(0.021)
	[0.000]		[0.000]
Proximity to initial distribution depots × Post-1870	-0.077***		-0.081***
	(0.024)		(0.022)
	[0.002]		[0.000]
Proximity to post-1870 distribution depots		-0.025	-0.035
		(0.029)	(0.028)
		[0.397]	[0.219]
Proximity to post-1870 distribution depot × Post-1870		0.069**	0.073**
		(0.031)	(0.030)
		[0.032]	[0.018]
Number of observations	476	476	476
Number of provinces	68	68	68
R-squared	0.623	0.596	0.637

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: province × year. All regressions are estimated by least squares with linear controls. Standard errors clustered at the province level in parentheses; *p*-values in brackets. The dependent variable is the province–year number of counters, divided by the province's number of millstones in 1870. The explanatory variable is defined in Section III and measured in days of travel time. Controls are: proximity to other institutional hubs, i.e., the negative travel time to the province's *compartimento* (statistical-division) capital under the 1871 census and to Rome; agro-climatic and economic controls, i.e., the standard deviation of historical temperature and precipitation (1500–1800), FAO-GAEZ attainable yields for wheat and maize, the post-1500 Caloric Suitability Index, log population in 1871, and the agriculture and industry shares of GDP in 1871.

A Falsification Test: Proximity as Access to Technology Supply. We take advantage of a distinctive feature of our historical setting: different depots stocked and supplied different models of counters (Section II.B): Turin and Naples supplied Italian and sleeve models; Florence supplied French and wooden-pole models. This feature yields a sharper test of why proximity matters. Diffusion of a given model should increase with proximity to the depot that supplied that model, but not with proximity to other depots—unless proximity brings something beyond access to the tax technology. Access to markets and the reach of the state would each raise installations of *every* model, not only of the model a depot supplied. We therefore estimate

$$\text{(2)}\quad \text{Technology}_{p,j,t} = \alpha_j + \beta^{\text{SUPPLIED}} \cdot \text{Proximity}_{p,j,t} + \beta^{\text{PLACEBO}} \cdot \text{Proximity}_{p,-j,t} + \mathbf{X}'_p \gamma + \varepsilon_{p,j,t}$$

where $\text{Technology}_{p,j,t}$ is the number of counters of model j per millstone in province p and year t, $\text{Proximity}_{p,j,t}$ measures proximity to the nearest depot supplying model j, and $\text{Proximity}_{p,-j,t}$ measures proximity to the nearest depot supplying models other than j. Model fixed effects α_j absorb time- and province-invariant characteristics of the counters, such as the total volume manufactured and the ease of installation. $\mathbf{X}$ controls for province-level characteristics, additional fixed effects or trends depending on the regression specification.

We report the results in Table 3. β^{SUPPLIED} is positive and statistically significant, β^{PLACEBO} is small and tightly estimated around zero, and we strongly reject equality between the two coefficients ($\beta^{\text{SUPPLIED}} = \beta^{\text{PLACEBO}}$). Provinces that are one walking-day closer to a depot supplying a model j are 5–8 pp more likely to have a counter of that model installed; proximity to depots supplying other models $-j$ has no such effect. The estimates are similar when each group of models is examined separately (Appendix Table B2). Proximity matters by easing access to counters, not through generic proximity to the administrative centers where the depots stood.

Access Implies Technology Use. Does higher distance to depots induce lower technology use, or greater post-delivery frictions, such as installation lags and damage in transit? We directly test for these two alternatives using data from the ministerial reports, which decompose each province's stock of counters into three categories: installed (our outcome so far), delivered but pending installation, and damaged.[39]

[39] We note that pending counters made up a modest share of the stock (14%, on average), which fell steadily from 29% in the first year of the policy. Over the same time span, the share of installed counters rose from 66% to 81%, while the share of damaged counters remained stable near 4%. This suggests that the technology proved appropriate for use on existing millstones: despite a slow start, the administration managed to put

Table 3. Access to technology supply, not proximity as such, drove diffusion ("model-specific first stage")

	(1)	(2)	(3)	(4)	(5)	(6)
Share of millstones equipped with a counter of model j						
Proximity to distribution depot of model j	0.067***	0.052***	0.060***	0.065***	0.063***	0.082***
	(0.008)	(0.010)	(0.010)	(0.012)	(0.009)	(0.014)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Proximity to distribution depot of other model $-j$	-0.021*	-0.027**	-0.018	-0.014	-0.015	0.002
	(0.012)	(0.011)	(0.012)	(0.013)	(0.015)	(0.012)
	[0.084]	[0.020]	[0.154]	[0.284]	[0.296]	[0.855]
Coefficient equality p-value	0.000	0.000	0.000	0.000	0.000	0.000
Mean of dependent variable	0.189					
Number of observations	1,904					
Number of provinces	68					
R-squared	0.456	0.498	0.504	0.507	0.518	0.524
Model fixed effects	✓	(✓)	(✓)	(✓)	(✓)	(✓)
Model-by-year fixed effects		✓	✓	✓	✓	✓
Proximity to other institutional hubs			✓	✓	(✓)	(✓)
Agro-climatic and economic controls			✓	✓	(✓)	(✓)
Pre-unification-state fixed effects				✓	(✓)	(✓)
Province fixed effects					✓	(✓)
Province-by-year fixed effects						✓

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: province × model × year. All regressions are estimated by least squares of Equation 2 with linear controls and fixed effects as indicated in the last seven rows of the table. (✓) marks controls and fixed effects subsumed by finer fixed effects in that column. Standard errors clustered at the province level in parentheses; p-values in brackets. The dependent variable is the province–model–year number of counters, divided by the province's number of millstones in 1870. The first explanatory variable is the model-specific counterpart of the variable defined in Section III; the second explanatory variable is negative travel time to the other model's distribution depot: (i) Florence for Italian or sleeve models; (ii) the nearest of Turin and Naples for French or wooden-pole models. Both are measured in days of travel time. 'Proximity to other institutional hubs' includes negative travel time to the province's *compartimento* (statistical-division) capital under the 1871 census and negative travel time to Rome. 'Agro-climatic and economic controls' include the standard deviation of historical temperature and precipitation (1500–1800), FAO-GAEZ attainable yields for wheat and maize, the post-1500 Caloric Suitability Index, log population in 1871, and the agriculture and industry shares of GDP in 1871.

If proximity only accelerated the timing of installation, it should predict installed counters but not the total supplied, and pending counters should accumulate far from depots (i.e., $\beta < 0$ with pending counters per millstone as outcome in Equation 1). If transport damaged counters, damage should rise with distance (i.e., $\beta < 0$ with damaged counters per millstone as outcome). Appendix Table B3 rejects both alternative accounts. Proximity predicts the total stock of counters as strongly as the installed stock. The small positive coefficients on pending and damaged counters are consistent with the larger volume shipped to nearer provinces, rather than with post-delivery frictions accumulating over distance. Proximity governed how many counters were supplied to a province—not

most counters into operation across the kingdom.

how quickly they were installed or whether they survived transport.

The evidence above consistently points to logistical constraints as determinants of counter diffusion. Depot proximity therefore provides plausibly exogenous variation in exposure to the tax technology, which we leverage to study tax compliance.

B. Tax Compliance

In this subsection, we estimate equations of the form

$$Y_{p,t} = \alpha_t + \beta \cdot \text{Proximity}_p + \mathbf{X}_p' \gamma + \varepsilon_{p,t} \tag{3}$$

where Y is a fiscal outcome. Unlike in Equation 1, Proximity_p is time-invariant in this analysis. The reason is that data on fiscal outcomes are available only from 1872, i.e., after the March 1870 royal decree fixed each province's depot assignment for the remainder of the milling tax. Identification rests on cross-sectional variation in proximity to the assigned depot, conditional on year fixed effects and, as in Table 1, an increasingly extensive set of controls. By 1872 the distribution network had reached full capacity, so this variation reflects the cross-sectional gap in technology diffusion that remained once the network had expanded.

Table 4 reports estimates of the effect of proximity to distribution depots on tax compliance. Provinces that are one day closer to their depot collected, on average, 0.18 to 0.23 more lire of milling tax revenue per capita (7 to 9% of the sample mean). Again, all estimates are significant at the 5 percent level and stable across specifications.

The results survive the inclusion of pre-unification-state fixed effects (Column 5), which absorb persistent differences in administrative traditions, fiscal capacity, and institutions across former polities. They are also robust to dropping each pre-unification state, one at a time (Appendix Figure B2), and each province (Appendix Figure B3), one at a time, from the estimation sample. Neither a single former polity nor a single province is responsible for the effect of proximity on tax compliance, reinforcing the view that the effect is not explained by pre-existing territorial differences or by any one influential geographic unit.

The Fiscal Return to Legibility and the Millers' Response. The increase in milling tax revenues is much larger than the cost associated with the policy implementation. The state spent 0.005 to 0.009 lire per capita more on technical services for the verification of counters, and employed 5 to 8 percent more tax officials, in provinces one day closer to their depot (Appendix Table B4). That additional spending came to under 5 percent of the additional revenues, a net fiscal return of at least 20 to 1. This comparison is the starting point of the welfare analysis we conduct in Section V.

The milling tax was cost-effective because the technology made an otherwise difficult-

Table 4. Tax technology raised tax compliance

	(1)	(2)	(3)	(4)	(5)
Tax collected per capita					
Proximity to distribution depot	0.212***	0.212***	0.183**	0.224***	0.228**
	(0.067)	(0.067)	(0.073)	(0.074)	(0.110)
	[0.002]	[0.002]	[0.014]	[0.003]	[0.042]
Mean of dependent variable	2.442				
Number of observations	828				
Number of provinces	69				
R-squared	0.069	0.357	0.369	0.508	0.530
Year fixed effects		✓	✓	✓	✓
Proximity to other institutional hubs			✓	✓	✓
Agro-climatic and economic controls				✓	✓
Pre-unification-state fixed effects					✓

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: province × year. All regressions are estimated by least squares of Equation 3 with linear controls and fixed effects as indicated in the last four rows of the table. Standard errors clustered at the province level in parentheses; *p*-values in brackets. The dependent variable is province–year milling tax revenue collected (in lire), divided by the province's population in 1871. The explanatory variable is the one defined in Section III for $t > 1870$, since the dependent variable is observed only from 1872 onward, and is measured in days of travel time. 'Proximity to other institutional hubs' includes negative travel time to the province's *compartimento* (statistical-division) capital under the 1871 census and negative travel time to Rome. 'Agro-climatic and economic controls' include the standard deviation of historical temperature and precipitation (1500–1800), FAO-GAEZ attainable yields for wheat and maize, the post-1500 Caloric Suitability Index, log population in 1871, and the agriculture and industry shares of GDP in 1871.

to-observe tax base more legible to the state. By mechanically recording millstone rotations, counters reduced the scope for under-reporting milled quantities. We provide evidence consistent with this mechanism using data on crop-specific tax revenues. The effect of depot proximity is concentrated on tax revenues from wheat, which faced a substantially higher tax rate, and is absent for maize (Appendix Table B5). The technology raised revenues precisely where the returns to under-reporting taxable quantities were greatest.

Greater legibility also drew a behavioral response by millers. Tax officials in provinces one walking-day closer to their assigned depot reported 11–20 percent more instances of counter tampering. By contrast, other inspection outcomes – such as ordinary verifications, mechanical malfunctions, and reapplications of counters for technical reasons – did not vary with depot proximity (Appendix Table B6). Since inspection effort would raise every category, not tampering alone, our estimates point to deliberate manipulation by millers. We use this behavioral response in the welfare analysis to bound millers' private cost of complying with the tax.

Ruling Out Alternative Mechanisms: State Targeting and the Economy. We rule out three alternative interpretations of our findings. First, the placement of counters, and their effects on tax compliance, could be the result of strategic targeting by the state to provinces with greater tax potential (e.g., Sánchez de la Sierra, 2020; Mayshar et al., 2022). Appendix Table B7 displays no such pattern: neither wheat nor maize suitability predicts technology diffusion.

Second, the effects of proximity on tax revenues may just reflect stronger monitoring by the state in provinces with more counters (Mastrorocco and Teso, forthcoming), rather than more counters themselves. Appendix Table B8 does not support this interpretation: provinces closer to distribution depots were not subject to more in-person inspections.

Third, by raising the tax burden on milling output, the technology may have led to either a contraction in milling activity or an upgrade in milling efficiency through the exit of unproductive mills. We test for this mechanism by using two sets of proxies: counts of mills and millstones (observed in 3 years, one pre- and two post-policy, which lets us employ a DiD design); imported and exported quantities of grain-based products (i.e., flour, bread, biscuits, and pasta, observed in 6 post-policy years, so we estimate Equation 3). Appendix Tables B9 and B10 show no effect on any of these outcomes.[40,41] These results complement the crop-specific evidence discussed above: the technology raised revenues by making the existing tax base more legible, rather than by expanding the underlying economic activity.

No Crowd-Out Effect on Other Taxes. While the technology increased revenues from the milling tax, this need not imply an increase in the state's overall fiscal capacity (Carrillo et al., 2017). Taxpayers more exposed to the counters could have responded by increasing evasion of less legible taxes, offsetting part of the revenue gain. We test for such fiscal crowd-out using data on the income tax, which accounted for roughly 15 percent of state revenues during our sample period. As discussed in Section II.A, the income tax was introduced in 1864 and, except for public salaries and pensions, relied on self-declared income. Unlike the milling tax, no technology directly verified the underlying tax base.

Because the income tax predates the milling tax, we can examine both pre-policy trends and post-policy responses using the following event-study specification:

$$Y_{p,t} = \alpha_p + \alpha_t + \sum_{\tau \neq -1} \beta_\tau \cdot \text{Close to Depot}_p \cdot \mathbf{1}\{t = \tau\} + \varepsilon_{p,t} \tag{4}$$

[40] While our results suggest no effect on quantity, we cannot rule out that the milling tax deteriorated the quality of flour, an hypothesis that has been put forward in historiographic literature (see, e.g., Aliberti, 1970).

[41] For robustness, we also look at provincial value added, total and by sector, in an event-study design. We find no evidence that broader economic activity responded to the tax technology (Appendix Table B11).

where p is province, t is year, and τ indicates the number of years before or after the introduction of the milling tax in 1869 (i.e., $\tau = 0$). Close to Depot$_d$ is a binary variable equal to one if the proximity to the assigned distribution depot in 1870 is above the sample median and zero otherwise.[42] Standard errors are clustered at the province level.

Appendix Figure B4 reports the event-study estimates for taxable income (Panel A) and income-tax revenues (Panel B).[43] Neither outcome exhibits differential pre-policy trends: provinces near distribution depots were not on different trajectories before the introduction of the milling tax. The post-policy responses are also undistinguishable from zero. Panel A provides no evidence that greater exposure to the counters induced taxpayers to report less income, while also complementing our earlier evidence of no differential effects on local economic activity. Panel B tests fiscal crowd-out directly: income-tax revenues do not decline in provinces closer to depots. Greater legibility of milling output did not result in a shift in evasion toward a less legible tax.

The absence of fiscal crowd-out is consistent with the two taxes falling largely on different segments of the population. A levy on the main ingredient of everyday staples, the milling tax was highly regressive, with revenues per capita flat with respect to GDP per capita. Income-tax revenues, instead, were roughly proportional to GDP per capita, in line with direct taxation reaching a narrower and wealthier pool of taxpayers (Appendix Table B12).[44]

Taken together, our findings indicate that the tax technology expanded the state's overall fiscal capacity by making milling output more legible to the state and without crowding out compliance with direct taxes.

C. Civic Compliance

The tax technology was conceived, provided, and administered by the state. Any dissatisfaction it generated was naturally attributable to the state itself. At the same time, the segment of the population that bore the milling tax was mostly excluded from the franchise in post-unification Italy and so had little say in political decisions. We therefore turn to compliance with the main non-fiscal obligation that the same state imposed on every able-bodied man of draft age: military service.

We leverage the spatial variation in exposure to the tax technology and estimate the

[42] The sample is a balanced panel of provinces, excluding Mantua and the provinces of Venetia, annexed in 1866, and Rome, annexed in 1870.

[43] Coefficients for 1871 and 1872 are currently unavailable because data for those years are still being digitized.

[44] Direct taxes on income, land, and buildings constituted a major component of the fiscal system, accounting for roughly 33 to 45 percent of total state revenues over our sample period (Ragioneria Generale dello Stato, 1909).

following event study

$$Y_{d,t} = \alpha_d + \alpha_t + \sum_{\tau \neq -1} \beta_\tau \cdot \text{Close to Depot}_d \cdot \mathbf{1}\{t = \tau\} + \varepsilon_{d,t} \tag{5}$$

where d is district (*circondario*), t is year, and τ indicates the number of years before or after the introduction of the milling tax in 1869 (i.e., $\tau = 0$). Y is the share of men registered on the draft lists who evaded compulsory military service. Close to Depot$_d$ is a binary variable equal to one if the proximity to the assigned distribution depot in 1870 is above the sample median and zero otherwise.[45] We cluster standard errors at the district level. The estimation sample is a balanced panel of districts (excluding those in Mantua and Venetia, first conscripted in 1866, and of Rome, first conscripted in 1871).

Identification of β_τ, the effect of proximity to distribution depots on draft evasion, requires that evasion in districts above and below median proximity would have been on parallel trends absent the tax technology. This is weaker than what the analysis of tax compliance in Section IV.B required: tax compliance exists only under the tax, so we compared provinces cross-sectionally, conditional on observables; military conscription predates the tax, so we can include district fixed effects to absorb any time-invariant difference across districts (hence across provinces), and we can test for parallel trends in the pre-tax periods.

