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Essays on Fiscal Modernization, Labor Coercion, State Capacity and Trade

DISSERTATION

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DOCTOR OF PHILOSOPHY

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by

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ABSTRACT OF THE DISSERTATION

Essays on Fiscal Modernization, Labor Coercion, State Capacity and Trade

By

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This dissertation consists of three essays in economic history and political economy. Each essay uses newly-collected data from colonial Indonesian archival sources to empirically study historically widespread aspects of taxation that have largely, but not completely, disappeared in modern times. The first two essays study taxation in the form of coerced labor, a practice known as corvée labor. The third essay studies tax collection done by private parties, a practice known as tax farming. This dissertation aims to shed light on what drove these practices and thereby aid our understanding of fiscal modernization, which is thought to be critical for long-run economic development. The key question that runs as a thread through the dissertation is: How and why did modern money-based and centralized fiscal institutions emerge?

The first essay is the first study to estimate the effect of state capacity expansion on corvée labor. To do so, I construct a new database covering eighteen Javanese provinces over thirty-two years (1874-1905) during the period of Dutch colonial rule. I document the importance of corvée labor and find that national-level policy centralized state finances by gradually replacing corvée with a monetary poll tax. At the same time, however, local state capacity expansion, primarily indigenous officials working as agents for the state, slowed the movement away from corvée. The relationship between state capacity expansion and fiscal

modernization therefore depends on what part of the state is expanding. Opposing interests of different state actors can be key in understanding fiscal modernization and public labor coercion, so it is imperative to break open the black box of state capacity and analyze specific actors within the state.

The second essay studies the impact of trade on corvée labor. To do so, I construct the first database on corvée labor usage and exports covering sixteen non-Javanese Indonesian provinces over forty-one years (1900-1940). I find trade expansions, especially of labor-intensive exports, reduce corvée usage. This effect runs through laborers buying themselves out of corvée duties. The fall in in-kind taxation is thus mirrored by a rise in monetary tax revenues. The opposite took place during the trade collapse of the Great Depression. The buy-out option enabled laborers to self-select out of corvée during trade booms, and into corvée during trade busts, without requiring stronger information-collection capabilities of the state. While some studies find a positive relationship between trade and private labor coercion, I argue public labor coercion follows a different logic due to the state's encompassing interest. The nature of the relationship between coercer and coerced is thus key in understanding labor coercion.

The third essay examines the relationship between tax farming and state capacity. To do so, I expand the first essay's database with data on tax farm revenues and non-indigenous Asian officials. The results indicate that state capacity expansion was related to less reliance on tax farming and that different segments of the state bureaucracy had different relationships with tax farming. Officials from the group traditionally excluded from tax farming (the indigenous) are most strongly related to reduced reliance on tax farming. In contrast, officials from the group traditionally wooed as tax farmers (the non-indigenous Asians) are related to increased reliance. This provides evidence for centralized fiscal institutions emerging when the state becomes less dependent on divide-and-rule strategies that route revenue streams through politically weak groups and away from potentially politically powerful groups.

Chapter 1

Labor Coercion and State Capacity: Evidence from Colonial Indonesia

1.1 Introduction

Fiscal modernization is thought to be key for long-run economic development (e.g. Dincecco 2009; He, 2013; Vries, 2015; Dincecco and Katz, 2016). Yet there are many aspects to this process which remain unclear. Prior to fiscal modernization, in-kind taxation in the form of corvée labor – an understudied type of labor coercion in which individuals were forced to work for the state without pay – was widespread.[1] From the state's perspective, such tax payments in labor suffered from several disadvantages as compared to tax payments in money in terms of fungibility, storability, and portability. However, limited monetization, labor productivity, and state capacity meant that states could not easily switch towards

[1]For example, it was used in ancient Egypt and Israel (Brown, 1994; Mendelsohn, 1962), the fifteenth-century Inca Empire (Mann, 2005), eighteenth-century England (Bogart, 2005), the nineteenth-century United States (Hunter, 1961), and twentieth-century British Africa (Frankema and Van Waijenburg, 2014), French Africa (Van Waijenburg, 2018) and China (Bernstein and Lü, 2003). Major projects such as the Great Pyramid of Giza and the Great Wall of China were partially built by corvée labor.

monetary taxation, at least, early in their development. As state capacity grew, so too did the ability to levy relatively information-intensive monetary taxes (e.g. Brewer, 1989; Besley and Persson, 2011, 2013, 2014; Johnson and Koyama, 2014, 2017; Dincecco, 2015). This would seem to have led to a reduction in corvée labor. However, the greater the state capacity, the greater the ability to effectively extract corvée labor, which would increase its prevalence all else equal. Moreover, we observe both forms of taxation side by side for long periods in countries' development. How, then, did state capacity affect the use of corvée labor overall and in relation to monetary taxes? Up until this point, the lack of large-scale corvée labor registers have kept us from understanding this key transition point in the process of fiscal modernization. I overcome this limitation by assembling a rich new panel of data for colonial Indonesia. With these data I present what is to my knowledge the first empirical study of corvée labor and state capacity.

Specifically, in this paper I ask what the effect of state capacity expansion is on corvée labor usage. I open the black box of state capacity by distinguishing between local and national state actors. Local officials to a large extent determined the level and allocation of corvée usage in Indonesia. Expansions of local state bureaucracies could thus increase corvée usage even as central state actors push for replacing corvée with higher monetary taxes. This tension between central and local state actors was accentuated by the differences between European officials, who aligned more with the national-level colonial bureaucracy, and indigenous officials, who were more rooted in local power structures and some of whom were local elites with a tradition of using coerced labor. The central colonial bureaucracy and the local officials were in a principal-agent relationship in which preferences and incentives were not necessarily aligned. In short, this paper posits two countervailing mechanisms: (i) higher state capacity entails higher administrative capacity that stimulates a shift into monetary taxation and thus a reduction of corvée labor; (ii) higher state capacity entails higher extractive capacity and thus an increase of corvée labor. The net effect is ultimately an empirical question.

I use primary colonial Indonesian records to construct a new dataset to empirically investigate this issue. I focus on the island of Java which contained the main population, administrative, and economic centers of the Dutch East Indies, modern-day Indonesia. Java is a good case study as unique province-level annual data on the actual usage of corvée labor, in addition to its legal maximum limits, have been preserved. Importantly, the colonial state on Java required hundreds of thousands of people to provide millions of labor days without pay. Moreover, the state relied on corvée labor differentially across space and time. The data show that in Java as a whole the use of corvée labor was declining, but with different timing and intensity across its provinces. At the same time, the use of a poll tax was increasing as it gradually replaced corvée.

I use a province-level panel data framework spanning thirty-two years (1874-1905) to test which effect of state capacity dominated. I proxy for local state capacity with the number of state officials in a province. Results show a positive and significant relationship between officials and corvée usage. Moreover, this positive relationship applies to indigenous officials, but not to European officials. These findings are robust across various specifications and standard error adjustments. State capacity proxies, such as counts of officials, may however be endogenous. To address endogeneity, I use an instrumental variables (IV) approach that exploits the differing importance, both across provinces and over time, of effective distance to the capital Batavia, modern-day Jakarta. The IV results are consistent with the OLS results in that I find local state capacity expansion increased corvée labor use. Moreover, this effect is driven by indigenous officials. The results are robust to the choice of instrument and across various specifications and standard error adjustments. These findings are evidence in favor of the extraction hypothesis in which state capacity expansion increases corvée labor.

However, since the use of corvée labor was on a downward trend across Java, local state capacity expansion only slowed this decline. What explains the overall downward trend? I argue several factors are at work. National policy actively stimulated the reduction of

corvée labor and its replacement by a poll tax. Such monetary taxation enhanced central control over both the level and the allocation of taxation. While local indigenous officials had significant leeway regarding the level and allocation of corvée, the level and allocation of the poll tax was determined by European officials to a much greater degree. Such a replacement, however, was conditional on sufficient information-collection capabilities of the state as well as productivity and monetization of local economies. The phasing out of labor duties was thus a gradual process. By 1916 the conditions were sufficiently met to enable the complete abolishment of the remaining corvée labor on Java. This part of the story is thus evidence in favor of the administrative capacity hypothesis in which state capacity expansion reduces corvée labor.

I argue that the interests of central state actors did not always coincide with those of local state actors regarding the revenue mix of the colonial state. National state actors favored centralization of taxation and viewed as key the replacement of labor duties with monetary taxation. Local state actors protected their position in the system by increasing the form of state-backed extraction over which they had most discretion: labor duties. These tensions between local and national state actors are key in understanding the countervailing forces underlying this facet of fiscal modernization.

This paper principally contributes to three literatures. First, by documenting the importance and patterns of corvée labor it contributes to the literature on labor coercion. While labor coercion is often studied in private and semi-private forms such as slavery, serfdom, and penal contracts (e.g. Nieboer, 1900; Domar, 1970; Nunn, 2008; Dell, 2010; Acemoglu and Wolitzky, 2011; Naidu and Yuchtman, 2013; Markevich and Zhuravskaya, 2018; Buggle and Nafziger, 2020; Dippel et al., 2020; Saleh, 2020), corvée labor is relatively understudied (e.g. Okia, 2017; Van Waijenburg, 2018). The historically widespread use of corvée, and the conceptual differences between private and public labor coercion, make understanding the rise and fall of corvée fundamental for understanding labor coercion more broadly.

Second, by unpacking the relationships between state capacity, corvée labor, and monetary taxation this paper contributes to the literatures on state capacity and taxation (e.g. Karaman and Pamuk, 2010, 2013; Besley and Persson, 2011; Sng and Moriguchi, 2014; Dincecco and Katz, 2016; Johnson and Koyama, 2017) and agency problems within the state and taxation (e.g. Sng, 2014; Ma and Rubin, 2019; Hao and Liu, 2020). The results indicate that state capacity expansion need not necessarily induce fiscal modernization. Rather, the type of state capacity expansion (e.g. central versus local) matters. Furthermore, fiscal modernization is not necessarily undertaken for efficiency or equity reasons or to increase overall tax revenues, but can be undertaken to alleviate principal-agent problems within the state in the center's favor and increase the center's share of taxation. It is key to open the black box of state capacity and analyze specific actors within the state. While certain state actors might push toward fiscal modernization, other state actors might push for the status quo in taxation.

Third, by detailing the different relationships European and indigenous officials had with different forms of taxation, and by explaining the rationale for the colonial Indonesian state initially relying on corvée while later replacing it with monetary taxation, this paper contributes to the literature on colonialism, specifically regarding the political economy of public finance (e.g. Frankema, 2010, 2011; Gardner, 2012; Wahid, 2013; Frankema and Van Waijenburg, 2014; Van Waijenburg, 2018; Xu, 2018, 2019; Frankema and Booth, 2019; Cogneau, Dupraz and Mesplé-Somps, 2021). I find that the reliance of the Indonesian colonial state on a large body of indigenous officials impacted its usage of different forms of taxation. Moreover, my findings indicate that merely looking at monetary taxation can lead to a distorted picture of the evolution of fiscal capacity, especially when such monetary taxation was partly replacing in-kind taxation such as corvée labor.

The next section sketches the historical and institutional context of corvée labor in Java. The third section introduces the data and describes the empirical patterns of corvée usage. The

fourth section describes the methodology. The fifth section discusses the results. The final section concludes. The appendix describes the data construction and contains additional regression tables.

1.2 Background

Forced labor has a long history on Java. For example, the seventeenth-century sultanate of Banten used coerced labor to construct irrigation networks and canals (Boomgaard, 1990). The sultanate of Mataram, meanwhile, relied on labor duties as its main form of taxation (Moertono, 1963). In the eighteenth century the Dutch East India Company required villagers in the Preanger regencies to cultivate coffee and sell it at preset prices to the company (Breman, 2015). The company also used corvée laborers to dredge canals and unload large ships (Bosma, 2013; De Zwart and Van Zanden, 2015). In 1810 the Great Post Road, which today still runs across Java from west to east in the form of the North Coast Road, was mainly built by forced labor (Ten Horn-Van Nispen and Ravesteijn, 2009).

In the 1830s the Cultivation System was established (Fasseur, 1992; Van Niel, 1992). Under this system peasants throughout Java were required to set aside a given proportion of their land for the cultivation of cash crops such as coffee, sugar, and indigo. These export crops were subsequently sold to the state at fixed prices. State officials determined the type and quantity of the export crops in each village. The colonial state enlisted local indigenous elites for enforcement through a sharing arrangement. Village heads received a certain share of the revenues from the export crops in order to make sure the villagers put time and effort into the forced cultivations (*cultuurdiensten*, 'cultivation services'). The Cultivation System was phased out from the 1870s onward.[2] Viewed over the longer term, cultivation services

[2]While the Cultivation System is estimated to have had long-run positive impacts on local economic development (Dell and Olken, 2020), it is also estimated to have increased contemporary mortality rates (De Zwart et al., 2020).

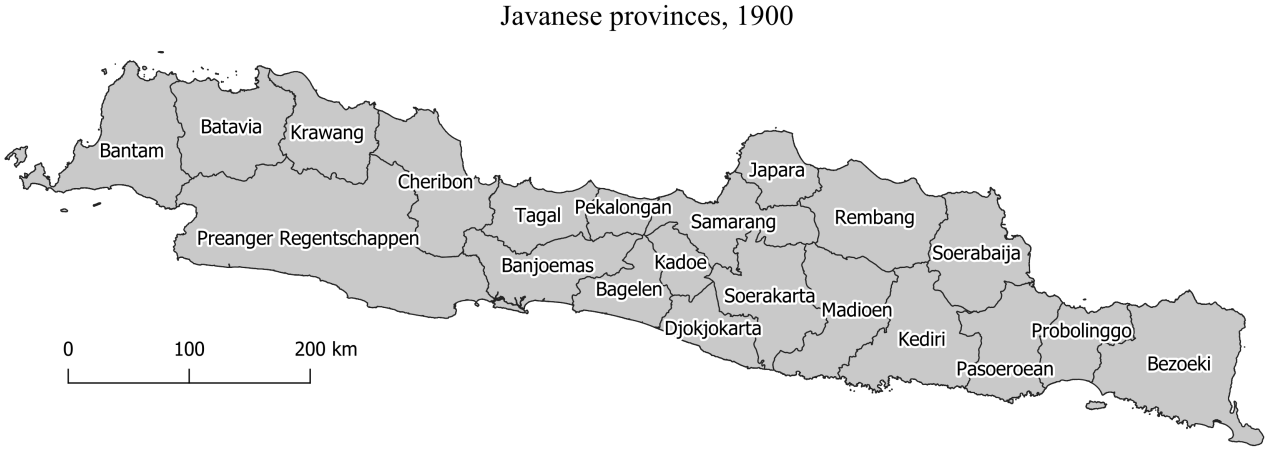


Figure 1.1

were only one version of forced labor in Java. Corvée labor was another, more enduring, version.

Corvée labor (*heerendiensten*, 'lord's services') was commonly used in 1870 and remained in use in the centrally-governed provinces on Java until the 1910s. Moreover, in the two Javanese princely states and outside Java corvée labor was still used in the 1940s (Booth, 1980). I mainly rely on hand-collected quantitative and qualitative data from the annual Colonial Reports (*Koloniale Verslagen*, henceforth KV) to examine this type of labor coercion at the provincial level in Java as the secondary literature on corvée is sparse. Figure 1.1 displays the Javanese provinces in 1900.

Corvée labor was applied to five broad categories: maintenance duty, construction duty, guard duty, personal duties to the local indigenous officials, and other duties.[3] Maintenance and construction duties were related to local infrastructure projects. Work was performed on roads, bridges, travel lodges, water works, storage houses, guard houses, and prisons.

[3]Javanese corvée labor could thus not be used in mines or on plantations. However, the state could require laborers to work for sugar refineries for a set output-dependent wage payment if those refineries claimed they could not find enough wage labor. This was still prevalent in the 1860s, but was phased out in the early 1870s (e.g. KV 1871, p. 170; KV 1872, p. 160; KV 1873, p. 217).

Guard duty mainly consisted of guarding roads at night, but storage houses, water works, and prisons were guarded too. Other duties consisted of operating ferries, delivering letters and grass for mail horses, and escorting prisoners and state officials.

A work day consisted of twelve hours. Each corvée laborer could be made to work a given maximum number of days per year. These maxima, as well as their legal bases, were regulated at the national level but differed across sub-provincial districts and across individuals. Usually this maximum depended on the type of land ownership, with owning rice paddies resulting in a higher assessment than, say, owning a garden would.[4] Over time the maxima were gradually reduced. The labor duties only applied to indigenous adult men of whom more than half were subject to such duties.

Laborers could pay off their labor duties or let others perform their duties for them. Relatively affluent villagers paid off their complete annual duties in advance, while others resorted to hiring full or partial substitutes (KV 1873, p. 94; KV 1874, p. 77). Some substitutes accepted food and lodging in lieu of monetary payments (KV 1872, p. 64). Sometimes people coordinated in hiring substitutes (KV 1901, p. 68). State officials were not always pleased with the substitutes, as many were either old men or teenage boys (KV 1873, p. 95; KV 1882, p. 74).[5]

Corvée labor was only to take place within eight *paal*, approximately twelve kilometers, from the laborer's village so he could return home at the end of the day. Nonetheless,

[4]In 1880 in Java, for example, 1.8 million men had a maximum duty of fifty-two days, 0.3 million men had a maximum duty of twenty-six days, and 0.2 million men had a maximum duty of thirteen days. In the pre-colonial sultanate of Mataram the level of labor duties also differed according to land ownership (Moertono, 1963, Appendix II).

[5]While paying off duties was geographically widespread, the extent in number of days is unknown. From 1905 onward the system of paying off corvée duties was formalized as agreements for specific villages and for specific tasks could be made (KV 1906, p. 139). Unfortunately the KVs contain no records of the extent of such agreements in terms of persons, days, and money involved, so their importance remains uncertain. Since such agreements increased in number from 1905 onward, paying off duties likely increased in importance over time and thus foreshadowed the eventual complete replacement of corvée with monetary taxation. In the non-Javanese Outer Islands the importance of paying off corvée duties greatly increased in the late 1910s and 1920s (Chapter 2 (Hup, 2021b)).

under certain circumstances laborers could be required to travel further and stay overnight, although this would have to be counted as a double work day (KV 1886, Appendix L, p. 1).[6] The laborer had to provide for his own meals, but when the labor was physically very demanding or when time was of the essence the local official was allowed to dispense food reimbursements.[7] Laborers could not be made to work more than the given maximum number of days. Still, if a work project hit unforeseen adversity then the local European official could ask the colony's Governor-General for permission to temporarily extend the number of work days.

This hierarchical bureaucracy reached down to the village level, but local indigenous officials possessed a significant degree of autonomy in setting and allocating corvée labor duties. While corvée labor was regulated at the national level, crucially for the evaluation of state capacity, the implementation of corvée labor therefore varied across time and space. With regards to road maintenance, for example, indigenous officials were responsible for day-to-day supervision. Minor repairs and the collection of surface material such as gravel were overseen by the *controleur*, the local European official who oversaw multiple villages. Since such *controleurs* were thinly spread, however, the actual on-the-ground oversight they were able to muster was limited. This left much autonomy in the hands of local indigenous officials. For major repairs the *controleur* first had to ask permission of the *resident*, the European official in charge of the province. The *controleur* collected records of used corvée days from the indigenous officials and amalgamated them into a report, following a standardized layout, that he sent to the *resident* before the fifteenth day of the subsequent month (KV 1891, Appendix O, p. 3). The *resident*, or rather his team of secretaries, subsequently

[6]This was the case, for example, in Banjoewangi where the postal road between its main city and the border with the neighboring province of Bezoeki went through sparsely populated areas. The villages close to the road could not provide enough labor so villages further away than eight *paal* were also assigned to maintaining the road (KV 1877, p. 85). Eventually this issue was dealt with by stationing convict laborers at four places along the road to maintain the road and thereby reduce the burden on local villagers (KV 1882, p. 74).

[7]There are also cases where compensation was given when localities were hit by tough economic times (KV 1890, p. 73; KV 1891, p. 79; KV 1897, p. 99). In general the compensation did not cover many tasks and did not amount to much.

combined the reports from the *controleurs* inside the province and sent such an overview to the central authorities in the capital. These detailed records provide essential data for an analysis of corvée labor usage.

For managing the implementation of corvée, local indigenous officials were partially paid in the form of corvée. The officials could use this 'payment' in the form of personal duties as they saw fit. Forcing the indigenous population to work for the state thus partially solved the state's revenue problem as laborers paid part of their taxes in kind through labor duties. From mid-1882 onward a poll tax of one guilder per person per year, that only applied to the men who were subject to corvée labor duties, replaced certain duties and reduced the maximum number of work days (Day, 1900).

The initial enactment of the poll tax in 1882 mainly aimed to abolish personal duties to certain indigenous officials, thereby limiting their autonomy in setting corvée burdens.[8] In return the affected officials received higher salaries. Moreover, the poll tax incentivized officials through a sharing arrangement. Their collector's fee was set at a uniform 8% of poll tax revenues. Under regulation and exhortation from the central authorities in Batavia the use of corvée labor was further streamlined and reduced. The maximum days a Javanese laborer could generally be made to work gradually declined from fifty-two in 1870 to forty-two in 1882 to twenty-four in the 1890s and to zero by 1916. By 1916 the state had completely phased out the remaining corvée duties in return for an increased poll tax.[9]

The replacement of corvée labor with a poll tax benefited the central state authorities through the higher fungibility, portability, and storability of money. In contrast to corvée labor, poll tax revenues could be more easily transferred to, and stored in, provincial capitals and

[8]However, village head services (*pantjendiensten*) that were part of village duties (*dessadiensten*) remained in place.

[9]However, outside Java more than 950,000 people were still obligated to work for the state for no pay. The use of corvée labor in the Outer Islands and in the two princely states on Java continued into the 1940s (Booth, 1980; Chapter 2 (Hup, 2021b)). The village services did continue on Java, but were also subject to tightening supervision and regulation from Batavia.

Batavia. This enabled the central authorities to more strictly control the aggregation and disbursement of revenues. For example, while the poll tax was collected by indigenous officials, they could not deviate from the centrally-set level of the poll tax and they subsequently had to hand in the funds at a designated site. The funds were generally under the control of the local European official, the *controleur* (KV 1896, p. 83).[10] The *controleur* also had to collect village-level assessment rolls, amalgamate them into district-level assessment rolls, and send these, along with explanatory notes on any potential increases or decreases, to the European official at the head of the province, the *resident* (Departement van het Binnenlandsch Bestuur, 1918). While the system of labor duties thus left significant control to local indigenous officials, the poll tax facilitated a firmer grip over taxation levels and allocation by the European bureaucracy.

This not only appealed to the colonial state's central authorities due to its distrust of indigenous officials, who constituted the vast majority of state officials, but also because it enabled cross-province transfers. While corvée labor had to be used locally, poll tax revenues could theoretically be spent anywhere. Even though regulations stated poll tax revenues ought to be spent locally, the net revenues of the poll tax (i.e. revenues minus collection fees and spending on local works) quietly flowed into the central coffers (Fokkens, 1914). In 1902 this was formalized by declaring the poll tax a regular tax whose net revenues would flow to the central budget (KV 1902, p. 165). This played into the hands of officials in Batavia and at the head of the provinces who had long criticized local officials for wasting corvée labor on unproductive tasks and for allowing shoddy work (e.g. KV 1874, p. 76; KV 1890, p. 73).[11]

Moreover, if the laborers engaged in productive activities on the days they would otherwise

[10] In some places where the *controleur* was stretched over too large an area, part of the funds were managed by local indigenous officials such as district and sub-district heads. In such cases, the central bureaucracy aimed to "strictly watch the indigenous heads' actions, to ensure no abuse of the funds" (KV 1896, p. 83, own translation).

[11] KV 1874, p. 77 relates the story of a corvée-built earthen dam in Krawang that broke at the first small flood. To improve the quality of dams, the report recommends expanding the department of hydraulic-engineering works so that dam construction can be performed under the guidance and supervision of qualified personnel.

have been performing corvée labor then the colonial Dutch state would also benefit through other taxes such as those on land and exports. In other words, the opportunity costs of corvée were born by the central authorities, who saw potential negative effects on other tax revenues, not by salaried local officials. Such an encompassing interest was openly acknowledged as an important rationale. After reducing labor duties in the province of Kadoe, for example, European officials who were part of a research group set up by the director of the civil service, contentedly noted villagers could now be more easily moved to work their fields in a regular and timely manner (KV 1891, p. 80). Similarly, the KVs care to report that in Pasoeroean the reduction in labor duties freed more labor to work on producing coffee and sugar and on constructing a nearby state railway (KV 1895, p. 99).

However, replacement of taxation in labor with taxation in money required local village economies to be sufficiently productive and monetized. Furthermore, to be able to levy and gradually increase the poll tax, the state first needed to collect local-level information on villages' capacity-to-pay. The state was careful in not shifting out of labor duties too quickly. Field research in 1890 in Bagelen, for example, concluded that all local duties could be abolished if the poll tax was increased from one guilder to nearly three guilders, but that such an increase was not yet possible given the economic conditions (KV 1891, p. 80). Some officials also complained about the rigid uniformity of the poll tax as compared to the labor duties that could flexibly adjust to local conditions (KV 1881, p. 73). Eventually central authorities recognized this problem and allowed for local adjustments of the poll tax as long as such adjustments were pre-approved by the central authorities (KV 1893, p. 75). All these facets indicate that even though a poll tax has relatively low information requirements for the state in comparison to more complex taxes such as income taxes, it does require more information on the part of the central state than corvée labor duties do, for example on local currency usage and earning capacity.

A related thorny issue concerned the supply of wage labor. In the absence of labor duties,

sufficient wage labor had to be available for the state to hire in order to be able to expand and maintain its network of roads and irrigation works. Moreover, the potentially large localized increases in wage labor demand due to abolishing corvée labor might drive up local wages. This would matter especially in areas where a significant part of the corvée laborers would not offer their labor on the wage labor market, at least not at the going wages. As an experiment in 1897, labor duties were further reduced in three districts in exchange for higher monetary taxation (KV 1898, p. 60). Wage labor was subsequently hired. However, wages rose to such an extent that the experiment was canceled in 1898 (KV 1899, p. 89). A few years later a compromise solution was decided on: certain corvée duties were abolished, but if not enough wage laborers were available at set wages then laborers who were subject to corvée duties could still be coerced to work. This work would now, however, be done at 'decent' wages (KV 1904, p. 175).

Lastly, the cancellation of the 1897 experiment was also due to two other reasons: the indigenous officials only cooperated minimally and the European official was stretched over too large an area to adequately monitor the wage labor. These last two issues illustrate the limits of colonial state capacity on Java. The corvée labor system relied on thousands of indigenous officials for implementation. These officials in return were rewarded with salaries, labor duties for personal use and the power and prestige that comes with overseeing large labor projects involving fellow villagers. In the poll tax system, in contrast, indigenous officials were more akin to mere tax collectors while European officials were in tighter control of setting the level of taxation and of dispensing the funds. Even when the responsibility for certain public works was devolved to indigenous village governments, it was made clear that the poll tax funds they received to hire wage labor came from the central budget (KV 1908, p. 141).

The replacement of corvée by the poll tax occurred as the colonial state gradually enhanced its capabilities by expanding its bureaucracy. This state expansion was in turn enabled by

improvements in communications and transportation through the introduction and spread of new technologies such as steams ships, railroads, and telegraph lines. Such improvements enabled the state to better manage an expanded bureaucracy, even in places far away from the capital or in the hinterlands. The state's bureaucratic expansion increased both its extractive capacity and its administrative capacity, its ability to gather and assess large amounts of information, and thereby enabled progress on fiscal modernization besides the poll-corvée replacement. For example, the state updated its Javanese land registry and applied it across Java, although spottily, by the 1910s. Moreover, it gathered detailed income statistics and levied the income tax from 1908 onward. By 1920 income tax coverage had expanded to a significant portion of the population. Contemporaneously, tax farming – the practice of leasing out collection of certain taxes to private parties – was generally phased out in favor of state-run tax collection (Wahid, 2013).

However, the replacement of the corvée system with a poll tax system was not merely about modernizing taxation, it was also about centralizing authority into the hands of a European bureaucracy that was more closely in tune with the wishes of Batavia. The dual character of Indonesia's colonial bureaucracy meant that European officials could potentially work their way up the European bureaucracy by faithfully implementing their superiors' wishes (Fasseur, 1993; Van den Doel, 1994). Local European officials were competing with each other for promotions to a more hospitable posting with better amenities, potentially a provincial capital or even Batavia. Such career incentives were much less pronounced for indigenous officials (Sutherland, 1979). Geographic circulation of indigenous officials was rare and, when it did occur, almost never crossed ethnic boundaries (Kuipers, 2020). While both these groups thus consisted of officials on the state's payroll, the European officials were more enmeshed into the national bureaucracy and therefore more strongly incentivized to work towards the wishes of the bureaucracy's upper hierarchy, in this case towards fiscal modernization and centralization by replacing corvée with a poll tax.

The tensions within the bureaucracy between those who aligned more with the national-level bureaucracy and those who were more rooted in local power structures, raises the question of the effect of state capacity expansion on corvée labor use. The colonial Indonesian state bureaucracy was not a unitary actor, but instead consisted of a multiplicity of actors with potentially different preferences and incentives with regards to types of taxation. National-level officials may have supported replacing corvée with a poll tax because it enhanced central control over the level and allocation of taxation, while local-level officials may have resisted exactly because they possessed a high degree of autonomy in setting the level and allocation of corvée. The effect of state capacity expansion on corvée labor may therefore differ depending on the which part of the state is expanding. In the case of colonial Java, local state capacity expanded unevenly across time and provinces. One prediction is that a province-level expansion in capacity increased its corvée usage relative to other provinces, even as increased state capacity across all provinces led to decreased usage of corvée. However, this effect of province-level capacity may differ after 1882 when the introduction of the poll tax altered the incentives facing local, mainly indigenous, officials. This paper investigates these differences by examining both overall expansions of state bureaucracies and by splitting such expansions into a relatively center-aligned component – the European officials – and a relatively locally-rooted component – the indigenous officials.

In the case of colonial Java, local state capacity expanded unevenly across time and provinces. One prediction is that a province-level expansion in capacity increased its corvee usage relative other provinces, even as increased state capacity across all provinces led to decreased usage of corvee. However, this effect of province-level capacity may differ after 1882 when the introduction of the poll tax altered the incentives facing local, mainly indigenous officials, changed. This paper investigates ...

1.3 Data

To empirically investigate corvée labor, I create the first dataset tracking actual usage in days across provinces. The dataset covers all Javanese provinces, except two indirectly-ruled princely states, and nearly five decades from 1870 to corvée's full replacement with a poll tax in 1916. Uniquely, the KVs contain province-level information on used corvée in number of days worked. This usage data, that goes beyond information on legal maximum limits on corvée, enables an empirical investigation of what drove corvée usage, how it varied over time and space, and for what purposes it was used. In addition to the number of used corvée days, I also collect poll tax revenues and the number of state officials from the KVs.

