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The Fiscal State in Africa: Evidence from a century of growth*

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Abstract

Why do large differences in fiscal capacity exist between states in the Global South? We construct a comprehensive new dataset of tax and revenue collection for 46 African polities from 1900 to 2015. Descriptive analyses show that many polities in Africa have been characterized by strong growth in real tax collection. As a next step, we employ these data to test theories of fiscal capacity in a long-run panel setting, using fixed-effects and causal estimation techniques. The results show democratic institutions and interstate warfare can increase fiscal capacity, while government turnover reduces it. However, these factors are conditional on the availability of debt financing and external aid, which by themselves reduce incentives to invest in fiscal capacity. Leveraging new data on exogenous movements in commodity prices, we show that resource income does not generally lead to lower capacity. These insights add important nuance to established theories of state-building.

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Abstract

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Keywords fiscal capacity · taxes · Africa · institutions · resources

1 Introduction

The importance of taxes to state development is rather self-evident. As Besley and Persson (2014, p. 100) note: "the power to tax is about much more than raising tax revenues; it is at the core of state development". This has been a central notion in the social sciences since Joseph Schumpeter founded the study of "fiscal states" one century ago. The fiscal analysis of state development has been particularly prevalent in the study of African polities. While some authors have described the state in Africa as "weak", barely able to collect enough revenues to fulfill its basic public functions (Herbst 2000; Bates 2001; Samatar and Samatar 2002), others have pictured the African polity as too "strong", invasive, and extractive (Frimpong-Ansah 1992; Young 1994; Mamdani 1996; Acemoglu et al. 2001). In one way or another, the fiscality of the state is seen as a root cause of the oft-diagnosed "failure of African independence" (Cabral 1973, p. 43).

Our contribution to this debate is twofold. First, we create a new empirical foundation for research into the development of the state in modern Africa.[1] We harness rich archival material to create a database of disaggregated government revenues from the early days of colonial rule until the present. We standardize the data across polities, both by classifying it according to modern standards and by creating an accompanying set of deflators. The latter allows us to express government revenue in a

[1]Despite promising recent work on individual country or period groupings (Prichard and Leonard 2010; Mansour 2014; Frankema and van Waijenburg 2014; Cogneau et al. 2018), we do not systematically know how much revenue African polities have historically collected.

comparable metric across time and space. To our knowledge, this represents the first long-run dataset of fiscal outcomes for a significant group of polities of the Global South. These new data show that, measured in real terms, African polities have been capable of realizing large gains in total revenues since independence, as well as in hard-to-collect taxes, a common measure of fiscal capacity.

Second, we explore the heterogeneity in the data along two dimensions— temporal and cross-sectional. Our historical analysis shows strong growth in fiscal capacity in times where there was insufficient trade to be taxed (such as the World Wars). Conversely, when alternative finance was available through aid and debt in the decade following World War II, investment in fiscal capacity was low. The waves of democratization in the 1960s and 2000s coincide with high rates of growth in fiscal capacity, whereas the reverse is true for the period of instability in the 1980s. The historical analysis also uncovers that a significant divergence across polities in fiscal capacity took place since independence.

To understand which factors determine country-specific paths, we distill theories of the fiscal state into four testable hypotheses drawing on work by Besley and Persson (2009, 2010), Tilly (1992), Moore (2004), and Stasavage (2011). Using a fixed-effects panel specification and causal estimation techniques, we find that democratic institutions increase investments in fiscal capacity only within ethnically homogenous societies. Interstate warfare provided an impetus for tax collection, but only for colonial polities. High government turnover reduces investments in fiscal capacity, especially in polities with less cohesive institutions. Finally, growth in fiscal capacity is conditional on the absence of alternative revenue sources, including debt and aid. This finding, however, does not extend to the availability of resource rents. Leveraging newly collected data on commodity prices and exports for the entire century, we show that exogenous increases in resource incomes do not lead to lower fiscal capacity, except for oil exporters.

In this paper, we define fiscal capacity as the tax revenue a government can collect in the long run. We follow much of the literature in measuring this concept through tax revenue generated by hard-to-collect taxes, net of cyclical effects (Besley and Persson 2014). This builds on the notion that hard-to-collect taxes will, after an initial investment, eventually provide a revenue stream that is both larger and less volatile than that generated by trade taxes or resource taxes.[2]

Fiscal capacity, of course, is still a concept with ambiguous normative implications. Governments can use taxes to increase funding for schools and infrastructure, as Mozambique did under Chissano in the 1990s, or Botswana did in the 1980s (Hillbom 2012). Reversely, funds can be frittered on prestige projects, or disappear in the pockets of corrupt government officials as they did in Mobutu's Zaire (MacGaffey et al. 1991). In many of the colonial polities we study, tax revenues were used to cement white minority rule over African populations. In the case of Rhodesia and South Africa, repressive minority rule continued even after independence, bolstered by an efficient fiscal apparatus

[2]For example, using modern cross-sectional data, Besley and Persson (2014) show that tax revenue expressed as a share of GDP is larger in countries with a higher proportion of income taxes in their revenue mix. Cagé and Gadenne (2018) document that the revenues of developing countries relying on trade taxes are considerably more volatile than those relying on direct taxes.