The history of the milling tax, together with our empirical exercises so far, supports this identifying assumption. First, both the introduction of the tax and its timing answered the budgetary needs of the new state, not local conditions or state targeting (Section IV.A). Tax policy in those years had no horizon beyond the next budget: the volatile deficit forecast of each budget session (*sessione di bilancio*) dictated the finance minister's proposals; at times, negotiations with parliament broke down, forcing governments to resign (Federico, 2010, pp. 200–201). The principle of a balanced budget had become a political imperative of the Historical Right; when the international financial crisis of April 1866 collapsed the value of Italy's bonds and the war against Austria strained public finances further, the state saw the milling tax as the last resort (Section II.B). Second, the spatial variation in proximity could hardly be anticipated: the locations of distribution depots and their provincial assignments date to March 1870, after tax collection had begun.

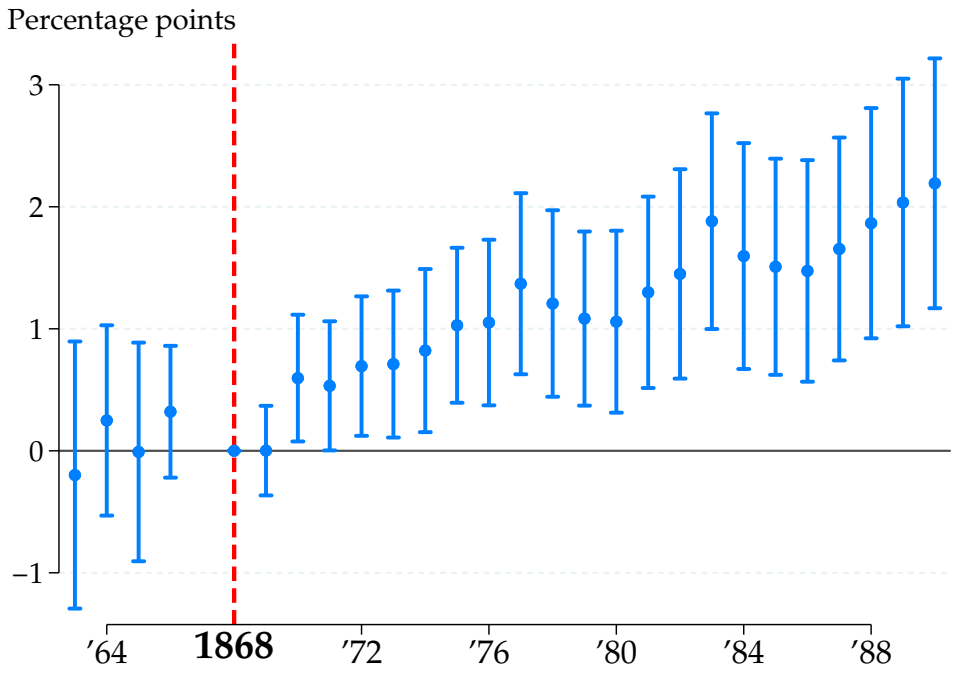
The pre-tax coefficients, before the vertical red line, in Figure 4 are consistent with

[45] We use proximity to the post-1870 distribution depots for two reasons. First, it allows us to exploit cross-sectional differences in exposure to the tax technology that persisted once the distribution network had reached its full capacity (Section IV.A). Proximity to the initial depots, by contrast, captures the early phase of diffusion (basically the first year of implementation), before the network expanded southward; consistent with this, Appendix Table B13 shows that proximity to the initial depots predicts lower civic compliance only during this early phase (1869–1870). Second, this choice is consistent with our analysis of tax compliance, allowing us to compare the same 'treatment' across fiscal and civic outcomes.

parallel trends and no anticipation.[46] The post-tax coefficients, after the red line, show the dynamic effects of proximity on civic compliance: districts with above-median proximity experienced a significant increase in draft evasion, an effect that arose in 1870 and grew steadily thereafter. It averaged 1.1 pp in the years when the counter network operated at its full capacity (42 percent of the counterfactual mean), and it did not fade after the repeal of the milling tax in 1884 (Appendix Table B14). This is consistent with the tax technology shifting citizens' beliefs about the state, beliefs that carried into obligations other than the tax and past the tax itself.

Because pre-unification states differed in their conscription traditions, one concern is that the results are driven by a single former polity. Yet the estimates survive excluding each pre-unification state, one at a time (Appendix Figure B5).

Figure 4. Tax technology undermined civic compliance



Notes: Event-study estimates with 90 percent confidence intervals from least-squares regressions of Equation 5. Unit of observation: district × year. Year and district fixed effects are included. Standard errors clustered at the district level. The dependent variable is the district–year share of men registered on the draft lists who evaded military service. The explanatory variable is an indicator for whether the district is above the sample median of proximity to its post-1870 assigned depot, interacted with year dummies. The vertical red line marks the omitted pre-event year relative to which coefficients are normalized.

[46] The null coefficient in 1869 provides additional evidence of no anticipation: although the milling tax was already in place, our treatment is defined by proximity to the post-1870 distribution depots, and districts subsequently more exposed to the technology show no differential change in civic compliance before this network was established.

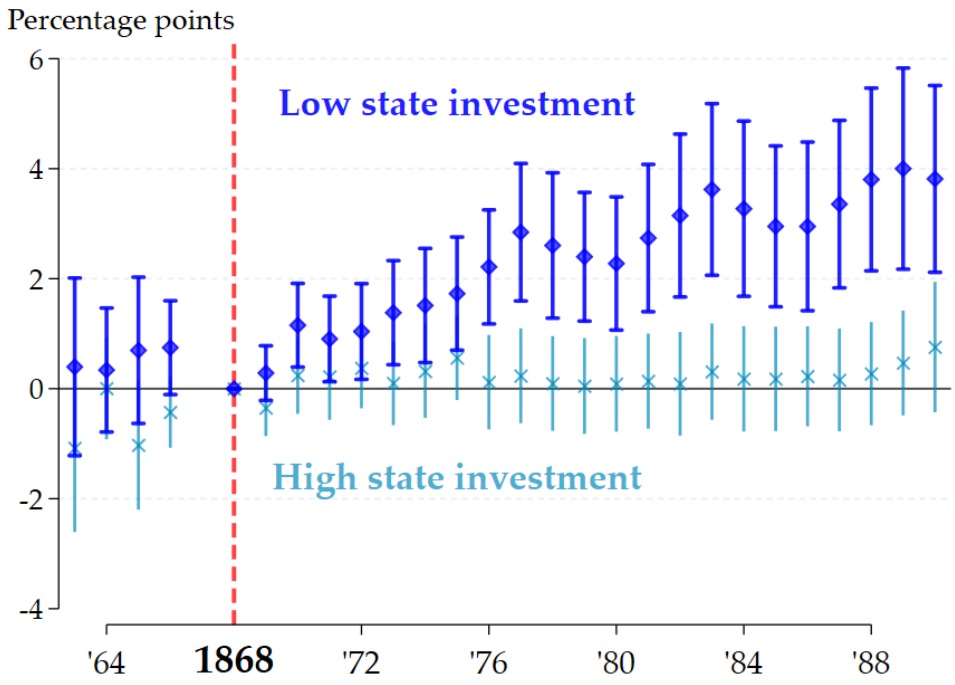
Reciprocity Governs the Response to the Tax Technology. Why did taxpayers respond to the tax technology by evading the draft? In the remainder of this subsection, we show that prior state investments shaped this response. We focus on railways, one of the largest and most salient of these state investments in the aftermath of unification. We split the sample by the median stock of railways in 1868, on the eve of the milling tax, and estimate event studies on draft evasion, as in Equation 5. The two sets of estimates are presented in Figure 5. In districts with railway density below the sample median, exposure to the tax technology increased draft evasion by 2 pp, on average (65 percent of the counterfactual mean); in districts above the median, instead, we detect no effect in any post-treatment year.[47] The increase in draft evasion in Figure 4 is thus driven by districts with little railway investment prior to the tax. This pattern is consistent with reciprocity: taxpayers accepted a heavier burden where the state had already delivered visible investments.

One concern with our interpretation is that railways may proxy for characteristics unrelated to the provision of public goods by the new Italian state. We address this concern with several exercises. First, the railway network could correlate with historical divides. Districts with little rail might simply be geographically remote or historically neglected by their pre-unification rulers. This is particularly relevant for draft evasion: districts with poorer transportation access may have offered greater opportunities to avoid detection or remain in hiding. If so, the heterogeneous effects we document would capture persistent disadvantages rather than citizens' response to what the new Italian state had provided. However, we find no heterogeneity when we split the sample by the median stock of railways in 1861, the year of unification (Appendix Figure B7). This stock largely reflects infrastructure inherited from the pre-unification states and therefore predates the investment choices of the new state.

Second, citizens may have responded to the availability of public infrastructure itself, irrespective of whether it was provided by the central government. We therefore repeat the analysis using road density in 1863, instead of railways. Roads provide a useful comparison because, unlike the state-planned and state-funded railway expansion (Bonfatti et al., 2021), responsibility for the ordinary road network was largely decentralized across provincial and municipal authorities (Buonanno et al., 2026). In 1863, provincial roads outnumbered state roads by more than ten to one; provinces financed between one-half and two-thirds of expenditures on provincial roads, with the central government covering the remainder. Municipalities were entirely responsible for investments in municipal roads (Martinez,

[47] In Appendix Figure B6, we compare the two sets of estimates formally. We interact the proximity terms in Equation 5 with a binary variable equal to one if a district is *below* the sample median of railway density and zero otherwise. Indistinguishable from zero before the tax, the estimated coefficients on this interaction term are positive and mostly statistically significant thereafter.

Figure 5. Tax technology undermined civic compliance where prior state investment was low



Notes: Event-study estimates with 90 percent confidence intervals from least-squares regressions of Equation 5. Unit of observation: district × year. Year and district fixed effects are included. Standard errors clustered at the district level. The dependent variable is the district–year share of men registered on the draft lists who evaded military service. The explanatory variable is an indicator for whether the district is above the sample median of proximity to post-1870 assigned depot, interacted with year dummies. Coefficients in dark and light blue are from separate regressions for districts below and above the sample median of railway density in 1868 (railway length in 1868 divided by land area), respectively. The vertical red line marks the omitted pre-event year relative to which coefficients are normalized.

2024). Road density therefore captures exposure to locally provided public infrastructure to a much greater extent than the railway network. We again find no heterogeneity in the effect of proximity to distribution depots (Appendix Figure B8).[48] Together, these results point to reciprocity toward the new state as underpinning the civic response of citizens.

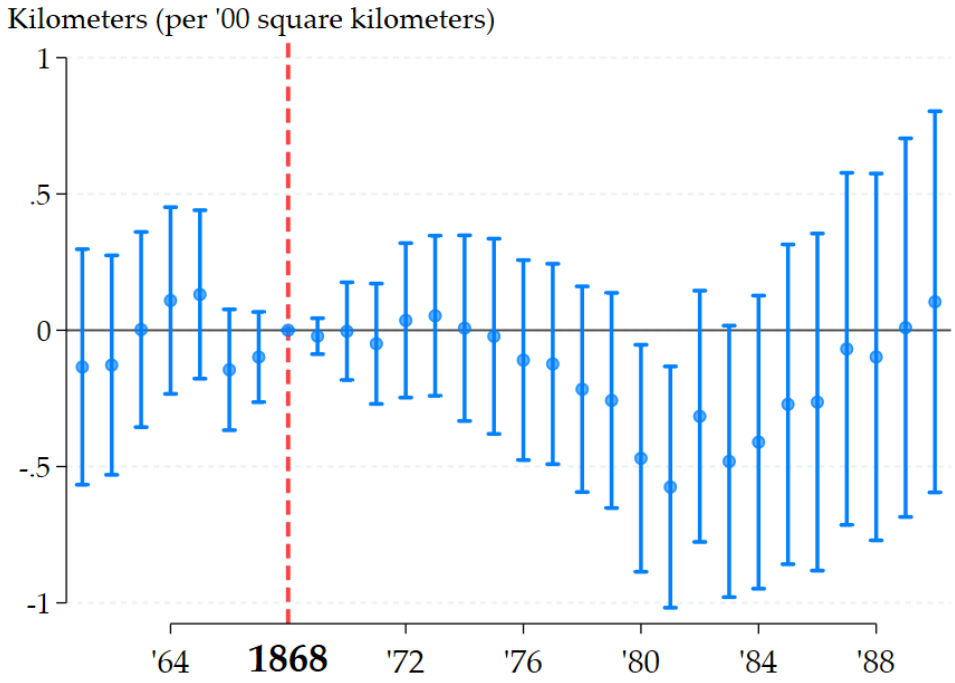
While taxpayers responded differently to the tax technology, the state could have earned their reciprocity by directing subsequent investments where the burden fell hardest. We find no evidence that it did, consistent with the revenues of the milling tax going mostly to close the budget deficit rather than to fund local investment. Figure 6 presents event-study estimates, as in Equation 5, with railway density as the outcome. Coefficients are indistinguishable from zero at almost every time period, both before and after the tax.

[48] Because road density is observed in 1863, before the milling tax and only two years after unification, this exercise also provides a complementary test against the interpretation that our railway measure merely captures inherited geographic remoteness.

The same pattern holds when splitting the sample by the median stock of railway density in 1868 used above (Appendix Figure B9), implying no reversion to the mean in railway expansion.

We draw two implications. First, railway expansion before the milling tax did not track the location of distribution depots, reassuring us that exposure to the technology did not follow prior state investment. Second, districts bearing a higher tax burden were not compensated with subsequent state investment. Civic compliance declined in districts that had received little before the milling tax and saw no compensation after it.

Figure 6. Tax technology brought no compensating state investment



Notes: Event-study estimates with 90 percent confidence intervals from least-squares regressions of Equation 5. Unit of observation: district × year. Year and district fixed effects are included. Standard errors clustered at the district level. The dependent variable is the district–year railway length (in kilometers) divided by the district's land area (in 100 square kilometers). The explanatory variable is an indicator for whether the district is above the sample median of proximity to post-1870 assigned depot, interacted with year dummies. The vertical red line marks the omitted pre-event year relative to which coefficients are normalized.

V. WELFARE ANALYSIS: TAX TECHNOLOGY UNDER NEGATIVE RECIPROCITY

The reduced-form evidence indicates that the tax technology raised tax compliance but weakened civic compliance. To quantify the overall welfare implications of these responses, we extend the model of optimal tax administration by Keen and Slemrod (2017). In

their canonical benchmark, the social planner weighs tax revenues against collection and compliance costs. In our extension, administrative interventions can also impose an externality on compliance with civic obligations beyond taxation, the strength of which is governed by citizens' reciprocity toward the state. The welfare impact of the tax technology therefore depends not only on its fiscal return but also on the civic response it provokes. We then derive sufficient statistics for welfare and express them as a marginal value of public funds (MVPF, Hendren and Sprung-Keyser, 2020). The reduced-form variation from Section IV identifies these statistics.

A. A Model of Tax and Civic Compliance

The Miller. Consider a locality with N^m millstones. Each millstone m grinds a fixed quantity of grain y per period.[49] The state does not observe y directly; when a counter is installed, it infers taxable output from the rotations recorded by the device. The miller operating the millstone owes the state t lire per unit of output legible to the state.[50]

Let $d_m \in \{0, 1\}$ indicate whether a counter is installed on millstone m. The miller does not choose d_m; he conceals $h_d \in [0, y]$ units of output, leaving a declared tax base $B_d = y - h_d$. For a millstone with no counter ($d = 0$), the state does not observe any output and concealment carries no cost, so $h_0 = y$ and $B_0 = 0$: the miller conceals all output and remits no tax.[51] For a millstone with a counter installed ($d = 1$), concealing output requires the miller to take a costly action, such as tampering, to make the device under-record. Let $\kappa(h)$ be this cost, increasing and convex: $\kappa(0) = 0, \kappa_h > 0, \kappa_{hh} > 0$.[52]

We assume that each unit of *concealed* milling output h raises the miller's payoff by t, the predetermined statutory tax per unit of *recorded* milling output. Appendix C.1 generalizes this benchmark along two margins: the share of the avoided remittance that enters the

[49] Fixing milling capacity and output isolates the effect of the tax technology that runs through legibility: the counter changes what the state can observe, not how much grain is milled. Consistent with this abstraction, in the reduced-form analysis, we detect no change in mills and millstones, in trade in grain-based products, or in value added.

[50] Writing the model at the millstone level implies either that each mill operates a single millstone or that milling output cannot be shifted costlessly within a mill between millstones with and without a counter. In 1870, there were 1.3 millstones per mill, so the first condition holds approximately. Second, selection into counter installation operated at the mill, not at the millstone, level. Counters were installed on site by tax officials, and the law prescribed a counter on every millstone, so each official visit equipped a mill in full. Mills escaped counters because remoteness or logistical frictions kept those officials away, not because millers chose which millstones to equip.

[51] The normalization is not essential. Without a counter, the miller could self-report under an alternative tax regime, leaving $B_0 > 0$. The expressions below would then depend on the incremental tax base $B_1 - B_0$ (Appendix C.1). We focus on the case $B_1 > B_0$, consistent with the tax technology increasing legibility and yielding positive revenue response (Section IV.B).