The data included in the records may understate total labor duties. Firstly, officials possibly undercounted the actual number of days worked (KV 1880, p. 72).[12] Secondly, four Javanese provinces contained private estates on which the landowner, not the state, collected labor duties.[13] Thirdly, the figures cover central government corvée, but not so-called village duties (*dessadiensten*) imposed by local indigenous village governments on which no comprehensive data were recorded.[14] Fourthly, the colonial state did not oversee the corvée labor in the

[12]However, the *resident* of Madura, an island province next to Java, was skeptical of such undercounting (KV 1881, p. 75). He thought the officials were more likely exaggerating, rather than downplaying, the actual number of days. They had two reasons for this: (i) to reduce the burden on villagers; (ii) to signal their diligence. However, this seems to have been a minority view.

[13]These estates were relics of the eighteenth and early nineteenth century when the colonial state sold stretches of land, along with 'feudal' privileges with regards to the people living on those lands, in order to raise revenue (Van Zanden, 2010). Data on these private estates are listed separately in the KVs from 1896 onward, but the accuracy of these statistics is probably lower. Moreover, for earlier years these numbers were (partly) incorporated into the overall province-level statistics. Disentangling these is thus not possible, so I include these figures throughout. These statistics do indicate that the vast majority of such 'private' corvée, usually around 95% of the total, took place in Batavia province.

[14]These village duties were not strictly regulated by the central authorities apart from instructing local officials to ensure they 'stayed within proper limits' (KV 1877, p. 85). Some European officials argued the village corvée was more onerous on the indigenous population than the central corvée was (KV 1889, p. 84). Subsequent field research by the department of the interior supported such claims by finding that indigenous village governments could use up to thirty-five million village service days in 1893 (KV 1895, Appendix P). KV 1901, p. 67 clarifies this number refers to the maximum allowed number of days, not the actual used number of days. However, it also notes that the count excludes certain types of work that were also performed as village duties but were completely unregulated. Still, the maximum lies significantly below the corvée labor maximum of nearly ninety-eight million days. However, the used number of days of corvée labor was far below its maximum. Whether that was the case with village service days is unclear.

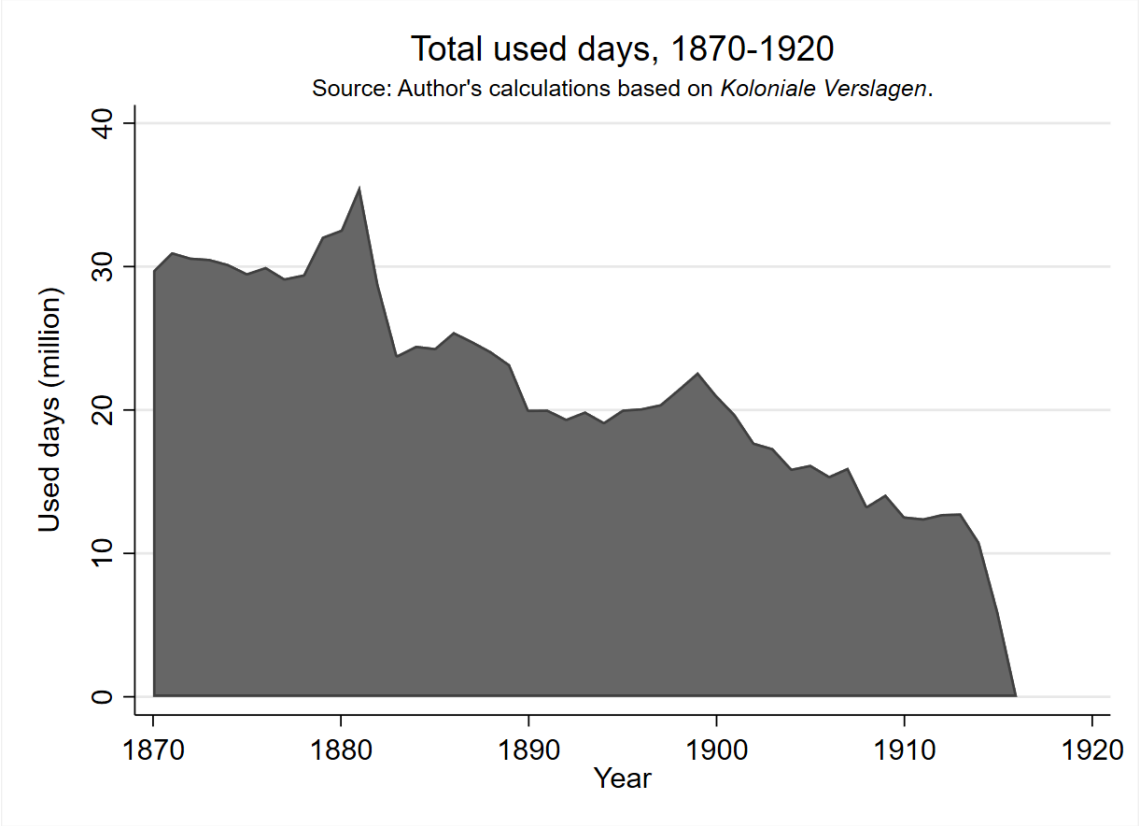


Figure 1.2

princely states of Djokjokarta and Soerakarta where indigenous elites directly managed the corvée labor system.[15]

Figure 1.2 displays the total days contributed by corvée labor on the nineteen directly-ruled Javanese provinces for the period 1870-1920. In 1880, about 34% of days were spent on maintenance duty, 28% on guard duty, 13% on construction duty, 19% on personal duties to the local indigenous officials, and the remaining 6% on other duties. Despite potentially representing a lower-bound estimate, Figure 1.2 shows that the use of corvée labor was an important component of state extraction as throughout the 1870s Javanese conscripts provided around thirty million days of labor. Valued at average unskilled labor wages the

Nonetheless, due to stricter regulation and supervision the village service days are believed to also be on a downward trend in this period.

[15]Booth (1980, Table 8) shows that as late as 1939 more than four million days of corvée labor were extracted by the indigenous elites in these two princely states.

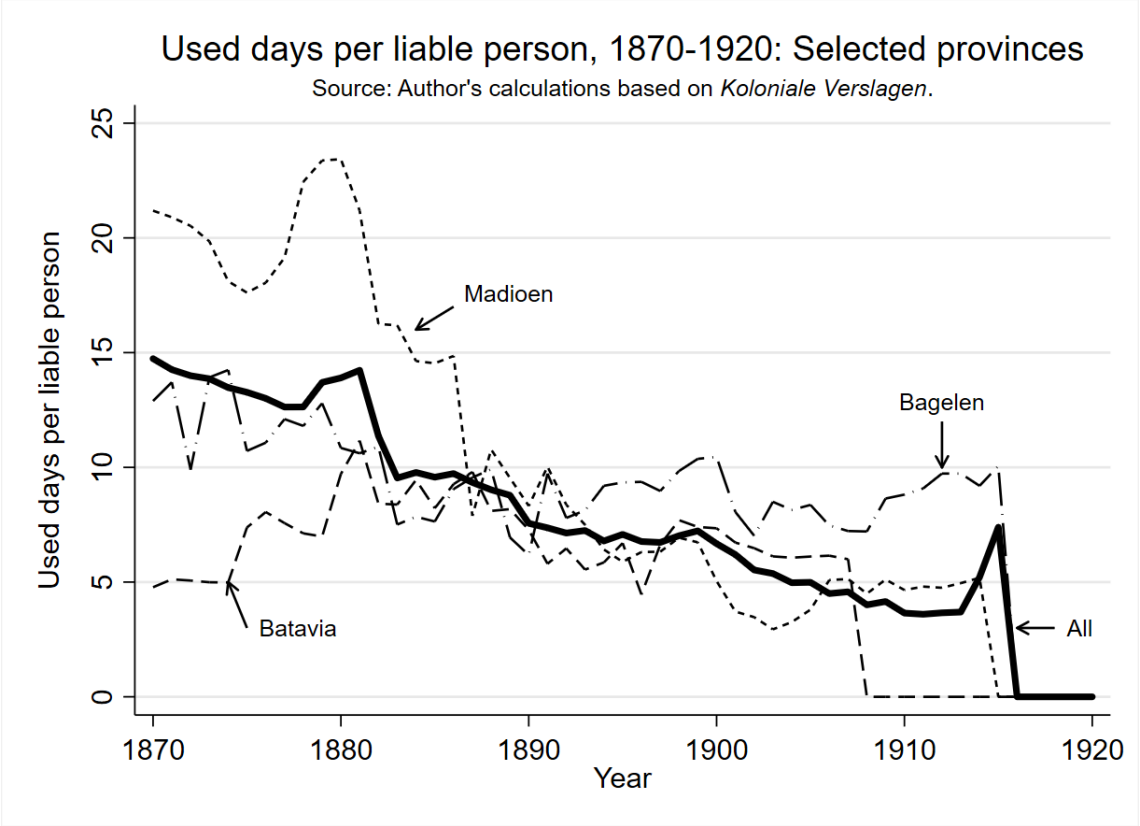


Figure 1.3

corvée labor equalled approximately a third of total tax revenues until 1882.[16] Alternatively, the thirty million days of labor is equivalent to around 120,000 people working for the state without pay twelve hours a day, five days a week, fifty weeks a year. With the adult indigenous male population in the directly-ruled Javanese provinces standing at about four million in 1870, this means 3% of this labor force was fully occupied by the corvée labor duties. In short, corvée labor represented both a sizable share of state revenues and of total labor usage.

Across approximately two million conscripts the thirty million labor days in the 1870s imply that the average conscript provided fifteen days per year. Across provinces, however, there

[16]I base this on tax revenues from Mellegers (2004). Note that tax revenues do not include the substantial revenues from product sales and monopolies. I do not include those here as the costs of those revenue streams are large but unclear, wherefore net revenues likely differed considerably from gross revenues. Still, in the same period the value of corvée equalled approximately ten percent of all gross revenues.

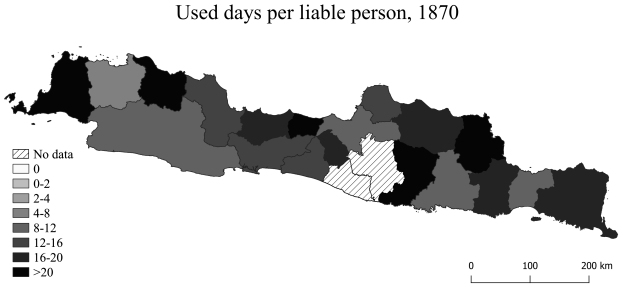
was much variation in the number of days used per laborer (see Figure 1.3). While across all Javanese provinces the average conscript worked about fifteen days in 1870, the average conscript in Madioen worked more than twenty days. In comparison, in colonial French Africa in 1913-1937 the average ranged from four days in Senegal to fifteen days in Congo (Van Waijenburg, 2018, Table 1A and 1B). As the population grew but the corvée labor days declined, the share of the labor force fully involved in corvée labor fell over time. The speed of this decline varied across provinces (see Figure 1.4).

The poll tax, first implemented in mid-1882, gradually replaced corvée. Figure 1.5 graphs the substitution between corvée labor (valued at province-year-specific average unskilled wages) and the poll tax. Induced by the poll tax implementation, used days fell by 11.9 million from 35.8 million in 1881 to 23.9 million in 1883. Induced by a poll tax hike, used days fell from 12.8 million in 1913 to zero in 1916. These two abrupt declines in corvée usage, together adding up to 24.7 million days, represent 69% of used days in 1881 and indicate the direct impact of the centrally-pushed poll tax on corvée usage. From 1883 to 1913 corvée usage fell by another 11.1 million days, partly due to further minor restrictions on corvée tasks financed by poll tax surpluses, but spread out over thirty years. In terms of monetary value, corvée usage dropped by 2.3 million guilders (1881-83), 6.0 million guilders (1883-1913), and 3.9 million guilders (1913-16). Contemporaneously, poll tax revenues rose by 2.3 million guilders (1881-83), 1.6 million guilders (1883-1913), and 5.4 million guilders (1913-16).

The poll-corvée replacement occurred as the colonial state enhanced its information-collection and -analysis capabilities by expanding its bureaucracy. Figure 1.6 shows that between 1874 and 1905 the number of officials in the eighteen provinces roughly doubled. While the number of European officials grew by nearly 90%, the number of indigenous officials more than doubled. Over the same period the indigenous population grew by about 60%. The density of officials thus increased. Figure 1.6 graphs this increasing density as a decrease in the number of adult indigenous men per official. A higher density of officials increased both the

Figure 1.4: Used days per liable person, 1870-1920
Source: Author's calculations based on *Koloniale Verslagen*.

(a)



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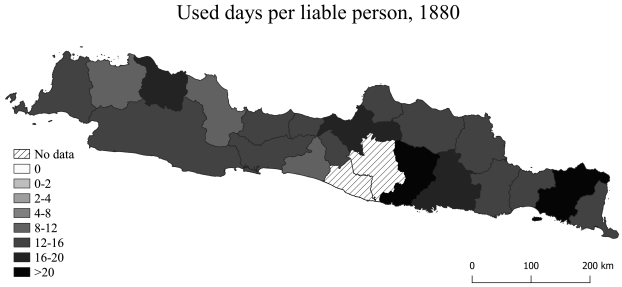

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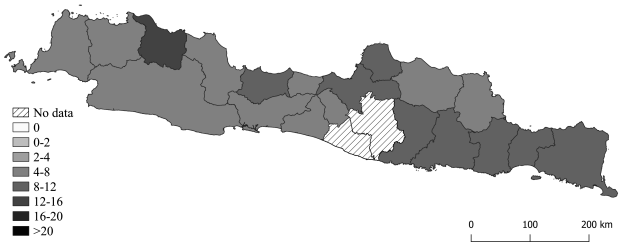

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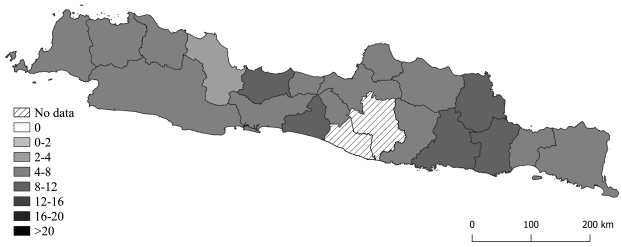

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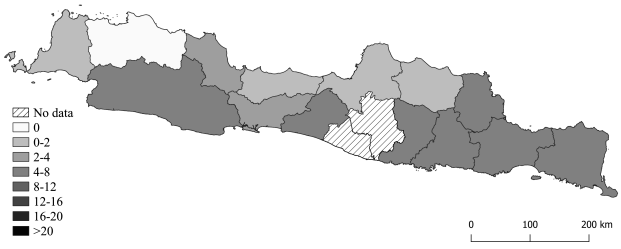

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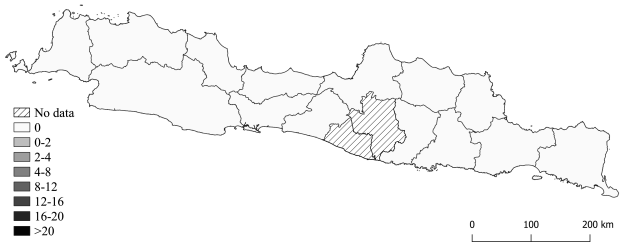

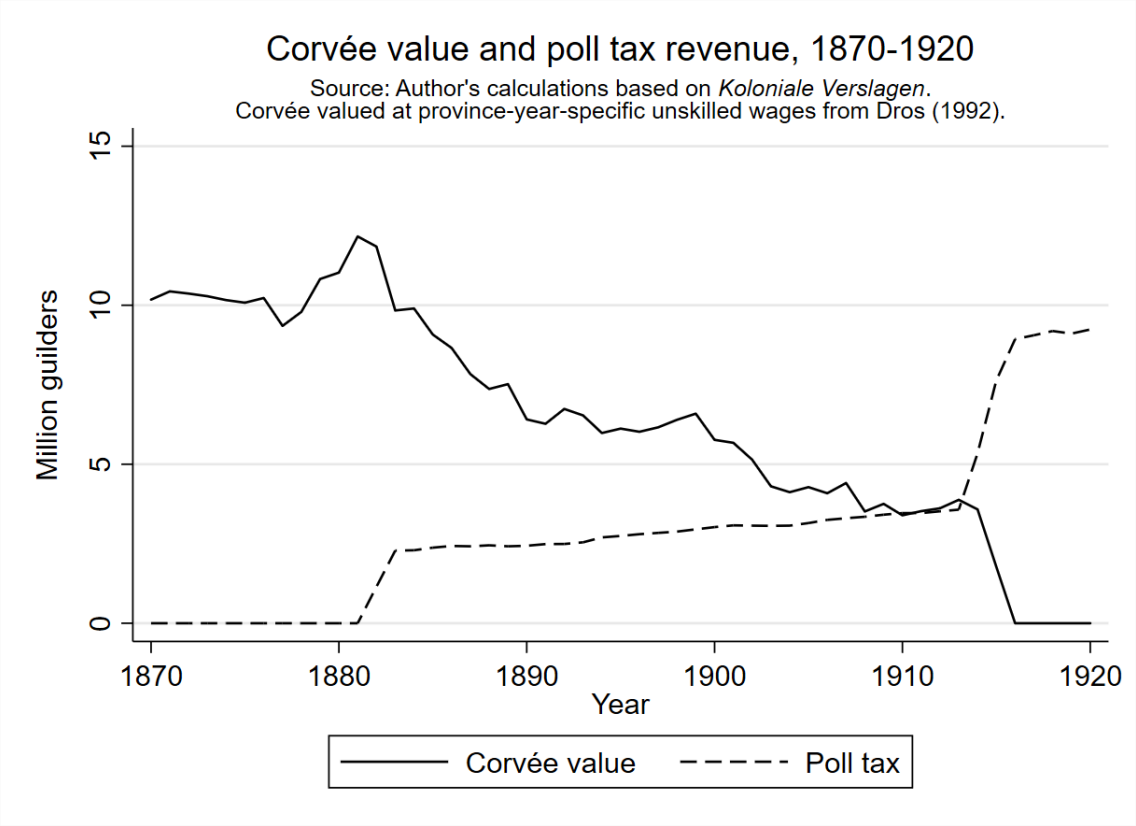

Figure 1.5

state's extractive capacity and its administrative capacity, its ability to gather and assess large amounts of information, and thereby enabled progress on changes in the tax structure such as the poll-corvée replacement, the updating of land registries, the levying of an income tax, and the replacement of tax farming with state-run tax collection.[17]

The count of officials consists of both European and indigenous officials stationed in a province. The indigenous officials constituted the vast majority of officials in the provinces, hovering around a share of 85%.[18] Here, 'indigenous' refers to the state's racial classification

[17]See Wahid (2013a, 2013b) and Chapter 3 (Hup, 2021c) for more on the replacement of tax farming with state-run tax collection.

[18]State-appointed indigenous heads above the village level are amongst the indigenous officials, but indigenous village heads are not for two main reasons. Firstly, the number of village heads is largely determined by the number of villages, irrespective of state policy. The small growth in their number of less than sixteen percent between 1883 and 1905, while the number of other indigenous officials nearly doubled, reflects this. Secondly, from 1883 on the KVs report a single number for both village heads and village council members. As village councils differed in size and roles across villages, often according to local customs, this raw number does not capture state size and capacity well. Moreover, from 1883 onward the KVs report indigenous heads

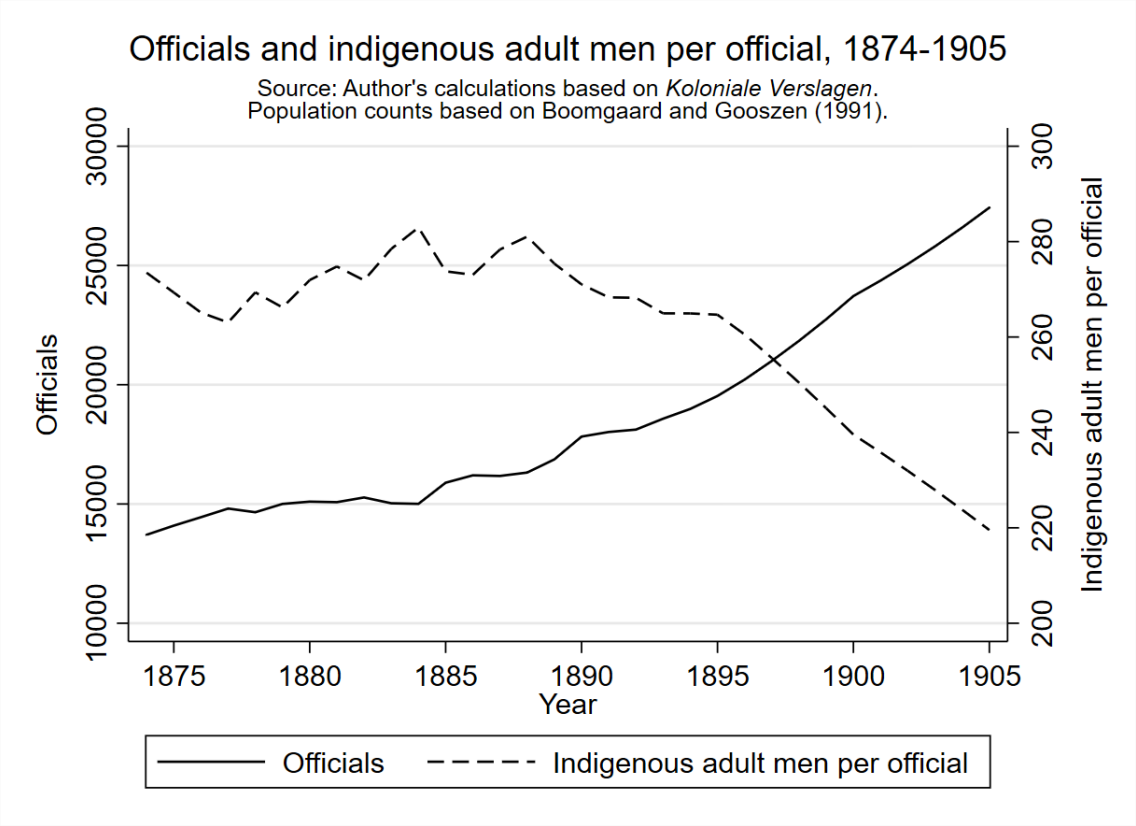

Figure 1.6

of the officials. These indigenous officials thus do not signify indirect rule as was practiced in the two Javanese princely states, Djokjokarta and Soerakarta, that are not included in the analysis due to a lack of data on corvée labor. All the included officials are on the payroll of the colonial state and part of its bureaucracy. They performed a wide variety of essential tasks including, but not limited to, collecting taxes, enforcing law and order, and overseeing public works projects.

above the village level and other indigenous officials as a single number. This indicates the state viewed these latter two groups similarly and viewed them as distinct from village heads and village council members.

1.4 Methodology

To disentangle the potentially countervailing forces within the state, I use a panel data framework of eighteen Javanese provinces across thirty-two years (1874-1905). To capture the national-level push, common across provinces, towards phasing out corvée in favor of a poll tax, I include year fixed effects. To proxy for local state capacity I use the number of state officials in a province. To also capture national-local tensions within state bureaucracies in each province, I separate the local officials into European and indigenous officials.

To investigate the relationship between state capacity and corvée labor, I thus estimate the following models:

$$Log(Days_{it}) = Log(Officials_{it})\beta + X'_{it}\gamma + \alpha_i + \lambda_t + \epsilon_{it} \tag{1.1}$$

And:

$$Log(Days_{it}) = Log(EuropeanOff_{it})\beta_1 + Log(IndigenousOff_{it})\beta_2 + X'_{it}\gamma + \alpha_i + \lambda_t + \epsilon_{it} \tag{1.2}$$

Where for province i and year t: $Days_{it}$ = used days of corvée labor, $Officials_{it}$ = number of officials, $EuropeanOff_{it}$ = number of European officials, $IndigenousOff_{it}$ = number of indigenous officials, X'_{it} = set of controls, α_i = province fixed effects, and λ_t = year fixed effects. I use logged variables in order to interpret the coefficients as elasticities and because provinces varied widely in used days of corvée labor and number of officials. The impact of one added official thus likely differed across provinces, so percentage changes are more suitable. In model (1), a positive coefficient estimate on the number of officials would indicate local state capacity expansion is related to increased corvée usage. In model (2), a larger positive coefficient on indigenous officials as compared to on European officials would provide

further evidence that the type of state capacity expansion matters for corvée usage and that within the state bureaucracy different actors can have different preferences over types of taxation and therefore over fiscal modernization.

The set of controls consists of certain factors that may be related to both state capacity and corvée labor use. For example, the expansion of industrial agriculture increased productivity and stimulated private demand for wage labor. At the same time, this industrialization was partly conditional on a strong state presence protecting European property rights as the majority of the involved capital came from Europe. To control for this channel I include the amount of privately-owned land-based steam power. Population growth, agricultural expansion and wage growth were also related to marginal labor productivity and wage labor supply as well as the provision of public goods such as law and order. To control for these factors I include population figures, the extent of rice paddies (*sawah*), and average unskilled daily wages. Lastly, province fixed effects control for unobserved heterogeneity across provinces (e.g. geography, pre-colonial institutions) and year fixed effects control for common developments across time (e.g. national-level policies, technological advances).

To permit valid inference in the presence of potential within-state and within-year (i.e. geographic) correlation in the errors, I use two-way (province and year) clustered Driscoll-Kraay standard errors (Driscoll and Kraay, 1998; Cameron et al., 2011). To correct potential downward bias in the cluster-robust standard errors due to the small number of clusters, I apply a small-sample correction. Following Cameron and Miller (2015), I further check for the potential problem of few clusters by using the wild cluster bootstrap method. Since shocks could be spatially dependent with decaying dependence across space, I follow Conley (1999) and Colella et al. (2019) by allowing for arbitrary dependence of the errors across provinces and years.[19] Results are robust to the wild cluster bootstrap method and to

[19]To implement this I use Colella et al.'s (2019) `acreg` Stata package. The distance matrix uses the longitude and latitude of each province's centroid. I set the spatial and temporal thresholds (i.e. distance and lag cutoffs) at 1,000 kilometers and ten years, respectively. Based on manually trying out different cutoff values for the basic specification, these cutoff values are more conservative (i.e. larger standard errors) while

spatial correction of errors. The findings are thus not affected by the exact method of computing standard errors. To avoid attributing undue influence to the effects observed in small provinces, I weight observations by the number of indigenous adult males. Results are robust to using no weights. Results are also robust to transforming variables into per capita terms and to using the value of used days as dependent variable.

Potential endogeneity troubles a causal analysis of the impact of state capacity on corvée labor usage. For example, the use of corvée labor for such public goods as security and infrastructure could in turn strengthen state capacity. Such a feedback loop raises the issue of reverse causality. Alternatively, if the state expands its ranks of officials in areas where it intends to modernize tax collection and phase out corvée labor, then this selection into treatment would introduce negative bias into the estimates. To address such issues I use an instrumental variables (IV) estimation in addition to an ordinary least squares (OLS) estimation. I exploit the differing importance, both across provinces and over time, of distance to the capital Batavia. I calculate the distance from the centroid of each province to the centroid of Batavia. Key is that while each province's distance from the capital is fixed, the practical importance of this distance varied over time.

Due to technological advancements and the spread of steam ships, railways, and telegraphs, the practical importance of distance shrank. While time is a rough proxy for advancement in technology and transportation, the number of steam ships and the number of steam ship passengers provide a more accurate reflection. To gauge robustness to the choice of instrument I thus instrument for the number of officials with three different instruments: the distance to Batavia divided by the year, the distance to Batavia divided by the number of Javanese steam ships, and the distance to Batavia divided by the number of steam ship passengers. The numerator varies cross-sectionally and the denominator varies temporally. I use ratios as these three variables all shrink the effective distance. The further time

at the same time being the largest practical distance cutoff value as the largest centroid-to-centroid distance is approximately 900 kilometers.

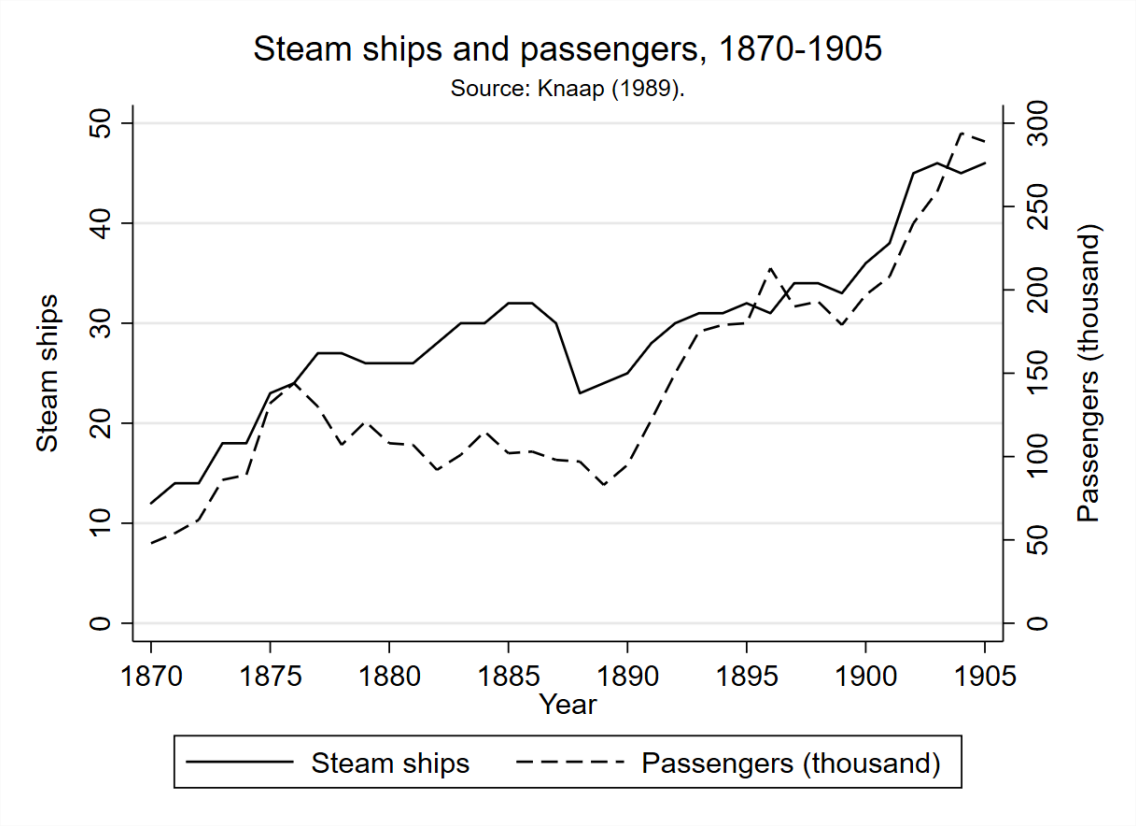

Figure 1.7

progresses, the more steam ships are active and the more passengers are ferried about, the further effective distance shrinks. Figure 1.7 graphs the increasing use of packet-boat service steam ships in Java. These ships were mainly concerned with transporting passengers and postal packets, rather than goods, and therefore especially relevant for the transport of and communication with officials scattered across Java (Knaap, 1989).