(Mkandawire 2010). While the theoretical framework we set out does not require the analysis of spending patterns, we do analyze the importance of representative institutions and suffrage. Moreover, despite the frequent misuse of government funds in African fiscal history, sufficient tax collection remains a necessary, though clearly not sufficient, condition for the extension of vital public services across the continent.

The remainder of the paper proceeds as follows. The next section describes the fiscal data we have collected. In section 3, we show through historical analysis how the novel data recast the debates surrounding African statehood. Section 4 sets up a theoretical and empirical framework that allows us to investigate the evolution of fiscal capacity formally. Section 5 presents the empirical results, while the last section concludes.

2 Data: Tax Collection and fiscal capacity

Despite the centrality of taxes to statehood and economic development, large gaps exist in our knowledge about trends in taxation in Africa.[3] These gaps emanate from two sources. First, while records of state revenues exist for almost all polities since 1900, they are scattered across numerous archives and their granularity varies substantially. Second, it is difficult to compare nominal revenues through time and across jurisdictions in the absence of reliable GDP data (Jerven 2013b). We propose a data harmonization strategy that solves both problems.

As Appendix B details, we have extracted heterogenous revenue data for more than 4,700 country-year combinations from a large variety of sources, mainly colonial budgets, revenue statements, and IMF Article IV consultation documents. Our first step is to break down any revenue data we find into its smallest constituent components, called items. These items, more than 135,000 data points, are usually revenue streams for individual taxes, such as income from a colonial hut tax or an export duty on copper. We then reclassify each item—-there more than 25,000 unique ones—according to modern IMF definitions into direct taxes, indirect taxes (subdivided into indirect taxes proper and trade taxes), non-tax ordinary revenue, resource income and extraordinary revenue. We then sum all items within each - now consistently coded - category.

Our concept of fiscal capacity builds on revenue from hard-to-collect taxes. While we exclude trade and resource taxes, we include direct taxes and indirect taxes proper. The latter category is largely comprised of Value Added Taxes (VAT). In developed economies, indirect taxes are often treated as easy taxes (Besley and Persson 2014), but this definition is not immediately transferable to developing countries, where institutions and technology need to be built up before VAT can effectively be collected (Fjeldstad et al. 2020). Indeed, the recent literature on capacity building in developing countries has focused precisely on measures to enhance the institutional capacity to collect VAT (Pomeranz 2015; Keen and Lockwood 2010).

[3] We survey the existing work in Appendix A.

Finally, we deflate nominal revenues. The choice of the deflator is motivated by a simple consideration: nominal incomes should be converted into real incomes using the prices of the relevant consumption basket. During much of our period, the main expenditure item for governments in Africa was their wage bill (Gardner 2012). We therefore take nominal wages as the deflator for the revenue series. For a polity i in year t we then have:

$$\text{real tax revenue per capita}_{i,\,t} = \frac{\text{nominal tax revenue}_{i,\,t}}{\text{nominal day wage}_{i,\,t}} \times \frac{1}{\text{population}_{i,\,t}} \tag{1}$$

Our approach has four further advantages. Firstly, as we use daily wages and normalize by population, our measure is expressed in a meaningful unit: the number of work days the government collects from each worker.[4] Secondly, we do not have to rely on inflation or nominal GDP estimates, which are rarely available before 1960 and often unreliable thereafter (Jerven 2013a). Wages, on the other hand, are relatively well-recorded, which is why they are regularly used in contexts with limited data availability (see e.g. Karaman and Pamuk 2013, in their study of fiscal capacity in early modern Europe). Thirdly, many colonial governments supplemented monetary taxes with forced labor (Fall 1993). Our method allows us to capture this important feature of the colonial state, as we can add estimates of forced labor days to the estimates for monetary taxes, expressed in labor days.[5] Fourthly, for periods where nominal GDP data is available, we can compare our metric to tax revenues as a share of GDP. The correlation between these measures is high and both produce comparable patterns. If anything, our deflator is less volatile, as it is not subject to idiosyncratic swings in GDP.[6]

In summary, our measure of real tax revenue provides us with a metric for resource collection by the state that is comparable between African polities and across the century. We achieve near-comprehensive coverage for a balanced sample of 41 African polities. We also work with a full sample encompassing 5 additional polities (Djibouti, Ethiopia, Liberia, Libya and Somalia), for which we have coverage after World War II only.[7]

3 Revenue patterns in African polities since 1900

Figure 1 summarizes the evolution of the fiscal state in Africa. Panel (a) reports the mean level of total ordinary revenues across polities, as well as the mean and median levels of fiscal capacity. Panel (b) shows the composition of revenues.