[52] We do not consider any additional hassle time cost from using the technology (Boning et al., 2025), as counters were provided at the state's expense and installed by tax officials, not requiring actions by millers.

miller's payoff, and the share of the resulting saving passed to customers through lower prices. Omitting milling revenues and costs that do not vary with concealment, the miller chooses

$$h_1 \in \arg\max_{0 \le h \le y} \{th - \kappa(h)\}$$

At an interior solution,

$$\kappa_h(h_1) = t$$

The State. Take the share of millstones equipped with a counter, $q \equiv \sum_{m=1}^{N^m} d_m / N^m \in [0, 1]$ ("counter reach" henceforth), as predetermined from the miller's perspective. The state collects milling-tax revenues

$$R(q) = N^m q t (y - h_1)$$

and the private cost of concealment is

$$K(q) = N^m q \kappa(h_1)$$

Hence a marginal increase in q raises revenue by $R_q = N^m t B_1 > 0$ but also carries concealment costs of $K_q = N^m \kappa(h_1) \ge 0$. This is the standard trade-off in the analysis of tax-administration interventions.

The Conscript. Considering the same locality, let N^c be the number of men registered on the annual draft lists ('conscripts' henceforth), and let I denote prior state investment. We take N^c and I as fixed with respect to q.

Each conscript c chooses whether to comply with the draft, with $e_c = 1$ denoting evasion. Complying meant appearing for the medical examination and, if drawn, serving. Let b_c be the conscript's *net material payoff* from evading: the burden he escapes, less the sanction he risks. b_c varies across conscripts according to a continuous cumulative distribution F; we leave F otherwise unrestricted and hold it fixed with respect to q.

Each conscript weighs the material benefit of evading the draft against the *civic disutility* of failing to fulfill a perceived obligation to the state. Following Besley (2020, 2021), we view this obligation as an expression of *intrinsic reciprocity*: conscripts are more willing to comply when they believe that the state reciprocates citizens' contributions.

Drawing on Besley and Dray (2024), we let the expected civic disutility from draft evasion depend on beliefs about the state, which are in turn shaped by the public fiscal signal ζ generated by the tax technology. Greater counter reach increases the probability that the locality receives this signal $\varphi(q)$, whose informativeness is held fixed. Conscripts combine the signal with their prior beliefs to form posterior beliefs π_ζ: technology changes beliefs about the state—and thereby draft evasion—through the adverse information it

conveys.[53]

Let $\psi(\pi_\zeta)$ denote the expected civic disutility of draft evasion given posterior beliefs π_ζ. A conscript evades when the net material benefit exceeds this disutility: $b_c \geq \psi(\pi_\zeta)$. We assume that material benefits have the same distribution F whether or not conscripts receive the signal. Taking counter reach $q \in [0,1]$ as predetermined from the conscript's perspective, the locality's expected draft-evasion rate is

$$D(q) \equiv \mathbf{E}[e_c \mid q] = \mathbf{E}_{\zeta|q}\left[\Pr\{b_c \geq \psi(\pi_\zeta) \mid q, \zeta\}\right] = \mathbf{E}_{\zeta|q}\left[1 - F\big(\psi(\pi_\zeta)\big)\right]$$

Hence a marginal increase in q weakly raises expected evasion by changing the probabilities of the possible information outcomes:

$$D_q = \sum_\zeta \left[1 - F\big(\psi(\zeta)\big)\right] \frac{\partial \Pr(\zeta \mid q)}{\partial q}$$

This civic response is the negative externality that we add to the analysis of tax administration. In Section V.B.4, we allow the strength of this response to vary with prior state investment: where investment had established confidence in the state's reciprocity, the same adverse signal could lower the perceived obligation to comply by less.

Welfare. To translate the fiscal and civic responses into a common metric of social welfare, we follow the MVPF approach. Let $S(q)$ denote the state spending on procuring, distributing, and monitoring the tax technology. For revenue-raising policies, the MVPF takes the conventional form

$$\frac{\text{WTP to avoid counters}}{\text{Net government revenue}} = \frac{R_q + K_q}{R_q - S_q}$$

The numerator is equal to the additional tax remitted to the state, plus the real resource cost of concealment.[54] The denominator is equal to the additional tax revenue, minus the associated state spending. The MVPF ratio therefore measures the welfare cost per lira the state collects; a lower MVPF represents a more efficient way of raising it. As a benchmark, a lump-sum tax that does not distort behavior has an MVPF of one: the state gains a lira for each lira of welfare it extracts from citizens.

Willingness to pay captures the *private* costs of tax compliance that taxpayers internalize, but it does not price the *social* costs of changes in civic compliance. Recent extensions of the MVPF framework admit externalities (Hahn et al., 2026) or internalities (Christian et al., 2026) that fall outside private benefits and government costs, by adding these social benefits to citizens' willingness to pay in the numerator. We add the uninternalized social costs

[53] Appendix C.2 illustrates how this information mechanism can arise from Bayesian updating.

[54] Under equal social weights on millers and customers, payments between them cancel, as does the division of the remittance savings from concealment.

of draft evasion $\nu N^c D_q$, where $\nu \geq 0$ is the planner's valuation, in private-consumption units, of preventing an additional draft evader. The planner treats the civic disutility of evasion as reflecting a perceived obligation rather than a preference. Reciprocity therefore enters welfare only through the draft evasion it induces, not as a separate consequence of resentment toward the state.[55]

Draft evasion also generates a fiscal externality through higher state spending on pursuit, prosecution, and imprisonment. Let $\mu > 0$ denote the expected increase in state spending generated by one additional draft evader. Because this spending reduces net government revenue, it enters the denominator of the MVPF, yielding

$$\text{MVPF} \equiv \frac{R_q + K_q + \nu\, N^c D_q}{R_q - S_q - \mu\, N^c D_q}$$

Sufficient Statistics. Define state spending, concealment costs, and draft evaders per additional lira of tax revenue as

$$\sigma_S \equiv \frac{S_q}{R_q} \qquad \sigma_K \equiv \frac{K_q}{R_q} \qquad \delta \equiv \frac{N^c D_q}{R_q}$$

We can therefore rewrite the MVPF as

$$\text{MVPF} \equiv \frac{\overbrace{1 + \sigma_K}^{\substack{\text{internalized} \\ \textit{private} \text{ cost}}} + \overbrace{\nu\, \delta}^{\substack{\text{uninternalized} \\ \textit{social} \text{ cost}}}}{\underbrace{1 - \sigma_S}_{\substack{\text{fiscal yield} \\ \text{of technology}}} - \underbrace{\mu\, \delta}_{\substack{\text{fiscal externality} \\ \text{from civic response}}}}$$

Our metric of social welfare nests the canonical model of tax administration. Absent a civic response, stronger administration (in our application, an expansion in counter reach) leaves draft evasion unchanged ($D_q = 0$), and the state trades off the net value of additional revenue against administrative and concealment costs, as in Keen and Slemrod (2017). Negative reciprocity introduces a wedge: at a given tax rate, stronger administration erodes compliance with a civic obligation outside the tax system. This wedge enters both sides of the MVPF: the fiscal externality from draft evasion decreases net government revenue in the denominator; the uninternalized social cost of draft evasion increases the numerator. The canonical model omits both consequences and therefore understates the welfare cost of the tax technology.

[55] Reciprocity could also have surfaced as lower compliance with other fiscal obligations. In Section IV.B, we look at the income tax, whose base was self-declared and not measured by any technology. Had the technology bred a broader resentment toward taxation, declared incomes should have fallen with proximity to counters' distribution depot: we find no evidence of such a response.

B. Quantifying the Welfare Cost of Tax Technology

We study a marginal increase in counter reach $\mathrm{d}q$, holding fixed prior state investment I, the number of millstones N^m, and the number of conscripts N^c. Throughout the empirical welfare analysis, a locality corresponds to a province, the finest level at which the fiscal outcomes are observed. Fiscal and civic outcomes are expressed per capita, counter reach is a millstone share, and tampering events are per millstone-year.

We proceed in four steps. In Section V.B.1, we discuss the identification of the sufficient statistics using the reduced-form variation. In Section V.B.2, we calibrate the planner's cost parameters using historical data. In Section V.B.3, we report our estimates and translate them into the MVPF, as a function of the planner's valuation of military service. In Section V.B.4, we examine how prior state investment shapes the MVPF.

1. Identification of Sufficient Statistics: Fiscal and Civic Responses

State Spending per Revenue Collected, $\sigma_S \equiv \frac{S_q}{R_q}$. State spending S on the tax technology comprises two sources of cost: the cost of procuring and installing counters (incurred once) and the cost of enforcing the tax thereafter, from maintaining the counters to inspecting mills and prosecuting violations. For procurement and first installation, which do not vary with proximity, we price the additional counters (per capita) required to go from zero to full counter reach, using archival documentation in Camera dei Deputati (1870b), and annualize them over the life of the tax (details in Appendix D.1).

For enforcement, we use the reduced-form effect of proximity to the assigned depot on administrative spending. Let β_q be the marginal effect of proximity on counter reach (Equation 1) and β_A be the coefficient of proximity on administrative spending A (Equation 3), the marginal effect is the Wald ratio, $A_q = \beta_A/\beta_q$. The numerator S_q adds to this effect the annualized cost of procuring and installing counters.

The denominator $R_q = \beta_R/\beta_q$ follows analogously from the reduced-form effect of proximity on tax revenues. Both marginal effects are identified under an exclusion restriction: conditional on year fixed effects and the controls of Section IV.A, proximity moved these outcomes only through counter reach.

Private Costs per Revenue Collected, $\sigma_K \equiv \frac{K_q}{R_q}$. Millers may incur private costs to conceal their milling output. Neither the decision to conceal nor its costs are observed. In Appendix D.2, we propose a strategy to bound them by revealed preference: a miller who tampered with his counter reveals that his concealment costs did not exceed the remittance he avoided, which caps those costs by the tax he would otherwise have remitted to the state. The upper bound of σ_K is $\frac{X_q}{1-X_q}$, where X_q measures recorded tampering

events per marginal counter-equipped millstone-year. As above, $X_q = \frac{\beta_X}{\beta_q}$ is a Wald ratio, recovered from the reduced-form effect of proximity on tampering (Equation 3), scaled by the first-stage effect of proximity on counter reach (Equation 1). The lower bound is zero, the case in which no marginal counter-equipped millstone concealed.

Draft Evaders per Revenue Collected, $\delta \equiv \frac{N^c D_q}{R_q}$. We recover the marginal effect of counter reach on draft evaders $E_q \equiv N^c D_q = \frac{\beta_E}{\beta_q}$ as the Wald ratio of the reduced-form effect of proximity on evaders and the first-stage effect of proximity on counter reach. The reduced form departs from the event-study design of Equation 5 in four respects. First, the dependent variable measures the number of evaders rather than the evasion rate. Second, the unit of observation is the province, at which we observe fiscal outcomes and into which we aggregate district counts. Third, consistent with the rest of the welfare analysis, exposure is the continuous measure of proximity between the province centroid and its assigned distribution depot. Fourth, a single post-tax coefficient replaces the event-study coefficients, since the welfare criterion requires an average marginal effect rather than year-by-year dynamics.

2. Historical Calibration of the Planner's Cost Parameters

State Spending per Draft Evader, μ. Draft evaders were pursued, prosecuted and, upon arrest or voluntary surrender, imprisoned. We price this state effort using historical sources on the cost of a prisoner and the expected days of custody implied by the probability of arrest or surrender and the sentencing schedule (details in Appendix D.3). We exclude state spending on prosecution, so our estimate is a lower bound.

Social Cost per Draft Evader, ν. The estimated civic response measures additional draft evaders per lira of revenue. Valuing its non-fiscal consequences requires the planner's valuation of preventing an additional evader, which our research design does not identify. To benchmark it, we turn to the historical conduct of the Italian state, which posted its own price for the release from military service (*affrancazione*). Under the law of 7 July 1866, a conscript could purchase release by paying the state a fee fixed by royal decree. Up to 1871, the fee stood at 3,200 lire.[56]

3. Estimates and the Marginal Value of Public Funds

Table 5 reports the estimates of our welfare analysis.

[56] The annual contingent was set by law and filled by lottery: men drawn within it formed the first category and served the full term, while the remaining fit conscripts formed the second category, liable but left at home. In July 1871, outright purchase was abolished; a conscript could purchase only the passage from the first category to the second at 2,500 lire.

On the fiscal margin, state spending absorbed 0.04 lire per additional lira of tax revenue. Concealment costs added at most 0.14 lire. Together, these estimates imply a conventional MVPF between 1.05 and 1.19: for every lira the state raised, it imposed between 1.05 and 1.19 lire of welfare cost on its citizens. From a fiscal standpoint, the technology was an efficient way to raise revenues.

On the civic margin, the technology induced 0.0002 additional draft evaders per additional lira of tax revenue. Each additional evader costs the state 166 lire in pursuit and prison spending. Accounting for this fiscal externality moves the welfare cost little: the MVPF rises to between 1.09 and 1.24.

Increased spending on evaders does not exhaust the social cost of draft evasion, however. Accounting for its non-fiscal consequences, the full MVPF increases linearly with the planner's valuation of preventing an additional draft evader ν: every hundred lire of valuation the planner attaches to preventing an evader adds 0.03 lire to the welfare cost of the marginal lira. At the benchmark valuation of 3,200 lire, the MVPF reaches 1.85 to 2.01.

A welfare cost approaching two lire per lira raised does not condemn the tax technology. First, a fiscal instrument is costly only relative to the set of alternative policies available to the planner for raising the same revenue. A lump-sum tax, whose welfare cost is one lira per lira, presumes a modern state that can see and tax its citizens. Second, on the eve of the milling tax, a balanced budget was widely treated as the condition of the new state's survival (Section II.A). Therefore, the planner likely valued a lira in the treasury more than a lira in private hands. For an emerging state such as nineteenth-century Italy, the imperative to build fiscal capacity and fund its basic functions may rationalize socially costly technologies.

Table 5. Technology and the Welfare Cost of Raising Revenue

Panel A. Reduced-Form Effects of Proximity		
β_q	Counter reach	0.1022
β_R	Tax revenue per capita	0.3217
β_A	State spending on enforcement per capita	0.0119
β_X	Tampering events per millstone	0.0125
β_D	Draft-evasion rate	0.0065
β_E	Draft evaders per capita	0.0001
Panel B. Marginal Effects of counter reach		
$R_q \equiv \beta_R/\beta_q$	Revenue collected	3.1489
$A_q \equiv \beta_A/\beta_q$	State spending on enforcement per capita	0.1161
$S_q \equiv \beta_A/\beta_q + p_q$	State spending	0.1407
$X_q \equiv \beta_X/\beta_q$	Tampering	0.1228
$D_q \equiv \beta_D/\beta_q$	Draft-evasion rate	0.0640
$E_q \equiv \beta_E/\beta_q$	Draft evaders	0.0007
Panel C. Sufficient Statistics		
$\sigma_S \equiv S_q/R_q$	State spending per revenue collected	0.0447
$\sigma_K \in [0,\ X_q/(1-X_q)]$	Private costs per revenue collected	[0, 0.1400]
$\delta \equiv E_q/R_q$	Draft evaders per revenue collected	0.0002
Panel D. Welfare		
MVPF($\mu = 0, \nu = 0$)	Conventional marginal value of public funds	[1.0468, 1.1933]
MVPF($\mu = 166, \nu = 0$)	Accounting for fiscal externality of draft evasion	[1.0884, 1.2408]
MVPF($\mu = 166, \nu = 3{,}200$)	Accounting for social cost of draft evasion	[1.8554, 2.0078]

Notes: Panel A reports the effects of proximity to distribution depots on the outcome in the second column. All regressions are estimated by least squares using the balanced sample of provinces (excluding Mantua, Rome, and Venetia): β_q follows Equation 1; β_R, β_A, and β_X follow Equation 3; β_D and β_E follow the province-level specification described in Section V.B.1. Counter reach is the number of counters divided by the province's number of millstones in 1870. Milling tax revenue collected, state spending (both in lire), and the number of draft evaders are divided by the province's population in 1871. Tampering events are measured per millstone. The draft-evasion rate is the share of conscripts (i.e., men registered on the draft lists) who evaded military service. Panel B scales these effects by the first-stage effect on counter reach in the first row of Panel A. 'State spending' S_q adds procurement and installation costs annualized over 1869–1884 to 'State spending on enforcement' A_q. Panel C reports sufficient statistics. Panel D reports welfare costs per lira of net revenue. Bracketed ranges reflect bounds on concealment costs, not confidence intervals.

4. Heterogeneity by Prior State Investment

Was the Italian state bound to trade off citizens' cooperation for fiscal capacity? That is, could it have raised the same revenue at a lower civic cost? The answer depends on state investment.