Each province differs in distance to Batavia, the administrative center, and this impacts the costs and benefits of employing officials. Shrinking distance directly impacts the costs of sending officials back-and-forth, of communication, and of monitoring. For example, officials far removed from the influence emanating from Batavia might be tempted to extract more corvée labor despite general proclamations calling for the gradual abolishment of labor duties. On the other hand, being distant from the capital, and its support in case of trouble, may also limit the official's extractive power vis-à-vis the population. The costs

and benefits of corvée labor are mainly determined by local conditions such as population density, industrialization, and labor productivity. While such factors could be affected by shrinking distance to the capital, my identifying assumption is that the model controls for them through including controls for population, land-based steam power, rice paddies, and wages in addition to province and year fixed effects. The instrument is plausibly excludable conditional on including the controls.

I loosen the assumption of conditional excludability by using Nevo and Rosen's (2012) Imperfect Instrumental Variable (IIV) procedure. The IIV procedure replaces the zero correlation assumption between the IIV and the unobserved error term with an assumption related to the sign of the correlation. Specifically, it assumes the IIV's correlation with the error term has the same sign as the endogenous variable's correlation with the error term. I assume the sign of the endogenous variable's correlation with the error term to be negative as I consider state bureaucratic expansion for the sake of fiscal modernization, and thus the phasing out of corvée labor, to be the most serious identification issue. Such selection into treatment would introduce negative bias into the OLS estimates. The IIV procedure also assumes the IIV's correlation with the error term is smaller than the endogenous variable's correlation with the error term. These two assumptions enable identification of bounds, instead of point estimates, for the parameters of interest.

The panel consists of eighteen Javanese provinces across thirty-two years. The capital province Batavia is excluded because of its special status as administrative center for the whole colony and because of its poor data on corvée labor due to large private estates.[20] The two indirectly-ruled princely states, Djokjokarta and Soerakarta, are not included due to a complete lack of data on corvée labor. I use the provincial boundaries of 1882 to 1900. I thus merge the provinces of Bezoeki and Banjoewangi, which were separate until 1882, and separate five provincial mergers that took place in 1901. The time coverage is limited to

[20] As noted in footnote 9, the vast majority of 'private' corvée took place in Batavia. Furthermore, it was the only province in which 'private' corvée vastly outweighed regular corvée.

1874-1905 due to data limitations on the province-level number of officials.

Besides the number of used corvée days, poll tax revenues, and state officials, I also collect the number of privately-owned land-based steam power from the KVs. For population figures I use the number of adult indigenous men as only this group was subject to labor duties. These figures are based on Boomgaard and Gooszen (1991). The extent of rice paddies is from Boomgaard and Van Zanden (1990). Average unskilled daily wages are from Dros (1992). The number of steam ships and steam ship passengers are from Knaap (1989). The appendix contains details on the data construction.

1.5 Results

Columns 1 and 2 in Panel A of Table 1.1 show the results of the main OLS regression using the number of used corvée labor days as the dependent variable and the number of officials as the main explanatory variable. Columns 3 and 4 separate the officials into European and indigenous officials. Each specification includes province and year fixed effects. Controls for indigenous adult males, land-based steam power, rice paddies (*sawah*), and unskilled average daily wages are added in columns 2 and 4.[21] The results are consistent across specifications: more officials are associated with more forced labor. This implies that for local state capacity expansion the extractive channel dominates the administrative channel. Every one percent increase in officials is associated with a 0.71 percent increase in corvée labor days. Moreover, this positive elasticity between officials and corvée runs through the indigenous officials. Panel B shows the results are robust to transforming variables into per capita terms.

Table 1.2 shows the IV results for the three different instruments. Panel A uses all officials, while Panel B uses only indigenous officials. Each specification includes province and year

[21] Since province-years without privately-owned land-based steam power exist, I add one to the amount of steam power to enable the logarithm.

Table 1.1: OLS estimates, corvée and officials

	(1)	(2)	(3)	(4)
Panel A: DV: Log(Days)				
Log(Officials)	0.71***	0.71***		
	(0.17)	(0.16)		
Log(European Officials)			-0.02	0.01
			(0.09)	(0.11)
Log(Indigenous Officials)			0.66***	0.62***
			(0.13)	(0.13)
Adjusted R^2	0.86	0.87	0.86	0.87
Panel B: DV: Log(Days PAM)				
Log(Officials PAM)	0.74***	0.77***		
	(0.20)	(0.18)		
Log(European Officials PAM)			-0.05	0.02
			(0.07)	(0.11)
Log(Indigenous Officials PAM)			0.67***	0.67***
			(0.16)	(0.14)
Adjusted R^2	0.84	0.84	0.84	0.84
Controls	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	576	576	576	576

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

DV means dependent variable. PAM means per indigenous adult male. Controls consist of the log of, respectively, indigenous adult males (not used in panel B), steam power, rice paddies, and wages. Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (18 province clusters, 32 year clusters). Observations are weighted by the number of indigenous adult males. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

fixed effects. Controls are added in columns 2, 4, and 6. For the preferred specifications with controls, the first-stage F-statistics range from 18 to 29, suggesting the estimates do not suffer from the weak instrument problem (Stock and Yogo, 2005). The IV estimates are qualitatively consistent with the OLS estimates, and robust to the choice of instrument, but the impact of officials on corvée labor days is larger: a one percent increase in officials increases corvée labor days by 1.40 to 2.08 percent. Again, this effect runs through the indigenous officials. Table 1.3 reports the IIV lower and upper bounds and the 95% confidence intervals for the same specifications. Since none of the confidence intervals contain zero, the IV results are robust to loosening the conditional excludability assumption.

Both the OLS and the IV results are also robust to using used corvée value as dependent variable (Table A.4 and Table A.5), using wild cluster bootstrap standard errors (Table A.6 and Table A.7), using spatial correction of standard errors (Table A.8 and Table A.9), using no population weights (Table A.10 and Table A.11), and excluding provinces one at a time (results not reported).

Given the results, three main questions require answering. First, what are the mechanisms underlying the positive impact of local state capacity expansion on corvée labor? Second, why are the IV estimates larger than the OLS estimates? Third, how to reconcile the finding of a positive effect with the gradual decline of corvée? To help answer these questions, the relationship between local officials and the revenues of the poll tax, corvée's replacement, needs to be explored first.

Table 1.4 and Table 1.5 contain estimates of the relationship between officials and poll tax revenues. Using OLS, Table 1.4 shows a small positive, but statistically insignificant, relationship between officials and poll tax revenues. Columns 3 and 4 show no strong relationship is detected when separating European and indigenous officials. Using IV, while Table 1.5 shows a weakly significant positive relationship for two of the instruments, this result disappears when only looking at indigenous officials.

Table 1.2: IV estimates, corvée and officials

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A1 Second Stage: DV: Log(Days)						
Log(Officials)	2.08***	1.84***	1.61***	1.40***	1.78***	1.56***
	(0.60)	(0.40)	(0.36)	(0.34)	(0.50)	(0.42)
Panel A2 First Stage: DV: Log(Officials)						
Instrument	-81.99***	-90.87***	-0.03***	-0.03***	-0.08***	-0.08***
	(23.20)	(17.88)	(0.01)	(0.01)	(0.02)	(0.01)
First-stage F	12.49	25.84	22.84	27.11	13.90	29.42
Panel B1 Second Stage: DV: Log(Days)						
Log(Indigenous Officials)	1.57***	1.47***	1.25***	1.14***	1.37***	1.27***
	(0.37)	(0.34)	(0.28)	(0.29)	(0.36)	(0.36)
Panel B2 First Stage: DV: Log(Indigenous Officials)						
Instrument	-108.30***	-114.12***	-0.04***	-0.04***	-0.10***	-0.10***
	(26.89)	(26.33)	(0.01)	(0.01)	(0.03)	(0.02)
First-stage F	16.21	18.79	20.13	19.12	15.12	18.33
Controls	No	Yes	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	576	576	576	576	576	576
Instrument	Distance/Year		Distance/Ships		Distance/Passengers	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

DV means dependent variable. Controls consist of the log of, respectively, indigenous adult males, steam power, rice paddies, and wages. Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (18 province clusters, 32 year clusters). Observations are weighted by the number of indigenous adult males. First-stage F is the Kleibergen-Paap rk Wald F statistic. Regarding the instruments, distance is in kilometers and passengers is in thousands. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

Table 1.3: Imperfect Instrumental Variable (IIV) estimates, corvée and officials

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: DV: Log(Days)						
Log(Officials)	(0.67, 1.99)	(0.65, 1.83)	(0.90, 1.55)	(0.86, 1.41)	(1.01, 1.71)	(1.00, 1.64)
	[0.48, 3.13]	[0.39, 2.83]	[0.53, 2.43]	[0.47, 2.27]	[0.62, 2.63]	[0.61, 2.55]
First-stage F	7.69	15.11	20.44	18.90	16.73	22.09
Panel B: DV: Log(Days)						
Log(Indigenous Officials)	(0.62, 1.78)	(0.58, 1.61)	(0.80, 1.40)	(0.74, 1.25)	(0.89, 1.52)	(0.86, 1.46)
	[0.46, 2.71]	[0.38, 2.45]	[0.50, 2.20]	[0.44, 2.00]	[0.56, 2.37]	[0.54, 2.29]
First-stage F	7.50	12.85	15.40	14.51	13.71	14.43
Controls	No	Yes	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	576	576	576	576	576	576
Instrument	Distance/Year		Distance/Ships		Distance/Passengers	

Estimates based upon Nevo and Rosen's (2012) Imperfect Instrumental Variable (IIV) procedure. First line reports the lower and upper bound. Second line reports the 95% confidence interval's lower and upper bound. Implemented using Clarke and Matta's (2018) `imperfectiv` Stata package. DV means dependent variable. Controls consist of the log of, respectively, indigenous adult males, steam power, rice paddies, and wages. First-stage F is the Kleibergen-Paap rk Wald F statistic. Regarding the instruments, distance is in kilometers and passengers is in thousands. The first line's upper bound and the first-stage F-statistic differ from those reported in Table 2 as several adjustments are lacking here. No population weighting is applied and standard errors are one-way (province) clustered without Driscoll-Kraay and small sample adjustments.

Table 1.4: OLS estimates, poll tax revenues and officials

	(1)	(2)	(3)	(4)
Panel A: DV: Log(Poll Tax)				
Log(Officials)	0.14	0.14		
	(0.23)	(0.22)		
Log(European Officials)			0.06	-0.03
			(0.08)	(0.06)
Log(Indigenous Officials)			0.08	0.09
			(0.20)	(0.20)
Adjusted R^2	0.96	0.96	0.96	0.96
Panel B: DV: Log(Poll Tax PAM)				
Log(Officials PAM)	0.17	0.21		
	(0.18)	(0.19)		
Log(European Officials PAM)			-0.07	-0.06
			(0.09)	(0.08)
Log(Indigenous Officials PAM)			0.12	0.15
			(0.15)	(0.16)
Adjusted R^2	0.83	0.84	0.83	0.84
Controls	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	414	414	414	414

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

DV means dependent variable. PAM means per indigenous adult male. Controls consist of the log of, respectively, indigenous adult males (not used in panel B), steam power, rice paddies, and wages. Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (18 province clusters, 23 year clusters). Since the poll tax was first levied in mid-1882, the time coverage starts at 1883, the first full year. Observations are weighted by the number of indigenous adult males. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

Table 1.5: IV estimates, poll tax revenues and officials

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A1 Second Stage: DV: Log(Poll Tax)						
Log(Officials)	0.94	0.94*	0.55	0.59	0.92	0.94*
	(0.60)	(0.52)	(0.41)	(0.39)	(0.58)	(0.51)
Panel A2 First Stage: DV: Log(Officials)						
Instrument	-105.98***	-109.51***	-0.03***	-0.03***	-0.07***	-0.07***
	(31.48)	(24.99)	(0.01)	(0.01)	(0.02)	(0.01)
First-stage F	11.34	19.20	19.75	18.58	15.36	24.39
Panel B1 Second Stage: DV: Log(Poll Tax)						
Log(Indigenous Officials)	0.70	0.75	0.42	0.49	0.70	0.78
	(0.47)	(0.43)	(0.33)	(0.33)	(0.47)	(0.45)
Panel B2 First Stage: DV: Log(Indigenous Officials)						
Instrument	-142.86***	-136.38***	-0.04***	-0.04***	-0.09***	-0.09***
	(38.00)	(33.57)	(0.01)	(0.01)	(0.02)	(0.02)
First-stage F	14.13	16.51	18.20	15.13	15.21	15.77
Controls	No	Yes	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	414	414	414	414	414	414
Instrument	Distance/Year		Distance/Ships		Distance/Passengers	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

DV means dependent variable. Controls consist of the log of, respectively, indigenous adult males, steam power, rice paddies, and wages. Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (18 province clusters, 23 year clusters). Since the poll tax was first levied in mid-1882, the time coverage starts at 1883, the first full year. Observations are weighted by the number of indigenous adult males. First-stage F is the Kleibergen-Paap rk Wald F statistic. Regarding the instruments, distance is in kilometers and passengers is in thousands. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

Table 1.4 and Table 1.5 show that the economically large and statistically significant relationships between local officials and corvée usage do not exist between local officials and poll tax revenues. These results support the notion that an important part of the rationale behind the poll tax was to restrict local officials', and particularly local indigenous officials', autonomy in setting and allocating taxes. Note that Table 1.1 indicates only local indigenous officials are related to increased corvée usage. Table 1.4 shows the poll tax replacement exactly thwarted such a relationship. Since local officials had much less leeway in setting the level of the poll tax, as compared to corvée, we would expect a smaller coefficient. Finding a coefficient statistically indistinguishable from zero means that the poll tax was successful in limiting local officials' autonomy in setting the level of this type of taxation.

In between the 1882 enactment and the 1914-1916 full replacement, Figure 1.5 also shows little annual variation in poll tax revenues. As intended with a uniform one guilder poll tax charge per laborer, poll tax revenues mainly tracked population growth and left less leeway for local officials, be they European or indigenous. Even local changes in the distribution of the poll tax burden across villages or individuals first had to go through a bureaucratic procedure requiring the approval of the province's top European official, the *resident* (Departement van het Binnenlandsch Bestuur, 1918). In other words, the poll tax was partly used as a tool of fiscal centralization. Another way of gauging this is to estimate the relationships that European and indigenous officials had with corvée usage before and after the 1882 poll tax implementation.

Table 1.6 reports estimates of regressions of used corvée days on officials in which the variables of interest are interacted with a poll tax dummy that switches on in 1883, the first full year of the poll tax after implementation in mid-1882. For all local officials together, although the point estimate on the interaction term is negative, columns 1 and 2 indicate no statistical evidence for a post-1882 change in the positive relationship between officials and corvée. Separating the local officials into European and indigenous officials, as in columns

Table 1.6: OLS estimates, corvée and officials with poll tax dummy interaction

	(1)	(2)	(3)	(4)
DV: Log(Days)				
Log(Officials)	0.78***	0.76***		
	(0.19)	(0.17)		
Poll Tax=1 × Log(Officials)	-0.08	-0.10		
	(0.15)	(0.13)		
Log(European Officials)			-0.10	-0.17*
			(0.08)	(0.10)
Poll Tax=1 × Log(European Officials)			0.18***	0.18***
			(0.06)	(0.05)
Log(Indigenous Officials)			0.94***	0.91***
			(0.14)	(0.15)
Poll Tax=1 × Log(Indigenous Officials)			-0.43**	-0.51***
			(0.16)	(0.16)
Controls	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	558	558	558	558
Adjusted R^2	0.86	0.87	0.87	0.88

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

DV means dependent variable. Controls consist of the log of, respectively, indigenous adult males, steam power, rice paddies, and wages. Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (18 province clusters, 31 year clusters). Observations are weighted by the number of indigenous adult males. The year 1882 is omitted as the poll tax was implemented in mid-1882. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

3 and 4, shows that this apparent overall lack of change hides two countervailing changes. While European officials' relationship with corvée becomes more positive, indigenous officials' relationship becomes less positive. Importantly, after the poll tax the strength of the relationship with corvée is weakened for both European and indigenous officials. In column 4, the preferred specification, European officials' coefficient estimate changes from -0.17 to 0.01 and indigenous officials' coefficient estimate changes from 0.91 to 0.40. While the changes are in opposite directions, in absolute size both coefficients are smaller after the poll tax is implemented.

This is further evidence for the poll tax aiding fiscal centralization. Not only is there no relationship between local officials and poll tax revenues, after the poll tax is implemented the relationship local officials had with corvée usage is also weakened. The explanation for this lies in poll tax regulations explicitly replacing certain corvée duties with wage labor paid for by poll tax revenues. For example, besides replacing personal duties to indigenous officials with salary raises, the 1882 poll tax implementation also reduced the maximum number of days any laborer could be made to provide corvée from fifty-two to forty-two and abolished several other corvée duties, such as those regarding building and maintaining prisons and transporting officials (KV 1882, p. 72). In an effort to spread knowledge of these changes to the corvée laborers, European officials were instructed to publicize the changes at meetings with indigenous chiefs and village councils and to ensure that several corvée laborers themselves were present too (KV 1882, p. 73). Use of corvée for purposes that were now prohibited also was to be punished. For example, a district head and a sub-district head were fired for such transgressions while another sub-district head was put on trial (KV 1884, p. 70). After the reductions and replacements local officials thus had less avenues to increase corvée duties, hence the weakened relationship after 1882.

What then are the mechanisms underlying the positive impact of local state capacity expansion on corvée labor? Between 1874 and 1905 the number of officials doubled while

the indigenous population grew by about 60%. The density of officials therefore increased. Throughout this period declarations from Batavia instructed provincial officials to reduce usage of corvée labor (e.g. KV 1895, p. 98). Such instructions were passed on down the bureaucratic hierarchy, but in the end it was up to local officials to decide whether to use coerced labor or wage labor to accomplish their goals. If no wage labor was available for hire at the wage the local official was willing to pay, then he could resort to corvée labor. Given such an opportunity, and considering the tight budgets many officials operated with, coercion was often preferred. By its nature, as a tax in the form of labor, corvée also had limited room for spatial and temporal transfers, particularly in comparison to monetary taxation. While corvée duties tended to be restricted to projects in and around the laborers' and officials' villages, poll tax revenues could be more easily siphoned off by the center. This indeed happened with the net poll tax revenues that flowed into Batavia's coffers. By the early 1900s such accumulated net poll tax revenues since 1882 summed up to about six million guilders, equivalent to more than two years of average annual poll tax revenues (Fokkens, 1914). While the central authorities thus benefited from phasing out corvée, local authorities saw benefits in continuing, and even expanding, corvée usage.

Corvée labor also built on pre-colonial tax practices and was enmeshed in local patronage systems in which officials could decide on who had to perform the labor. Individual exemptions were handed out, presumably in return for favors, increasing the burden on the remaining laborers (KV 1874, p. 77). Contemporaneous observers also stressed another advantage: corvée labor can immediately be used to solve urgent local problems (Rose, 1879). If heavy rains washed away roads or damaged irrigation networks, for example, the local state official could appoint villagers to come out for emergency repairs without being constrained by limited supply of wage labor, by lack of funds to hire wage labor, or by slow bureaucratic procedures. Along with the decline of corvée, local officials partly lost such flexibility. The 8% collector's fee officials received for collecting the poll tax supports the notion that these officials lost something of value to them with the phasing out of corvée labor and that they

needed to be compensated in some fashion in order to implement the new system.

In short, increasing the number and density of state officials enlarged both the abilities and the ambitions of the state. Corvée labor was a flexible lever for local officials to use in accordance with their preferences. In and around provincial capitals, for example, laborers often had to work a number of days approaching their legal maximum (KV 1882, p. 74). The province of Banjoemas experienced a particularly egregious example of this in the 1870s when so many officials were located in the sparsely-populated capital district that most of the allowed corvée days of local villagers were spent on personal duties to certain indigenous officials. Corvée laborers from the neighboring district subsequently had to be called in to complete the required maintenance of roads and bridges. Problematically, many of these workers had to walk more than twenty kilometers to arrive at their designated work places (KV 1873, p. 95). This was in defiance of the central bureaucracy in Batavia which circumscribed a maximum work radius around a worker's village of about twelve kilometers. The higher burden in and around places with a high density of state officials also led to the use of convict laborers in such places to lighten the labor imposition on villagers (KV 1896, p. 82).

Given the positive coefficient estimates, why are the IV estimates larger than the OLS estimates? I argue two main potential reasons underlie this difference. First, there could be negative bias in the OLS estimates due to the bureaucracy being expanded where the state intends to modernize taxation. Second, the IV estimates could be larger than the OLS estimates due to heterogeneous treatment effects. Table 1.4 and Table 1.5 indicate that the first explanation is likely not highly relevant. Nonetheless, the small positive coefficients do indicate a weak positive relationship between local officials and poll tax revenues, thus indicating there might be minor negative bias in the OLS estimates due to this.

A more likely reason, however, lies in heterogeneous treatment effects making the IV estimator identify a weighted average of local average treatment effects (LATEs) instead of the

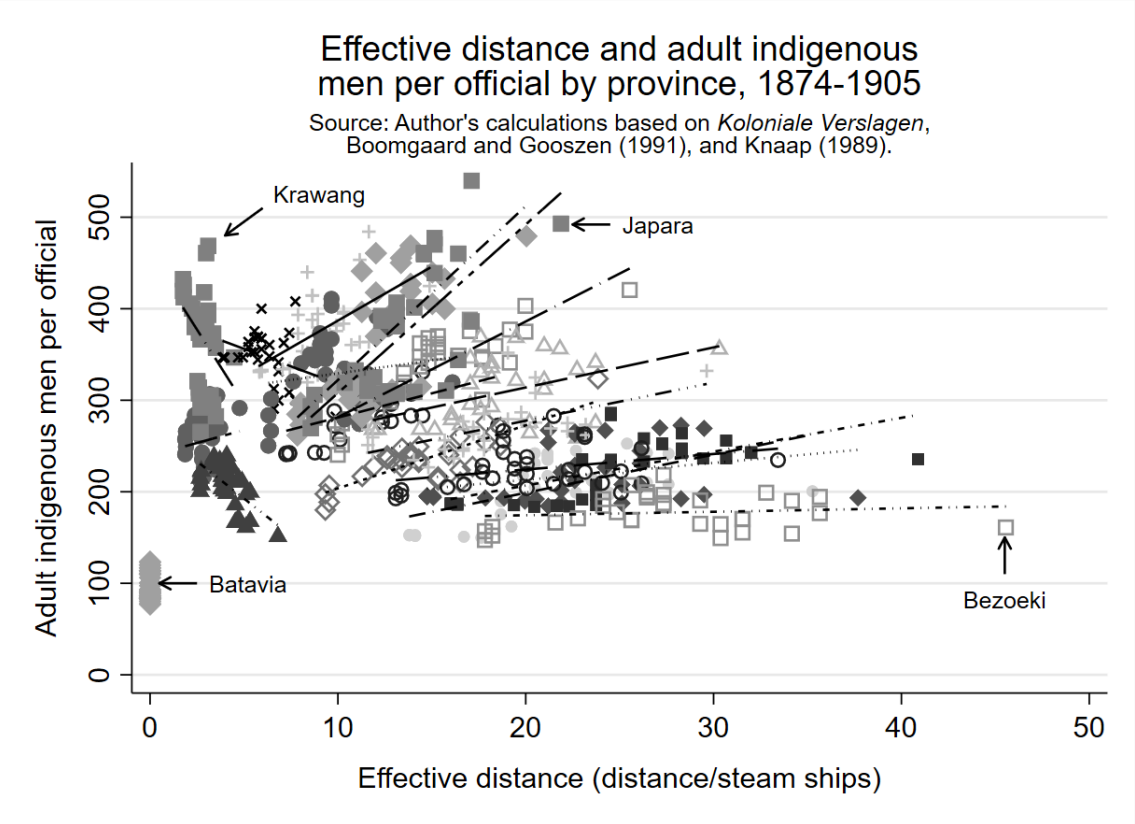

Figure 1.8

average treatment effect (ATE) (Imbens and Angrist, 1994). Shrinking effective distance could mainly impact relatively faraway provinces. For example, provinces near the capital Batavia are well-served by 'old' technologies (e.g. horse and carriage), while the faraway provinces become much closer due to the introduction and spread of new technologies (e.g. steam ships, railroads, telegraph communications). As such new technologies mainly reduce the costs of stationing officials far from the center, they chiefly affect state capacity in distant places. Moreover, adding officials in distant places might have a relatively large positive impact on corvée labor as monitoring and control by the center is still less than in places near the capital.

Shrinking effective distance mainly stimulating official density in relatively faraway provinces is corroborated by Figure 1.8 which, for each province, graphs the number of adult indigenous men per official and effective distance as measured by the ratio of distance and number of

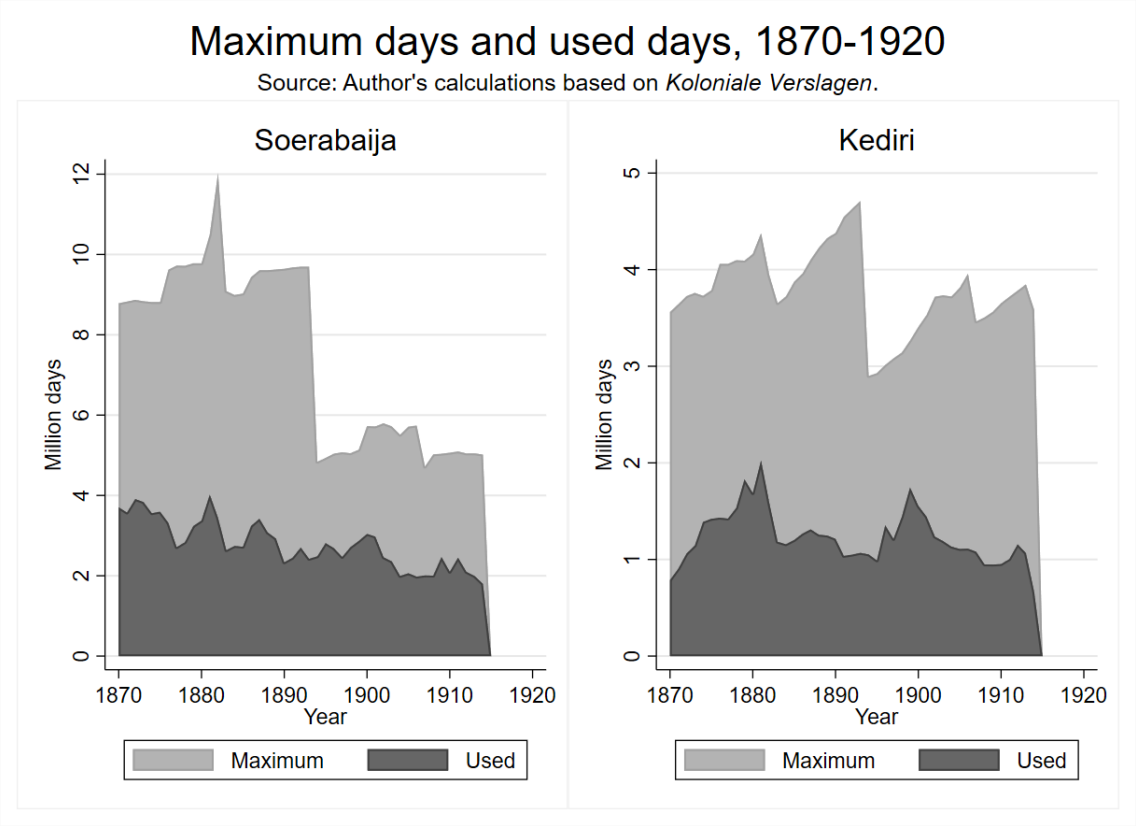

Figure 1.9

steam ships. As effective distance shrinks, provinces move leftward on the graph. As the number of adult indigenous men per official falls (i.e. density of officials increases), provinces move downward. All provinces move leftward, but not all provinces move downward. Figure 1.8 shows that the faraway places mainly show increasing (e.g. Japara) or constant (e.g. Bezoeki) official density as distance shrinks, while the only three provinces that experience decreasing official density (e.g. Krawang) are relatively close to the capital. In these latter three provinces growth in population outpaced growth in the number of officials.

Lastly, how to reconcile the finding of a positive effect with the gradual decline of corvée? As illustrated by Figure 1.2 and Figure 1.5, the use of corvée labor decreased and it was eventually completely phased out. However, most of this decline was common across the provinces and is picked up by the year fixed effects. There was thus a general shift out of corvée labor, but this was not directly due to the expanding local bureaucracies. Rather, this

was due to national state policy both boosting and adjusting to such changes as deepening monetization, expanding trade opportunities, and spreading industrialization while at the same time purporting to reel in local officials and enhancing central control over taxation. Including year fixed effects enables disentangling the national-level push towards abolishment from the local-level push towards increased usage. Table A.2 and Table A.3 report OLS and IV estimates without including year fixed effects. Without year fixed effects and controls, the relationship between officials and corvée is negative and statistically significant in certain specifications. When including controls most of these estimates become statistically indistuingishable from zero. Not including year fixed effects thus entails the countervailing national-level and local-level pushes cannot be distinguished from each other, leaving a net zero effect.

From the early 1890s on the central authorities viewed the labor duties as temporary, to be abolished once local conditions allowed for further poll tax increases to replace the remaining corvée (KV 1894, p. 78). Batavia, in conjunction with the top provincial officials who were deeply enmeshed in the European bureaucracy, periodically reduced the maximum number of days local officials could extract per conscript. However, such legal changes did not always have an impact on the actual use of corvée labor. Figure 1.9 illustrates this by graphing maximum days and used days for the provinces Soerabaija and Kediri. Maximum days in both these provinces were decreased in the 1890s, but used days did not respond. Ultimately, Batavia used a poll tax hike to abolish corvée on Java to get rid of the remaining nearly thirteen million days still being used by 1913. In the end, the tendency of local officials to use corvée labor could only temporarily stem the tide towards full replacement with monetary taxation. Along the path of fiscal modernization they lost some power and prestige and became more dependent on the center for transfers of funds.

1.6 Conclusion

Using a newly-constructed province-level database for colonial Java, this study makes visible the importance of corvée labor, shows corvée's gradual phaseout, and finds that different parts of the state had countervailing effects on corvée usage. The downward trend in corvée usage was due to national policy actively stimulating the reduction of corvée labor and its replacement by a poll tax. Such monetary taxation enhanced central control over both the collection and the allocation of revenue as local officials, who had significant leeway regarding the level and use of corvée, were more constricted regarding the poll tax. Such a replacement, however, was conditional on sufficient information-collection capabilities of the state as well as productivity and monetization of local economies. The phasing out of labor duties was thus a gradual process. By 1916 the conditions were sufficiently met to enable the complete abolishment of the remaining corvée labor on Java.

Over the same time period, local state capacity expansion increased the use of corvée labor. A one percent increase in local state officials increased corvée labor use by more than one percent. This effect runs through indigenous officials, who were more rooted in local power structures, not through European officials, who aligned more with the national-level colonial bureaucracy. In short, the interests of central state actors did not always coincide with those of local state actors regarding the revenue mix of the colonial state. State capacity expansion at the national level induced movement out of corvée labor while state capacity expansion at the local level simultaneously slowed the shift away from corvée labor. National state actors favored centralization of taxation and viewed as key the replacement of labor duties with monetary taxation. Local state actors protected their position in the system by increasing the form of state-backed extraction over which they had most discretion: labor duties.