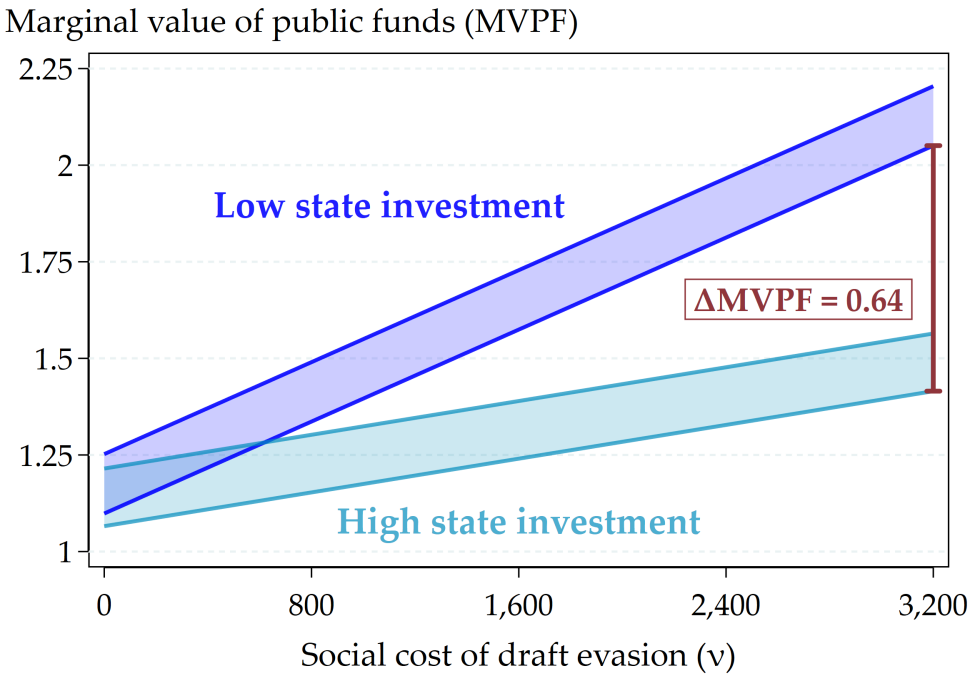
Motivated by the heterogeneous effects in Figure 5, we allow the civic response to the technology, δ, to vary with prior state investment, I. Past provision of public services or infrastructure shapes prior beliefs about the state by providing evidence that it reciprocates citizens' contributions. When past provision has established sufficient confidence in the state's reciprocity, the adverse information conveyed by the tax technology can induce a smaller decline in the civic disutility of draft evasion.[57]

Empirically, we proxy I by railway density in 1868 and split the sample at its median. We re-estimate the civic externality $\delta(I)$ separately in each subsample and evaluate welfare, holding the other sufficient statistics fixed. In provinces below the median, the technology induced 0.00027 additional draft evaders per additional lira of revenue (one additional evader per 3,700 lire); in provinces above it, 0.00010 evader per lira (one additional evader per 9,800 lire).

The welfare consequences of this heterogeneity are presented in Figure 7. We plot the MVPF in each subsample against the planner's valuation of preventing an additional draft evader. If the planner does not internalize the social cost of evasion ($\nu = 0$), the two subsamples differ only by 0.03, the fiscal externality alone. The gap between MVPFs widens linearly in ν. At the benchmark valuation of 3,200 lire per evader, the technology cost 2.05 lire per lira in locations with low state investment, 0.64 more than what it cost under high investment. Where state investment had come before the tax, the technology met a weaker civic response and cost society less.

[57] Appendix C.2 illustrates how prior beliefs can shape this response and gives sufficient conditions under which prior state investment attenuates the marginal effect of counter reach on draft evasion.

Figure 7. Tax technology imposed a higher welfare cost where prior state investment was low



VI. CONCLUSION

This paper explores a trade-off that emerging polities face in the effort to build state capacity: technologies strengthening compliance with one state demand (paying taxes) may inadvertently weaken voluntary compliance with others (serving in the military). In our setting, the trade-off is sharpest where the state extracted tax revenues before investing in public services.

We apply recent advances in welfare analysis to price this compliance trade-off in the presence of non-fiscal externalities. The newborn Italian state bought fiscal capacity partly with civic compliance: at its own valuation, nearly two lire of social welfare for each lira raised. A technology this socially costly may still have been the best instrument available to a young state that needed revenues more than civic compliance—and lacked the legibility and legitimacy of a modern state. This exercise illustrates what welfare analysis can recover from archival data.

We focus on one dimension of civic compliance, which is especially relevant in historical times. Serving in the military was, for most men, the principal obligation to the state beyond paying taxes. But the mechanism we identify is general: it works through beliefs about the state, of which compliance with military service is only one expression. Modern states

ask far more of their citizens than nineteenth-century Italy did, from voting to obeying public-health mandates and environmental rules. Where beliefs shape cooperation with the state across a wider range of domains, the civic cost of a coercive technology may be greater.

A new state needs a reliable and steady source of revenue to fund public services. Yet the instruments within its reach can erode the very cooperation those services would earn. Where extraction precedes investment, a state may fall into a low-capacity trap: it must coerce because compliance is low, and coercion lowers future compliance. Which policies can lift a state out of that trap, and at what price, remain open questions for future research.

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SUPPLEMENTAL APPENDIX

FOR "*THROUGH THE MILL*:
TAX TECHNOLOGY AND COMPLIANCE WITH A NEW STATE"

BY CATERINA ALFONZO AND MATTEO RUZZANTE

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A. DATA: SOURCES AND CONSTRUCTION

Figure A1. Example of Table from Ministerial Report on Milling Tax

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Allegato 5.

Quadro comparativo della tassa versata

Quadro per

Num. d'ordine	PROVINCIE	Popolazione	Tassa versata in tesoreria		Tassa media per abitante		Differenza fra il 1874 ed il 1873	
			1874	1873	1874	1873	in più	in meno
1	Alessandria	683,361	1,584,203 06	1,359,202 80	2 38	1 99	225,000 26	»
2	Ancona	262,349	751,133 65	743,599 72	2 86	2 83	7,533 93	»
3	Aquila	332,784	810,850 13	756,392 01	2 43	2 27	54,458 12	»
4	Arezzo	234,645	683,160 31	680,123 95	2 91	2 90	3,036 36	»
5	Ascoli	203,004	496,652 30	434,956 34	2 44	2 14	61,695 96	»
6	Avellino	375,691	998,233 24	819,856 87	2 66	2 18	178,376 37	»
7	Bari	604,540	1,567,557 49	1,745,701 68	2 59	2 89	»	178,144 19
8	Belluno	175,282	214,373 35	201,687 84	1 22	1 15	12,685 51	»
9	Benevento	232,008	765,685 79	701,167 13	3 30	3 02	64,518 66	»
10	Bergamo	368,152	1,222,534 55	1,041,975 14	3 32	2 83	180,559 41	»
11	Bologna	439,232	1,596,214 02	1,458,515 89	3 63	3 32	137,698 13	»
12	Brescia	456,023	1,255,366 39	1,121,061 35	2 75	2 46	134,305 04	»
13	Cagliari	393,208	329,955 26	268,074 29	0 84	0 68	61,880 97	»
14	Caltanissetta	230,066	766,479 59	771,444 36	3 33	3 35	»	4,964 77
15	Campobasso	364,208	997,523 54	908,641 35	2 74	2 49	88,882 19	»
16	Caserta	697,403	1,618,421 40	1,464,515 27	2 32	2 10	153,906 13	»
17	Catania	495,415	1,298,125 09	1,305,696 88	2 62	2 64	»	7,571 79
18	Catanzaro	412,226	831,336 53	824,478 61	2 01	2 00	6,857 92	»
19	Chieti	339,986	872,636 39	976,738 78	2 57	2 87	»	104,102 39
20	Como	477,642	781,088 26	724,132 80	1 63	1 52	56,955 46	»
21	Cosenza	440,468	814,324 26	844,995 15	1 85	1 92	»	30,670 89
22	Cremona	300,595	633,774 67	634,457 19	2 10	2 11	»	682 52
23	Cuneo	618,232	1,813,795 50	1,522,548 85	2 93	2 46	291,246 65	»
24	Ferrara	215,369	363,062 51	296,025 86	1 69	1 37	67,036 65	»
25	Firenze	766,824	1,962,715 90	1,962,022 84	2 56	2 56	693 06	»
26	Foggia	322 758	1,137,923 57	1,032,729 05	3 52	3 20	105,194 52	»
27	Forlì	234,090	653,295 02	633,707 43	2 79	2 71	19,587 59	»
28	Genova	716,759	2,275,157 67	2,137,803 82	3 17	2 98	137,353 85	»
29	Girgenti	289,018	1,052,728 34	1,009,423 18	3 64	3 49	43,305 16	»
30	Grosseto	107,457	317,045 24	316,224 04	2 95	2 94	821 20	»
31	Lecce	493,594	1,108,850 16	1,120,779 69	2 24	2 27	»	11,929 53
32	Livorno	118,851	182,038 97	175,341 74	1 53	1 48	6,697 23	»
33	Lucca	280,399	816,713 11	748,689 69	2 91	2 67	68,023 42	»
34	Macerata	236,994	737,777 79	688,350 05	3 11	2 90	49,427 74	»
35	Mantova	288,942	886,868 71	875,388 08	3 07	3 03	11,480 63	»
	Da riportarsi	13,207,575	34,197,601 76	32,306,449 72			2,229,218 12	338,066 08

B. EVIDENCE: ADDITIONAL RESULTS AND ROBUSTNESS

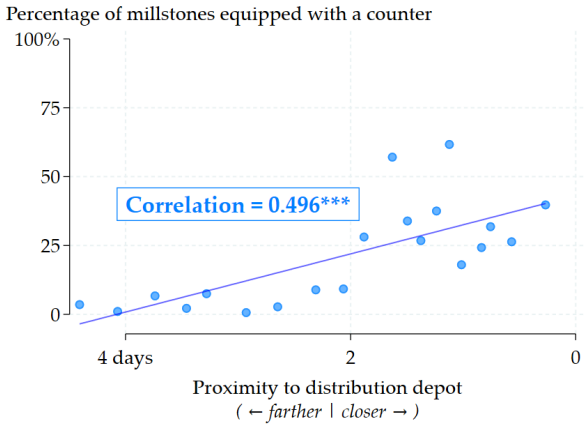
Table B1. First stage, weighted by millstones

	(1)	(2)	(3)	(4)	(5)	(6)
Share of millstones equipped with a counter						
Proximity to distribution depot	0.165**	0.162**	0.171**	0.129***	0.142***	0.061**
	(0.071)	(0.071)	(0.069)	(0.041)	(0.024)	(0.028)
	[0.022]	[0.027]	[0.016]	[0.002]	[0.000]	[0.031]
Weighted mean of dependent variable	0.490					
Number of observations	476					
Number of provinces	68					
R-squared	0.217	0.321	0.339	0.593	0.768	0.916
Year fixed effects		✓	✓	✓	✓	✓
Proximity to other institutional hubs			✓	✓	✓	(✓)
Agro-climatic and economic controls				✓	✓	(✓)
Pre-unification-state fixed effects					✓	(✓)
Province fixed effects						(✓)

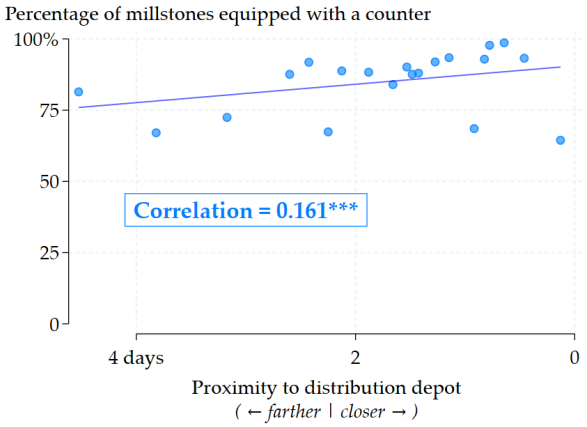
Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: province × year. All regressions are estimated by least squares of Equation 1 with linear controls and fixed effects as indicated in the last five rows of the table, and are weighted by the number of millstones in 1870. (✓) marks controls and fixed effects subsumed by finer fixed effects in that column. Standard errors clustered at the province level in parentheses; *p*-values in brackets. The dependent variable is the province–year number of counters, divided by the province's number of millstones in 1870. The explanatory variable is defined in Section III and measured in days of travel time. 'Proximity to other institutional hubs' includes negative travel time to the province's *compartimento* (statistical-division) capital under the 1871 census and negative travel time to Rome. 'Agro-climatic and economic controls' include the standard deviation of historical temperature and precipitation (1500–1800), FAO-GAEZ attainable yields for wheat and maize, the post-1500 Caloric Suitability Index, log population in 1871, and the agriculture and industry shares of GDP in 1871.

Figure B1. Expansion and persistent gaps in technology diffusion

(a) Early phase (1870)



(b) Full capacity (1873–)



Notes: The figure presents binscatter plots and linear fit lines of counter diffusion against proximity to distribution depots. The y-axis reports the province–year number of counters, divided by the province's number of millstones in 1870. The x-axis reports proximity to distribution depots, measured in days of travel time, as described in Section III. In the left panel, proximity is defined as the negative of the minimum travel time to Turin or Florence; in the right panel, it is defined as the negative travel time to the depot assigned to each province under the 1870 Decree.

Table B2. Model-specific first stage, by counter model

	Models installed			
	Italian models		French models	
	(1)	(2)	(3)	(4)
Proximity to distribution depot of Italian models	0.056**	0.067**		-0.028
	(0.026)	(0.033)		(0.021)
	[0.033]	[0.047]		[0.176]
Proximity to distribution depot of French models		0.015	0.055***	0.041**
		(0.023)	(0.016)	(0.018)
		[0.521]	[0.001]	[0.025]
Coefficient equality *p*-value		0.045		0.000
Number of observations	952	952	952	952
Number of provinces	68	68	68	68
R-squared	0.557	0.557	0.306	0.309

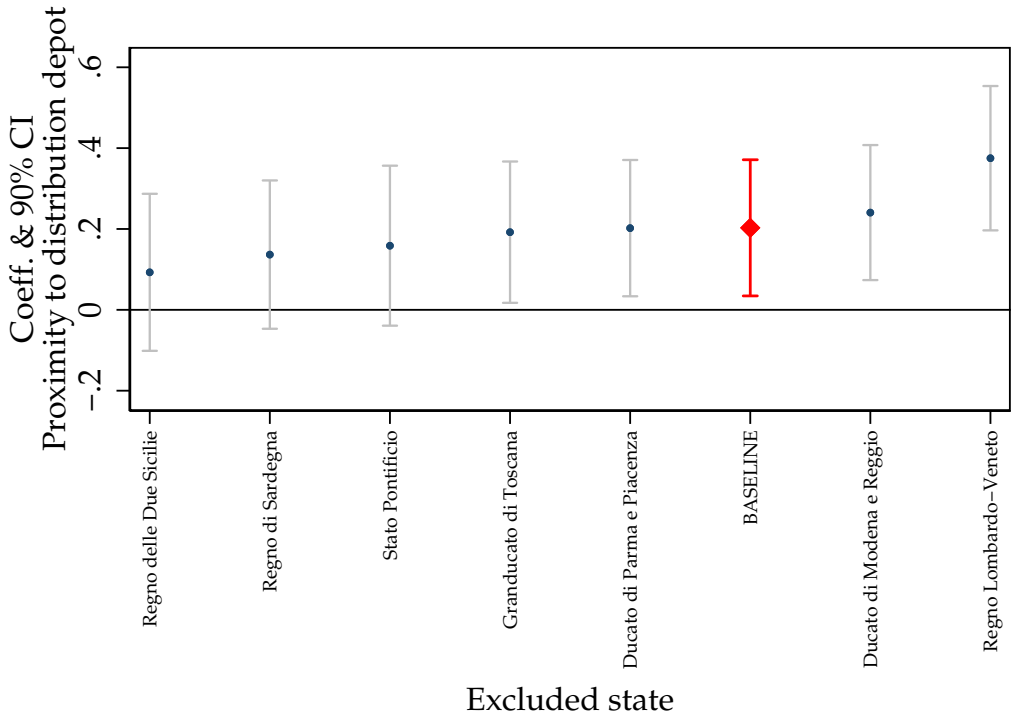
Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: province × model × year. For simplicity, 'Italian models' also include sleeve models, and 'French models' also include wooden-pole models, as these models share the same distribution depots. All regressions are estimated by least squares of Equation 2 with linear controls (as indicated below), model and year fixed effects, separately for Italian and French models. Standard errors clustered at the province level in parentheses; *p*-values in brackets. The dependent variable is the province–model–year number of counters, divided by the province's number of millstones in 1870. The explanatory variables are the model-specific counterpart of the variable defined in Section III, i.e., the negative travel time to: (i) Florence for Italian or sleeve models; (ii) the nearest of Turin and Naples for French models. Controls are: proximity to other institutional hubs, i.e., the negative travel time to the province's *compartimento* (statistical-division) capital under the 1871 census and to Rome; agro-climatic and economic controls, i.e., the standard deviation of historical temperature and precipitation (1500–1800), FAO-GAEZ attainable yields for wheat and maize, the post-1500 Caloric Suitability Index, log population in 1871, and the agriculture and industry shares of GDP in 1871.