The tensions between local and national state actors are key in understanding the countervailing forces underlying this facet of fiscal modernization and indicate that increasing state

capacity need not necessarily induce fiscal modernization. The relationships between state capacity and taxation levels, between state capacity and fiscal modernization, may depend on the internal workings of the state bureaucracy and on the type of state capacity expansion. Moreover, increasing monetary tax revenues may hide falling in-kind tax revenues. Alternatively, increasing monetary tax revenues for the central state may hide falling in-kind tax revenues for local state actors. Measurements of tax revenues per capita that solely rely on taxes that end up in central coffers may thus be problematic in certain settings. The historically widespread use of corvée labor and other in-kind taxes helps to give this study's findings wider resonance. Opening the black box of state capacity and analyzing specific actors within the state is thus important to understand the co-evolution of state capacity, public labor coercion, and fiscal modernization.

Chapter 2

Labor Coercion and Trade: Evidence from Colonial Indonesia

2.1 Introduction

Throughout human history coercive labor relationships – relationships in which the threat or use of force is key – constituted a major part of labor transactions. Such coercion took many different forms: slavery, serfdom, indentured labor, penal contracts, conscription, prison labor, and corvée labor, for example. Coerced labor was arguably more important than 'free' wage labor in many settings such as (plantation) agriculture, shipping, armies, and public works. Today about twenty-five million people are still subject to forced labor (ILO, 2017). What then determines the use of labor coercion?

Due to the hidden nature of much coercion in modern times, rigorous evidence on the determinants of coercion is scarce. To circumvent such data availability issues, this paper studies a legal form of labor coercion: corvée labor in colonial Indonesia. Corvée labor is a form of taxation in labor in which individuals work for the state without pay for several

weeks or months each year. Indonesian corvée labor was mainly imposed on adult indigenous males for building and maintaining roads, bridges, and irrigation works. Minor tasks involved piloting ferries, escorting state officials and prisoners, delivering letters, building roadside shelters, and transporting government goods.

This type of coercion was historically widespread. For example, major projects such as the Great Pyramid of Giza and the Great Wall of China were partially built by corvée labor, and deep into the twentieth century corvée labor remained in use in China as well as European colonies throughout Africa and Asia (Bernstein and Lü, 2003; Frankema and Van Waijenburg, 2014; Van Waijenburg, 2018; Chapter 1 (Hup, 2021a)). Moreover, corvée labor is still practiced today in certain developing countries (Olken and Singhal, 2011; ILO, 2017)

Corvée labor is one of the least information-intensive forms of taxation. For example, in comparison to a poll tax, another tax with low information requirements, a corvée labor tax does not require the state to have information on local currency usage and earning capacity. Furthermore, a corvée tax can be levied in economies characterized by low levels of productivity and monetization. From the state's vantage point, however, tax payments in labor also suffer from several disadvantages as compared to tax payments in money in terms of fungibility, storability, and portability. The literature views low levels of labor productivity, currency usage, and state capacity as factors limiting states in shifting towards more information-intensive monetary taxation (e.g. Brewer, 1989; Besley and Persson, 2011, 2013, 2014; Johnson and Koyama, 2014, 2017; Dincecco, 2015). Such shifting is an important aspect of strengthening fiscal capacity, which in turn is a key aspect of long-run development (e.g. Dincecco, 2009; He, 2013; Vries, 2015; Dincecco and Katz, 2016).

This paper studies the impact of trade on corvée labor. I focus on trade because large global price shocks, and resulting changes in local production quantities, aid identification of the impact of productivity changes. I focus on Indonesia due to its large size and unique data availability. A lack of large-scale corvée labor registers have limited empirical studies of this

key form of labor coercion. I overcome this limitation by assembling the first dataset on corvée labor usage and exports. This new province-level dataset for colonial Indonesia spans sixteen provinces and forty-one years (1900-1940). It includes annual data on province-level corvée usage and, from 1920 onward, counts of laborers that bought off their corvée duties. This corvée data is combined with annual data on province-level product-specific exports. To the best of my knowledge, this combination of data is only available for colonial Indonesia due to two peculiarities: (i) Dutch colonial rules on record keeping of corvée usage, and; (ii) the island geography of Indonesia with each non-Javanese province having its own harbors and therefore its own customs collection points with attendant record-keeping archive. I use contemporary Colonial Reports and trade overviews to manually construct this database covering hundreds of thousands of laborers and dozens of different export products.

To guide the empirics, I build a simple political economy model of taxation choice that embeds a trade-off between taxation in labor and taxation in monetary currency. Corvée labor usage reduces the labor available for private production, hence monetary tax revenue based on private production drops. The more productive a laborer is in private production, or the higher his output price is, the higher the opportunity costs of corvée labor are for both the state and the laborer. This encompassing interest, which stems from the coercer being the state rather than a private employer, differs from Acemoglu and Wolitzky's (2011) seminal model of private labor coercion. The opportunity costs of corvée labor induce the state to limit its usage, even though corvée labor is nominally free, and make corvée usage sensitive to productivity changes. If laborers have a high marginal revenue product, the state prefers not to use corvée and instead tax laborers' output. A more productive laborer also prefers to work and would choose a contract that allows them to pay a monetary tax instead of corvée. The model thus predicts a negative relationship between labor productivity and labor coercion in the form of corvée.

A trade boom is such a period of high marginal productivity. I hypothesize that an ex-

port boom, especially in labor-intensive goods, reduces corvée labor use. This effect can run through two channels: (i) higher opportunity costs for the state, and; (ii) higher outside options for the laborers. While the state might want to reduce corvée impositions on productive laborers, it faces an information problem. It cannot accurately gauge the productivity of all the hundreds of thousands of laborers liable to corvée. The so-called 'ransom' option solved this by allowing laborers to buy themselves out of corvée duties. The buy-out option meant the state could rely on laborers' private information. More productive laborers faced higher opportunity costs of being submitted to corvée and would thus be more willing to pay off such duties. By allowing laborers to self-select out of corvée, the state reduced its information-collection costs while still achieving a shift from in-kind to monetary taxation. Furthermore, the buy-out option enabled corvée usage to flexibly adjust to the waxing and waning of economic opportunities.

To gauge the effect of productivity changes on corvée usage, I use variations in province exports. Part of the variation in exports is driven by price shocks, and these price shocks are exogenous from the perspective of individual laborers who make the ransom decision. As the size and composition of export goods production differed across provinces, provinces were differentially affected by changes in world market prices. Given the many products there are many different annual price shocks, so the identification can leverage exogenous variation in shocks even when exposure is potentially endogenous (Borusyak, Hull and Jaravel, 2019).

I decompose used corvée days into three parts to separate corvée usage into two variables (liable persons and maximum days per liable) which were mainly affected by governmental policy choices and one variable (used days as a share of maximum days) which was mainly affected by ransom decisions of individual laborers. I regress the log of used days per capita, and each component separately, on the log of exports per capita and province and year fixed effects. To mitigate endogeneity concerns, I construct a measure of potential exports by multiplying lagged quantities with current prices across dozens of products.

The results show that trade expansions, especially of labor-intensive exports, reduce corvée usage. In particular, the boom in exports in the late 1910s and early-to-mid 1920s can nearly fully explain the large drop in corvée usage. Moreover, this effect runs through laborers buying themselves out of their corvée duties. The fall in in-kind taxation is thus mirrored by a rise in monetary taxation. The number of people buying themselves out of corvée grew throughout the 1920s trade boom and peaked in 1929 at nearly one million ransomers. The opposite took place during the trade collapse of the Great Depression as the number of ransomers dropped rapidly.

The finding of a negative relationship between trade and labor coercion, particularly between labor-intensive exports and coercion, is perhaps surprising as it stands in contrast to studies that focus on coercion in the form of slavery (e.g. Saleh, 2020). I argue that private labor coercion and public labor coercion differ due to the state's encompassing interest which meant that corvée reductions implied higher tax revenues. In contrast, labor coercion in private production tends to increase with trade as trade opportunities increase the employers' returns to such coercion. The nature of the relationship between coercer and coerced, as well as the purpose of coercion, thus matter for the relationship between trade and coercion. While trade booms may increase coercion in slaveholder-slave relationships, such booms can reduce coercion in state-taxpayer relationships. The flipside is that trade busts can increase state coercion.

This paper is the first to empirically investigate the relationships between fiscal modernization, corvée, and trade, and by doing so principally contributes to three literatures. First, by documenting the importance and patterns of corvée labor it contributes to the literature on labor coercion. While labor coercion is often studied in private and semi-private forms such as slavery, serfdom, and penal contracts (e.g. Nieboer, 1900; Domar, 1970; Nunn, 2008; Dell, 2010; Acemoglu and Wolitzky, 2011; Naidu and Yuchtman, 2013; Markevich and Zhuravskaya, 2018; Buggle and Nafziger, 2020; Dippel et al., 2020; Saleh, 2020), corvée labor is

relatively understudied (e.g. Okia, 2017; Van Waijenburg, 2018; Chapter 1 (Hup, 2021a)). The historically widespread use of corvée, and the conceptual differences between private and public labor coercion, make understanding the rise and fall of corvée fundamental for understanding labor coercion more broadly. Furthermore, while corvée labor and certain forms of labor coercion, such as prison labor, are still in use around the world today, data is often scarce. Studying historical episodes with accurate data can thus aid our understanding of what drives labor coercion.[1]

Second, by estimating the impact of trade on corvée usage this paper contributes to the literature on trade and economic development (e.g. Acemoglu, Johnson and Robinson, 2005; Findlay and O'Rourke, 2007). Moreover, the newly-constructed dataset on annual province-product level exports for all Outer Island Indonesian provinces in the period 1900-1940 has many potential applications. As booms and busts in trade remain key features of the world today, learning from major historical events, such as the Great Depression trade collapse, remains important for better grasping what drives development.

Third, by pinpointing labor productivity as an important rationale for the colonial Indonesian state relying on corvée differentially across time and space, the paper contributes to the literature on public finance and fiscal modernization. High information-collection costs are often seen as a barrier for moving towards centralized monetary taxation, both historically and currently, and partially explain the usage of solutions such as tax farming and fiscal decentralization (e.g. Toma and Toma, 1992; Kiser, 1994; Coşgel and Miceli, 2009; Besley and Persson, 2014; Johnson and Koyama, 2014; Khan, Khwaja and Olken, 2016). To understand how a shift to higher monetary taxation was achieved in Indonesia, without requiring further information-collection capabilities of the state, is therefore important. The new dataset also clarifies the potential importance of non-monetary taxation in settings such as colonial Indonesia. As emphasized by Olken and Singhal (2011), studies on taxation and

[1]Similarly, historical data can help shed light on the consequences of labor coercion (e.g. Poyker, 2019).

fiscal capacity should thus be careful not to miss this type of taxation.

The next section builds a simple model of public labor coercion. The third section sketches the context of corvée labor in the Indonesian Outer Islands. The fourth section introduces the data. The fifth section explains the methodology. The sixth section discusses the results. The final section concludes. The appendix briefly describes the data construction and contains additional regression tables.

2.2 Model

Acemoglu and Wolitzky (2011) describe two seminal theories linking labor productivity with labor coercion through labor scarcity. Domar (1970) claims that labor productivity and labor coercion are positively related. This channel works through labor scarcity: where labor is scarce, the marginal product of labor high, so labor input is expensive and output prices are high. This induces employers to use coercion to on the one hand reduce costs and on the other increase output in order to benefit from high prices. In contrast, the neo-Malthusian theory (e.g. North and Thomas (1971)) postulates the opposite: where labor is scarce, laborers' outside options are high, reducing employers' power and their use of coercion. These two theories help explain different historical scenarios. The price effect as identified by Domar explains the import of slaves and indentured labor to relatively labor-scarce areas such as Brazil, the Caribbean, and the southern United States, while the outside options effect as identified by the neo-Malthusians explains the decline of feudalism in Western Europe after the Black Death and the use of coercive institutions in European colonies with abundant labor. Acemoglu and Wolitzky (2011) unite these theories into one where either effect can dominate depending on the direct effect of labor scarcity on the difference between the output price and the outside option.

Different forms of labor coercion have been used throughout history. While labor coercion is often studied in private and semi-private forms such as slavery, serfdom, and penal contracts (e.g. Nieboer, 1900; Domar, 1970; Nunn, 2008; Dell, 2010; Acemoglu and Wolitzky, 2011; Naidu and Yuchtman, 2013; Markevich and Zhuravskaya, 2018; Buggle and Nafziger, 2020; Dippel, Greif and Trefler, 2020; Saleh, 2020), public labor coercion in the form of corvée is less studied (e.g. Okia, 2017; Van Waijenburg, 2018; Chapter 1 (Hup, 2021a)). This type of public labor coercion was, however, historically widespread. Large public works such as the Great Pyramid of Giza and the Great Wall of China, for example, were partially built by corvée labor. Over time, corvée labor was often replaced by monetary taxation. Corvée labor was gradually phased out in Europe and the United States in the eighteenth and nineteenth centuries, but in other places, such as French Africa and the Dutch East Indies, it persisted until at least the 1940s.

The models considered by Domar (1970), North and Thomas (1971), and Acemoglu and Wolitzky (2011) need to be modified for corvée labor because such labor involves the state as coercer. Unlike private employers, the state has an encompassing interest in the economy's output as it taxes in various ways. This implies the state considers the opportunity costs of using coercion even though corvée labor is nominally free of charge. If people have to perform corvée labor on roads and canals for a month, then they cannot produce their usual economic output. As the state taxes such output through taxes on production and exports, the state faces an opportunity cost of reduced monetary taxation by using corvée labor. Throughout history and across countries an important aspect of fiscal modernization has been the phasing out of corvée labor in favor of monetary taxation. What stimulates such a shift?

The opportunity costs of corvée labor limit the state's usage of corvée. If laborers are highly productive in private activity, then the state would rather not levy labor duties and instead tax the fruits of private activities through production and export taxes. The state can

incorporate this feature by allowing laborers to pay off their duties. The colonial state in Indonesia explicitly built such a buy-out, or 'ransom', feature into their corvée labor system. This uses the individual's information on his marginal labor productivity in private activity. In periods of high marginal productivity corvée labor would thus be low, and vice versa.

Consider a simple perfect information model in which a state taxes both a person's output and the person's labor directly through corvée duties. The state aims to maximize its total utility from both forms of taxation. If the state uses corvée labor, the involved laborers have less time available for private production, hence output and monetary tax revenue drop. For purely non-altruistic reasons the state thus faces a trade-off between monetary taxation and corvée labor. Moreover, a higher tax rate causes a deadweight loss and the state is limited in setting its tax rate and forced labor rate by the laborers' subsistence, or no-revolt, constraint. How do the optimal tax rate and corvée labor rate respond to private output price shocks?

The state chooses the forced labor rate f and the tax rate τ and maximizes

$$\max_{f,\tau} \quad \tau r(\tau) pA(1-f) + h(f) \tag{2.1}$$

subject to the subsistence constraint

$$(1-\tau) r(\tau) pA(1-f) - s \geq 0 \tag{2.2}$$

Where: $r(\tau)$ = percentage of output produced for a given τ; p = price of laborer's private output; A = productivity of laborer's private labor input; $h(f)$ = utility from corvée labor for a given forced labor rate f, and; s = laborer's subsistence consumption. Note that the value of the laborer's output depends on the deadweight loss, the output price, the laborer's private productivity, and the forced labor rate. A higher output price thus increases the laborer's total output value, for any given labor input, and can be interpreted similarly as a

productivity shock.

Price p, productivity A, and subsistence consumption s are all strictly greater than zero. Both f and τ are rates bounded between zero and one, hence $0 \leq f \leq 1$ and $0 \leq \tau \leq 1$. I assume diminishing marginal product to corvée labor: $h'(f) > 0$ and $h''(f) < 0$. This captures the tendency for the initial projects to be built, maintained and guarded by corvée labor to strongly benefit the state (e.g. the first road allowing for vehicular traffic, the first dam enabling a regulated irrigation network), whereas additional projects run into diminishing returns. The $r(\tau)$ term is bounded between zero and one, $0 \leq r(\tau) \leq 1$, with $r(0) = 1$ and $r(1) = 0$. Following McGuire and Olson (1996), I assume higher tax rates to be more incentive-distorting: $r'(\tau) < 0$ and $r''(\tau) < 0$. The $r(\tau)$ term can be interpreted as a measure of fiscal capacity where low-capacity states face a lower $r(\tau)$, for any τ, as compared to high-capacity states.

Since the population in the provinces investigated in this study was steadily growing in the first decades of the twentieth century, the inhabitants were likely not living exactly at subsistence level.[2] I therefore focus on a non-binding constraint. Under a non-binding constraint, the first-order conditions are:

$$\tau^* = -\frac{r(\tau^*)}{r'(\tau^*)} \tag{2.3}$$

$$h'(f^*) = \tau^* r(\tau^*) pA \tag{2.4}$$

Equation (3) indicates the optimal tax rate is independent of price changes. Equation (4) indicates the optimal forced labor rate responds negatively to increases in price, labor productivity, and fiscal capacity (i.e. higher $r(\tau)$ for any τ, in other words a smaller distortionary impact for any level of monetary taxation).[3] Note that while the tax rate is independent of

[2]There is, however, conflicting evidence on whether health outcomes were improving (Baten, Stegl, and Van der Eng, 2013; Foldvari et al., 2013).

[3]The second-order condition for f^* to give a maximum is that $\frac{d[-\tau r(\tau) pA + h'(f)]}{df} < 0$ when evaluated at

price changes, tax revenues are not.

What are potential mechanisms underlying the comparative statics? This paper focuses on trade-related price shocks as important factors impacting corvée labor. The data section (see Figure 2.2) shows a sharp drop in corvée usage during the post-World War I trade boom followed by a sharp rise around the onset of the Great Depression. This suggests world market price changes, rather than slow-moving changes in fiscal capacity or labor productivity, were the main factors underlying these abrupt fluctuations.

The model assumes the state knows the laborer's private output value which is a combination of price, productivity, and labor input. In reality the state did not know the private output potential of each of the hundreds of thousands of persons liable to corvée. Due to this imperfect information it was difficult for the state to optimize its forced labor usage. The ransom system was exactly designed to remedy this lack of central information. As individuals had a more accurate notion about their own potential production value they would be willing to buy off, to ransom, their corvée duty if the cost of doing so was below their expected private production value. The ransom system thus enabled highly productive individuals to self-select out of corvée. This benefited the state as using such individuals for corvée had high opportunity costs in terms of lost output and lost tax revenues. The ransom system also benefited the involved laborers as evidenced by many laborers deciding to buy off their corvée duties. The ransom system thus aligned incentives between the state and the individuals liable to corvée. Moreover, it allowed for flexible adjustments across individuals and across space and time as economic opportunities waxed and waned.

While the ransom system utilized individuals' information, the state also used two direct policy levers, the number of liable and the maximum days per liable, to affect corvée us-

f^*. The second derivative works out as $h''(f)$ and by assumption $h''(f) < 0$. The second-order condition for τ^* to give a maximum is that $\frac{d[r(\tau)pA(1-f)+\tau r'(\tau)pA(1-f)]}{d\tau} < 0$ when evaluated at τ^*. The second derivative works out as: $\frac{d[r(\tau)p(1-f)+\tau r'(\tau)p(1-f)]}{d\tau} = 2r'(\tau) + \tau r''(\tau)$. By assumption $r'(\tau) < 0$ and $r''(\tau) < 0$, so the second-order condition for τ^* to give a maximum is satisfied too.

age. These direct levers were less targeted to individuals and more intended as broad-brush measures. The maximum days per liable was gradually reduced across the four decades spanning 1900 to 1940 (see Figure 2.8). For most provinces the number of liable was not majorly adjusted and mainly tracked population growth (see Figure 2.10). For the small island provinces of Belitung and Bangka, however, corvée labor was fully replaced by a poll tax in 1919 and 1920, respectively. On those two islands the number of liable was thus reduced to zero. A similar replacement occurred on Java between 1914 and 1916 (Chapter 1 (Hup, 2021a)). Such broad-brush measures can be seen as adjustments to general improvements in economic development, fiscal capacity, and labor productivity.

In short, the model helps explain a major pattern in the data: a post-World War I trade boom concurrent with a corvée collapse, and a Great Depression trade bust concurrent with a corvée resurgence. The next section sketches the context of corvée labor in the Indonesian Outer Islands.

2.3 Background

This study focuses on Indonesian provinces outside Java, known collectively as the Indonesian Outer Islands, between 1900 and 1940. Figure 2.1 displays the Indonesian Outer Island provinces in 1940. Outer Island corvée labor was managed under two different institutional arrangements which can be roughly classified as direct-rule corvée and indirect-rule corvée. Direct-rule corvée was tracked by the colonial state through its officials. Indirect-rule corvée, in contrast, was without direct oversight from the colonial bureaucracy. Direct-rule corvée labor could be further divided into two kinds: (i) labor for the central state (*heerendiensten*), and; (ii) labor for the village (*dessadiensten*). Due to data availability constraints this paper focuses on direct-rule corvée for the central state.

Figure 2.1

While the specifics of such corvée labor differed across provinces and across time, several general facts can be outlined. Corvée labor was mainly used for building and maintaining roads, bridges, and irrigation works. Lesser tasks involved piloting ferries, escorting state officials and prisoners, delivering letters, building roadside shelters, and transporting government goods. Over time the type of tasks that corvée laborers could be assigned was reduced (e.g. IV 1932, p. 42).

In general, every adult and able indigenous male was subject to corvée duties. Exceptions were made for persons of certain stature (e.g. village heads) and profession (e.g. school teachers). The laborers were not paid, except for certain tasks and in certain cases where a compensation of less than half the usual daily wages was paid. The number of days laborers could be required to work was capped at a given maximum per year. While this maximum differed across individuals, across provinces, and across time, the supremum was set at fifty-two days a year (i.e. one day a week). Initially, a work day lasted for twelve hours and included travel to the work site. The maximum distance of the work site to the laborer's village was officially capped at lengths varying from ten to thirty kilometers (Hamerster 1918, p. 838), although exceptions could be requested and the maximum was sometimes exceeded.

Laborers were generally not provided with transport or food. Over time restrictions increased on the maximum number of days per year, hours per day, distance to the work site, and who was liable to perform corvée labor (IV 1932, p. 43).

Laborers could hire substitutes to perform their corvée duties for them. Relatively well-off laborers presumably usually did so. Laborers could also directly pay off, or 'ransom', part or all of their duties to the local officials. At first this was only allowed in certain places, but in 1918 a general rule enabled it everywhere (KV 1919, p. 131). Such ransoming could be done individually or as a whole village. Village officials collected the ransom and kept eight percent as compensation.

The indigenous village officials were also responsible for the implementation and supervision of the major part of corvée duties. The local European official, the *controleur*, was responsible for coordinating the duties across the multiple villages in his catchment area. While official policy aimed to even the burden of labor duties across villages and individuals, in practice the burden could differ widely. For some individuals the actual burden even exceeded the maximum legal burden (Liefrinck 1917, p. 22). Factors such as natural events (e.g. landslides, rainstorms), distance (e.g. requiring longer travel to work sites), corruption (e.g. certain individuals being unlawfully exempted), and expanding state presence (e.g. requiring more infrastructure construction), all underlay such inter-village and inter-individual differences (e.g. Vervolg 1914, pp. 204-5, 211; Liefrinck 1917, p. 24; Hamerster 1918, p. 1013).

Productivity was another major factor underlying differences in corvée burdens. In the province of East Sumatra, for example, where export crop production was a key sector, ransoming of duties was already common in the early 1900s. A government report stated that the "extent of ransoming can be viewed as a measure of development" (KV 1920, p. 46). In the island provinces of Belitung and Bangka, where tin and pepper exports were major parts of the local economies, corvée was completely replaced with a poll tax in 1919 and

1920, respectively. Similar arrangements were already implemented in the provincial capitals of Bengkulu and Celebes in the nineteenth century (IV 1931, p. 73). Such replacements followed an earlier pattern established in Java (Chapter 1 (Hup, 2021a)).

Productivity differences also led to differing corvée burdens within the year. While certain tasks had to be done year-round, such as minor road maintenance, official policy aimed to allocate major tasks, such as the construction of new roads and bridges, to the periods in the year when laborers were less busy in private production. As most laborers worked in agriculture this meant the major tasks should be scheduled to coincide with the agricultural slack periods for the particular villages in question (Jellesma 1903, p. 18; Hamerster 1918, p. 836). Making laborers perform the majority of their corvée duties in such periods meant lower opportunity costs in terms of foregone private production. As the state benefited from such private production, particularly through export duties, it was incentivized to minimize corvée labor demand when private labor demand was high. These instructions were, however, not always fully followed by local officials. Presumably this was due to local officials being wage earners and not residual claimants on export duty revenues. Relatedly, officials argued taxation in labor was preferred by the laborers themselves over taxation in money, especially in areas where labor demand was cyclical (IV 1931, p. 72). The fact that many laborers did not choose to ransom themselves, especially in the early 1900s, was seen as evidence for such preferences (as was the case for Java, see Van Deventer 1904, p. 143).

Nonetheless, the high corvée burden was an oft-cited grievance of the laborers. Laborers' grievances covered several other facets as well (e.g. Joustra 1917, p. 14; Hamerster 1918, p. 1022). First, the unequal burden across individuals within the same village was contentious. Second, the wastefulness of the tasks which corvée laborers could be required to do was also a point of contention. For example, necessary maintenance of small grass paths connecting villages was often foregone in favor of unnecessary hardening and widening of sparsely-used roads (Hamerster 1918, p. 1028). Such roads, however, eased the state's task of maintaining

control (Joustra 1917, p. 13).

Still, laborers were forced to supply their labor and could be punished for refusing to work and for not finishing tasks on time. In West Sumatra, for example, maximum penalties ranged from a fine of fifteen guilders, which would represent several weeks of wages for most people, to six days imprisonment with labor (Staatsblad 1914, no. 731). In certain places the number of convictions for dereliction in corvée duties covered a sizable part of the population. In the Palembang district of Bovenlanden, for example, 9,340 laborers out of a total of about 62,000 were convicted in 1915 (Liefrinck 1917, p. 44). On top of a fine or imprisonment with labor, the laborers often also had to make good the uncompleted corvée days. While this was officially forbidden by the top official of Palembang province, the state turned a blind eye because it feared many people might otherwise prefer the six days of imprisonment, which came with free food and shelter, over the corvée labor.

Overall, corvée labor duties were one method the state could use to extract what it wanted from the populace. As mechanization was not widespread and because wage labor could be scarce and expensive, being able to force inhabitants to supply labor for free, as a form of taxation, was useful for the state. Relatedly, alternative forms of taxation such as land taxes and income taxes were difficult to levy given the limitations on economic development and state capacity in the Indonesian Outer Islands. Customs revenues, in contrast, were relatively easy to collect as the colonial state controlled the harbors and the seas. As trade boomed in the 1910s and 1920s, customs revenues became the largest form of monetary tax revenues in the Outer Islands. At the same time, such a trade boom increased the opportunity costs of using corvée labor and allowed more laborers to ransom themselves. Corvée labor and trade were therefore tightly connected. The next section introduces the data on corvée labor and trade.

2.4 Data

I collect data on corvée labor and product-specific exports for sixteen Indonesian provinces across forty-one years (1900-1940). All Outer Island provinces are included except Riau and West Borneo as no corvée under direct-rule supervision took place there. The corvée data mainly come from the annual Colonial Reports (*Koloniale Verslagen* and *Indische Verslagen*, henceforth KV). For some cases the annual Statistical Overviews (*Statistische Jaaroverzichten*) are used as supplements. The export data come from annual trade overviews (*Statistiek van den handel* and *Jaaroverzicht in- en uitvoer*). The appendix describes the data construction in detail.

Regarding corvée, I collect data on five main variables: (i) used days; (ii) maximum days; (iii) number of liable; (iv) number of ransomers, and; (v) ransom payments. Used days is the number of days spent on corvée duties. Maximum days is the maximum number of days that could legally have been spent on corvée duties. This maximum is the multiplication of the maximum number of days a liable person could legally be made to work and the number of such liable persons. A certain number of liable persons bought off their corvée duties. The KVs call such people 'ransomers' paying 'ransom'. Outer Island corvée duties levied as village services or under the auspices of indirect rule are not included as these were without direct oversight from, and recording by, the central colonial authorities. Lastly, as six provinces first appear in the corvée sources after 1900, and two provinces abolish corvée in favor of a poll tax in 1919 and 1920, the panel is unbalanced.

Regarding exports, I collect data on the quantity and value of over a hundred different products. I aggregate these products into thirty-five product groups to ensure consistent coverage over the years. I select the products to ensure a high coverage of total export value in each province in each year. These data cover at minimum 95.7% of all provinces' exports in any given year. I do not include provinces on Java and Madura for two reasons: (i) corvée

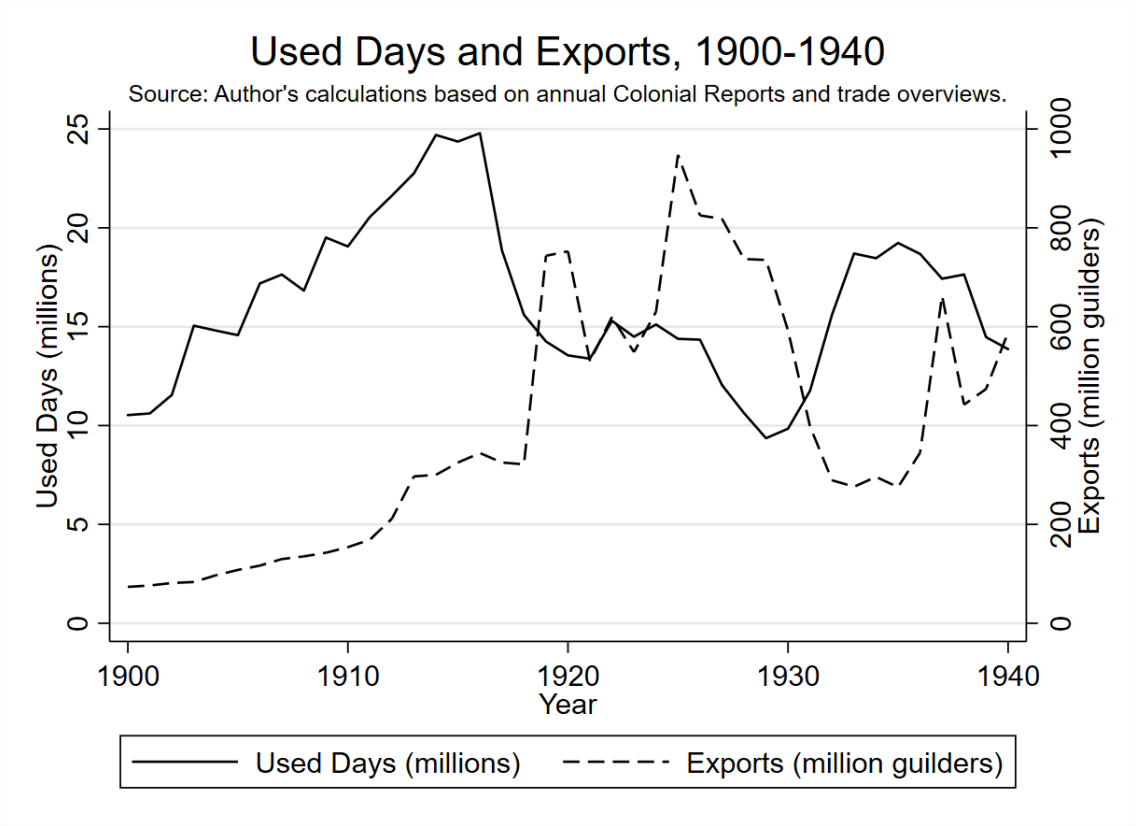

Figure 2.2

was phased out by 1916, and; (ii) no accurate province-level export data is available as many Javanese products were first transported overland to one of the main Javanese harbors, Batavia, Surabaya and Semarang, before export. As each Outer Island province had its own harbors, and much less overland inter-provincial transport took place, this problem is considerably less for the Outer Island provinces.