Table B3. Higher technology diffusion is not driven by lower installation time or lower damage in transit

	(1)	(2)	(3)	(4)	(5)
Total					
Proximity to distribution depot	0.058**	0.041*	0.045**	0.055**	0.082***
	(0.026)	(0.021)	(0.021)	(0.022)	(0.021)
	[0.028]	[0.054]	[0.035]	[0.013]	[0.000]
Mean of dependent variable	0.816				
Number of observations	476				
Number of provinces	68				
R-squared	0.044	0.507	0.510	0.620	0.694
Not yet applied					
Proximity to distribution depot	0.026***	0.023***	0.014*	0.026***	0.018*
	(0.008)	(0.007)	(0.007)	(0.008)	(0.009)
	[0.001]	[0.003]	[0.056]	[0.002]	[0.052]
Mean of dependent variable	0.151				
Number of observations	476				
Number of provinces	68				
R-squared	0.046	0.148	0.208	0.282	0.341
Damaged					
Proximity to distribution depot	0.012***	0.011***	0.007***	0.010***	0.011***
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
	[0.000]	[0.000]	[0.005]	[0.001]	[0.002]
Mean of dependent variable	0.046				
Number of observations	476				
Number of provinces	68				
R-squared	0.035	0.086	0.137	0.202	0.216
Year fixed effects		✓	✓	✓	✓
Proximity to other institutional hubs			✓	✓	✓
Agro-climatic and economic controls				✓	✓
Pre-unification-state fixed effects					✓

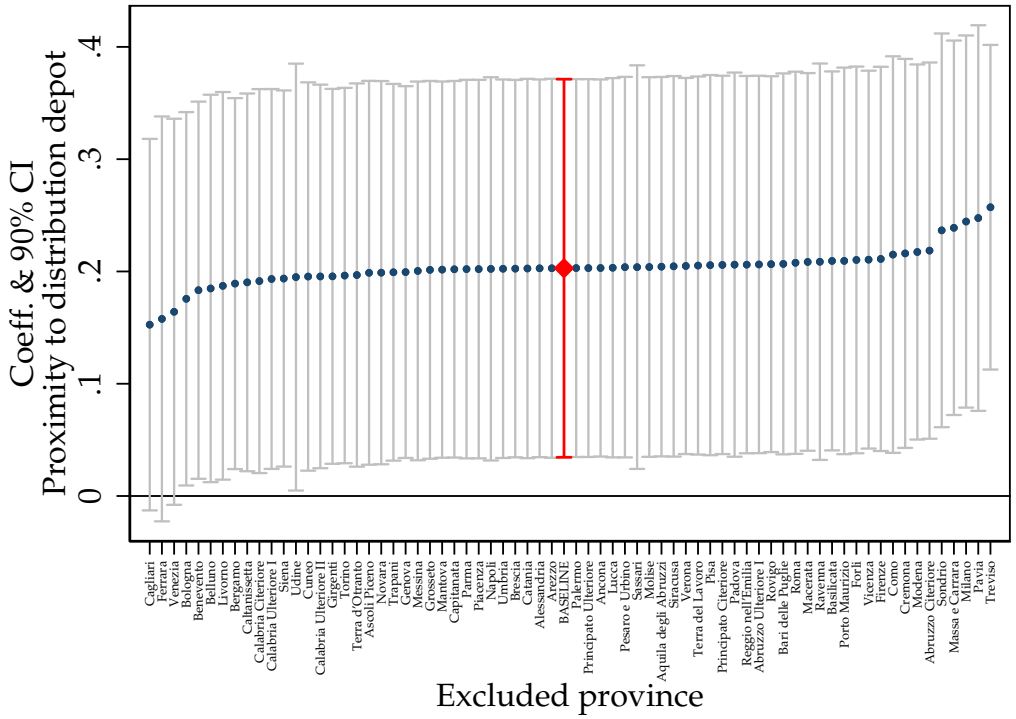
Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: province × year. All regressions are estimated by least squares of Equation 1 with linear controls and fixed effects as indicated in the last four rows of the table. Standard errors clustered at the province level in parentheses; *p*-values in brackets. The dependent variables are the province–year number of counters delivered, pending installation or damaged, divided by the province's number of millstones in 1870. The explanatory variable is defined in Section III and measured in days of travel time. 'Proximity to other institutional hubs' includes negative travel time to the province's *compartimento* (statistical-division) capital under the 1871 census and negative travel time to Rome. 'Agro-climatic and economic controls' include the standard deviation of historical temperature and precipitation (1500–1800), FAO-GAEZ attainable yields for wheat and maize, the post-1500 Caloric Suitability Index, log population in 1871, and the agriculture and industry shares of GDP in 1871.

Figure B2. No single pre-unification state is responsible for the effect on tax compliance ("leave-one-out")



Notes: The figure plots the proximity coefficients and 90 percent confidence intervals from least squares of Equation 3 with linear controls (as indicated below), year and pre-unification state fixed effects. Each coefficient is obtained by re-estimating the model after excluding the pre-unification state indicated on the x-axis. Unit of observation: province × year. Standard errors are clustered at the province level. The dependent variable is province–year milling tax revenue collected (in lire), divided by the province's population in 1871. The explanatory variable is the one defined in Section III for $t > 1870$, since the dependent variable is observed only from 1872 onward, and is measured in days of travel time. Controls are: proximity to other institutional hubs, i.e., the negative travel time to the province's *compartimento* (statistical-division) capital under the 1871 census and to Rome; agro-climatic and economic controls, i.e., the standard deviation of historical temperature and precipitation (1500–1800), FAO-GAEZ attainable yields for wheat and maize, the post-1500 Caloric Suitability Index, log population in 1871, and the agriculture and industry shares of GDP in 1871.

Figure B3. No single province is responsible for the effect on tax compliance ("leave-one-out")



Notes: The figure plots the proximity coefficients and 90 percent confidence intervals from least squares of Equation 3 with linear controls (as indicated below), year and pre-unification state fixed effects. Each coefficient is obtained by re-estimating the model after excluding the province indicated on the x-axis. Unit of observation: province × year. Standard errors are clustered at the province level. The dependent variable is province–year milling tax revenue collected (in lire), divided by the province's population in 1871. The explanatory variable is the one defined in Section III for $t > 1870$, since the dependent variable is observed only from 1872 onward, and is measured in days of travel time. Controls are: proximity to other institutional hubs, i.e., the negative travel time to the province's *compartimento* (statistical-division) capital under the 1871 census and to Rome; agro-climatic and economic controls, i.e., the standard deviation of historical temperature and precipitation (1500–1800), FAO-GAEZ attainable yields for wheat and maize, the post-1500 Caloric Suitability Index, log population in 1871, and the agriculture and industry shares of GDP in 1871.

Table B4. Expenditures

	(1)	(2)	(3)	(4)	(5)
Staff size per '000 capita					
Proximity to distribution depot	0.006**	0.005**	0.004*	0.005**	0.006***
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
	[0.016]	[0.039]	[0.075]	[0.017]	[0.007]
Mean of dependent variable	0.076				
Number of observations	687				
Number of provinces	69				
R-squared	0.043	0.384	0.423	0.578	0.644
Technical service expenditures per capita					
Proximity to distribution depot	0.007*	0.007*	0.005	0.007*	0.009*
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)
	[0.076]	[0.078]	[0.213]	[0.086]	[0.083]
Mean of dependent variable	0.141				
Number of observations	758				
Number of provinces	69				
R-squared	0.034	0.088	0.159	0.360	0.475
Ordinary administrative expenditures per capita					
Proximity to distribution depot	0.013***	0.013***	0.009**	0.008	0.000
	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)
	[0.004]	[0.004]	[0.032]	[0.197]	[0.986]
Mean of dependent variable	0.114				
Number of observations	758				
Number of provinces	69				
R-squared	0.035	0.180	0.221	0.511	0.562
Total expenditures per capita					
Proximity to distribution depot	0.020***	0.020***	0.012**	0.013	0.009
	(0.007)	(0.007)	(0.006)	(0.008)	(0.009)
	[0.004]	[0.005]	[0.049]	[0.115]	[0.328]
Mean of dependent variable	0.253				
Number of observations	758				
Number of provinces	69				
R-squared	0.053	0.202	0.291	0.501	0.552
Year fixed effects		✓	✓	✓	✓
Proximity to other institutional hubs			✓	✓	✓
Agro-climatic and economic controls				✓	✓
Pre-unification-state fixed effects					✓

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: province × year. All regressions are estimated by least squares of Equation 3 with linear controls and fixed effects as indicated in the last four rows of the table. Standard errors clustered at the province level in parentheses; *p*-values in brackets. The dependent variables are the province–year number of personnel employed, or the province–year spending on technical service, ordinary administration and total (in lire), all divided by the province's population in 1871. The explanatory variable is defined in Section III and measured in days of travel time. 'Proximity to other institutional hubs' includes negative travel time to the province's *compartimento* (statistical-division) capital under the 1871 census and negative travel time to Rome. 'Agro-climatic and economic controls' include the standard deviation of historical temperature and precipitation (1500–1800), FAO-GAEZ attainable yields for wheat and maize, the post-1500 Caloric Suitability Index, log population in 1871, and the agriculture and industry shares of GDP in 1871.

Table B5. Tax compliance, by cereal

	(1)	(2)	(3)
Share of millstones equipped with a counter			
Wheat suitability	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
	[0.303]	[0.240]	[0.657]
Maize suitability	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
	[0.222]	[0.730]	[0.391]
Mean of dependent variable	0.757		
Number of observations	476		
Number of provinces	68		
R-squared	0.527	0.565	0.622
Tax collected per capita			
Wheat suitability	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
	[0.317]	[0.665]	[0.571]
Maize suitability	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
	[0.779]	[0.892]	[0.846]
Mean of dependent variable	2.442		
Number of observations	828		
Number of provinces	69		
R-squared	0.307	0.446	0.500
Year fixed effects	✓	✓	✓
Agro-climatic and economic controls		✓	✓
Pre-unification-state fixed effects			✓

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: province × year. All regressions are estimated by least squares of Equation 3 with linear controls and fixed effects as indicated in the last four rows of the table. Standard errors clustered at the province level in parentheses; *p*-values in brackets. The dependent variables are the province–year crop-specific (wheat vs. maize) milling tax revenue collected (in lire), divided by the province's population in 1871. The explanatory variable is the one defined in Section III for $t > 1870$, since the dependent variable is observed only from 1872 onward, and is measured in days of travel time. 'Proximity to other institutional hubs' includes negative travel time to the province's *compartimento* (statistical-division) capital under the 1871 census and negative travel time to Rome. 'Agro-climatic and economic controls' include the standard deviation of historical temperature and precipitation (1500–1800), FAO-GAEZ attainable yields for wheat and maize, the post-1500 Caloric Suitability Index, log population in 1871, and the agriculture and industry shares of GDP in 1871.

Table B6. Higher technology diffusion increases detection of counter tampering, but not other inspection outcomes

	(1)	(2)	(3)	(4)	(5)
Ordinary verification per capita					
Proximity to distribution depot	2.254	2.252	1.737	3.993	3.890
	(4.513)	(4.542)	(4.458)	(4.823)	(5.674)
	[0.619]	[0.622]	[0.698]	[0.411]	[0.495]
Mean of dependent variable	77.992				
Number of observations	619				
Number of provinces	69				
R-squared	0.003	0.022	0.038	0.259	0.380
Malfunctions per capita					
Proximity to distribution depot	-0.273	-0.272	-0.187	-0.221	0.191
	(0.213)	(0.215)	(0.230)	(0.203)	(0.183)
	[0.205]	[0.210]	[0.418]	[0.282]	[0.301]
Mean of dependent variable	2.043				
Number of observations	619				
Number of provinces	69				
R-squared	0.019	0.100	0.117	0.186	0.241
Reapplication of counters per capita					
Proximity to distribution depot	-0.650**	-0.650**	-0.460	-0.568**	0.107
	(0.282)	(0.284)	(0.297)	(0.273)	(0.285)
	[0.024]	[0.025]	[0.126]	[0.041]	[0.710]
Mean of dependent variable	2.869				
Number of observations	619				
Number of provinces	69				
R-squared	0.051	0.145	0.182	0.259	0.334
Violations per capita					
Proximity to distribution depot	0.045***	0.045***	0.042***	0.035*	0.026
	(0.013)	(0.013)	(0.014)	(0.017)	(0.016)
	[0.001]	[0.001]	[0.005]	[0.051]	[0.114]
Mean of dependent variable	0.236				
Number of observations	619				
Number of provinces	69				
R-squared	0.047	0.109	0.111	0.220	0.449
Year fixed effects		✓	✓	✓	✓
Proximity to other institutional hubs			✓	✓	✓
Agro-climatic and economic controls				✓	✓
Pre-unification-state fixed effects					✓

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: province × year. All regressions are estimated by least squares of Equation 3 with linear controls and fixed effects as indicated in the last four rows of the table. Standard errors clustered at the province level in parentheses; *p*-values in brackets. The dependent variables are the province–year number of ordinary verifications, malfunctions, reapplications of counters, or violations reported following in-person audits, all divided by the province's population in 1871. The explanatory variable is defined in Section III and measured in days of travel time. 'Proximity to other institutional hubs' includes negative travel time to the province's *compartimento* (statistical-division) capital under the 1871 census and negative travel time to Rome. 'Agro-climatic and economic controls' include the standard deviation of historical temperature and precipitation (1500–1800), FAO-GAEZ attainable yields for wheat and maize, the post-1500 Caloric Suitability Index, log population in 1871, and the agriculture and industry shares of GDP in 1871.

Table B7. Higher technology diffusion is not driven by state targeting to provinces with higher tax potential

	(1)	(2)	(3)
Share of millstones equipped with a counter			
Wheat suitability	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
	[0.303]	[0.240]	[0.657]
Maize suitability	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
	[0.222]	[0.730]	[0.391]
Mean of dependent variable	0.757		
Number of observations	476		
Number of provinces	68		
R-squared	0.527	0.565	0.622
Tax collected per capita			
Wheat suitability	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
	[0.317]	[0.665]	[0.571]
Maize suitability	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
	[0.779]	[0.892]	[0.846]
Mean of dependent variable	2.442		
Number of observations	828		
Number of provinces	69		
R-squared	0.307	0.446	0.500
Year fixed effects	✓	✓	✓
Agro-climatic and economic controls		✓	✓
Pre-unification-state fixed effects			✓

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: province × year. All regressions are estimated by least squares with linear controls and fixed effects as indicated in the last three rows of the table. Standard errors clustered at the province level in parentheses; *p*-values in brackets. The dependent variables are the province–year number of counters, divided by the province's number of millstones in 1870; or the province–year milling tax revenue collected (in lire), divided by the province's population in 1871. The explanatory variables are the FAO-GAEZ attainable yields for wheat and maize. 'Agro-climatic and economic controls' include the standard deviation of historical temperature and precipitation (1500–1800), the post-1500 Caloric Suitability Index, log population in 1871, and the agriculture and industry shares of GDP in 1871.

Table B8. Higher technology diffusion does not induce higher enforcement through inspections

	(1)	(2)	(3)	(4)	(5)
Total number of inspections per capita					
Proximity to distribution depot	1.336	1.335	1.059	3.170	4.199
	(4.867)	(4.899)	(4.878)	(5.143)	(5.917)
	[0.785]	[0.786]	[0.829]	[0.540]	[0.480]
Mean of dependent variable	82.967				
Number of observations	619				
Number of provinces	69				
R-squared	0.001	0.025	0.039	0.263	0.378
Year fixed effects		✓	✓	✓	✓
Proximity to other institutional hubs			✓	✓	✓
Agro-climatic and economic controls				✓	✓
Pre-unification-state fixed effects					✓

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: province × year. All regressions are estimated by least squares of Equation 3 with linear controls and fixed effects as indicated in the last four rows of the table. Standard errors clustered at the province level in parentheses; p-values in brackets. The dependent variable is the province–year number of in-person audits, divided by the province's population in 1871. The explanatory variable is defined in Section III and measured in days of travel time. 'Proximity to other institutional hubs' includes negative travel time to the province's *compartimento* (statistical-division) capital under the 1871 census and negative travel time to Rome. 'Agro-climatic and economic controls' include the standard deviation of historical temperature and precipitation (1500–1800), FAO-GAEZ attainable yields for wheat and maize, the post-1500 Caloric Suitability Index, log population in 1871, and the agriculture and industry shares of GDP in 1871.

Table B9. No significant effects of tax technology on milling industry

	Mills per per capita		**Millstones per capita**		**Millstones per mill**	
	(1)	(2)	(3)	(4)	(5)	(6)
Proximity to initial distribution depot × 1870	-0.0014		-0.0016		0.0375	
	(0.0014)		(0.0015)		(0.0758)	
	[0.3262]		[0.2655]		[0.6222]	
Proximity to initial distribution depot × 1882	-0.0009		-0.0009		0.1059	
	(0.0008)		(0.0008)		(0.0926)	
	[0.2648]		[0.2606]		[0.2565]	
Proximity to final distribution depot × 1870		-0.0018		-0.0019		0.0634
		(0.0017)		(0.0017)		(0.0729)
		[0.2814]		[0.2743]		[0.3876]
Proximity to final distribution depot × 1882		-0.0009		-0.0010		-0.0092
		(0.0011)		(0.0011)		(0.0954)
		[0.4048]		[0.3467]		[0.9233]
Mean of dependent variable	0.0024	0.0024	0.0034	0.0034	1.7328	1.7328
Number of observations	195	195	195	195	195	195
Number of provinces	68	68	68	68	68	68
R-squared	0.850	0.852	0.844	0.845	0.823	0.822

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: province × year. All regressions are estimated by least squares in a three-year panel, with year and province fixed effects. Standard errors clustered at the province level in parentheses; *p*-values in brackets. The dependent variables are the province–year number of operating mills or millstones, divided by the province's population in 1871; or the province–year number of operating millstones per mill. The explanatory variables are indicators for whether the province is above the sample median of proximity to initial or post-1870 assigned depot, interacted with year dummies (1866 serves as omitted category).

Table B10. Imports and exports

	(1)	(2)	(3)	(4)	(5)
Imports					
Proximity to distribution depot	-0.020	-0.020	-0.012	-0.016	-0.051
	(0.068)	(0.069)	(0.080)	(0.107)	(0.126)
	[0.772]	[0.773]	[0.877]	[0.881]	[0.685]
Mean of dependent variable	0.192				
Number of observations	414				
Number of provinces	69				
R-squared	0.001	0.002	0.003	0.133	0.177
Exports					
Proximity to distribution depot	-0.117	-0.117	-0.073	-0.113	0.074
	(0.136)	(0.137)	(0.110)	(0.184)	(0.246)
	[0.394]	[0.397]	[0.508]	[0.541]	[0.764]
Mean of dependent variable	0.510				
Number of observations	414				
Number of provinces	69				
R-squared	0.004	0.011	0.016	0.288	0.313
Year fixed effects		✓	✓	✓	✓
Proximity to other institutional hubs			✓	✓	✓
Agro-climatic and economic controls				✓	✓
Pre-unification-state fixed effects					✓

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: province × year. All regressions are estimated by least squares of Equation 3 with linear controls and fixed effects as indicated in the last four rows of the table. Standard errors clustered at the province level in parentheses; *p*-values in brackets. The dependent variables are the province–year imported and exported quantities (in kg) of grain-based products (i.e., flour, bread, biscuits, and pasta), divided by the province's population in 1871. The explanatory variable is defined in Section III and measured in days of travel time. 'Proximity to other institutional hubs' includes negative travel time to the province's *compartimento* (statistical-division) capital under the 1871 census and negative travel time to Rome. 'Agro-climatic and economic controls' include the standard deviation of historical temperature and precipitation (1500–1800), FAO-GAEZ attainable yields for wheat and maize, the post-1500 Caloric Suitability Index, log population in 1871, and the agriculture and industry shares of GDP in 1871.