Figure 2.2 graphs the used corvée days and exports for all of the Outer Islands. Corvée peaks at nearly twenty-five million days during the First World War and then steeply declines to below fifteen million days towards the end of the war and the beginning of the post-war trade boom. In the mid-to-late 1920s exports grow further and corvée drops to under ten million days. With the onset of the Great Depression in 1929 this pattern reverses: trade collapses and corvée steeply increases. Figure 2.2 thus indicates a negative relationship between corvée usage and exports. This patterns is clearer for certain individual provinces, such as West

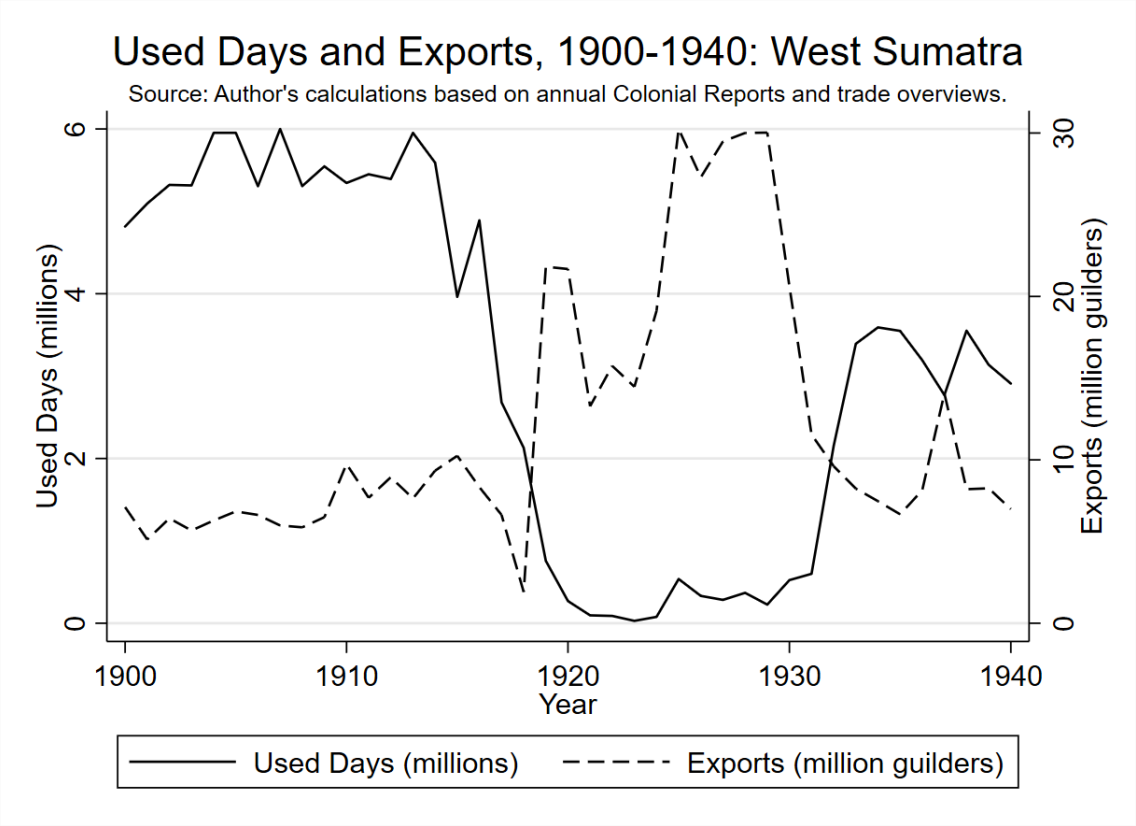

Figure 2.3

Sumatra where corvée usage hovered around six million days in the 1910s before collapsing to near zero in the 1920s and then surging back to over three million in the 1930s (see Figure 2.3).

Underlying the booms and busts of exports were booms and busts of prices for different products at different times. Figure 2.4 graphs the price indices for petroleum, pepper, and rubber. Together these three product categories constituted an important slice of total export value, ranging from 25% in 1902 up to 75% in 1917 and 1940. All these products' prices rose during and immediately after the First World War and fell subsequently, but only petroleum peaked in 1919. Rubber boomed again in the 1920s and peaked in 1925. Meanwhile, pepper experienced a more drawn-out boom and bust peaking in 1928. All these products' prices bottomed out in the early-to-mid 1930s.

In line with Figure 2.4, Figure 2.5 shows the export value peak around 1919 mainly being driven by oil products and the peak around 1925 by rubber. Underlying this overall picture are large differences in export good specialization across provinces. West Sumatra, for example, experienced large booms in oily seeds (e.g. coconuts, peanuts) in the late 1910s and a coffee boom in the late 1920s (see Figure 2.6). Palembang, in contrast, mainly exported oil products (see Figure 2.7). Both provinces partook in the rubber boom in the mid-1920s. Provinces were thus differentially affected by price shocks through differential specialization. Furthermore, products differed in labor intensity. As a major export category, oil products stand out in this respect. In 1930 in the whole of Sumatra, for example, less than 20,000 indigenous males were involved in producing petroleum, while more than 1.6 million indigenous males were involved in agriculture (Volkstelling 1930 Deel IV, 1935, p. 96). Petroleum thus only involved about 1% of the indigenous male Sumatran population, while agriculture involved more than 80%. Because so few laborers were involved in the petroleum industry, petroleum price shocks likely did not influence the ransoming decisions of many laborers. In contrast, price shocks to relatively labor-intensive products such as coffee and pepper likely did affect many laborers' ransoming decision. It is therefore useful to separate oil and non-oil exports.

What might explain the seemingly negative relationship between corvée and exports? Two actors are of import here: the state and the corvée laborer. During an export boom the marginal productivity of laborers in export-related production is high. The opportunity costs of corvée in terms of lost monetary taxation, especially on exports, are thus high as well. This induces the state to reduce its corvée usage. The Indonesian colonial state did so in two main ways: (i) reduce the annual maximum number of days a corvée laborer could be made to work, and; (ii) enable laborers to 'ransom' (i.e. pay off) their corvée duty in cash. The first measure ensured that local officials, who did not necessarily have the same encompassing interest in the economy's overall production as the central government, did not harm private production too much by levying too high a corvée duty. The second measure

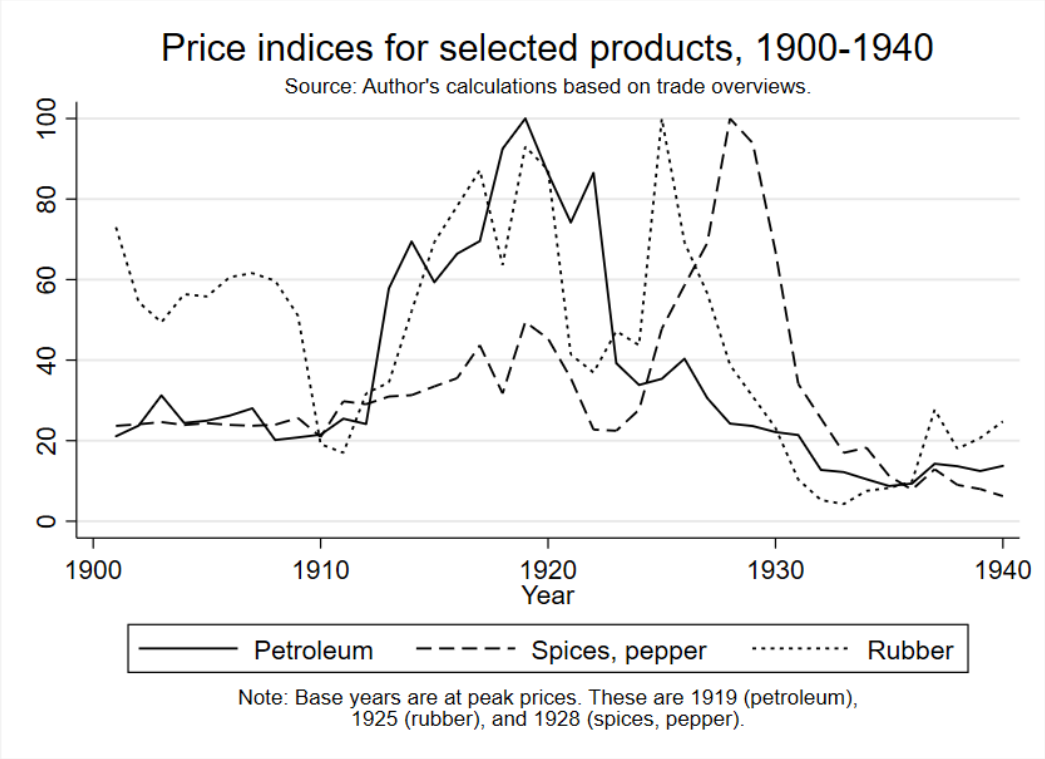

Figure 2.4

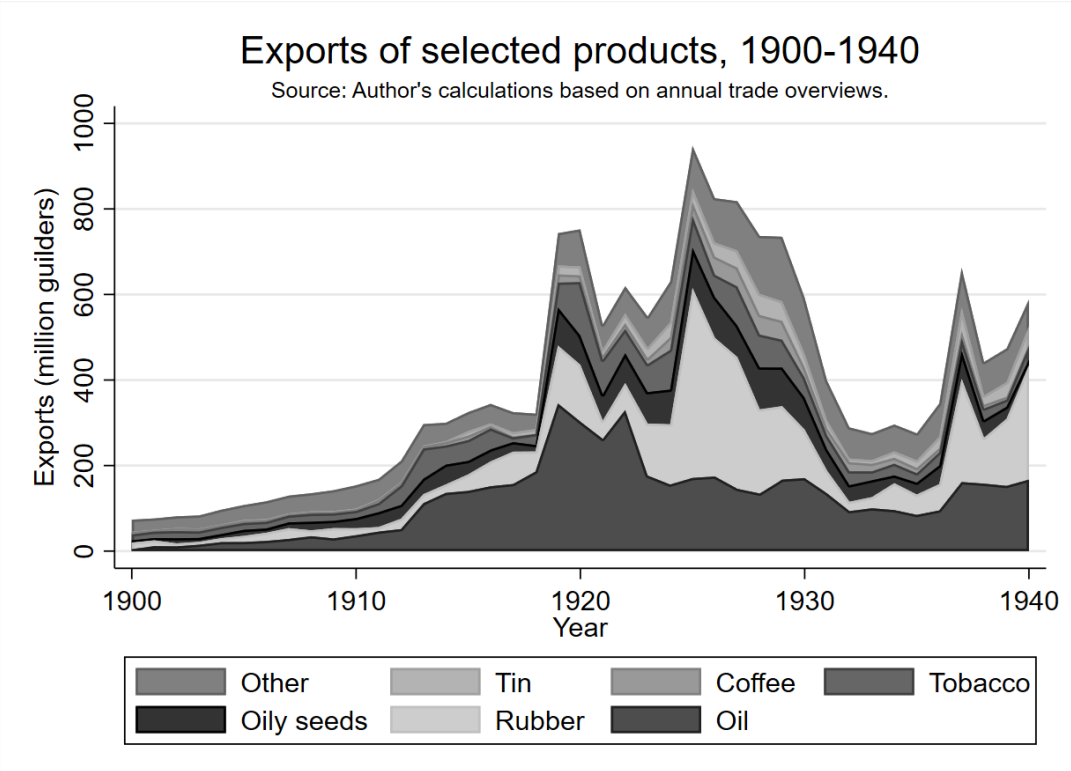

Figure 2.5

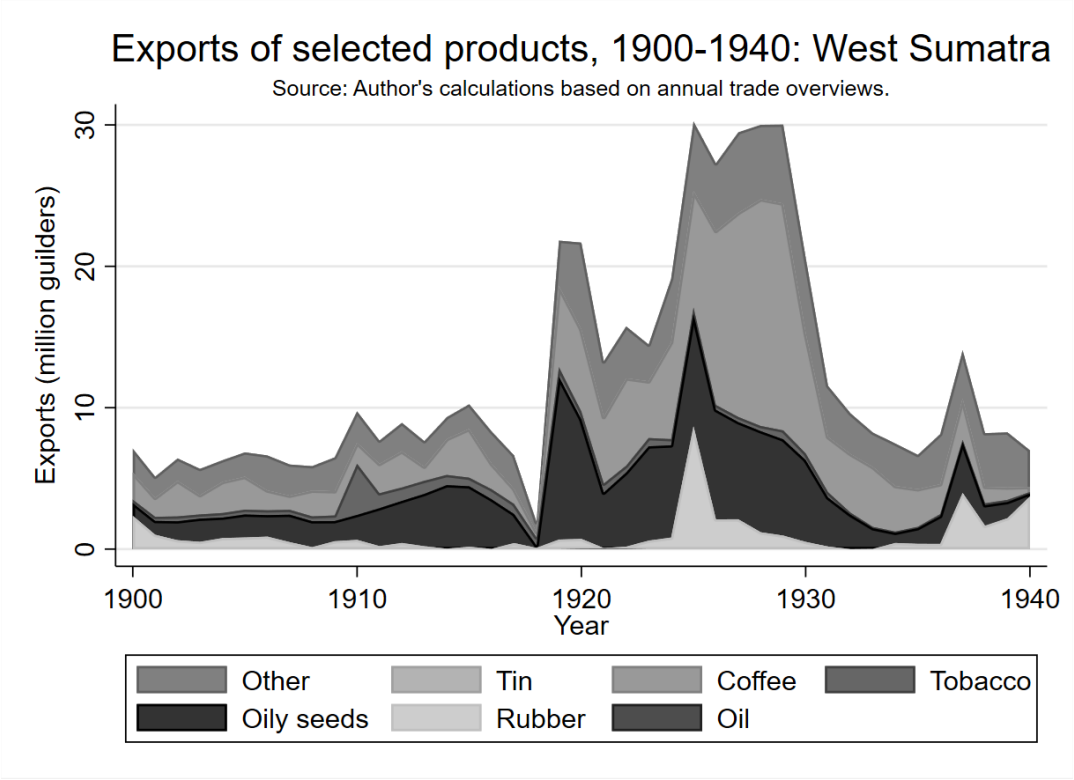

Figure 2.6

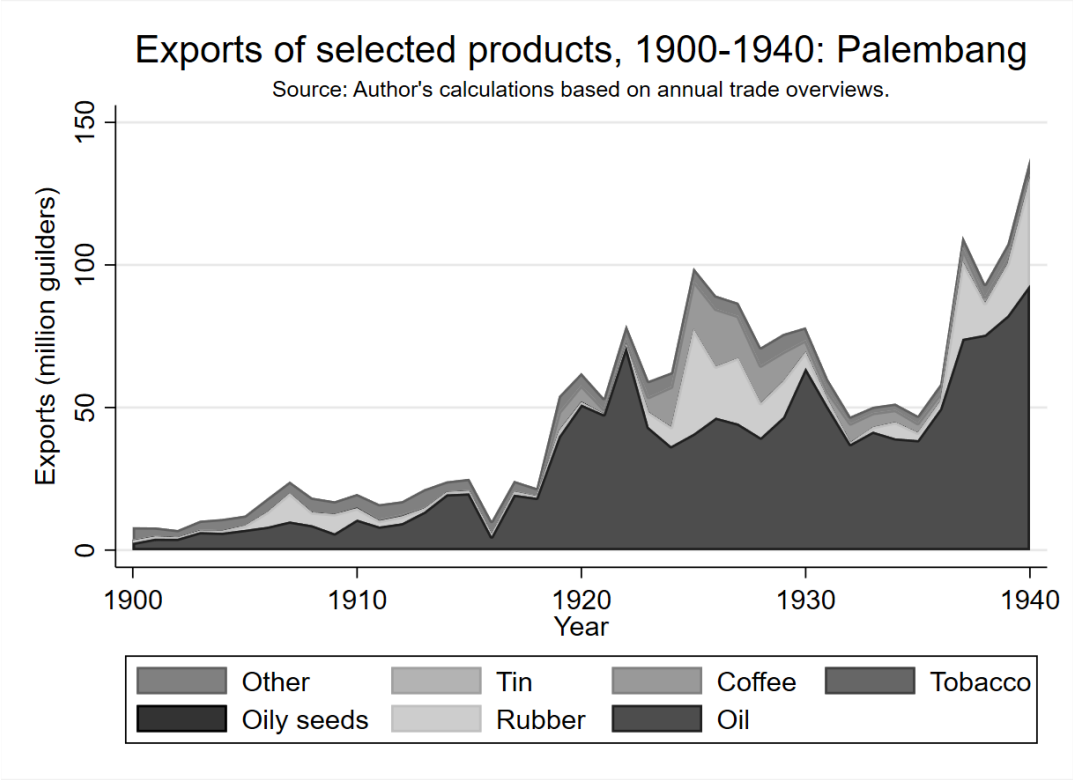

Figure 2.7

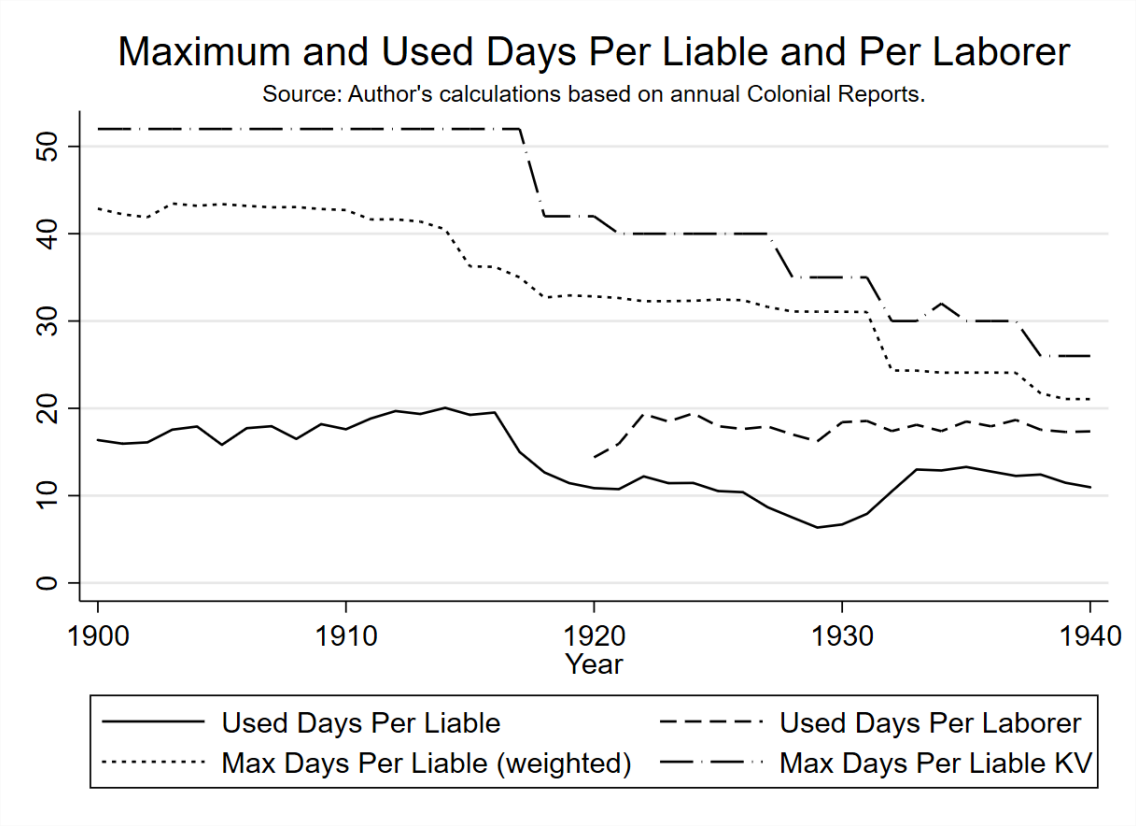

Figure 2.8

ensured that the most-productive laborers would self-select out of corvée when their marginal productivity in production would exceed the ransom. Moreover, the ransom payments could then be used to hire wage labor to do the work previously done by corvée laborers.

Figure 2.8 shows the movement over time of maximum days and used days per liable person and per laborer. The liable consist of two groups: laborers and ransomers. Tracking the distinction between per liable and per laborer is possible from 1920 on as statistics on ransomers become available. The top line shows the supremum of maximum days per liable across all provinces. In the 1900s and early 1910s this stood at fifty-two days per year (i.e. one day per week). In 1918 this fell to forty-two and kept on falling until reaching twenty-six in 1938. The maximum differed across provinces. In West Sumatra, for example, it already dropped from fifty-two to thirty-five in 1915. As changes in provinces with many liable strongly affect movements in overall corvée use, the second line from the top shows

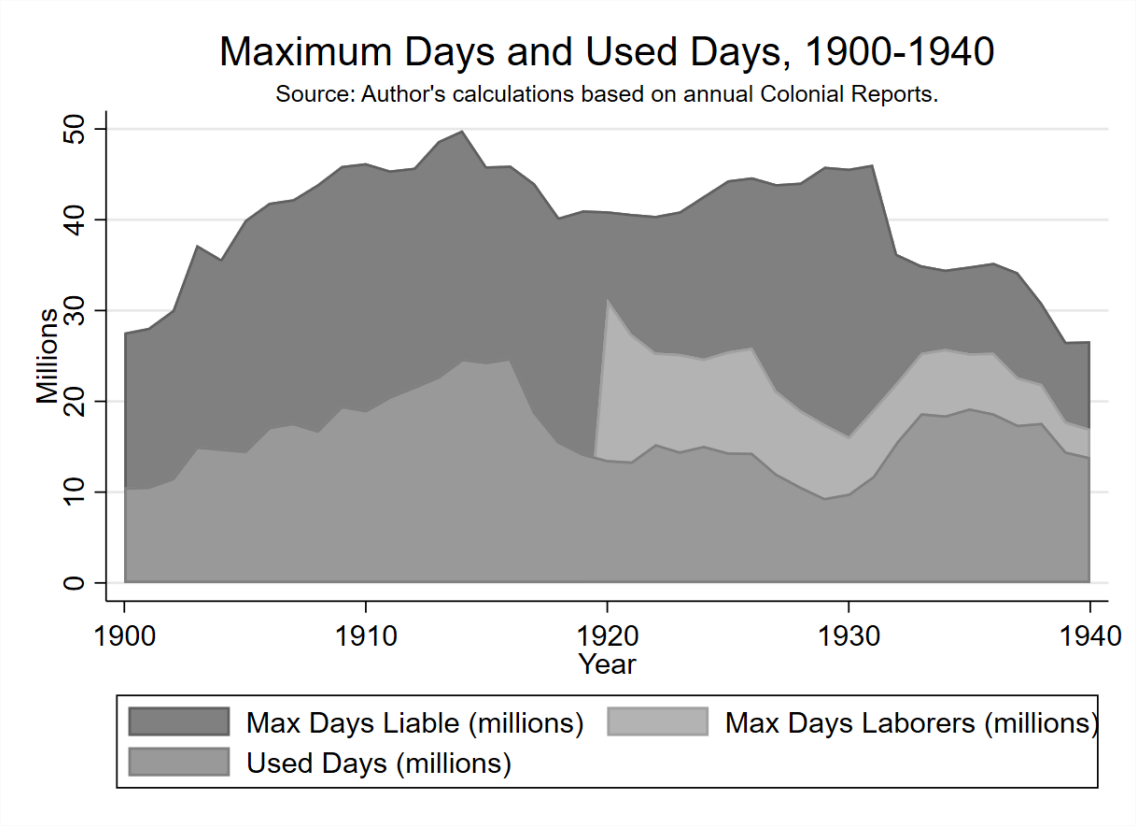

Figure 2.9

the liable-weighted average of maximum days per liable. This weighted average drops from forty-two in the early 1910s to thirty-three by 1918, before stabilizing around thirty-two in the 1920s and then dropping to twenty-four in 1932 and twenty-one in 1938.

The bottom line shows the used days per liable. This drops from twenty in 1916 to eleven in 1921. It bottoms out at six in 1929 and then shoots up to thirteen by 1933. The second line from the bottom shows the used days per laborer, where laborers are the liable who did not ransom themselves. The used days per laborer hovers between fifteen and twenty between 1920 and 1940. On average, the liable who did not pay off their corvée were thus made to work a relatively stable number of days throughout the trade boom and bust. The drop in corvée in the 1920s is thus not mainly due to a drop in days per laborer, but due to a drop in the number of laborers as individuals paid off their corvée duties.

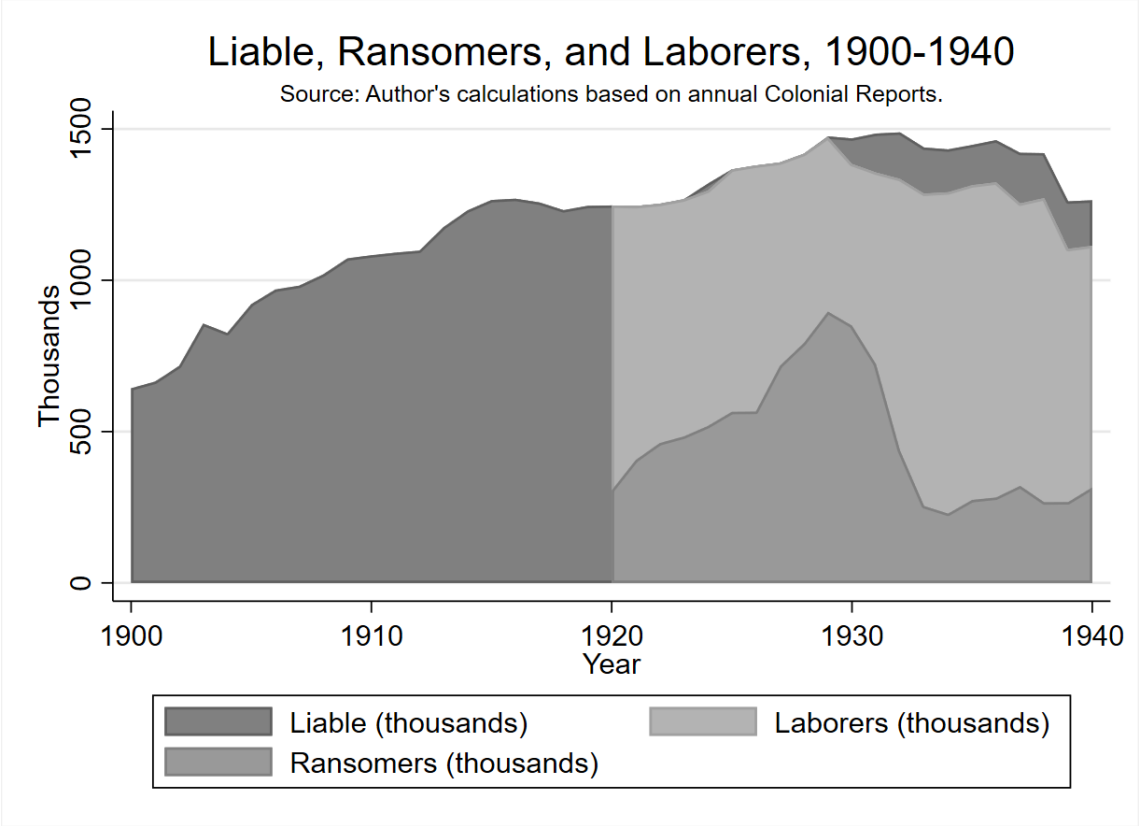

Figure 2.10

Figure 2.9 shows the maximum total days of the liable drop between 1914 and 1918 as the maximum days per liable decreased in many provinces. Throughout the 1920s, however, this number rose despite minor reductions in the maximum days per liable. The maximum total days rose as the number of liable persons grew along with the general population. In the 1930s the maximum total days dropped as the maximum days per liable were strongly reduced in 1932 and 1938 (see the weighted line in Figure 2.8). Figure 2.9 also shows the maximum total days of the laborers drop in the 1920s and rise with the onset of the Great Depression in 1929. As the maximum total days of the liable does not show the same movements, these changes in the maximum total days of the laborers are due to an increase in ransomers and a commensurate decrease in laborers in the 1920s trade boom and a reversal in the Great Depression.

Figure 2.10 graphs the number of liable, ransomers, and laborers. While the number of

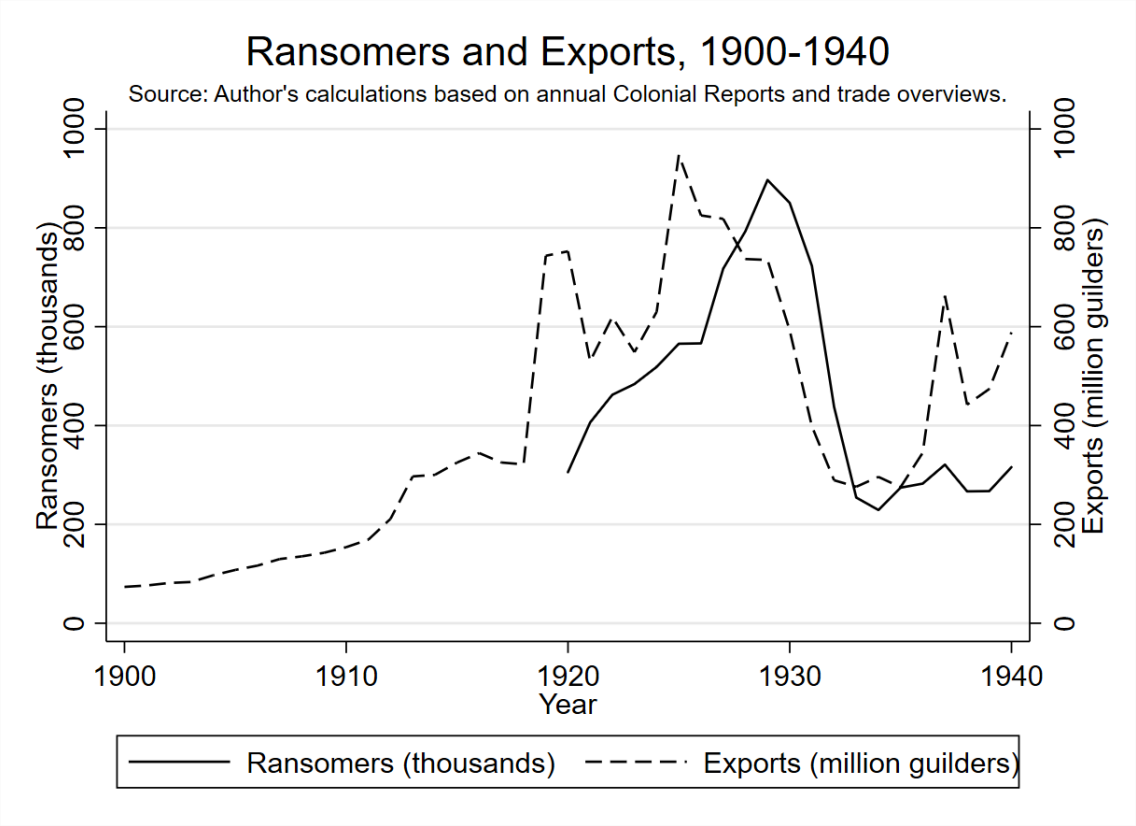

Figure 2.11

liable steadily grew until the late 1930s, the number of laborers reached a nadir in 1929 as the number of ransomers peaked. With the onset of the Great Depression the number of ransomers dropped precipitously from a peak of 900,000 to a low of 230,000 in 1934. Figure 2.11 shows the number of ransomers closely following exports. These ransomers presumably based their decision to ransom on backward-looking expectations. They took the prices they faced as fixed and adjusted their behavior accordingly. In times of a rubber price boom, for example, they could decide to ransom their corvée duty so they could spend more time harvesting rubber. Price shocks to tradable goods, exogenous from the perspective of the corvée laborers and linked to corvée through the ransom system, can thus help causally identify the effect of labor productivity on labor coercion.

2.5 Methodology

To empirically investigate the relationship between corvée labor and trade, I decompose used corvée days into three parts: number of liable persons, maximum days per liable, and used days as a share of maximum days. The number of liable persons constitutes the base, the maximum days per liable constitutes the legal limit of usage, and the used days as a share of maximum days constitutes the actual intensity of usage. This decomposition separates corvée usage into two variables – number of liable persons and maximum days per liable – which were mainly affected by governmental policy choices and one variable – used days as a share of maximum days – which was mainly affected by decisions of individual laborers. The decomposition enables a closer look into what particular facet of corvée drives the changes in usage.

The number of liable persons is a slow-changing long-term policy lever that generally tracks population growth and is only abruptly affected by completely switching to monetary taxation. Such a switch to a poll tax took place on Java in the 1910s and on the islands Belitung and Bangka in 1919 and 1920, respectively, but did not take place in the other Outer Islands. The maximum days per liable was the principal governmental policy lever. Adjustments to this legal maximum were made often (see Figure 2.8) with the view to reduce corvée usage and partially switch to monetary taxation. Finally, used days as a share of maximum liable days captures individual behavior. As laborers had the choice to buy off their corvée duties, this measure of corvée usage intensity is responsive to individual decisions on ransoming. To account for population growth I put corvée usage in per capita terms.

Formally:

$$\frac{Used\ Days}{Population} = \frac{Liable}{Population} * \frac{Maximum\ Days}{Liable} * \frac{Used\ Days}{Maximum\ Days} \tag{2.5}$$

Taking logs:

$$Log(\frac{Used\ Days}{Population}) = Log(\frac{Liable}{Population}) + Log(\frac{Maximum\ Days}{Liable}) + Log(\frac{Used\ Days}{Maximum\ Days}) \quad (2.6)$$

I regress the log of used days per capita, and each component separately, on the log of exports per capita and province and year fixed effects. The main regression specifications are thus:

$$Log(\frac{Used\ Days_{it}}{Population_{it}}) = Log(\frac{Exports_{it}}{Population_{it}})\beta + \lambda_t + \alpha_i + \epsilon_{it} \quad (2.7)$$

$$Log(\frac{Liable_{it}}{Population_{it}}) = Log(\frac{Exports_{it}}{Population_{it}})\beta + \lambda_t + \alpha_i + \epsilon_{it} \quad (2.8)$$

$$Log(\frac{Maximum\ Days_{it}}{Liable_{it}}) = Log(\frac{Exports_{it}}{Population_{it}})\beta + \lambda_t + \alpha_i + \epsilon_{it} \quad (2.9)$$

$$Log(\frac{Used\ Days_{it}}{Maximum\ Days_{it}}) = Log(\frac{Exports_{it}}{Population_{it}})\beta + \lambda_t + \alpha_i + \epsilon_{it} \quad (2.10)$$

Where for each province i and year t: $Exports_{it}$ = value of exports; $Population_{it}$ = size of indigenous population; λ_t = year fixed effects, and; α_i = province fixed effects.

As the size and composition of export crop production differed across provinces, provinces were differentially affected by changes in world market prices. For example, at the peak of the rubber boom in 1925, rubber products constituted about 92% of export value in Tapanuli, 58% in East Sumatra, 29% in West Sumatra, and 0% in Bali and Lombok. Given the many products there are many different annual price shocks, so the identification can leverage exogenous variation in shocks even when exposure is potentially endogenous (Borusyak, Hull and Jaravel, 2019). To further mitigate endogeneity concerns, I use the product-specific data to construct a measure of potential exports by multiplying lagged quantities with current

prices across dozens of products:

$$Potential\ Exports_{it} = \sum_{n} Quantity_{in,t-1} * Price_{n,t}$$

Where for each province i and year t: n = product category (e.g. rubber, tobacco, coffee). I aggregate the more than one hundred specific products into thirty-five product categories to ensure full coverage over the years as the definition and specificity of products in the trade statistics differed over time. I re-estimate equations (7)-(10) using this measure of potential exports.

To distinguish the major capital-intensive export from other exports, I separate exports into oil and non-oil exports. As the oil industry only involved a tiny fraction of laborers, booms and busts in oil products likely barely impacted aggregate ransoming decisions even though oil products constituted a large fraction of total exports. Using oil-only exports as the regressor thus serves as a placebo test. Relatedly, the relationship between corvée and non-oil exports should be stronger and clearer than the relationship between corvée and overall exports.

To permit valid inference in the presence of potential within-state and within-year (i.e. geographic) correlation in the errors, I use two-way (province and year) clustered Driscoll-Kraay standard errors (Driscoll and Kraay, 1998; Cameron, Gelbach and Miller, 2011). To correct potential downward bias in the cluster-robust standard errors due to the small number of clusters, I apply a small-sample correction. Since provinces differed substantially in the number of people subject to direct-rule corvée, and to avoid attributing undue influence to the effects observed in small provinces, I weight observations by the number of liable persons.

2.6 Results

Panel A of Table 2.1 shows the regression estimates with exports per capita as the main explanatory variable. Column 1 shows a negative, but statistically insignificant, point estimate for the overall relationship between exports per capita and used days per capita. Columns 2 to 4 break down used days into its three components. Columns 2 and 3 show the number of liable persons per capita and the maximum number of days per liable were not significantly related to exports. Column 4 shows every one percent increase in exports per capita is associated with a marginally significant 0.18 percent decrease in intensity of corvée usage per capita.

Table 2.1's Panel B uses non-oil exports per capita. As expected, this increases both the statistical and economic significance of the estimates. Column 1 indicates every one percent increase in non-oil exports per capita is associated with a 0.22 percent decrease in corvée usage per capita. Columns 2 to 4 suggest that this negative relationship does not operate through policy adjustments in the number of liable per capita or the maximum days per liable (columns 2 and 3), but through adjustments in the intensity of corvée usage (column 4).

Panels A and B of Table 2.2 show the regression estimates with potential exports per capita and non-oil potential exports per capita, respectively, as the main explanatory variable. The results are consistent with those in Table 2.1. Every one percent increase in non-oil potential exports per capita decreases corvée usage per capita by 0.18 percent. Again, this relationship operates fully through adjustments in the intensity of corvée usage (column 4).

The results are robust to using lagged exports (see Table B.2), further lagging potential exports (see Table B.3), and not applying a per capita adjustment (see Table B.4 and Table B.5).

Table 2.1: OLS estimates, corvée and exports per capita with and without oil

	Log(Used Days per Capita)	Log(Liable per Capita)	Log(Max Days / Liable)	Log(Used Days / Max Days)
Panel A: Log(Exports per Capita)				
Log(Exports per Capita)	-0.18	-0.01	0.01	-0.18*
	(0.12)	(0.05)	(0.04)	(0.10)
Adjusted R^2	0.49	0.84	0.84	0.44
Panel B: Log(Exports per Capita, No Oil)				
Log(Exports per Capita, No Oil)	-0.22**	-0.01	-0.00	-0.21**
	(0.09)	(0.04)	(0.03)	(0.09)
Adjusted R^2	0.50	0.84	0.84	0.45
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	545	545	545	545

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (16 province clusters, 41 year clusters). Observations are weighted by the number of liable persons. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

Table 2.2: OLS estimates, corvée and potential exports per capita with and without oil

	Log(Used Days per Capita)	Log(Liable per Capita)	Log(Max Days / Liable)	Log(Used Days / Max Days)
Panel A: Log(Potential Exports per Capita)				
Log(Potential Exports per Capita)	-0.13	0.00	0.01	-0.14
	(0.12)	(0.04)	(0.04)	(0.09)
Adjusted R^2	0.49	0.83	0.84	0.44
Panel B: Log(Potential Exports per Capita, No Oil)				
Log(Potential Exports per Capita, No Oil)	-0.18***	0.01	-0.00	-0.18***
	(0.05)	(0.04)	(0.03)	(0.03)
Adjusted R^2	0.49	0.83	0.84	0.44
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	535	535	535	535

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (16 province clusters, 40 year clusters). Observations are weighted by the number of liable persons. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

As a placebo test, I use oil exports and oil potential exports as main regressors. Table 2.3 shows there is no association between oil exports and corvée usage. This is likely due to the capital-intensive nature of oil production, relative to the production of other major export goods. For example, petroleum production only involved about 1% of the indigenous male Sumatran population, while agriculture involved more than 80%. Price shocks to petroleum would therefore hardly have impacted many laborers' earnings opportunities, thereby leaving unchanged laborers' decisions regarding buying off corvée duties.

The results indicate that booms in relatively labor-intensive export products reduced corvée usage. The magnitude of the results is substantial. Given that non-oil exports per capita more than tripled between 1916 and 1925, and corvée usage per capita dropped by a bit over half in the same period, the increase in non-oil exports explains nearly all of the drop in corvée usage. As this relationship ran through changes in intensity of usage (captured by column 4), not through policy adjustments in the number of liable per capita or the maximum days per liable (captured by columns 2 and 3), the main driver was likely individual laborers buying off their corvée duties. As the ransom system enabled laborers to self-select out of corvée, it was likely essential in allowing corvée to flexibly adjust to the waxing and waning of economic opportunities induced by trade booms and busts. Importantly, the ransom system utilized laborers' private information on their marginal earnings possibilities and thereby could achieve flexible changes in corvée usage without requiring stronger information-collection and -analysis capabilities of the state.

Table 2.4 reports the results from regressing ransom revenues and number of ransomers on exports, all in per capita terms, and confirms the positive link between exports and ransoming. Consistent with the strong negative link between non-oil exports and corvée usage, Table 2.4 shows a strong positive link between non-oil exports and ransoming. Every one percent increase in non-oil exports per capita is associated with a 0.49 percent increase in ransom revenues per capita and a 0.57 percent increase in number of ransomers per

Table 2.3: OLS estimates, corvée and oil-only exports and potential exports per capita

	Log(Used Days per Capita)	Log(Liable per Capita)	Log(Max Days / Liable)	Log(Used Days / Max Days)
Panel A: Log(Exports per Capita, Oil)				
Log(Exports per Capita, Oil)	0.01	-0.00	0.01	0.01
	(0.03)	(0.00)	(0.00)	(0.03)
Adjusted R^2	0.49	0.84	0.84	0.43
Observations	545	545	545	545
Panel B: Log(Potential Exports per Capita, Oil)				
Log(Potential Exports per Capita, Oil)	0.03	0.00	0.01	0.03
	(0.03)	(0.00)	(0.00)	(0.03)
Adjusted R^2	0.49	0.84	0.84	0.44
Observations	535	535	535	535
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (16 province clusters, 40-41 year clusters). Observations are weighted by the number of liable persons. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

Table 2.4: OLS estimates, ransom(ers) and exports per capita with and without oil

	Log(Ransom per Capita)	Log(Ransomers per Capita)
Panel A: Log(Exports per Capita)		
Log(Exports per Capita)	0.52*	0.66*
	(0.28)	(0.33)
Adjusted R^2	0.69	0.62
Panel B: Log(Exports per Capita, No Oil)		
Log(Exports per Capita, No Oil)	0.49**	0.57**
	(0.22)	(0.24)
Adjusted R^2	0.69	0.61
Province FE	Yes	Yes
Year FE	Yes	Yes
Observations	268	268

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (13 province clusters, 21 year clusters). Observations are weighted by the number of liable persons. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

capita. Using potential exports per capita retains the magnitude of the coefficients, but the underlying lagged quantities result in a more noisy estimate that is not statistically significant at the usual levels (see Table 2.5). Note that the lack of data on ransoming before 1920 means the late 1910s trade boom is not captured. Not having access to this information is likely contributing to the estimates being more noisy than those on corvée usage.

The finding of a negative relationship between trade and labor coercion, particularly between relatively labor-intensive exports and coercion, stands in contrast to certain other studies (e.g. Lowes and Montero, 2018; Saleh, 2020). Distinctions between different forms of coercion are key here. Labor coercion in private production, as investigated by Lowes and Montero (2018) and Saleh (2020), can increase with trade as trade opportunities increase the employers' returns to such coercion. For example, cotton price increases may induce plantation holders to acquire more slave labor. Such findings are in line with Domar's (1970)

Table 2.5: OLS estimates, ransom(ers) and potential exports per capita with and without oil

	Log(Ransom per Capita)	Log(Ransomers per Capita)
Panel A: Log(Potential Exports per Capita)		
Log(Potential Exports per Capita)	0.55	0.69
	(0.35)	(0.40)
Adjusted R^2	0.69	0.62
Panel B: Log(Potential Exports per Capita, No Oil)		
Log(Potential Exports per Capita, No Oil)	0.52	0.58
	(0.32)	(0.34)
Adjusted R^2	0.69	0.61
Province FE	Yes	Yes
Year FE	Yes	Yes
Observations	268	268

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (13 province clusters, 21 year clusters). Observations are weighted by the number of liable persons. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

expectation of a positive relationship between productivity and coercion.

Labor coercion in taxation, however, follows another logic due to the encompassing interest of the state. Unlike private producers that have to pay laborers (higher) wages in the absence of coercion, the state can tax in alternative manners in the absence of coercion. Furthermore, corvée was not used to produce goods for sale, but to labor on infrastructural works such as roads. Price booms in products such as coffee and rubber thus did not mean increased returns to corvée, but increased opportunity costs of corvée. By allowing laborers to buy off their corvée duties, the state therefore benefited in two ways: (i) direct ransom payments, and; (ii) indirect higher tax revenues through export duties and other production-related taxes. Although the ransom data does not allow looking at individual laborers, it was likely the laborers with high outside private labor returns who bought off their corvée duties in order to reap those relatively high returns. After all, only for laborers whose marginal earnings

exceeded the ransom payment would it pay to ransom themselves.

As trade booms, particularly booms in labor-intensive products, increased private labor returns, they decreased coercion in the form of corvée. My findings are thus in line with the neo-Malthusian view in which laborers with high outside wages face less coercion, but they also differ from the canonical view on coercion as a matter between employer and employee. The ability of the state to benefit from reducing coercion by reaping higher tax revenues is key to understand why trade booms reduced this specific form of labor coercion. It was beneficial for the state to find a way to relieve the most-productive laborers of corvée duties as productive activities they would undertake in lieu of corvée were taxed by the state. In contrast to slaveholders who were generally limited to keeping coerced labor focused on certain tasks in a specific area, such as cotton-picking on a plantation, the state could relieve laborers from corvée duties and still benefit from those laborers' private production.

2.7 Conclusion

By constructing a new database on corvée labor usage and product-specific exports for sixteen Indonesian provinces across four decades, this study is the first to empirically investigate the effects of trade booms and busts on the usage of corvée labor, an important form of public labor coercion. Results indicate this type of coercion decreases with trade booms as laborers react to increased private returns by paying off corvée duties. In particular, the boom in exports in the late 1910s and early-to-mid 1920s nearly fully explains the large concurrent drop in corvée usage. The buy-out system thus enabled corvée usage to flexibly adjust to the waxing and waning of economic opportunities. This in turn allowed a flexible shift towards monetary taxation that enabled taxpayers to increase their private production, and the state to increase its tax revenue, at a time when it was most advantageous to do so, namely during a trade boom. Moreover, the opposite took place during the trade collapse of

the Great Depression. The trade collapse increased state-ordained labor coercion as laborers ceased to ransom themselves. The ransom system utilized laborers' private information on their productivity to effect these movements between taxation in labor and taxation in currency. Furthermore, since presumably only laborers whose opportunity costs exceeded the ransom payment would ransom themselves, such self-selection out of corvée reduced the negative impact of corvée labor usage on private production. Particular aspects of fiscal modernization can thus be achieved without stronger information-collection capabilities of the state.

In general, these results highlight that fiscal modernization, labor coercion, and trade are tightly intertwined. Since corvée was historically widespread, increasing productivity, potentially trade-induced, may have been a major reason for phasing out corvée elsewhere too. Specifically, the finding of a negative relationship between trade and corvée indicates that the nature of the relationship between coercer and coerced, as well as the purpose of coercion, matters for the relationship between trade and coercion. While trade booms may increase coercion in slaveholder-slave relationships (e.g. Saleh, 2020), such booms can reduce coercion in state-taxpayer relationships. The flipside is that trade busts can increase state coercion. Since state coercion, also in the form of corvée labor, is still present today (Olken and Singhal, 2011; ILO, 2017), the 2020 economic downturn may have induced a shift into such coercion. Studying such relationships in modern-day settings, especially by collecting better data, would be a worthwhile avenue for further research.

Chapter 3

Tax Farming and State Capacity: Evidence from Colonial Indonesia

3.1 Introduction

How did modern and centralized fiscal institutions emerge? Fiscal modernization is thought to be key for long-run economic development (e.g. Dincecco 2009; He, 2013; Vries, 2015; Dincecco and Katz, 2016). Yet there are many aspects to this process which remain unclear. Prior to centralized tax collection by state actors, tax collection by private parties – a practice known as tax farming – was widespread. Furthermore, tax farming is still used in modern times in certain countries. While much work in economics has focused on optimal taxation (see Mankiw et al. (2009) for a review), relatively little work has focused on optimal tax administration (see Slemrod and Yitzhaki (2002) for a review). However, the administration of taxation is a key facet of fiscal capacity and therefore of long-run development.

Tax administration refers to who is in charge of tax collection, how the tax collector is rewarded and what type of taxes are collected (e.g. excise, customs, income taxes). Two

general categories can be distinguished: wage contracts and rent contracts (Cosgel and Miceli, 2009).[1] With wage contracts the state sets up a bureaucracy and uses salaried employees to collect taxes. The employees hold no residual rights to the tax revenue and solely earn wages. With rent contracts the state sells tax collection rights to private parties who then hold residual rights to the tax revenue. These private parties can 'farm' the tax and are thus known as tax farmers. What then determines the use of private tax collection over public tax collection?

The literature argues that private collection offers several advantages from the ruler's or state's perspective. Importantly, tax collection is costly. It requires an expensive administrative and monitoring system to enable key tasks such as measuring the tax base and limiting stealing and slacking by tax collectors. Costs of establishing and maintaining such a system are potentially prohibitive for low-capacity states ruling over fragmented and heterogeneous areas. The state also faces uncertain tax revenues due to unexpected shocks and due to deficient tax base measurement and monitoring. Auctioning tax collection rights to the highest bidder enables the state to receive a guaranteed lump-sum payment upfront while at the same time outsourcing administrative and monitoring costs to the tax farmer. Given these relationships, I hypothesize that the state reduces reliance on tax farming as it improves its ability to measure the tax base and to operate a large bureaucracy. In other words, increasing state capacity reduces reliance on tax farming. This aligns with a literature that posits a positive relationship between state capacity and the state's ability to levy relatively information-intensive forms of taxation (e.g. Brewer, 1989; Besley and Persson, 2011, 2013, 2014; Johnson and Koyama, 2014, 2017; Dincecco, 2015).

Another advantage of tax farming for the state is that it enables the state to curb potential local political competitors by not running revenue streams through them but through private

[1]Share contracts could be seen as a third category. These are a mix between wage contracts and rent contracts where private parties buy the right to collect taxes, but then share the revenue with the state according to a pre-specified sharing rule.

parties. Through farming out taxes at auction to the highest bidders, who historically often were traders with local information-gathering networks, the state can circumvent potentially insubordinate powerholders such as local gentry. This particularly applies when the largest traders and tax farmers are from a politically weak minority. As the state's capacity grows it has less need to rely on tax farming for security reasons and it can gradually replace it with state-run collection.

I argue these theories help explain tax administration in colonial Indonesia where tax farming was an important source of revenue for the state throughout the nineteenth century, but was replaced by state-run administration during the 1890s to 1910s. Furthermore, ethnic Chinese dominated in tax farming while constituting a small minority of the population. Employing them as tax farmers was useful for a thin colonial state that was wary of rebellion and faced many limitations on its power. Due to this divide-and-rule aspect of tax farming, I hypothesize that the type of state capacity expansion matters for reliance on tax farming. Focusing on the state bureaucracy, I hypothesize that the relationship between state capacity and tax farming depends on the identity of the bureaucrat. Bureaucrats from the group traditionally excluded from tax farming would strongly push for reduced usage of tax farming. In contrast, bureaucrats from the group purposefully wooed by the state as tax farmers would do so less strongly or may even push for more usage. Bureaucrats from the group in power would work towards replacing tax farming with state-run administration, but would do so less strongly than the excluded group. They would thus have a relationship with tax farming in between those of the excluded and wooed groups.

I collect a rich new province-level panel of data spanning thirty-two years (1874-1905) for colonial Indonesia to test the hypotheses. I focus on the island of Java which contained the main population, administrative, and economic centers of the Dutch East Indies, modern-day Indonesia. I collect data on tax farm revenues as a measure of tax farm reliance. I collect data on the number of state officials in a province to proxy for local state capacity. I

divide these local bureaucracies into the three ethnic groupings into which the colonial state divided the population: indigenous, European, and 'foreign oriental' (hereafter, Asian). The Asian group mainly consisted of ethnic Chinese. Most tax farmers were also ethnic Chinese. I posit that the negative relationship between state capacity and tax farming most strongly applies to indigenous officials as they had less to lose from phasing out tax farming and more to gain from shifting towards state-run collection as they constituted the vast majority of the colonial state bureaucracy. For similar reasons, I posit that the relationship is least negative for Asian officials.

Results indicate a negative and significant relationship between the state officials and tax farm revenues. Furthermore, this relationship only applies to indigenous and European officials, not to Asian officials. Moreover, the negative relationship is strongest for indigenous officials. To address endogeneity concerns, I use an instrumental variables (IV) approach that exploits the differing importance, both across provinces and over time, of effective distance to the capital Batavia, modern-day Jakarta. The IV results are consistent with the OLS results. These findings provide evidence for state capacity expansions enabling reductions in tax farm reliance. They also indicate that the identity of state bureaucrats, and therefore the type of state capacity expansion, matters for the types of taxation a state uses.

The next section discusses historical and current cases of tax farming. The third section specifically describes tax farming in colonial Indonesia. The fourth section introduces the data. The fifth section describes the methodology. The sixth section discusses the results. The final section concludes. The appendix briefly describes the data construction and contains additional regression tables.

3.2 Tax Farming: Historical and Current Cases

The first reported use of tax farming comes from ancient Mesopotamia, but it has been widely used across time and space (Jang, 2012). For example, it was used in ancient Greece, the Roman Empire, the Ottoman Empire, many early modern European countries like France, England, Spain, the Netherlands, and Prussia, and in Southeast Asian colonies such as Indonesia and Malaya (Dick and Butcher, 1993; Kiser, 1994; Ma, 2003; White, 2004; Cosgel and Miceli, 2009; Wahid, 2012). In modern times, it has been used in Tunisia, Indonesia, Uganda, and Tanzania, amongst others (Azabou and Nugent, 1988; Kiser and Baker, 1994; Iversen et al., 2006; Fjeldstad et al., 2008). In the United States since the 1980s state governments use private debt collection agencies in the collection of unpaid and undisputed tax debt (Jang and Eger, 2018).

Historically, tax farming was generally used to collect indirect taxes rather than direct taxes (Kiser, 1994). In France, indirect taxes were farmed from the thirteenth century until the French Revolution. Direct taxes were occasionally farmed, but were usually collected under state administration. Before the sixteenth century many different tax farmers competed for the contracts at the parish level. However, over time the tax farmers consolidated due to state policy. Such consolidation aimed to take advantage of economies of scale and to allow the tax farmers to act as major lenders to the state. By 1598 the salt taxes and the customs were consolidated into a single tax farm, and in 1604 the sales taxes followed course. However, in 1609 there still remained close to fifty major tax farms and many minor ones. Consolidation of the farms continued until 1723 when the separate farms merged into a single Company of General Farms. Tax farm revenue as a share of total crown revenue increased from 24 percent in 1643 to more than 45 percent in 1768. The consolidation of tax farms, however, also meant less competition between tax farmers and therefore more opportunities for overextraction. Tax farmers were depicted as exploiters of the poor and accused of being "bloodsuckers of the nation" who "drink in golden cups of tears of the unfortunates"

(Matthew, 1958, p. 273 as cited in Ma, 2003, p. 448). The decline of tax farmers started with reforms in the 1780s shifting several important indirect taxes (e.g. excise taxes, stamp duties) to state administration. The revenue share contributed by tax farmers had fallen to 30 percent by 1788 (Kiser, 1994). The French Revolution of 1789 ushered in the demise of the tax farms as tax farmers were assaulted on the streets, the General Farms' building in Paris was attacked and 28 tax farmers were executed at the guillotine.

In England, experiments with state administration and tax farming continued for centuries, but indirect taxes were solely farmed from the late sixteenth century to the late seventeenth century. As in France, a gradual consolidation of tax farms took place. In 1604 the customs were unified into the Great Farm and by 1640 other small farms like those on sweet wines and currants had merged with the Great Farm which then constituted 37 percent of the crown's total revenue (Johnson and Koyama, 2014). During the English Civil War (1642-1651) the tax farmers were expropriated by Parliament, but with the Restoration of 1660 tax farming was renewed. However, opposition to tax farming continued and in the 1670s the customs came under state administration. Tax farming was brought to a final end in the 1680s when the excise and the hearth tax were brought under state administration in 1683 and 1684, respectively (Kiser, 1994).

In the early modern period Spain employed tax farming for most taxes, sometimes even using it to collect an important direct tax – the tax on sheep flocks. As in the cases of France and England, the state experimented much so multiple transitions can be noted. From 1558 to 1583 the wool export tax was collected by state officials, after which it was farmed out. Still, between 1583 and 1640 there were five more transitions between tax farming and state administration for this particular tax. Eventually, tax farming was decided on. In 1667 only seven percent of state revenue was collected by central state officials (Kiser, 1994). Yet, no consolidation of tax farmers took place in Spain (Jang, 2012). Tax farmers remained fragmented in the Netherlands too where the farms were geographically small and

most contracts lasted between six months and a year (Johnson and Koyama, 2014). In the Netherlands tax farming was abolished in 1806 when it was replaced by a Napoleonic system of central administration.

Prussia represents an unusual case in that it did not mainly use rent contracts (i.e. tax farming) but share contracts. Beginning in the 1680s, excise taxes were collected by state officials who were incentivized through share contracts which stipulated the share of tax revenue they could keep. These proportionally-paid state tax collectors thus were partial residual claimants. However, tax farming was used for renting crown lands, and intermittently for certain indirect taxes between 1766 and 1786 (Ma, 2003). At the turn of the nineteenth century the tax administration was bureaucratized with the move from share contracts to wage contracts in the wake of reforms in 1794, 1804, and 1806 (Kiser and Schneider, 1994).

In the Ottoman Empire tax farming continued from the fifteenth to the late nineteenth century. Peculiarly, the Ottomans did not solely farm customs and indirect taxes, but also farmed land taxes. This land tax, the tithe, was the most important direct tax and before the late sixteenth century it was collected by military men in lieu of their salaries. Afterwards, however, it was farmed out. This is special as land taxes, which are levied upon immobile assets, are supposedly easier to collect than indirect taxes. Moreover, as land contains a substantial capital component, the opportunity for short-term extraction by the tax farmer at the cost of long-term development is a danger. To combat this the Ottoman state's tax farming also distinguished itself from other cases in the length of its contracts. The state developed lifetime contracts for the tax farmers in order to incentivize them to retain the long-term productivity of the land and its laborers (Cizakca, 1993; Ma, 2003). In the nineteenth century tax farmers were gradually replaced with state employees. Lifetime tax farming was abolished in the 1840s, while short-term tax farming was abolished in the wake of the First World War (Pamuk, 2004).

Several Southeast Asian countries continued the practice of tax farming into the twentieth

century. Colonial Malaya, colonial Indonesia, Thailand, and Cambodia all employed tax farmers to collect indirect taxes (Dick and Butcher, 1993; Cooke, 2007; Wahid, 2012, 2013a, 2013b). In all these cases the tax farmers were mainly local ethnic Chinese businessmen. In Cambodia tax farms were only introduced in the late 1860s and lasted for less than thirty years. While the tax farms lasted for only a brief time, they covered a wide variety of activities and goods. Amongst others, there were tax farms for raw opium, cooked opium, rice wine, sugar, fish, pork, gambling, pawnshops, and rice (Cooke, 2007). This wide coverage of tax farms existed in Indonesia too. The Indonesian case is described in more detail in the next section.

Currently, tax farming is still in use in several developing countries. In Uganda, for example, private collection of taxes has been on the rise since the 1990s (Iversen et al., 2006). Tax farmers mainly collect market dues, fish landing site fees, and car park fees which together constitute an important part of revenue for cash-strapped municipalities. Under Uganda's form of fiscal decentralization, local councils organize the auctions and tender out the tax farms on the basis of a 'reserve price' which is based on an estimate of the revenue potential of the tax farm in question. All districts organize public sealed-bid auctions, but the duration and content of the contracts vary across districts. Typical contracts last between three to twelve months. While the tax farmers are supposed to competitively bid for the contract, the highest bidder does not necessarily win the contract as there is much room for discretion by the auction board. This leaves room for political interference and corruption. The winning bidder usually has connections with the auction board, and the winning bid is also often substantially below the revenue potential as measured by Iversen et al. (2006). The spread between the revenue potential and the winning bid is supposedly shared between the tax farmer and the corrupt auction board members. Due to these problems the particular form of tax administration is still in flux in several developing countries.[2] Tanzania has experimented

[2]For a recent experiment in Pakistan with share contracts for bureaucratic tax collectors, see Khan et al. (2016).

with private collection of local government revenues since 1996, but the results are mixed there as well (Fjeldstad et al., 2008).