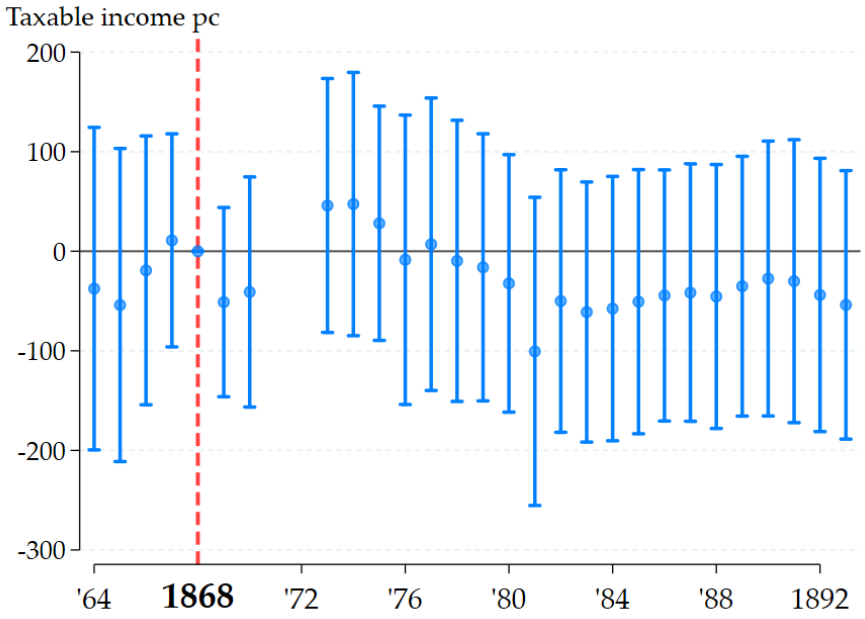
Table B11. Tax technology does not have clear macroeconomic effects

	Log GDP per capita				Share of total GDP		
	Total	**Agriculture**	**Industry**	**Services**	**Agriculture**	**Industry**	**Services**
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Proximity to distribution depot × 1881	0.054	0.047	0.009	0.036	0.008	0.001	-0.009
	(0.047)	(0.034)	(0.017)	(0.029)	(0.010)	(0.004)	(0.009)
	[0.258]	[0.170]	[0.595]	[0.224]	[0.453]	[0.742]	[0.300]
Proximity to distribution depot × 1901	-0.066	0.008	0.006	-0.028	-0.015	0.007	0.008
	(0.052)	(0.061)	(0.034)	(0.040)	(0.013)	(0.008)	(0.011)
	[0.211]	[0.899]	[0.872]	[0.478]	[0.242]	[0.346]	[0.465]
Proximity to distribution depot × 1911	-0.125*	0.027	-0.031	-0.050	-0.023	0.020*	0.003
	(0.064)	(0.076)	(0.040)	(0.048)	(0.016)	(0.012)	(0.013)
	[0.054]	[0.722]	[0.443]	[0.296]	[0.159]	[0.100]	[0.812]
Number of observations	276	276	276	276	276	276	276
Number of provinces	69	69	69	69	69	69	69
R-squared	0.874	0.921	0.963	0.896	0.923	0.886	0.882

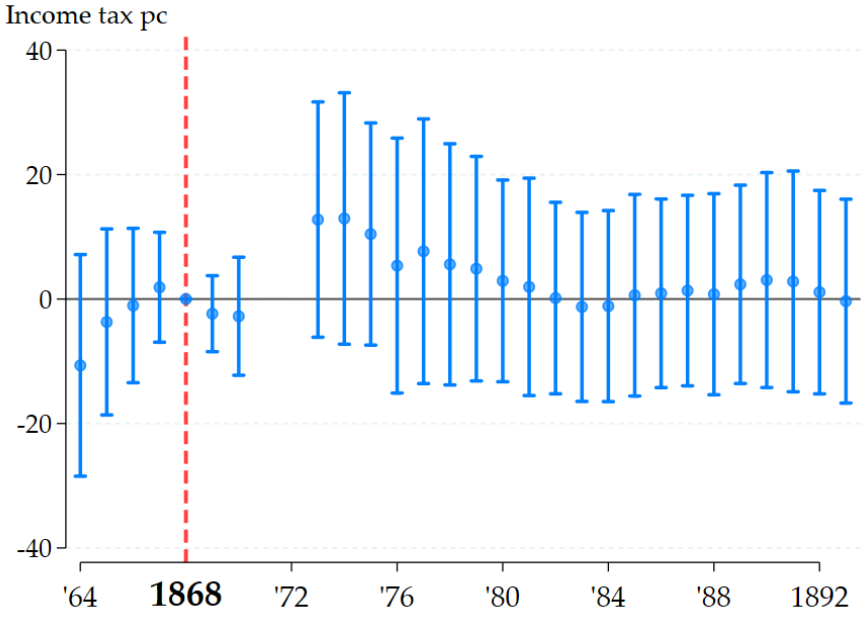
Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: province × year. All regressions are estimated by least squares in a four-year panel, with year and province fixed effects. Standard errors clustered at the province level in parentheses; *p*-values in brackets. The dependent variables are the (ln) province–year GDP both total and divided by sector, divided by the province's population in 1871; or the province–year share of sectoral GDP. The explanatory variable is an indicator for whether the province is above the sample median of proximity to post-1870 assigned depot, interacted with year dummies (1871 serves as omitted category).

Figure B4. Tax technology did not crowd out compliance with less legible taxes

(a) Effect on taxable income



(b) Effect on income tax



Notes: Event-study estimates with 90 percent confidence intervals from least-squares regressions of Equation 5. Unit of observation: province × year. Year and province fixed effects are included. Standard errors clustered at the province level. The dependent variables are the province–year self-declared taxable income or the income tax collected (in lire), all divided by the number of taxpayers. The explanatory variable is an indicator for whether the province is above the sample median of proximity to post-1870 assigned depot, interacted with year dummies. The vertical red line marks the omitted pre-event year relative to which coefficients are normalized.

Table B12. Milling tax is highly regressive; income tax is not

	(1)	(2)	(3)	(4)
Milling tax				
Log GDP per capita	0.002	0.002	0.096	0.261
	(0.239)	(0.241)	(0.194)	(0.198)
p-value of one-sided test for $\beta < 1$	0.000	0.000	0.000	0.000
Number of observations	827			
Number of provinces	69			
R-squared	0.000	0.276	0.439	0.507
Income tax				
Log GDP per capita	0.923***	0.941***	1.016***	0.580***
	(0.336)	(0.336)	(0.181)	(0.213)
p-value of one-sided test for $\beta < 1$	0.409	0.431	0.534	0.026
Number of observations	1,877			
Number of provinces	68			
R-squared	0.063	0.366	0.711	0.763
Year fixed effects		✓	✓	✓
Agro-climatic and economic controls			✓	✓
Pre-unification-state fixed effects				✓

Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: province × year. All regressions are estimated by least squares with linear controls and fixed effects as indicated in the last three rows of the table. Standard errors clustered at the province level in parentheses. The dependent variables are the province–year (ln) milling tax revenue collected (in lire), divided by the province's population in 1871; or the province–year (ln) income tax revenues collected (in lire), divided by the number of taxpayers. The explanatory variable is the (ln) province–year total GDP, divided by the province's population in 1871. 'Agro-climatic and economic controls' include the standard deviation of historical temperature and precipitation (1500–1800), FAO-GAEZ attainable yields for wheat and maize, the post-1500 Caloric Suitability Index, log population in 1871, and the agriculture and industry shares of GDP in 1871.

Table B13. Initial diffusion of tax technology affected civic compliance *only* in the initial phase

	Rate of draft evasion			
	Unbalanced sample		Unbalanced sample	
	(1)	(2)	(3)	(4)
Above-median proximity to initial distribution depot × Post	0.008	0.006		
	(0.005)	(0.006)		
	[0.146]	[0.261]		
Above-median proximity to initial depot × 1869–1870			0.009**	0.008*
			(0.004)	(0.004)
			[0.047]	[0.084]
Above-median proximity to initial depot × 1871–1884			0.008	0.007
			(0.005)	(0.006)
			[0.140]	[0.239]
Above-median proximity to initial depot × Post-1884			0.008	0.005
			(0.007)	(0.007)
			[0.256]	[0.443]
Number of observations	5,500			
Number of districts	206			
R-squared	0.773	0.776	0.773	0.776
Year and district fixed effects	✓	✓	✓	✓

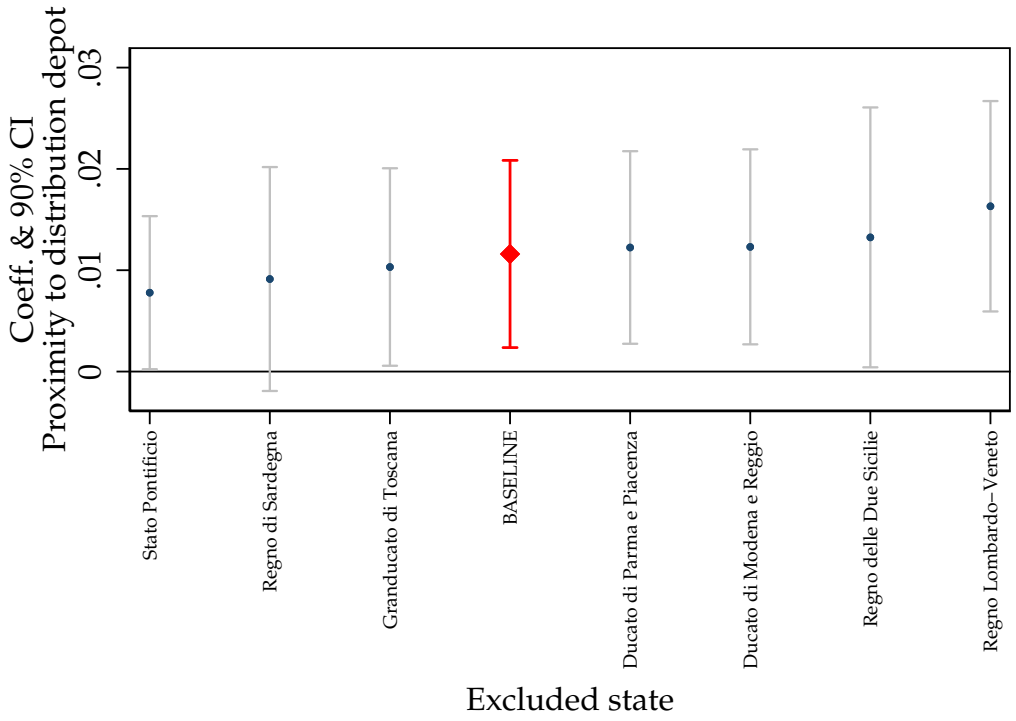
Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: district × year. All regressions are estimated by least squares, with fixed effects as indicated in the last row of the table. Standard errors clustered at the district level in parentheses; *p*-values in brackets. The dependent variable is the district–year share of men registered on the draft lists who evaded military service. The explanatory variable is an indicator for whether the province is above the sample median of proximity to initial distribution depot, interacted with period dummies (pre-1869 serves as omitted category).

Table B14. Diffusion of tax technology undermined civic compliance

	Rate of draft evasion			
	Unbalanced sample		Unbalanced sample	
	(1)	(2)	(3)	(4)
Above-median proximity to post-1870 distribution depot × Post	0.011**	0.012**		
	(0.006)	(0.006)		
	[0.043]	[0.039]		
Above-median proximity to post-1870 depot × 1869–1870			0.004	0.002
			(0.004)	(0.005)
			[0.359]	[0.616]
Above-median proximity to post-1870 depot × 1871–1884			0.011**	0.011*
			(0.005)	(0.006)
			[0.044]	[0.060]
Above-median proximity to post-1870 depot × Post-1884			0.014**	0.017**
			(0.007)	(0.007)
			[0.037]	[0.011]
Number of observations	5,500			
Number of districts	206			
R-squared	0.774	0.778	0.775	0.779
Year and district fixed effects	✓	✓	✓	✓

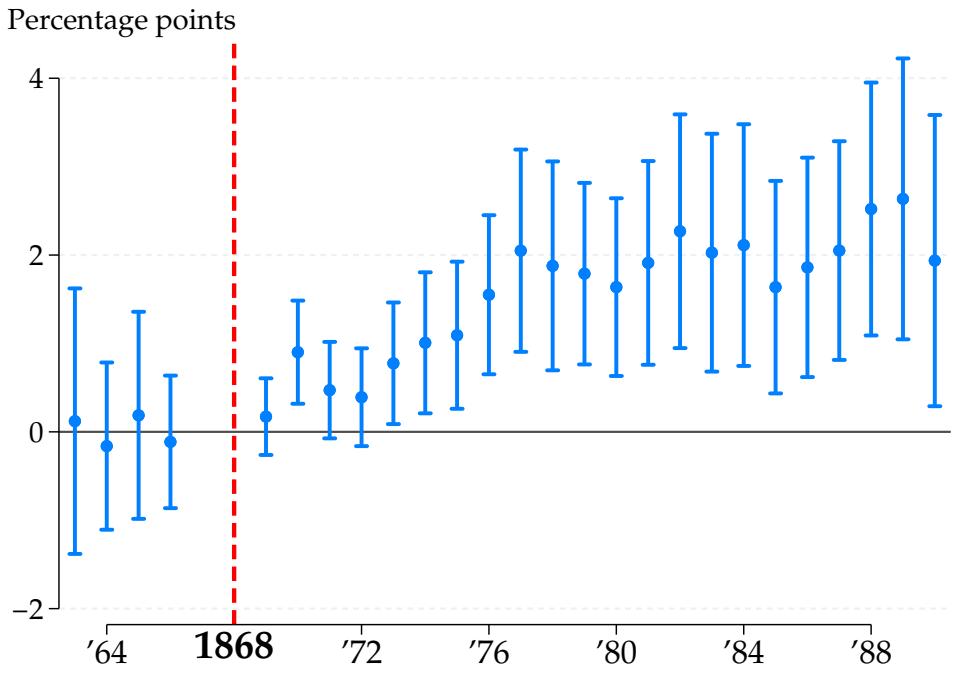
Notes: *Significant at 10%. **Significant at 5%. ***Significant at 1%. Unit of observation: district × year. All regressions are estimated by least squares, with fixed effects as indicated in the last row of the table. Standard errors clustered at the district level in parentheses; *p*-values in brackets. The dependent variable is the district–year share of men registered on the draft lists who evaded military service. The explanatory variable is an indicator for whether the province is above the sample median of proximity to post-1870 distribution depot, interacted with period dummies (pre-1869 serves as omitted category).

Figure B5. No single pre-unification state is responsible for the effect on civic compliance ("leave-one-out")



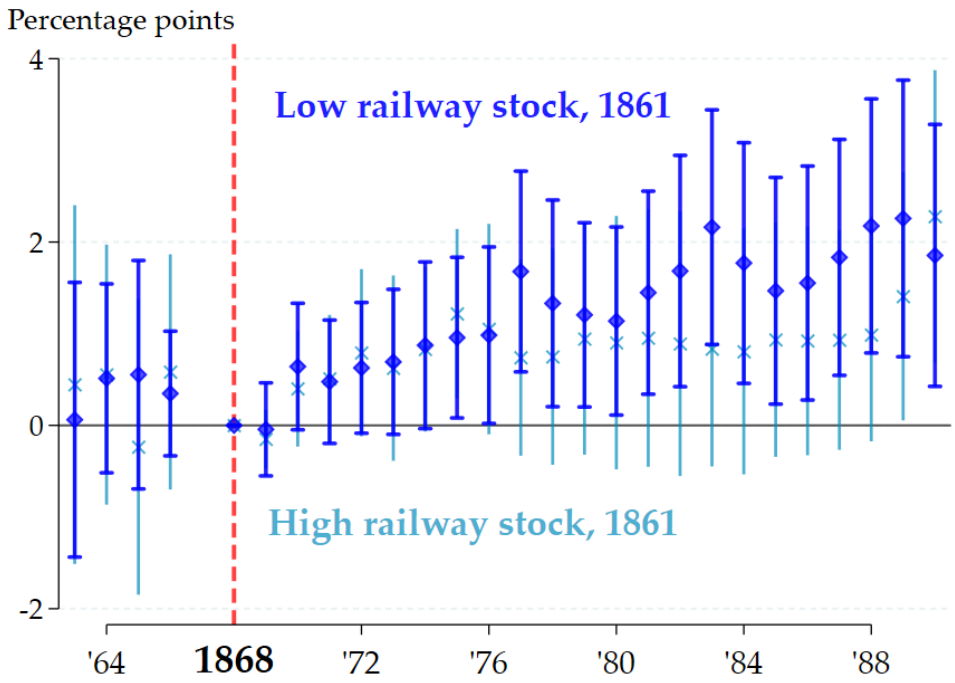
Notes: The figure plots the proximity coefficients and 90 percent confidence intervals from least squares with year and district fixed effects. Each coefficient is obtained by re-estimating the model after excluding the pre-unification state indicated on the x-axis. Unit of observation: district × year. Standard errors are clustered at the district level. The dependent variable is the district–year share of men registered on the draft lists who evaded military service. The explanatory variable is an indicator for whether the district is above the sample median of proximity to post-1870 distribution depot, interacted with a post-1869 dummy.