3.3 Tax Farming in Colonial Indonesia

Tax farming has pre-colonial roots in Indonesia, but the Dutch colonial state institutionalized it and spread it across the archipelago (Wahid, 2012). It was first introduced in Batavia on the island of Java, the main city for the Dutch East India Company's (*Vereenigde Oost-Indische Compagnie*, hereafter VOC) activities in Asia. In 1620 the VOC farmed out the right to collect the gambling tax to a prominent ethnic Chinese merchant. With the VOC's victory in the Java War of 1741-1743, tax farming spread to Java's northeast coast as the Sultanate of Mataram ceded the right to collect certain taxes to the VOC. As the eighteenth century progressed tax farming constituted a growing proportion of the VOC's revenue, leading some to state that it represented "the largest part of the Company's revenues on these coasts" (De Jonge and Van Deventer, 1909, p. 356 as cited in Wahid, 2012, p. 258). The dependence on tax farming was likely related to weak state capacity implying poor monitoring of and widespread corruption of VOC officials (for an analysis of such corruption in the VOC, see Adams, 1996).

In the early nineteenth century the Dutch state took over from the VOC and continued the use of tax farming. In 1810 more than twenty different taxes were farmed out. Among others, these included taxes on forest exploitation, fish markets and exports, vegetable stalls, slaughter of cattle, rice, traditional theatrical performances, gambling, sugar exports, and opium sales (Wahid, 2012). The auctions were publicly announced and the contracts generally lasted for one to three years and stipulated periodical payments. Chinese merchants dominated at the auctions. Their dominance was strengthened by an 1819 government decree prohibiting indigenous regents from conducting trade and other business while turning

them into salaried state officials with restricted claims to land and other traditional rights. By using tax farmers the colonial state could thus decouple revenue streams from such local powerholders. The ethnic Chinese were economically powerful, but they only made up about one percent of the Javanese population and held little political sway. In the late nineteenth century the share of tax farmers who were Chinese hovered around 80%. About four to six percent of adult ethnic Chinese males were tax farmers while less than 0.1% of adult indigenous males were tax farmers. Chinese dominance of the tax farming system may thus not only be related to their dominance in trade and their high degree of organization, but also to the fact that they represented less of a threat to the colonial state due to their minority status.

By co-opting the elites of different societal groups, the colonial state was able to maintain and gradually expand its grip. Tax farming fit into this system as it allowed the state to make use of Chinese organizations like the Chinese Council (*Kong Koan*) to operationalize tax farming. The Chinese Council consisted of the Chinese elite and they were often wealthy businessmen as well as tax farmers or their guarantors. The wealthy businessmen who participated in the auctions, particularly in the big opium farm auctions, thus possessed both economic resources in the form of capital and trade networks, and political resources in the form of official positions and prestige (Rush, 1983). Such community leaders faced lower collection costs, both due to the wealth of information they possessed but also because of social norms and interactions. The latter mainly applied within their own Chinese community. Moreover, exactly because of such social norms, overextraction by tax farmers would also be limited. The state could therefore expect high bids and relatively low required investments in overextraction monitoring. This mechanism might explain why certain small tax farms were specifically targeted at the Chinese community: the Chinese gambling farm, the Chinese tobacco farm, and the Chinese poll tax farm. However, in several general tax farms overextraction was a problem. The market farm was particularly notorious for tax farmers allegedly charging up to six times the official tax rate. It was abolished in the 1850s

after sparking discontent and unrest among the Javanese (Wahid, 2012).

From the 1830s onwards the colonial government split the tax farm administration into two: the opium tax farm (*opiumpacht*) and the small tax farms (*kleine verpachte middelen*) (Wahid, 2013a). The opium farm grew to be the greatest tax farming revenue source. Raw opium was imported from Bengal and Turkey by the state-affiliated Netherlands Trading Society (*Nederlandsche Handel-Maatschappij*) which had a monopoly on opium imports. The tax farmers then bought the opium and refined it for consumption before selling it in their specially-designated opium shops. In the case of the opium farm the tax farmers thus did not have to collect taxes on other people's activities. The tax farmers were the producers and sellers themselves. Such a system might have been devised to prevent the problem of overextraction as well as to retain stronger governmental control over the opium market. The potential social costs of widespread opium use were already recognized, but seen as perhaps more important was the protection of the government monopoly on opium imports. By dividing Java into separate non-overlapping opium farms the producers and distributors of refined opium were exactly known. This made it easier to combat the problem of smuggling. Moreover, as local opium monopolists the tax farmers themselves had every incentive to combat opium smuggling in their turfs. Opium farms therefore often employed a network of local thugs and spies and cultivated connections with indigenous village-level power structures (Rush, 1983).

Opium farm auctions were grand occasions drawing high- and low-rank officials (Wahid, 2013a). It also drew the wealthiest and most powerful Chinese in Java. For the winning bidder it meant not only gaining the tax farm, but also gaining prestige and patronage. Moreover, it provided a method to circumvent the government-decreed pass system which prohibited ethnic Chinese traders from accessing the countryside. In this case, complementarities thus existed between a tax farmer's trading business and a tax farmer's tax farming business. Before participating in the auction, the potential tax farmer formed a partner-

ship (a *kongsi*) and assessed the farm's revenue potential by studying local conditions. The partnership, which was often based on familial relationships, appointed two guarantors who, along with the tax farmer, would bind themselves to the farm contract. Behind such a partnership there was often a network of partnerships that provided funding to finance the farms. The government in the role of auctioneer, meanwhile, assessed the partnership's reliability and ability to deliver the monthly required tax fee. Moreover, it also tried to maintain competition by preventing extended domination of certain partnerships. The winning partnership often used sub-farming to operate the farm. Due to a prolonged economic crisis in the 1880s fifteen out of the nineteen opium farms went bankrupt. This decline in the tax farms' perceived reliability, as well as mounting criticism of the system among political circles in the Netherlands, increased the pressure for reform. As an experiment, in 1894 the opium farm on the small island of Madura was replaced by a state monopoly. From 1897 the tax farm system was gradually replaced with a state monopoly on the whole of Java. By 1904 this transition was completed, and by 1911 a similar transition was accomplished in the outlying islands (Wahid, 2013a).

The small tax farms contained many different farms, but the most important was the salt farm. The salt farm practices also contained problematic aspects. The tax farmers were accused of price and weight manipulation and providing usurious credit to salt-producing peasants (Wahid, 2013a). Partly because of these reasons, the state replaced the salt farm with a government monopoly in 1882. However, due to local opposition it was not fully enforced in the entire archipelago until 1914. Other tax farms gradually came to an end too. For example, the forest products farm was abolished in 1862, the fish farm and the palm sugar farm in 1863, the Chinese poll tax farm in 1865, the liquor farm in 1872, the slaughter farm in 1894, the tobacco farm in 1896, the Chinese gambling farm and the pawnshop farm in the 1890s, and the toll-gate and river crossing farm in 1915. As the auctions of small tax farms took place in different administrative residencies a tax farm might be abolished in Batavia, but still continue in a regional town. While most of the small tax farms were

replaced by direct taxation and collection by state officials, the salt farm and the pawnshop farm were replaced by a state monopoly as in the case of the opium farm.

The state-run monopolies and the direct tax collection required a larger bureaucracy. For example, the opium monopoly was supported by a special opium police force to combat smuggling. As in the old tax farm system, Java was subdivided into multiple regions. Each region had a Dutch civil servant as manager and chief of the opium police. Each regional head had a team of one inspector-in-chief, two inspectors, two deputy-inspectors and one additional staff member. All these were Dutch officials. The Dutch opium depot managers also hired several indigenous assistant-collectors to help in the distribution of opium to specially-designated opium dens which were run by indigenous officials. Similarly, the pawnshop monopoly and the transformation of small tax farms into directly-collected taxes also required an increase in the number of officials. Through this growth in the administrative capacities of the colonial state, the state gradually became more independent of two groups it had partially relied on for control: Chinese middlemen and indigenous elites (Van Zanden, 2010).

3.4 Data

I collect tax farm revenues, state-run revenues, and the number of Asian officials from the *Koloniale Verslagen* (henceforth, KV). Tax farm revenues consist of opium farm revenues and small tax farm revenues. State-run revenues consist of the state-run opium monopoly (*Opiumregie*) net revenues and the state-collected slaughter tax revenues. I use the database constructed in Chapter 1 (Hup, 2021a), also based on the KVs, for the number of indigenous and European officials and the amount of land-based steam power. Population figures are based on Boomgaard and Gooszen (1991). The extent of rice paddies is from Boomgaard and Van Zanden (1990). Average unskilled daily wages are from Dros (1992).

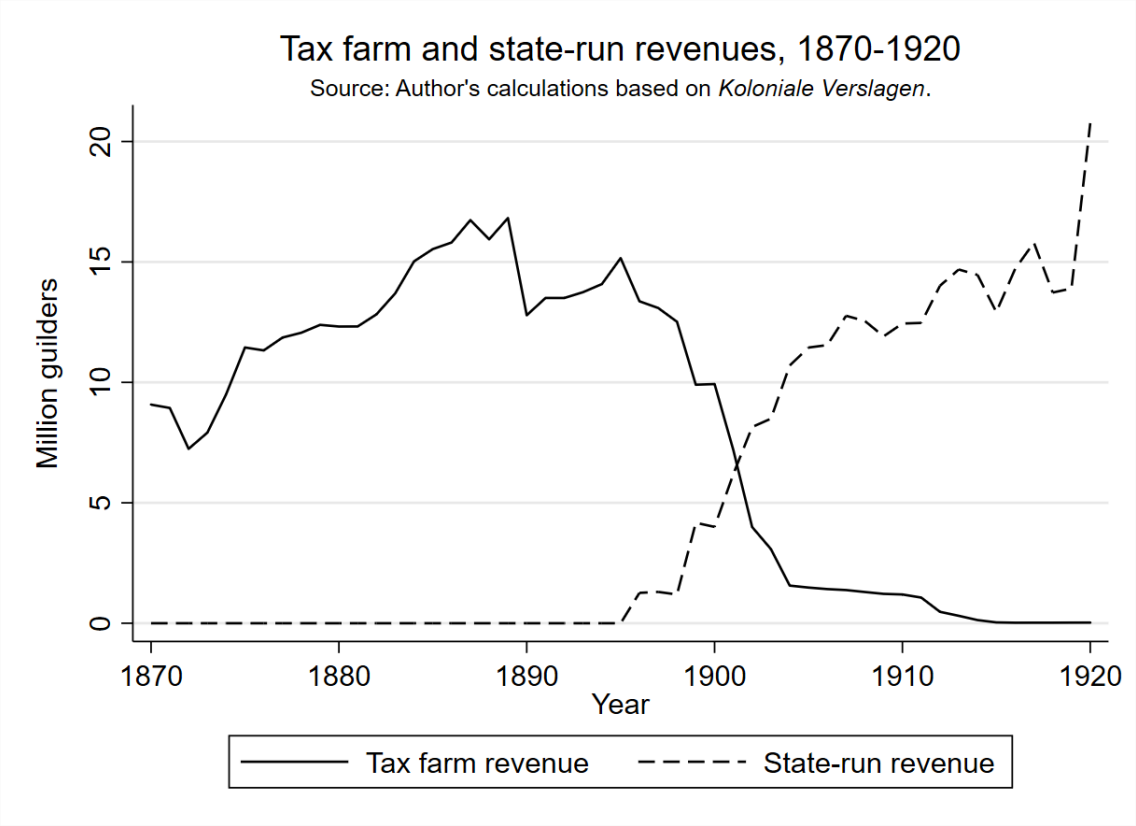

Figure 3.1

Figure 3.1 displays tax farm and state-run revenues on Java between 1870 and 1920.[3] Tax farm revenues generally rise until the late 1880s when the first sharp drop occurs. Revenues then stabilize until the mid-1890s after which revenues quickly collapse as tax farms are replaced by state-run monopolies and tax collection.

The tax farming phaseout occurred as the colonial state enhanced its information-collection and -analysis capabilities by expanding its bureaucracy. Figure 3.2 shows that between 1874 and 1905 the number of officials on Java roughly doubled from just over 15,000 to just over 30,000. These officials are persons employed by and on the payroll of the colonial state

[3]Java refers to all nineteen directly-ruled provinces. The two indirectly-ruled princely states, Djokjokarta and Soerakarta, are not included. Tax farm revenues include the following farms: opium, cattle slaughter, pawnshop, gambling, liquor, pig slaughter, toll bridges and river crossings, birds' nests, Chinese poll tax, indigenous businesses, tobacco, traditional theatrical performances, small islands, and forests. State-run revenues include (net) revenues from the opium monopoly and the slaughter taxes.

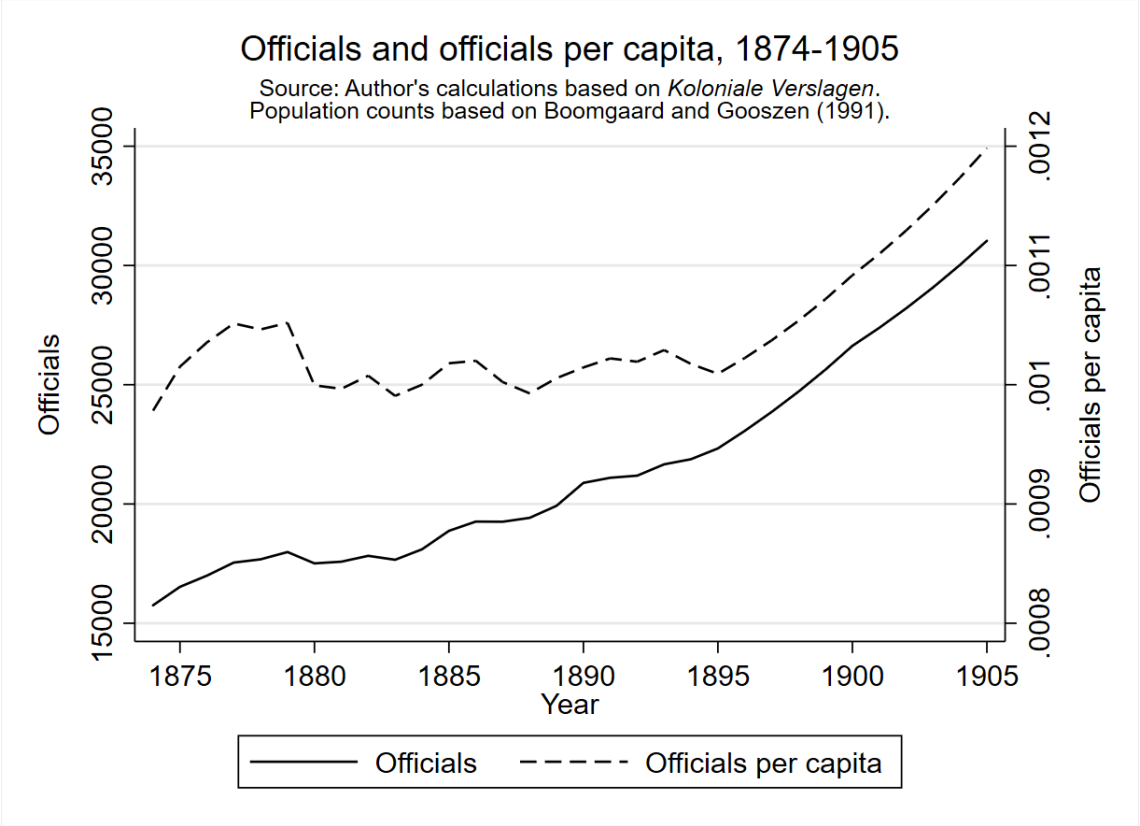

Figure 3.2

above the village-level.[4] Until about 1895 the growth in officialdom aligns with population growth as indicated by the stagnant number of officials per capita. After 1895 the number of officials grows quicker than the number of inhabitants. Note that the colonial state remains thin throughout this period. In 1905 the 0.0012 officials per capita translates into about 833 inhabitants per official. As most officials were adult men, and assuming a 25% population share of adult men, this means less than 1 in 200 adult men were officials.

Most officials were ethnically indigenous. They constituted 77.9% of officials in 1874 and 79.5% in 1905. European officials meanwhile constituted 20.6% in 1874 and 19.9% in 1905. The remainder, 1.5% in 1874 and 0.6% in 1905, were Asian officials. The terms 'indigenous', 'European', and 'Asian', refer to the state's racial classifications. The indigenous officials thus do not signify indirect rule as was practiced in the two Javanese princely states, Djokjokarta

[4]Village heads and village council members are thus not included.

and Soerakarta, that are not included here. All the included officials are on the payroll of the colonial state and part of its bureaucracy. They performed a wide variety of essential tasks including, but not limited to, collecting taxes, enforcing law and order, and overseeing public works projects.

3.5 Methodology

I use a panel data framework of nineteen Javanese provinces across thirty-two years (1874-1905) to investigate the relationship between tax farming and state capacity. To proxy for local state capacity I use the number of state officials in a province. To also capture potential differences across ethnic groups within state bureaucracies in each province, I separate the local officials into indigenous, European and Asian officials.

To investigate the relationship between state capacity and tax farming, I thus estimate the following models:

$$Log(TaxFarm_{it}) = Log(Officials_{it})\beta + X'_{it}\gamma + \alpha_i + \lambda_t + \epsilon_{it} \tag{3.1}$$

And:

$$Log(TaxFarm_{it}) = Log(IndiOff_{it})\beta_1 + Log(EuroOff_{it})\beta_2 + Log(AsiaOff_{it})\beta_3 + X'_{it}\gamma + \alpha_i + \lambda_t + \epsilon_{it} \tag{3.2}$$

Where for province i and year t: $TaxFarm_{it}$ = tax farm revenue, $Officials_{it}$ = number of officials, $IndiOff_{it}$ = number of indigenous officials, $EuroOff_{it}$ = number of European officials, $AsiaOff_{it}$ = number of Asian officials, X'_{it} = set of controls, α_i = province fixed effects,

and λ_t = year fixed effects. I use logged variables in order to interpret the coefficients as elasticities and because provinces varied widely in tax farm revenues and number of officials. In model (1), a positive coefficient estimate on the number of officials would indicate local state capacity expansion is related to decreased tax farm revenues. In model (2), different coefficients across groupings of officials would indicate that within the state bureaucracy different groups can have differing interests and preferences with respect to types of taxation and fiscal modernization.

The set of controls consists of certain factors that may be related to both state capacity and tax farming. For example, industrialization, agricultural expansion and wage levels may all be related to tax farm revenues as they relate to overall economic activity. At the same time, such factors may be related to state capacity as improvements may depend on a strong state presence protecting property rights and providing security. To control for these channels I include the amount of privately-owned land-based steam power, the extent of rice paddies (*sawah*), and average unskilled daily wages. Lastly, province fixed effects control for unobserved heterogeneity across provinces (e.g. geography, pre-colonial institutions) and year fixed effects control for common developments across time (e.g. national-level policies, technological advances).

To permit valid inference in the presence of potential within-state and within-year (i.e. geographic) correlation in the errors, I use two-way (province and year) clustered Driscoll-Kraay standard errors (Driscoll and Kraay, 1998; Cameron et al., 2011). To correct potential downward bias in the cluster-robust standard errors due to the small number of clusters, I apply a small-sample correction. To avoid attributing undue influence to the effects observed in small provinces, I weight observations by the number of inhabitants. Results are robust to using no weights. Results are also robust to allowing arbitrary dependence of the errors across provinces and years.

As potential endogeneity troubles a causal analysis of the impact of state capacity on tax

farming, I also use an instrumental variables (IV) estimation. I use the same IV as developed in Chapter 1 (Hup, 2021a).

The panel consists of nineteen Javanese provinces across thirty-two years. The two indirectly-ruled princely states, Djokjokarta and Soerakarta, are not included due to their special status and poorer data. I use the provincial boundaries of 1882 to 1900. I thus merge the provinces of Bezoeki and Banjoewangi, which were separate until 1882, and separate five provincial mergers that took place in 1901. The time coverage is limited to 1874-1905 due to data limitations on the province-level number of officials.

3.6 Results

Columns 1 and 2 in Panel A of Table 3.1 show the results of the main OLS regression using tax farm revenues as the dependent variable and the number of officials as the main explanatory variable. Columns 3 and 4 separate the officials into indigenous, European, and Asian officials. Each specification includes province and year fixed effects. Controls for the number of inhabitants, land-based steam power, rice paddies (*sawah*), and unskilled average daily wages are added in columns 2 and 4.[5] Panel B shows the results are robust to transforming variables into per capita terms.

The results are consistent across specifications with and without controls: more officials are associated with less tax farm revenues. In the preferred specification with controls (column 2), every one percent increase in officials is associated with a 1.87 percent decrease in tax farm revenues. Columns 3 and 4 indicate this negative elasticity between officials and tax farming only applies to indigenous and European officials, and applies most strongly to indigenous officials. In the preferred specification with controls (column 4), every one

[5]Since province-years without privately-owned land-based steam power exist, I add one to the amount of steam power to enable the logarithm.

Table 3.1: OLS estimates, tax farm revenue and officials

	(1)	(2)	(3)	(4)
Panel A: DV: Log(Tax Farm Revenue)				
Log(Officials)	-1.42**	-1.87***		
	(0.61)	(0.58)		
Log(Indigenous Officials)			-1.35**	-1.43***
			(0.57)	(0.47)
Log(European Officials)			-0.13	-0.51*
			(0.35)	(0.25)
Log(Asian Officials)			0.27*	0.15*
			(0.15)	(0.08)
Adjusted R^2	0.88	0.90	0.89	0.90
Panel B: DV: Log(Tax Farm Revenue per Capita)				
Log(Officials per Capita)	-1.04*	-1.25***		
	(0.54)	(0.42)		
Log(Indigenous Officials per Capita)			-0.96**	-1.01**
			(0.45)	(0.36)
Log(European Officials per Capita)			-0.14	-0.35*
			(0.24)	(0.19)
Log(Asian Officials per Capita)			0.25*	0.17
			(0.14)	(0.11)
Adjusted R^2	0.89	0.89	0.89	0.90
Controls	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	608	608	608	608

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

DV means dependent variable. Controls consist of the log of, respectively, number of inhabitants (not used in panel B), steam power, rice paddies, and wages. Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (19 province clusters, 32 year clusters). Observations are weighted by the number of inhabitants. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

percent increase in indigenous officials is associated with a 1.43 percent decrease in tax farm revenues. Meanwhile, every one percent increase in European officials is associated with only a 0.51 percent decrease in tax farm revenues. In contrast to these negative point estimates, increases in Asian officials are positively associated with tax farm revenues: every one percent increase in Asian officials is associated with a 0.15 percent increase in tax farm revenues.

Table 3.2 shows the IV results for the three different instruments. Panel A uses all officials, while Panel B uses only indigenous officials. Each specification includes province and year fixed effects. Controls are added in columns 2, 4, and 6. For the preferred specifications with controls, the first-stage F-statistics range from 18 to 32, suggesting the estimates do not suffer from the weak instrument problem (Stock and Yogo, 2005). The IV estimates are qualitatively consistent with the OLS estimates, and robust to the choice of instrument, but the impact of officials on tax farm revenues is larger: a one percent increase in (indigenous) officials decreases tax farm revenue by about 3 to 4 percent.

Both the OLS and the IV results are robust to using spatial correction of standard errors (Table C.2 and Table C.3), using no population weights (Table C.4 and Table C.5), and excluding provinces one at a time (results not reported).

The findings provide evidence for both hypotheses: (1) state capacity expansion is related to decreased reliance on tax farming, and; (2) the type of state capacity expansion matters for reliance on tax farming. Bureaucrats from the group traditionally excluded from tax farming (i.e. the indigenous) have a strong negative relationship with tax farming. In contrast, bureaucrats from the group purposefully wooed by the state as tax farmers (i.e. the Asians) have a positive relationship with tax farming. Also, bureaucrats from the group in power (i.e. the Europeans) have a negative relationship with tax farming that lies between those of the excluded and wooed groups.

Table 3.2: IV estimates, tax farm revenue and officials

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A1 Second Stage: DV: Log(Tax Farm Revenue)						
Log(Officials)	-4.44**	-3.86***	-4.37**	-3.91***	-4.81**	-4.18***
	(2.08)	(1.12)	(1.76)	(1.18)	(1.92)	(1.19)
Panel A2 First Stage: DV: Log(Officials)						
Instrument	-86.86***	-93.07***	-0.03***	-0.03***	-0.08***	-0.09***
	(20.53)	(16.34)	(0.01)	(0.01)	(0.02)	(0.02)
First-stage F	17.91	32.46	22.99	26.05	18.38	32.45
Panel B1 Second Stage: DV: Log(Tax Farm Revenue)						
Log(Indigenous Officials)	-3.38**	-3.02**	-3.36**	-3.07**	-3.80**	-3.43***
	(1.56)	(1.13)	(1.43)	(1.16)	(1.51)	(1.19)
Panel B2 First Stage: DV: Log(Indigenous Officials)						
Instrument	-114.29***	-118.90***	-0.04***	-0.04***	-0.10***	-0.11***
	(24.25)	(23.92)	(0.01)	(0.01)	(0.02)	(0.02)
First-stage F	22.22	24.72	19.33	18.95	18.54	21.18
Controls	No	Yes	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	608	608	608	608	608	608
Instrument	Distance/Year		Distance/Ships		Distance/Passengers	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

DV means dependent variable. Controls consist of the log of, respectively, number of inhabitants, steam power, rice paddies, and wages. Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (19 province clusters, 32 year clusters). Observations are weighted by the number of inhabitants. First-stage F is the Kleibergen-Paap rk Wald F statistic. Regarding the instruments, distance is in kilometers and passengers is in thousands. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

3.7 Conclusion

Using a newly-constructed province-level database for colonial Java, this study finds that state capacity expansion was related to decreasing reliance on tax farming and that different segments of the state bureaucracy had different relationships with tax farming. The decreasing reliance on tax farming was due to state policy pushing for replacement of tax farming with state-run monopolies and tax collection. Such a replacement strengthened central control over taxation. This replacement required sufficient information-gathering and -analysis capabilities of the state, as well as sufficient monitoring of the bureaucrats now in charge of these revenue streams, so it was a gradual process spread over roughly two decades starting from the early 1890s.

A one percent increase in local state officials decreased tax farm revenues by more than one percent. This negative relationship runs through indigenous and European officials, not Asian officials. The latter group stemmed from a population traditionally wooed by the colonial state as tax farmers, partially with an eye on diverting control over revenue streams away from indigenous powerholders. As the colonial state expanded it became less dependent on middlemen such as tax farmers, and could gradually take control of the farmed-out monopolies and taxes. As it did so, this particularly offered opportunities to the erstwhile excluded group: the indigenous. As they made up the majority of state officials, and were barely involved in tax farming and therefore had little to lose from phasing out tax farming, they benefited from and pushed for putting farmed-out revenue streams under bureaucratic control.

As found in Chapter 1 (Hup, 2021a) in the case of corvée labor, local state actors tended to solidify their position in the system by increasing forms of state-backed extraction over which they had more control (i.e. indigenous officials and corvée) and by decreasing forms of state-backed extraction over which they had less control (i.e. indigenous officials and tax

farming). The Indonesian colonial state was not a unitary actor. Within the state bureaucracy different, and potentially conflicting, interests over the structure of taxation existed. The historically widespread use of tax farming helps to give this study's findings wider resonance. Relationships between state capacity and taxation levels, between state capacity and fiscal modernization, may depend on the internal workings of the state bureaucracy and on the type of state capacity expansion. Studying (groups of) actors within the state can thus be worthwhile in better understanding what drives changes in the structure of taxation and a state's fiscal capacity.

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Appendix A

Appendix for Chapter 1

A.1 Data Construction

Table A.1 lists the variable definitions and sources. I collect days, poll tax revenues, officials, and steam power from the Colonial Reports (*Koloniale Verslagen*, henceforth KV). Days are the 'performed daily services' (*gepresteerde dagdiensten*). I value used days at province-year specific average unskilled daily wages from Dros (1992). See below for details on the wages.

The regressions' time period is limited to 1874-1905 as the KVs do not report the number of officials in each province before 1873 and after 1905. Due to the incomplete data on officials for 1873, I do not include that year. For some provinces in some years the number of officials is not reported or misreported. In those cases I estimate in one of two ways: (i) adjustment by using notes in the KVs, or; (ii) interpolation by using the previous known year, the next known year, and a geometric growth rate. For the period 1873-1895 the KVs report the number of officials each year. For the period 1896-1905, however, the KVs only report it every five years. Therefore I interpolate the years 1896-1899 and 1901-1904. I also interpolate the number of European officials for Soerabaija in the years 1876-1880 and the

Table A.1: Variable definitions and sources

	Definition	Source
Outcome variables		
Days	Used days of corvée labor.	*Koloniaal Verslag*, various issues.
Days Value	Value of used days of corvée labor.	Author's calculation.
Poll Tax	Poll tax revenues.	*Koloniaal Verslag*, various issues.
Main explanatory variable		
Officials	Number of European and indigenous state officials.	*Koloniaal Verslag*, various issues.
Control variables		
Adult Males	Number of adult indigenous males.	Boomgaard and Gooszen (1991).
Steam Power	Amount of privately-owned land-based steam power.	*Koloniaal Verslag*, various issues.
Sawah	Arable rice paddies in thousand hectares.	Boomgaard and Van Zanden (1990).
Wages	Average unskilled daily wages in cents.	Dros (1992).
Instrumental variables components		
Distance	Kilometers between province's centroid and Batavia's centroid.	Author's calculation.
Ships	Number of Javanese steam ships.	Knaap (1989).
Passengers	Number of passengers on Javanese steam ships in thousands.	Knaap (1989).

number of indigenous officials for Samarang 1881 and Soearabaija 1883. Lastly, I adjust the number of indigenous officials for Bantam 1883, Tagal 1880, and Bagelen 1874.

The KVs list European officials as 'persons in the civil service and paid by the government' (*personen in 's lands burgerlijken dienst en door het Gouvernement bezoldigd*). For the period 1874-1882, the KVs list indigenous officials as 'paid heads of standing', 'government-appointed but unpaid heads of standing', and as 'paid officials' (respectively *hoofden van rang, bezoldigd*, *hoofden van rang, door het Gouvernement aangesteld maar niet bezoldigd*, and *bezoldigde beambten*). From 1883 on the KVs list indigenous officials as 'heads of the populace and officials in the government's service' (*hoofden der bevolking en ambtenaren of beambten in dienst van het Gouvernement*). As described in the text, my count of the number of indigenous officials does not include village heads and village council members (*hoofden van dessa's of kampongs, zoomede de nog in functie zijnde leden van het dessa- of kampong-bestuur*).

My count of steam power concerns the amount of heated surface area in square meters produced by privately-owned land-based steam boilers (*stoomketels*). For the years 1888-1896 the KVs do not list land-based steam power for each province separately from ship-based steam power. The Java-wide total ship-based steam power, however, is given. Moreover, for 1897 both land- and ship-based steam power are listed for each province. I assume provinces without ship-based steam power in 1897 also had none in 1888-1896. I therefore only estimate the land-based steam power in 1888-1896 for each province that had non-zero ship-based steam power in 1897. I do so by using the land-based steam power in 1887 and 1897 and a geometric growth rate. This applies to Cheribon, Tagal, Pekalongan, Samarang, Soerabaija, Pasoeroean, Probolinggo, and Bezoeki. I adjust Bezoeki 1884 for an error in the KV.