Figure B6. Tax technology undermined civic compliance where state investment was low: comparison of heterogeneous effects



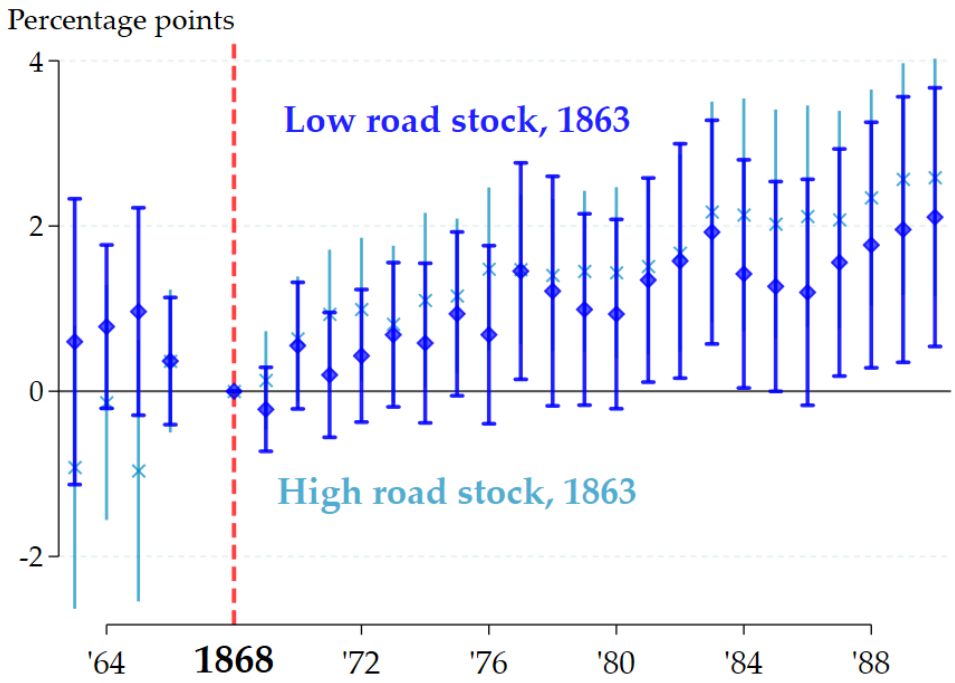
Notes: Event-study estimates with 90 percent confidence intervals from least-squares. Unit of observation: district × year. Year and district fixed effects are included. Standard errors clustered at the district level. The dependent variable is the district–year share of men registered on the draft lists who evaded military service. The explanatory variable is the (triple) interaction between: an indicator for whether the district is above the sample median of proximity to post-1870 assigned depot, an indicator for whether the district is below the sample median of railway density in 1868 (railway length in 1868 divided by land area), and year dummies.

Figure B7. Heterogeneous effect on civic compliance reflects low investment of the new state, not low inherited stock at unification



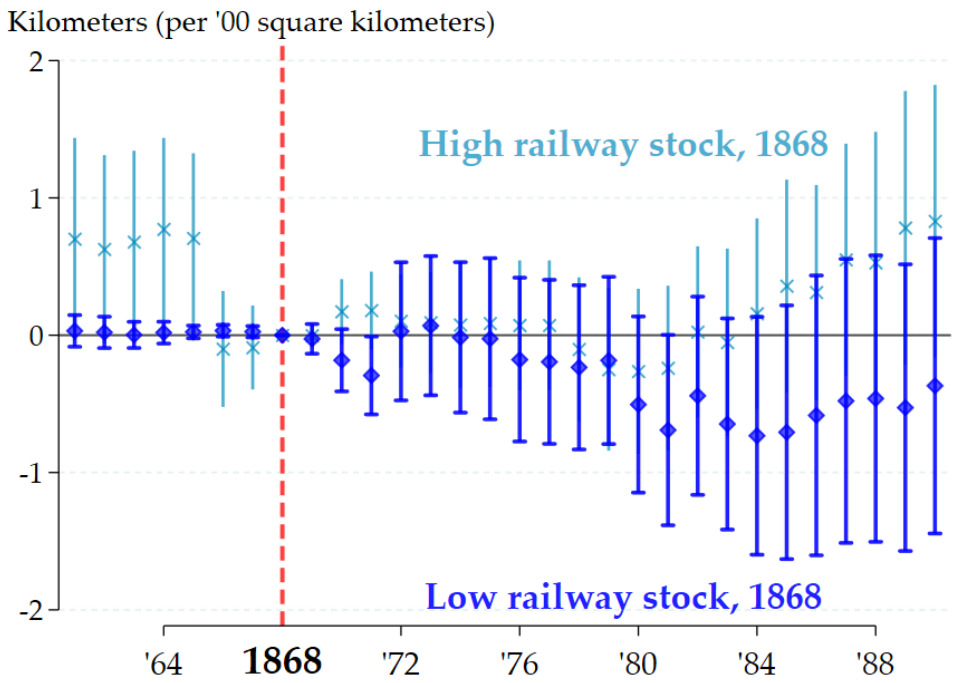
Notes: Event-study estimates with 90 percent confidence intervals from least-squares regressions of Equation 5. Unit of observation: district × year. Year and district fixed effects are included. Standard errors clustered at the district level. The dependent variable is the district–year share of men registered on the draft lists who evaded military service. The explanatory variable is an indicator for whether the district is above the sample median of proximity to post-1870 assigned depot, interacted with year dummies. Coefficients in dark and light blue are from separate regressions for districts below and above the sample median of railway density in 1861 (railway length in 1861 divided by land area), respectively. The vertical red line marks the omitted pre-event year relative to which coefficients are normalized.

Figure B8. No heterogeneous effect on civic compliance by locally provided public investment



Notes: Event-study estimates with 90 percent confidence intervals from least-squares regressions of Equation 5. Unit of observation: district × year. Year and district fixed effects are included. Standard errors clustered at the district level. The dependent variable is the district–year share of men registered on the draft lists who evaded military service. The explanatory variable is an indicator for whether the district is above the sample median of proximity to post-1870 assigned depot, interacted with year dummies. Coefficients in dark and light blue are from separate regressions for districts below and above the sample median of road density in 1863 (road length in 1863 divided by land area), respectively. The vertical red line marks the omitted pre-event year relative to which coefficients are normalized.

Figure B9. No reversion to the mean in railway expansion



Notes: Event-study estimates with 90 percent confidence intervals from least-squares regressions of Equation 5. Unit of observation: district × year. Year and district fixed effects are included. Standard errors clustered at the district level. The dependent variable is the district–year railway length (in kilometers) divided by the district's land area (in 100 square kilometers). The explanatory variable is an indicator for whether the district is above the sample median of proximity to post-1870 assigned depot, interacted with year dummies. Coefficients in dark and light blue are from separate regressions for districts below and above the sample median of railway density in 1868 (railway length in 1868 divided by land area), respectively. The vertical red line marks the omitted pre-event year relative to which coefficients are normalized.

C. MODEL: EXTENSIONS

C.1. The Miller's Problem: Incentive to Conceal and Tax Incidence

> *"frauds will grow in proportion to the profit that drives the miller to attempt them"*
>
> — *La Rassegna Settimanale*, "La diminuzione del macinato" (1878)
> (own translation from Italian)

Under the milling tax, customers were the statutory taxpayers, while the miller controlled the output recorded by the counter, collected the charge, and remitted it to the state. The model of tax compliance in Section V.A assumes that, when choosing how much output to conceal, the miller *fully* internalizes the reduction in tax remittances owed to the state. This assumption describes the miller's best response to the tax technology, but does not characterize who receives the resulting remittance saving in equilibrium.

Here, we show that: first, given fixed output, a sufficient condition for the miller to fully internalize the remittance saving is that he take the milling price as given when choosing concealed output. Second, we distinguish this incentive to conceal from tax incidence, i.e., how remittance savings are divided between millers and customers in equilibrium.

Profit Maximization. Let ξ denote *ordinary* milling costs per unit, separate from concealment costs $\kappa(h)$, and let p be the tax-inclusive price charged to customers. Conditional on true output y, the miller's profit from operating a counter-equipped millstone is

$$\text{Profit}(h; p) \equiv (p - \xi)y - t(y - h) - \kappa(h)$$

Because an atomistic miller takes p as given when choosing the amount of output h to conceal, suppressing the terms that do not vary with h simplifies the miller's problem to

$$h_1 \in \arg\max_{0 \le h \le y} \text{Profit}(h; p) = \arg\max_{0 \le h \le y} \left\{ th - \kappa(h) \right\}$$

Hence the miller's problem in the main text follows from profit maximization: it implies that the marginal return to concealing an additional unit equals the statutory tax rate t. Importantly, it does not assume that the miller ultimately retains the remittance saving as a rent.

Incentive to Conceal. To allow for partial internalization, let $\alpha \in [0, 1]$ denote the fraction of a marginal avoided remittance that the miller internalizes when choosing the amount of output to conceal. An atomistic miller operating a counter-equipped millstone then chooses

$$h_1(\alpha) \in \arg\max_{0 \le h \le y} \{\alpha t h - \kappa(h)\}$$

The benchmark in the main text sets $\alpha = 1$. At an interior solution,

$$\kappa_h\big(h_1(\alpha)\big) = \alpha t$$

Strictly convex concealment costs imply that concealed units $h_1(\alpha)$ are weakly increasing in α (strictly increasing at an interior solution). A miller who internalizes more of the remittance saving therefore conceals more output.

The parameter α affects two margins entering welfare: the marginal revenue gain of counter reach R_q and the marginal real resource cost K_q. To express these margins, hold fixed the tax base B_0 of a millstone without a counter, and let $B_1(\alpha) = y - h_1(\alpha)$ be the tax base of a millstone with a counter. Maintaining the assumption that concealment is costless without a counter, the marginal effects of counter reach become

$$R_q(\alpha) = N^m t\big[B_1(\alpha) - B_0\big] \qquad K_q(\alpha) = N^m \kappa\big(h_1(\alpha)\big)$$

The normalization used in the main text is the special case $B_0 = 0$. A higher α weakly increases concealment, thereby weakly reducing R_q and weakly increasing K_q (these changes are strict at an interior solution).

Tax Incidence. The miller's concealment problem treats p as given when an atomistic miller chooses h. Tax incidence instead concerns how the equilibrium milling price responds to concealment across millers and, therefore, who captures the resulting saving.

Relative to full compliance, concealment at $h_1(\alpha)$ reduces remittances by $t h_1(\alpha)$ and costs $\kappa\big(h_1(\alpha)\big)$ in real resources, where α is the share of the marginal reduction that the miller internalizes when choosing concealment. The resulting joint net private saving of customers and the miller is

$$\Delta\Pi(\alpha) \equiv t h_1(\alpha) - \kappa\big(h_1(\alpha)\big)$$

Let $\theta \in [0,1]$ denote the share of this saving passed to customers through a lower equilibrium milling price. The miller retains the remaining share $(1-\theta)\Delta\Pi(\alpha)$.

The benchmark in the main text $\alpha = 1$ does not imply that the miller retains the saving. For example, under competitive zero profit, the equilibrium price is

$$p_1 = \xi + \frac{t\big[y - h_1(1)\big] + \kappa\big(h_1(1)\big)}{y} = \xi + t - \frac{\Delta\Pi(1)}{y}$$

Relative to the full-compliance price $\xi + t$, the price falls by $\Delta\Pi(1)/y$. Hence, competitive zero profit implies $\theta = 1$ even though $\alpha = 1$: each atomistic miller internalizes the full marginal remittance saving when choosing concealment, even though competition ultimately passes the saving entirely to customers.

In general, α governs the miller's concealment decision, whereas θ allocates the resulting net private saving between millers and customers. They need not be equal and need not sum to one. Conditional on α and fixed output, changing θ only reallocates this saving without changing h_1, R_q, or K_q.

Implications for Welfare. With equal social weights on millers and customers, the division of $\Delta\Pi(\alpha)$ between them is a transfer: their joint private gain is the same for every θ. Consequently, no additional incidence term enters the sufficient statistics for the welfare

analysis in Section V.A. The welfare impact of counter reach remains

$$\text{MVPF}(\alpha) \equiv \frac{R_q(\alpha) + K_q(\alpha) + \nu\, N^c D_q}{R_q(\alpha) - S_q - \mu\, N^c D_q}$$

independently of θ. Estimating it does not require any additional input under equal social weights: the reduced-form estimate of R_q already incorporates the response at the prevailing α, while the bound on K_q derived in Appendix D.2 remains valid for every $\alpha \in [0, 1]$. If millers and customers instead receive different social weights, θ becomes relevant for distributional welfare analysis.

C.2. The Conscript's Problem: State Conduct, Beliefs about Reciprocity, and Draft Evasion

> *"the government [...] in levying the milling and slaughter tax comes daily into direct contact with the proletariat and confronts it in a hateful fashion"*
>
> — Karl Marx, "The Communism of the Rheinischer Beobachter" (1847)

In our model of civic compliance, the tax technology conveys information that can change conscripts' beliefs about the state and, in turn, their decision whether to evade the draft:

$$\underbrace{q}_{\text{technology}} \xrightarrow[\text{to information}]{\text{exposure}} \underbrace{\zeta}_{\text{signal}} \xrightarrow[\text{updating}]{\text{Bayesian}} \underbrace{\pi_\zeta}_{\text{beliefs}} \longrightarrow \underbrace{\psi(\pi_\zeta)}_{\text{civic disutility}} \longrightarrow \underbrace{e_c}_{\text{evasion choice}}$$

In this appendix, we illustrate this mechanism in a Bayesian model with two state types and reciprocal preferences. Our approach builds on Besley and Dray (2024), who connect citizens' beliefs about *a policy's benefits* to their willingness to comply with it. We take this idea further by connecting distinct state demands through beliefs about *the state's reciprocity*: experience of taxation can reframe the perceived obligation to perform military service. This connection is motivated by the literature on the reciprocal nature of state–citizen relations (Besley, 2020, 2021).[58]

Experiencing the State. The state has a fixed type, $\lambda \in \{\text{REC}, \text{NON-REC}\}$. Type REC is willing to reciprocate citizens' contributions, whereas type NON-REC is not. Conscripts in a locality do not observe the state's type and share a prior probability that it is reciprocal, $\pi_0 \in (0, 1)$, which they may update upon receiving a public fiscal signal ζ.

In our historical setting, counter reach $q \in [0, 1]$ determines the locality's exposure to information about state conduct. The fiscal signal is common to all conscripts within the locality and arrives with probability $\varphi(q) \in [0, 1]$, where $\varphi(0) = 0$ and $\varphi_q(q) > 0$ in the

[58] A related illustration in Weigel (2020, Online Appendix A3.3.2) links tax collection to beliefs about state capacity and political participation. Fernández et al. (2025) model how exposure to public goods changes beliefs about government expenditure efficiency and thereby tax compliance, distinguishing this information channel from reciprocity arising from the direct economic benefits of public goods.

interior $q \in (0, 1)$. Greater counter reach increases the probability of receiving the signal, while the informativeness of the received signal is held fixed.

We focus on exposure to the adverse fiscal experience realized in our historical setting. The locality receives the adverse signal, $\zeta = -$, with probability $\varphi(q)$. Importantly, receiving the signal does not reveal the state's type with certainty, but provides evidence about the state's willingness to reciprocate. If no signal arrives, the locality receives no new information, $\zeta = \varnothing$, and conscripts retain their prior beliefs, π_0. At a given counter reach, the probability of receiving the signal is the same under either state type and does not depend on the fiscal experience the signal reveals.[59]

Updating Beliefs. Given counter reach q, conscripts use the signal received to learn about the state's *unobserved type*. They update their beliefs, taking into account how likely the *observed experience* is under each state type. In particular, an adverse experience can arise under either state type, but is less likely under a reciprocal state: under type λ, it occurs with probability r_λ, where $0 < r_{\text{REC}} < r_{\text{NON-REC}} \leq 1$. According to the posterior beliefs, they then decide whether to evade the draft.

Let π_ζ denote the posterior probability that conscripts assign to the reciprocal state type after observing ζ. Bayes' rule gives

$$\pi_- = \frac{\pi_0 \, r_{\text{REC}}}{\pi_0 \, r_{\text{REC}} + (1 - \pi_0) \, r_{\text{NON-REC}}}$$
$$\pi_\varnothing = \pi_0$$

Receipt of a signal is equally likely under both state types, so its probability $\varphi(q)$ cancels from the numerator and denominator of π_-. With two state types, π_ζ fully describes the posterior beliefs introduced in the main text. The non-reciprocal state type receives the remaining probability $1 - \pi_\zeta$.

The adverse public signal lowers the perceived likelihood of a reciprocal state, so that $0 < \pi_- < \pi_0$. Conscripts become less confident in the state's reciprocity, without learning its type with certainty.

Evading the Draft. For simplicity, assume that evasion carries civic disutility $\psi_{\text{REC}} > 0$ when the state is reciprocal and zero when the state is not reciprocal. Given the posterior beliefs, expected civic disutility is equal to

$$\psi(\pi_\zeta) = \psi_{\text{REC}} \, \pi_\zeta$$

Following the model in Section V.A, each conscript knows his net material benefit b_c from evading and chooses to evade when such benefit exceeds this disutility: $b_c \geq \psi_{\text{REC}} \, \pi_\zeta$. The

[59] The fiscal pressures behind the milling tax (Section II.A) make the tax technology compatible with either state type: even a reciprocal state may resort to a socially costly technology when it urgently needs to finance its basic functions and has limited policy alternatives, as in Italy after the 1866 financial crisis. The evidence that counter diffusion mainly reflected production and logistical constraints, rather than strategic targeting by the state (Section IV.A), motivates treating deployment as given. In our model, conscripts learn about the state from their experience of taxation, as induced by counters, rather than from the extent of counter diffusion itself. Counters affect beliefs and draft evasion because they make exposure to this experience more likely.

draft evasion rate following signal $\zeta \in \{-, \varnothing\}$ becomes

$$D_\zeta \equiv \Pr\{b_c \geq \psi_{\text{REC}}\, \pi_\zeta \mid \zeta\} = 1 - F\left(\psi_{\text{REC}}\, \pi_\zeta\right)$$

The signal lowers the expected civic disutility and therefore weakly increases draft evasion:

$$D_\varnothing \leq D_-$$

The signal reaches the locality with probability $\varphi(q)$, giving an expected draft-evasion rate of

$$D(q) = [1 - \varphi(q)]D_\varnothing + \varphi(q)D_-$$

and a marginal effect of counter reach equal to

$$\begin{aligned} D_q(q) &= \varphi_q(q)\left[D_- - D_\varnothing\right] \\ &= \varphi_q(q)\left[F\left(\psi_{\text{REC}}\pi_0\right) - F\left(\psi_{\text{REC}}\pi_-\right)\right] \geq 0 \end{aligned}$$

Greater counter reach weakly raises expected draft evasion by exposing more localities to adverse information. This response does not require the state to be non-reciprocal: it follows from how conscripts interpret the fiscal experience (they infer that the state is less likely to be reciprocal than they previously believed), with reciprocal preferences held fixed.

The expression in brackets, $F\left(\psi_{\text{REC}}\pi_0\right) - F\left(\psi_{\text{REC}}\pi_-\right)$, is the share of conscripts who switch from compliance to evasion upon receiving the signal. The signal lowers civic disutility from $\psi_{\text{REC}}\pi_0$ to $\psi_{\text{REC}}\pi_-$, so these conscripts have material benefits satisfying

$$\psi_{\text{REC}}\pi_- \leq b_c < \psi_{\text{REC}}\pi_0$$

Weighing the State's Past. Past experience of public services or infrastructure can provide evidence that the state reciprocates citizens' contributions and give them reason to expect that tax revenues will support public provision. We now introduce this channel by allowing prior state investment to shape conscripts' prior beliefs about the state. Where past investment has established sufficiently strong confidence in the state's reciprocity, the same signal may lead to a smaller revision in these beliefs and, in turn, have a smaller effect on the perceived obligation to comply.[60]

Let I denote prior state investment, predetermined with respect to counter deployment. The prior probability that the state is reciprocal, $\pi_0(I)$, increases with investment, so that $\pi_{0,I} > 0$. We maintain the information assumptions above conditional on I and allow investment to affect the civic response only through $\pi_0(I)$, holding all other model primitives fixed.

[60] Past provision could also affect civic compliance by increasing the precision of prior beliefs. In an alternative model of learning about the state's propensity to reciprocate, greater precision at a fixed prior mean can make a new experience receive less weight and induce a smaller belief revision. Prior state investment would then attenuate the increase in draft evasion induced by counter reach. We favor the modeling choice in the text because past provision likely changed citizens' expectations of state reciprocity, a change that holding the prior mean fixed would exclude.

To characterize how prior investment shapes the belief revision, define the likelihood ratio of the fiscal experience as

$$\mathcal{L} \equiv \frac{r_{\text{REC}}}{r_{\text{NON-REC}}} \in (0, 1)$$

A smaller $\mathcal{L}$ means that the adverse fiscal experience is less likely under a reciprocal state relative to a non-reciprocal state. The experience therefore provides stronger evidence against reciprocity and, for a given prior belief, produces a larger revision in beliefs.

We hold $\mathcal{L}$ fixed across investment levels to isolate the role of prior beliefs: conscripts interpret the same fiscal evidence from different starting beliefs, $\pi_0(I)$. While past investment informs these starting beliefs, we do not additionally allow it to change the state's conduct within each type: the probabilities r_λ of the fiscal experience remain fixed.The posterior probability that the state is reciprocal can then be written as

$$\pi_-(I) = \frac{\pi_0(I)\mathcal{L}}{\pi_0(I)\mathcal{L} + 1 - \pi_0(I)}$$

Higher investment raises both the prior and posterior probabilities assigned to a reciprocal state. The heterogeneity of interest concerns how investment affects the size of the belief revision, $\pi_0(I) - \pi_-(I)$, and thus the change in civic disutility. Differentiating this revision with respect to investment gives

$$\pi_{0,I} - \pi_{-,I} = \pi_{0,I}\left(1 - \frac{\mathcal{L}}{[1 - (1 - \mathcal{L})\pi_0(I)]^2}\right)$$

This derivative is negative if and only if

$$\pi_0(I) > \frac{1}{1 + \sqrt{\mathcal{L}}}$$

The threshold condition requires prior confidence in the state's reciprocity to be sufficiently strong relative to the evidence conveyed by the adverse fiscal experience. When the prior exceeds this threshold, higher investment reduces how much confidence conscripts lose in the state's reciprocity after the same signal. Hence, expected civic disutility falls by less. Below the threshold, higher investment implies a larger loss of confidence.[61]

Recall that counter reach changes only the probability of receiving the signal $\varphi(q)$. At a given I, the marginal effect of counter reach on draft evasion becomes

$$\begin{aligned} D_q(q; I) &= \varphi_q(q)\big[D_-(I) - D_\varnothing(I)\big] \\ &= \varphi_q(q)\left[F\big(\psi_{\text{REC}}\pi_0(I)\big) - F\big(\psi_{\text{REC}}\pi_-(I)\big)\right] \geq 0 \end{aligned}$$

[61] We do not estimate prior beliefs or the informational strength of the fiscal experience. Our model derives conditions under which prior investment attenuates the belief revision; these conditions need not hold in every locality.

Conscripts who switch from compliance to evasion have material benefits satisfying

$$\psi_{\text{REC}}\pi_{-}(I) \leq b_c < \psi_{\text{REC}}\pi_0(I)$$

Higher investment raises both evasion cutoffs, shifting the interval toward higher material benefits. Under the threshold condition derived above, higher investment also reduces the fall in civic disutility and so narrows this interval. Suppose that F has a continuous density f over an interval containing both cutoffs. Differentiating $D_q(q; I)$ with respect to investment gives

$$D_{qI} = \varphi_q \psi_{\text{REC}} \pi_{0,I} \left[f\left(\psi_{\text{REC}}\pi_0\right) - \frac{\mathcal{L}}{[1-(1-\mathcal{L})\pi_0]^2} f\left(\psi_{\text{REC}}\pi_{-}\right) \right]$$

The threshold condition implies $\pi_{-,I} > \pi_{0,I} > 0$, i.e., the lower cutoff rises faster with investment than the upper cutoff. To the extent that f is weakly decreasing between the two evasion cutoffs, these conditions imply $D_{qI} \leq 0$: prior investment weakly attenuates the marginal effect of counter reach on draft evasion by shaping beliefs about the state's reciprocity.

The reduced-form analysis in Section IV.C estimates this heterogeneity directly. The welfare analysis in Section V.B.4 uses these estimates to compare the marginal value of public funds across two levels of prior state investment, defined by the sample median of railway density.

D. MODEL: QUANTIFICATION

D.1. Pricing Counters

The first ministerial report on the milling tax (Camera dei Deputati, 1870b) records the per-counter price paid by the central government to manufacturers. This price varies by procurement phase and by counter model.

- **Italian (Thiabaud–Calzone) counters** were priced at 61 lire (L thereafter) for the first batch (14,000 counters, September–October 1868) and at 56.25L for the second batch (30,000 counters, early 1870).
- **French (Flechet) counters** were priced under five contracts with two manufacturers: 1,000 large-model counters for poles of 105 mm at 85 L (Ducommun of Mulhouse, 11 October 1868); 1,000 small-model counters for 80 mm at 85L and 1,000 large-model counters with only five numbered dials at 90L (Ducommun and La Buire of Lyon, 17 March 1869); 3,000 small-model counters at 85L (La Buire, 17 March 1869); and 15,000 small-model counters at 82L (La Buire, 16 June 1869).
- **Sleeve (*nel bossolo*) counters** were priced at 80 L for a first batch (100 counters from Allemano, Turin, 26 November 1869) and at 65L for a second batch of 2,000 (Officina Galileo and Officina del Pignone, Florence, early 1870).
- **Wooden-pole (*per pali in legno*) counters** were all priced at 90 L for 1,000 units (Officina Galileo, Florence, 18 August 1869).

We price each counter model using the quantity-weighted average.

To each counter price, we add its installation costs, also listed in the first ministerial report: testing (3.13 lire per counter), installation (10.85), and adaptation to the millstone (9.00). The overall costs are therefore **80.74L for an Italian counter, 106.08L for a French counter, 112.98L for a wooden-pole counter, and 88.69L for a sleeve counter**.

We construct a model-weighted average price $\bar{p}$, where the weights are the shares of applied counters of each model during the milling tax (roughly 72% Italian, 24% French, 4% sleeve, and less than 1% for wooden-pole counters). Because a unit increase in counter reach q implies $\bar{m}$ additional counters per capita, the annualized capital cost per unit of counter reach is

$$p_q = \bar{p}\,\bar{m}\,/\,T$$

with $T = 16$ being the operating life of the tax (1869–1884).

D.2. Bounding Concealment Costs

Based on the historical evidence, our model features compliance costs: millers may conceal their output to shrink the tax base and so remit less to the state. Neither the concealment nor the resources it absorbs, $K(q)$, are observed in the data. In this appendix, we show how observed acts of deliberate counter tampering can be used to derive bounds on $\sigma_K = K_q/R_q$, one of the sufficient statistics for welfare. The cost of concealment itself remains unidentified.

We maintain three assumptions. First, each tampering event recorded in our data corresponds to a distinct counter-equipped millstone–year. Second, a counter-equipped millstone with no tampering event declares, and remits taxes on, all of its output that year. Third, within a province, tampering and complying millstones grind the same quantity of grains y and so owe the state the same full-compliance liability ty. The last assumption restricts heterogeneity within a province, but allows both output and tax base to vary across provinces.

Consider a province and suppress its index. Let N^m be the province's fixed number of millstones, and let $X(q)$ be recorded tampering events per millstone. Because a unit rise in q equips every millstone with a counter, $X_q \equiv \partial X/\partial q$ is the marginal share of newly covered millstones that tamper. Our first assumption above caps X_q at one; we impose the cap strictly, which keeps marginal revenue positive.

Let $a_x \in (0, 1]$ be the share of the millstone's full-compliance tax liability avoided in tampering event x, equal in the model to the share of output concealed, and let k_x be the resources the event absorbs. By choosing concealment, the miller reveals that the event's resource cost does not exceed the avoided remittance

$$k_x \leq tya_x$$

which follows from the condition $\kappa_h(h_1) = t$ of Section V.A, evaluated in event x. This inequality follows by comparing the miller's payoff from the chosen action with the feasible option of recording all output; it does not require observing the concealment cost.

Writing $\overline{a}$ for the average of a_x among the marginally induced events, and aggregating over the newly equipped millstones,

$$K_q \leq tyN^m X_q \overline{a} \qquad R_q = tyN^m \left(1 - X_q \overline{a}\right)$$

where $\overline{a}$ is the average of tax liability avoided among the marginally induced events. Since $\overline{a} \leq 1$,

$$\sigma_K \equiv \frac{K_q}{R_q} \leq \frac{X_q \overline{a}}{1 - X_q \overline{a}} \leq \frac{X_q}{1 - X_q}$$

The full-compliance liability is common within a province, so ty cancels, and tampering events alone determine the bound. The last inequality is attained when every tampering millstone conceals all of its output for that year. Partial concealment therefore makes the bound tighter.

Revealed preference supplies no positive lower bound on the private resources used in tampering: the chosen action can have an arbitrarily small positive cost. We therefore use

$$0 \leq \sigma_K \leq \frac{X_q}{1 - X_q}$$

as the benchmark identified set. In levels, the corresponding bound is

$$0 \leq K_q \leq \frac{R_q X_q}{1 - X_q}$$

D.3. Historical Calibration of the Fiscal Externality of Draft Evasion

Our welfare analysis allows draft evasion to carry a fiscal externality: evasion was a crime under Italian law, and the state spent resources to pursue and punish it. In this appendix, we estimate the additional lire of state spending generated by an additional draft evader, μ. Specifically, we price the cost of pursuing an evader and the cost of holding him in prison. The latter is equal to the share of evaders imprisoned, multiplied by the expected days of custody and the price of a prisoner-day.

Cost of Pursuit. The state spent resources to locate draft evaders, whether or not the pursuit resulted in arrest. It paid a reward of 50 lire for each successful arrest, granted to *carabinieri* and other agents of public security and extended in 1863 to enlisted soldiers assigned to this service (Royal Decree no. 1301, 31 May 1863). The reward reveals what an arrest was worth to the state, but does not measure the state resources actually spent to secure one. We take 50 lire as a revealed-preference measure of the cost of pursuit per evader, incurred regardless of whether an arrest followed.

Cost of Imprisonment. We recover the share of draft evaders imprisoned from Daltri (1988), who follows evaders from the classes of 1839–41 in the districts of Cesena and Rimini. Arrest and voluntary surrender each returned 38 percent of evaders to the state; the remaining 24 percent remained fugitives, died in hiding, were amnestied without presenting, or were absolved by the Ministry of War. Among those who surrendered voluntarily, 48 percent did so under the amnesties of 1868 or 1870; the remaining 52 percent returned outside one. Among evaders who were examined by a physician, 58 percent were found fit for service. Every evader who was arrested or who was surrendered outside an amnesty stood trial and served a sentence; those presenting under an amnesty were held without one.

These shares refer to cohorts called between 1860 and 1862 in two districts in Romagna, a window that predates our welfare analysis sample and may not be nationally representative. State recovery likely improved as the conscription apparatus matured and varied across Italy, depending on factors such as enforcement intensity, terrain, and proximity to a border. We consult a second source (Torre, 1876), which reports the composition of recoveries for the draft year ending 30 September 1875. Arrests accounted for 46 percent of recoveries, against 50 percent in Daltri (1988). We prefer Daltri (1988) as it reconstructs subsequent outcomes for a fixed cohort, rather than one-year flows out of the outstanding stock of evaders. We treat the resulting μ as an order of magnitude rather than a point estimate.

Expected time in prison follows from the sentencing schedule. The recruitment law punished evaders taken by arrest with one to two years of imprisonment, those surrendering within one year of the declaration with two to six months, those surrendering later with six months to one year, and those found unfit for service with one month to one year (Law of 20 March 1854, art. 176). Draft evaders presenting under the amnesty of 1870 were held about a month and released without prosecution (Marcellini, 2023). We evaluate the statutory ranges at their midpoints and the amnestied branch at thirty days. We then weight these durations by the arrest, voluntary surrender, amnesty, and fitness shares above. Expected time in prison is then 247 days (Table D1).

The records do not identify the institution in which each sentence was served. As a

common benchmark, we take the realized price per prisoner-day in judicial prison from the Annuario Statistico Italiano (Ministero di Agricoltura, Industria e Commercio, 1884, p. 567): weighted by presence-days, the average over 1870 to 1879 was 0.618 lire net of prison-labor receipts (0.635 lire gross).[62] The official series reports an average rather than a directly observed marginal cost. However, the state paid contractors a fixed rate per day of presence, which also covered food, clothing, and bedding, so this average price approximates the marginal cost of an additional day in prison. Guards and buildings lay outside the contract and did not vary with the marginal prisoner; we treat their costs as fixed at this margin.

Table D1. Expected Days of Prison per Recovered Draft Evader

	Share of imprisoned	Statutory range	Midpoint (days)	Contribution (days)
Arrested, fit	0.289	1–2 years	548	158.4
Arrested, unfit	0.212	1–12 months	198	42.0
Surrendered and tried, fit	0.149	2–6 months	120	17.9
Surrendered and tried, unfit	0.110	1–12 months	198	21.8
Surrendered under amnesty	0.240	—	30	7.2
Expected prison	1.000			247.2

Notes: Shares in the second column are from Daltri (1988). Statutory ranges in the third column are from the Law of 20 March 1854, art. 176. The number of days in custody under the amnesty in the fourth column is from Marcellini (2023). Contributions in the fifth column are the product of the share and the midpoint.

Summing the two components, we obtain a μ of 166 lire per evader: 50 in pursuit and 116 in prison (0.76 probability of imprisonment $\times$ 247.2 expected days $\times$ 0.618 lire). The estimate omits adjudication costs, and is therefore a lower bound on total state spending on evaders. Replacement costs are excluded from the fiscal externality and instead inform the planner's valuation of the social cost of draft evasion ν in Section V.B.3.

[62] Judicial prisons (*carceri giudiziarie*) held defendants awaiting judgment and prisoners serving terms not exceeding six months; prisoners sentenced to longer terms were ordinarily transferred to penal institutions (*case di pena*) once the judgment became final (Royal Decree 27 January 1861, no. 4681; Royal Decree 13 January 1862, no. 413). Recovered evaders therefore entered judicial custody before any transfer. The judicial-prison rate directly prices all pre-trial detention, all terms not exceeding six months, and the period before transfer for longer sentences. Because transfer dates are unavailable, we apply this rate throughout. For comparison, the weighted average price per prisoner-day, net of prison labor, was 0.676 lire in male penal institutions, and 0.867 lire gross (Ministero di Agricoltura, Industria e Commercio, 1884, pp. 568, 576–577).

ADDITIONAL REFERENCES CITED IN THE APPENDICES

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