I estimate the number of adult indigenous males using indigenous population figures from Boomgaard and Gooszen (1991, Tables 2, 4, and 5). I assume a quarter of the total indigenous

population consisted of adult males. I base this on population figures for 1895 which show adult males constituted a quarter of the total indigenous population on Java (KV 1897, Appendix A, p. 32). Since Boomgaard and Gooszen (1991) do not list the indigenous population for the years 1896-1899, 1901-1904, and 1906-1911, I interpolate these years using the previous known year, the next known year, and a geometric growth rate.

The extent of arable rice paddies (*sawah*) in thousand hectares is from Boomgaard and Van Zanden (1990, Tables 3A.1 and 3A.2).

The average unskilled daily wages in cents are those for plantation coolies from Dros (1992, Table 5.4). Dros (1992) lists the minimum and maximum observed wage in a given province-year. I take the average of both. Due to missing observations I estimate for all provinces in 1874 and 1896 and for the following province-years: Bantam 1891; Krawang 1901-1902; Tagal 1877, 1888, 1901-1902; Samarang 1878, 1901-1902; Japara 1888-1890, 1901-1902; Rembang 1877, 1893, 1895; Pasoeroean 1893-1895; Probolinggo 1890, 1901-1902; Bagelen 1887, 1901-1905, and; Kediri 1894. I also adjust Samarang 1889 since the reported minimum and maximum, around three times those in the previous and the next year, seem like reporting errors. In all these cases, except for 1874, I use the arithmetic average of the previous known and next known wage. For 1874 I use the 1875 wages. Note that there are cases in which the minimum and maximum wages in a particular province did not change from one year to the next.

I calculate the distance in kilometers between a province's centroid and Batavia's centroid. I take the number of steam ships and steam ship passengers from Knaap (1989, Tables 6A and 6B). The steam ships belonged to the packet-boat service companies Netherlands-Indies Steamboat Company (until 1890) and Royal Packet-boat Service Company (from 1891 on).

I use the provincial boundaries of 1882 to 1900. I thus merge the provinces of Bezoeki and Banjoewangi, which were separate until 1882, and separate five provincial mergers that took

place in 1901. The merged pairs concern Batavia-Krawang, Pekalongan-Tegal, Samarang-Japara, Pasoeroean-Probolinggo, and Kadoe-Bagelen. To separate the mergers I assign to each province in a pair the share of that province in the pre-merger pair's total of days, steam power, adult males, and *sawah* in 1898-1900.

Since the KVs report Krawang's used corvée days separately, I do not use this share assignment for Batavia-Krawang. Boomgaard and Van Zanden (1990), however, list only one *sawah* figure for Batavia and Krawang for the whole period 1874-1905. For 1874-1880 they label it Krawang and for 1880-1905 Batavia. I take the difference in 1880, the year of overlap, to be Batavia's *sawah* and assume it to be fixed. I then estimate Krawang's *sawah* for 1874-1905 by subtracting Batavia's estimate from the given total.

To separate the number of officials I use each province's share in 1893-1895 as officials in 1900 are only reported in the post-merger status. This is due to the 1900 numbers being reported in KV 1902, at which time the mergers had already taken place.

For wages in the merged Bezoeki-Banjoewangi province pre-1882, I average the minimum and maximum of both provinces. For days, officials, adult males, steam power, and *sawah* in pre-1882 Bezoeki-Banjoewangi, I add both provinces' figures.

A.2 Additional Regression Tables

Table A.2 and Table A.3 report OLS and IV estimates without including year fixed effects. Table A.4 and Table A.5 report OLS and IV estimates with used corvée value as dependent variable. Table A.6 and Table A.7 report OLS and IV estimates with wild cluster bootstrap standard errors. Table A.8 and Table A.9 report OLS and IV estimates with spatial correction of standard errors. Table A.10 and Table A.11 report OLS and IV estimates with no population weights.

Table A.2: OLS estimates, corvée and officials, no year FE

	(1)	(2)	(3)	(4)
Panel A: DV: Log(Days)				
Log(Officials)	-0.68***	0.09		
	(0.19)	(0.20)		
Log(European Officials)			-0.41***	-0.09
			(0.13)	(0.19)
Log(Indigenous Officials)			-0.31*	0.10
			(0.16)	(0.16)
Adjusted R^2	0.73	0.77	0.74	0.77
Panel B: DV: Log(Days PAM)				
Log(Officials PAM)	-0.63	0.40		
	(0.48)	(0.33)		
Log(European Officials PAM)			-0.62	-0.13
			(0.38)	(0.24)
Log(Indigenous Officials PAM)			-0.41	0.36
			(0.45)	(0.27)
Adjusted R^2	0.22	0.58	0.27	0.58
Controls	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	No	No	No	No
Observations	576	576	576	576

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

DV means dependent variable. PAM means per indigenous adult male. Controls consist of the log of, respectively, indigenous adult males (not used in panel B), steam power, rice paddies, and wages. Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (18 province clusters, 32 year clusters). Observations are weighted by the number of indigenous adult males. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

Table A.3: IV estimates, corvée and officials, no year FE

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A1 Second Stage: DV: Log(Days)						
Log(Officials)	-0.81***	-0.26	-0.76***	-0.21	-0.72***	-0.12
	(0.21)	(0.46)	(0.18)	(0.31)	(0.22)	(0.39)
Panel A2 First Stage: DV: Log(Officials)						
Instrument	-167.78***	-95.19***	-0.05***	-0.02***	-0.15***	-0.06***
	(16.00)	(10.81)	(0.01)	(0.00)	(0.03)	(0.01)
First-stage F	109.98	77.53	44.51	24.45	32.82	21.11
Panel B1 Second Stage: DV: Log(Days)						
Log(Indigenous Officials)	-0.77***	-0.21	-0.72***	-0.18	-0.67***	-0.10
	(0.22)	(0.39)	(0.18)	(0.27)	(0.21)	(0.34)
Panel B2 First Stage: DV: Log(Indigenous Officials)						
Instrument	-177.48***	-114.63***	-0.06***	-0.03***	-0.15***	-0.07***
	(19.30)	(19.73)	(0.01)	(0.01)	(0.03)	(0.02)
First-stage F	84.55	33.75	37.20	18.29	29.52	15.93
Controls	No	Yes	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	No	No	No	No	No
Observations	576	576	576	576	576	576
Instrument	Distance/Year		Distance/Ships		Distance/Passengers	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

DV means dependent variable. Controls consist of the log of, respectively, indigenous adult males, steam power, rice paddies, and wages. Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (18 province clusters, 32 year clusters). Observations are weighted by the number of indigenous adult males. First-stage F is the Kleibergen-Paap rk Wald F statistic. Regarding the instruments, distance is in kilometers and passengers is in thousands. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

Table A.4: OLS estimates, corvée value and officials

	(1)	(2)	(3)	(4)
Panel A: DV: Log(Corvée Value)				
Log(Officials)	0.69***	0.64***		
	(0.20)	(0.19)		
Log(European Officials)			0.16	0.13
			(0.12)	(0.14)
Log(Indigenous Officials)			0.54***	0.51***
			(0.15)	(0.15)
Adjusted R^2	0.86	0.87	0.86	0.87
Panel B: DV: Log(Corvée Value PAM)				
Log(Officials PAM)	0.63***	0.64***		
	(0.18)	(0.17)		
Log(European Officials PAM)			0.11	0.13
			(0.12)	(0.13)
Log(Indigenous Officials PAM)			0.51***	0.52***
			(0.14)	(0.14)
Adjusted R^2	0.87	0.87	0.87	0.87
Controls	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	576	576	576	576

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

DV means dependent variable. PAM means per indigenous adult male. Controls consist of the log of, respectively, indigenous adult males (not used in panel B), steam power, and rice paddies. Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (18 province clusters, 32 year clusters). Observations are weighted by the number of indigenous adult males. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

Table A.5: IV estimates, corvée value and officials

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A1 Second Stage: DV: Log(Corvée Value)						
Log(Officials)	1.74***	1.70***	1.16**	1.13**	1.41**	1.37***
	(0.49)	(0.38)	(0.44)	(0.41)	(0.52)	(0.47)
Panel A2 First Stage: DV: Log(Officials)						
Instrument	-81.99***	-91.22***	-0.03***	-0.03***	-0.08***	-0.08***
	(23.20)	(17.66)	(0.01)	(0.01)	(0.02)	(0.01)
First-stage F	12.49	26.67	22.84	29.26	13.90	29.93
Panel B1 Second Stage: DV: Log(Corvée Value)						
Log(Indigenous Officials)	1.31**	1.35***	0.90**	0.91**	1.08**	1.11**
	(0.47)	(0.42)	(0.41)	(0.38)	(0.49)	(0.45)
Panel B2 First Stage: DV: Log(Indigenous Officials)						
Instrument	-108.30***	-115.16***	-0.04***	-0.04***	-0.10***	-0.10***
	(26.89)	(27.20)	(0.01)	(0.01)	(0.03)	(0.02)
First-stage F	16.21	17.93	20.13	18.91	15.12	17.52
Controls	No	Yes	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	576	576	576	576	576	576
Instrument	Distance/Year		Distance/Ships		Distance/Passengers	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

DV means dependent variable. Controls consist of the log of, respectively, indigenous adult males, steam power, and rice paddies. Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (18 province clusters, 32 year clusters). Observations are weighted by the number of indigenous adult males. First-stage F is the Kleibergen-Paap rk Wald F statistic. Regarding the instruments, distance is in kilometers and passengers is in thousands. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

Table A.6: OLS estimates, corvée and officials, wild cluster bootstrap

	(1)	(2)	(3)	(4)
Panel A: DV: Log(Days)				
Log(Officials)	0.71	0.71		
	(0.002)	(0.002)		
	[0.43, 0.97]	[0.39, 1.02]		
Log(European Officials)			-0.02	0.01
			(0.852)	(0.950)
			[-0.24, 0.20]	[-0.27, 0.27]
Log(Indigenous Officials)			0.66	0.62
			(0.003)	(0.001)
			[0.43, 0.88]	[0.37, 0.89]
Panel B: DV: Log(Days PAM)				
Log(Officials PAM)	0.74	0.77		
	(0.011)	(0.015)		
	[0.31, 1.18]	[0.34, 1.18]		
Log(European Officials PAM)			-0.05	0.02
			(0.542)	(0.890)
			[-0.26, 0.15]	[-0.26, 0.27]
Log(Indigenous Officials PAM)			0.67	0.67
			(0.010)	(0.005)
			[0.32, 1.03]	[0.32, 0.99]
Controls	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	576	576	576	576

Wild cluster bootstrap p-values in parentheses and wild cluster bootstrap 95% confidence intervals in square brackets. The bootstrap is based on the regressions in Table 1. Implemented using Roodman et al.'s (2019) `boottest` Stata package. DV means dependent variable. PAM means per indigenous adult male. Controls consist of the log of, respectively, indigenous adult males (not used in panel B), steam power, rice paddies, and wages.

Table A.7: IV estimates, corvée and officials, wild cluster bootstrap

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A Second Stage: DV: Log(Days)						
Log(Officials)	2.08	1.84	1.61	1.40	1.78	1.56
	(0.014)	(0.025)	(0.020)	(0.021)	(0.010)	(0.022)
	[0.54, 4.08]	[0.77, 3.27]	[0.56, 2.69]	[0.42, 2.57]	[0.70, 3.28]	[0.47, 2.80]
Panel B Second Stage: DV: Log(Days)						
Log(Indigenous Officials)	1.57	1.47	1.25	1.14	1.37	1.27
	(0.014)	(0.027)	(0.025)	(0.027)	(0.014)	(0.035)
	[0.68, 2.60]	[0.57, 2.67]	[0.41, 2.21]	[0.20, 2.36]	[0.54, 2.39]	[0.32, 2.41]
Controls	No	Yes	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	576	576	576	576	576	576
Instrument	Distance/Year		Distance/Ships		Distance/Passengers	

Wild cluster bootstrap equal-tailed p-values in parentheses and wild cluster bootstrap 95% confidence intervals in square brackets. The bootstrap is based on the regressions in Table 2. Implemented using Roodman et al.'s (2019) `boottest` Stata package. DV means dependent variable. Controls consist of the log of, respectively, indigenous adult males, steam power, and rice paddies. Regarding the instruments, distance is in kilometers and passengers is in thousands.

Table A.8: OLS estimates, corvée and officials, spatial correction of standard errors

	(1)	(2)	(3)	(4)
Panel A: DV: Log(Days)				
Log(Officials)	0.71***	0.71***		
	(0.12)	(0.13)		
Log(European Officials)			-0.02	0.01
			(0.07)	(0.08)
Log(Indigenous Officials)			0.66***	0.62***
			(0.10)	(0.10)
Panel B: DV: Log(Days PAM)				
Log(Officials PAM)	0.74***	0.77***		
	(0.15)	(0.15)		
Log(European Officials PAM)			-0.05	0.02
			(0.07)	(0.08)
Log(Indigenous Officials PAM)			0.67***	0.67***
			(0.13)	(0.12)
Controls	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	576	576	576	576

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Estimator allows for arbitrary dependence of the errors across provinces and across years. Implemented using Colella et al.'s (2019) `acreg` Stata package. Standard errors are heteroskedastic- and autocorrelation-consistent (HAC) with a linear decay in distance (Bartlett). Distance matrix is based on the longitude and latitude of each province's centroid. Distance cutoff is 1,000 kilometers. Time lag is ten years. DV means dependent variable. PAM means per indigenous adult male. Controls consist of the log of, respectively, indigenous adult males (not used in panel B), steam power, rice paddies, and wages. Observations are weighted by the number of indigenous adult males.

Table A.9: IV estimates, corvée and officials,
spatial correction of standard errors

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A1 Second Stage: DV: Log(Days)						
Log(Officials)	2.08***	1.84***	1.61***	1.40***	1.78***	1.56***
	(0.45)	(0.33)	(0.32)	(0.29)	(0.36)	(0.32)
Panel A2 First Stage: DV: Log(Officials)						
Instrument	-81.99***	-90.87***	-0.03***	-0.03***	-0.08***	-0.08***
	(15.43)	(13.84)	(0.01)	(0.01)	(0.01)	(0.01)
First-stage F	28.31	41.90	35.82	38.61	29.97	43.29
Panel B1 Second Stage: DV: Log(Days)						
Log(Indigenous Officials)	1.57***	1.47***	1.25***	1.14***	1.37***	1.27***
	(0.30)	(0.27)	(0.25)	(0.24)	(0.28)	(0.27)
Panel B2 First Stage: DV: Log(Indigenous Officials)						
Instrument	-108.30***	-114.12***	-0.04***	-0.04***	-0.10***	-0.10***
	(17.47)	(17.21)	(0.01)	(0.01)	(0.02)	(0.02)
First-stage F	27.51	34.67	29.63	31.94	24.56	30.17
Controls	No	Yes	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	576	576	576	576	576	576
Instrument	Distance/Year		Distance/Ships		Distance/Passengers	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Estimator allows for arbitrary dependence of the errors across provinces and across years. Implemented using Colella et al.'s (2019) `acreg` Stata package. Standard errors are heteroskedastic- and autocorrelation-consistent (HAC) with a linear decay in distance (Bartlett). Distance matrix is based on the longitude and latitude of each province's centroid. Distance cutoff is 1,000 kilometers. Time lag is ten years. DV means dependent variable. Controls consist of the log of, respectively, indigenous adult males, steam power, rice paddies, and wages. Observations are weighted by the number of indigenous adult males. First-stage F is the Kleibergen-Paap rk Wald F statistic. Regarding the instruments, distance is in kilometers and passengers is in thousands.

Table A.10: OLS estimates, corvée and officials, no weights

	(1)	(2)	(3)	(4)
Panel A: DV: Log(Days)				
Log(Officials)	0.66***	0.64***		
	(0.16)	(0.16)		
Log(European Officials)			-0.00	-0.01
			(0.08)	(0.09)
Log(Indigenous Officials)			0.61***	0.58***
			(0.14)	(0.13)
Adjusted R^2	0.87	0.88	0.87	0.88
Panel B: DV: Log(Days PAM)				
Log(Officials PAM)	0.61***	0.66***		
	(0.18)	(0.17)		
Log(European Officials PAM)			-0.05	-0.01
			(0.08)	(0.10)
Log(Indigenous Officials PAM)			0.57***	0.60***
			(0.15)	(0.14)
Adjusted R^2	0.82	0.82	0.82	0.82
Controls	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	576	576	576	576

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

DV means dependent variable. PAM means per indigenous adult male. Controls consist of the log of, respectively, indigenous adult males (not used in panel B), steam power, rice paddies, and wages. Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (18 province clusters, 32 year clusters). Observations are not weighted. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

Table A.11: IV estimates, corvée and officials, no weights

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A1 Second Stage: DV: Log(Days)						
Log(Officials)	1.99***	1.83***	1.55***	1.41***	1.71***	1.64***
	(0.63)	(0.54)	(0.45)	(0.44)	(0.48)	(0.46)
Panel A2 First Stage: DV: Log(Officials)						
Instrument	-68.75**	-76.51***	-0.03***	-0.03***	-0.07***	-0.07***
	(24.03)	(18.33)	(0.01)	(0.01)	(0.02)	(0.02)
First-stage F	8.19	17.41	18.78	17.69	15.33	20.21
Panel B1 Second Stage: DV: Log(Days)						
Log(Indigenous Officials)	1.78***	1.61***	1.40***	1.25***	1.52***	1.46***
	(0.53)	(0.44)	(0.40)	(0.37)	(0.44)	(0.42)
Panel B2 First Stage: DV: Log(Indigenous Officials)						
Instrument	-76.87**	-87.04***	-0.03***	-0.03***	-0.08***	-0.08***
	(27.63)	(22.69)	(0.01)	(0.01)	(0.02)	(0.02)
First-stage F	7.74	14.71	13.46	13.90	12.55	13.80
Controls	No	Yes	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	576	576	576	576	576	576
Instrument	Distance/Year		Distance/Ships		Distance/Passengers	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

DV means dependent variable. Controls consist of the log of, respectively, indigenous adult males, steam power, rice paddies, and wages. Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (18 province clusters, 32 year clusters). Observations are not weighted. First-stage F is the Kleibergen-Paap rk Wald F statistic. Regarding the instruments, distance is in kilometers and passengers is in thousands. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

Appendix B

Appendix for Chapter 2

B.1 Data Construction

Table B.1 lists the variable definitions and sources. I collect used days, maximum days, liable, ransom, and ransomers from reports produced by the colonial state in Indonesia (*Koloniale Verslagen*, *Indische Verslagen*, and *Statistische Jaaroverzichten*, henceforth KV, IV, and SJ). Used days are the 'exacted daily services' (*gevorderde dagdiensten*). I collect export statistics from trade overviews produced by the colonial state in Indonesia (*Statistiek van den handel* and *Jaaroverzicht in- en uitvoer*, henceforth SH and JA)). Indigenous population figures are based on Boomgaard and Gooszen (1991) and Bosma (2011). Due to gaps or errors in the sources certain province-years for certain variables are estimated.

Table B.1: Variable definitions and sources

	Definition	Source
Components of variables		
Used Days	Used days of corvée labor.	KV, IV and SJ, various issues.
Maximum Days	Maximum days of corvée labor.	KV, IV and SJ, various issues.
Liable	Number of people liable to corvée.	KV, IV and SJ, various issues.
Ransom	Ransom revenues.	KV, IV and SJ, various issues.
Ransomers	Number of ransomers.	KV, IV and SJ, various issues.
Exports	Value of exports.	SH and JA, various issues.
Potential Exports	Value of potential exports.	Author's calculation based on SH and JA, various issues.
Population	Size of indigenous population.	Boomgaard and Gooszen (1991) and Bosma (2011).

B.2 Additional Regression Tables

Table B.2 uses lagged exports, Table B.3 further lags potential exports, and Table B.4 and Table B.5 do not apply a per capita adjustment.

Table B.2: OLS estimates, corvée and lagged non-oil exports

	Log(Used Days per Capita)	Log(Liable per Capita)	Log(Max Days / Liable)	Log(Used Days / Max Days)
Panel A: Log(Exports per Capita, No Oil (t-1))				
Log(Exports per Capita, No Oil (t-1))	-0.23**	-0.01	0.00	-0.21***
	(0.09)	(0.04)	(0.03)	(0.07)
Adjusted R^2	0.50	0.83	0.84	0.45
Observations	535	535	535	535
Panel B: Log(Exports per Capita, No Oil (t-2))				
Log(Exports per Capita, No Oil (t-2))	-0.20**	-0.02	0.00	-0.18**
	(0.08)	(0.04)	(0.03)	(0.06)
Adjusted R^2	0.49	0.83	0.84	0.44
Observations	525	525	525	525
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (16 province clusters, and 39-40 year clusters). Observations are weighted by the number of liable persons. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

Table B.3: OLS estimates, corvée and lagged non-oil potential exports

	Log(Used Days per Capita)	Log(Liable per Capita)	Log(Max Days / Liable)	Log(Used Days / Max Days)
Log(Potential Exports per Capita, No Oil (t-2))	-0.08	0.02	0.00	-0.10***
	(0.08)	(0.05)	(0.02)	(0.03)
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.47	0.91	0.84	0.44
Observations	525	525	525	525

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (16 province clusters, and 39 year clusters). Observations are weighted by the number of liable persons. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

Table B.4: OLS estimates, corvée and exports, no per capita adjustment

	Log(Used Days)	Log(Liable)	Log(Max Days / Liable)	Log(Used Days / Max Days)
Panel A: Log(Exports)				
Log(Exports)	-0.12	0.01	0.02	-0.15*
	(0.12)	(0.06)	(0.04)	(0.08)
Adjusted R^2	0.47	0.91	0.84	0.44
Panel B: Log(Exports, No Oil)				
Log(Exports, No Oil)	-0.15**	0.02	0.01	-0.18**
	(0.06)	(0.05)	(0.03)	(0.07)
Adjusted R^2	0.47	0.91	0.84	0.44
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	545	545	545	545

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (16 province clusters, 41 year clusters). Observations are weighted by the number of liable persons. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

Table B.5: OLS estimates, corvée and potential exports, no per capita adjustment

	Log(Used Days)	Log(Liable)	Log(Max Days / Liable)	Log(Used Days / Max Days)
Panel A: Log(Potential Exports)				
Log(Potential Exports)	-0.08	0.02	0.02	-0.12
	(0.13)	(0.06)	(0.04)	(0.07)
Adjusted R^2	0.47	0.91	0.84	0.44
Panel B: Log(Potential Exports, No Oil)				
Log(Potential Exports, No Oil)	-0.10**	0.03	0.01	-0.15***
	(0.05)	(0.05)	(0.03)	(0.03)
Adjusted R^2	0.47	0.91	0.84	0.44
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	535	535	535	535

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (16 province clusters, 40 year clusters). Observations are weighted by the number of liable persons. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

Appendix C

Appendix for Chapter 3

C.1 Data Construction

Table C.1 lists the variable definitions and sources. I collect tax farm revenues, state-run revenues, and the number of Asian officials from the *Koloniale Verslagen* (henceforth, KV). Tax farm revenues consist of opium farm revenues and small tax farm revenues. State-run revenues consist of the state-run opium monopoly (*Opiumregie*) net revenues and the state-collected slaughter tax revenues. I use the database constructed in Chapter 1 (Hup, 2021a), also based on the KVs, for the number of indigenous and European officials and the amount of land-based steam power. Population figures are based on Boomgaard and Gooszen (1991). The extent of rice paddies is from Boomgaard and Van Zanden (1990). Average unskilled daily wages are from Dros (1992). Due to gaps or errors in the sources certain province-years for certain variables are estimated.

Table C.1: Variable definitions and sources

	Definition	Source
Outcome variable		
Tax Farm Revenue	Tax farm revenue (opium farm and small tax farms).	KV, various issues.
Main explanatory variable		
Officials	Number of indigenous, European, and Asian state officials.	Hup (2021a) and KV, various issues.
Control variables		
Population	Number of inhabitants.	Boomgaard and Gooszen (1991).
Steam Power	Amount of privately-owned land-based steam power.	Hup (2021a).
Sawah	Arable rice paddies in thousand hectares.	Boomgaard and Van Zanden (1990).
Wages	Average unskilled daily wages in cents.	Dros (1992).
Instrumental variables components		
Distance	Kilometers between province's centroid and Batavia's centroid.	Hup (2021a).
Ships	Number of Javanese steam ships.	Knaap (1989).
Passengers	Number of passengers on Javanese steam ships in thousands.	Knaap (1989).

C.2 Additional Regression Tables

Table C.2 and Table C.3 report OLS and IV estimates with spatial correction of standard errors. Table C.4 and Table C.5 report OLS and IV estimates with no population weights.

Table C.2: OLS estimates, tax farm revenue and officials, spatial correction of standard errors

	(1)	(2)	(3)	(4)
Panel A: DV: Log(Tax Farm Revenue)				
Log(Officials)	-1.42***	-1.87***		
	(0.41)	(0.38)		
Log(Indigenous Officials)			-1.35***	-1.43***
			(0.37)	(0.31)
Log(European Officials)			-0.13	-0.51***
			(0.23)	(0.19)
Log(Asian Officials)			0.27***	0.15**
			(0.10)	(0.07)
Panel B: DV: Log(Tax Farm Revenue per Capita)				
Log(Officials per Capita)	-1.04***	-1.25***		
	(0.35)	(0.29)		
Log(Indigenous Officials per Capita)			-0.96***	-1.01***
			(0.29)	(0.25)
Log(European Officials per Capita)			-0.14	-0.35**
			(0.19)	(0.17)
Log(Asian Officials per Capita)			0.25***	0.17**
			(0.10)	(0.08)
Controls	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	608	608	608	608

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Estimator allows for arbitrary dependence of the errors across provinces and across years. Implemented using Colella et al.'s (2019) `acreg` Stata package. Standard errors are heteroskedastic- and autocorrelation-consistent (HAC) with a linear decay in distance (Bartlett). Distance matrix is based on the longitude and latitude of each province's centroid. Distance cutoff is 1,000 kilometers. Time lag is ten years. DV means dependent variable. Controls consist of the log of, respectively, number of inhabitants (not used in panel B), steam power, rice paddies, and wages. Observations are weighted by the number of inhabitants.

Table C.3: IV estimates, tax farm revenue and officials, spatial correction of standard errors

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A1 Second Stage: DV: Log(Tax Farm Revenue)						
Log(Officials)	-4.44***	-3.86***	-4.37***	-3.91***	-4.81***	-4.18***
	(1.28)	(0.79)	(1.19)	(0.88)	(1.19)	(0.84)
Panel A2 First Stage: DV: Log(Officials)						
Instrument	-86.86***	-93.07***	-0.03***	-0.03***	-0.08***	-0.09***
	(13.70)	(12.54)	(0.01)	(0.01)	(0.01)	(0.01)
First-stage F	40.19	47.84	34.69	31.82	40.28	49.59
Panel B1 Second Stage: DV: Log(Tax Farm Revenue)						
Log(Indigenous Officials)	-3.38***	-3.02***	-3.36***	-3.07***	-3.80***	-3.43***
	(0.95)	(0.72)	(0.93)	(0.78)	(0.95)	(0.79)
Panel B2 First Stage: DV: Log(Indigenous Officials)						
Instrument	-114.29***	-118.90***	-0.04***	-0.04***	-0.10***	-0.11***
	(15.82)	(16.09)	(0.01)	(0.01)	(0.02)	(0.02)
First-stage F	38.41	38.57	25.10	23.11	28.53	29.39
Controls	No	Yes	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	608	608	608	608	608	608
Instrument	Distance/Year		Distance/Ships		Distance/Passengers	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Estimator allows for arbitrary dependence of the errors across provinces and across years. Implemented using Colella et al.'s (2019) `acreg` Stata package. Standard errors are heteroskedastic- and autocorrelation-consistent (HAC) with a linear decay in distance (Bartlett). Distance matrix is based on the longitude and latitude of each province's centroid. Distance cutoff is 1,000 kilometers. Time lag is ten years. DV means dependent variable. Controls consist of the log of, respectively, number of inhabitants, steam power, rice paddies, and wages. Observations are weighted by the number of inhabitants. First-stage F is the Kleibergen-Paap rk Wald F statistic. Regarding the instruments, distance is in kilometers and passengers is in thousands.

Table C.4: OLS estimates, tax farm revenue and officials, no weights

	(1)	(2)	(3)	(4)
Panel A: DV: Log(Tax Farm Revenue)				
Log(Officials)	-1.17**	-1.55**		
	(0.55)	(0.58)		
Log(Indigenous Officials)			-1.09**	-1.24**
			(0.50)	(0.48)
Log(European Officials)			-0.22	-0.41*
			(0.29)	(0.22)
Log(Asian Officials)			0.19	0.12
			(0.13)	(0.08)
Adjusted R^2	0.89	0.90	0.89	0.90
Panel B: DV: Log(Tax Farm Revenue per Capita)				
Log(Officials per Capita)	-0.74*	-0.94**		
	(0.40)	(0.38)		
Log(Indigenous Officials per Capita)			-0.70*	-0.77**
			(0.38)	(0.34)
Log(European Officials per Capita)			-0.19	-0.32*
			(0.21)	(0.17)
Log(Asian Officials per Capita)			0.17	0.12
			(0.12)	(0.09)
Adjusted R^2	0.88	0.88	0.88	0.88
Controls	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	608	608	608	608

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

DV means dependent variable. Controls consist of the log of, respectively, number of inhabitants (not used in panel B), steam power, rice paddies, and wages. Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (19 province clusters, 32 year clusters). Observations are not weighted. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.

Table C.5: IV estimates, tax farm revenue and officials, no weights

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A1 Second Stage: DV: Log(Tax Farm Revenue)						
Log(Officials)	-4.05**	-4.15***	-3.89**	-4.00***	-4.57***	-4.56***
	(1.85)	(1.34)	(1.56)	(1.22)	(1.53)	(1.30)
Panel A2 First Stage: DV: Log(Officials)						
Instrument	-74.81***	-76.86***	-0.03***	-0.03***	-0.07***	-0.07***
	(21.31)	(16.10)	(0.01)	(0.01)	(0.02)	(0.02)
First-stage F	12.32	22.79	16.47	15.89	18.76	21.80
Panel B1 Second Stage: DV: Log(Tax Farm Revenue)						
Log(Indigenous Officials)	-3.58**	-3.59**	-3.46**	-3.48***	-4.16**	-4.12***
	(1.67)	(1.33)	(1.43)	(1.20)	(1.48)	(1.35)
Panel B2 First Stage: DV: Log(Indigenous Officials)						
Instrument	-84.60***	-88.86***	-0.03***	-0.03***	-0.08***	-0.08***
	(25.49)	(21.31)	(0.01)	(0.01)	(0.02)	(0.02)
First-stage F	11.02	17.38	11.44	11.67	13.87	13.83
Controls	No	Yes	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	608	608	608	608	608	608
Instrument	Distance/Year		Distance/Ships		Distance/Passengers	

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

DV means dependent variable. Controls consist of the log of, respectively, number of inhabitants, steam power, rice paddies, and wages. Standard errors are two-way (province and year) clustered Driscoll-Kraay standard errors with a bandwidth of three. Small sample correction is applied due to small number of clusters (19 province clusters, 32 year clusters). Observations are not weighted. First-stage F is the Kleibergen-Paap rk Wald F statistic. Regarding the instruments, distance is in kilometers and passengers is in thousands. Implemented using Baum et al.'s (2010) `ivreg2` Stata package.