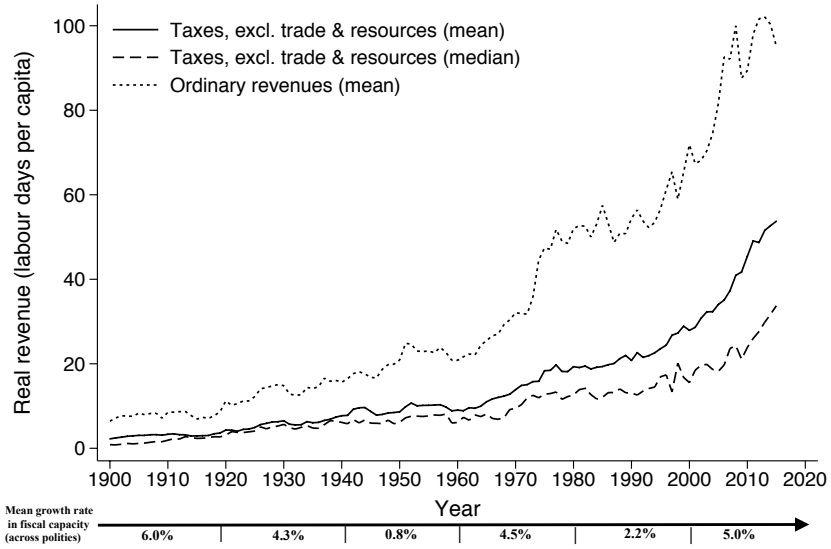
[4] Appendix B.2 provides more information on the wage data. Note that it does not matter if wages are administratively set, as long as labor is actually remunerated at this rate. We are interested in the purchasing power of the government at the going wage rate, not the determination of this rate.

[5] The exact extent of forced labor is not known, as colonial authorities did not keep systematic records of these practices. We estimate lower and upper bounds on the basis of the secondary literature, in particular van Waijenburg (2018).

[6] See Appendix B.2 for a comparison of deflators.

[7] There are only three currently sovereign states on the African continent which we do not include separately in either the balanced or the full sample: South Sudan, which is included with Sudan until its secession in 2011; Eritrea, which is included with Ethiopia from 1952 until independence in 1993; Equatorial Guinea, as access to Spanish archives was deficient.

(a) Revenue levels



(b) Revenue composition

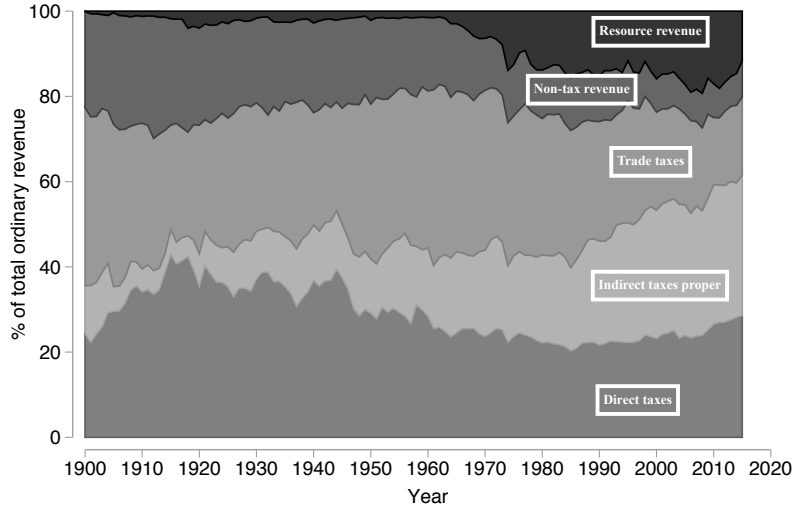


Figure 1: Real taxation, revenues, and their composition in Africa over the last century

Notes: For details on the data construction, see Appendix B. Figures are averaged for all polities of the balanced sample (41 polities). Appendix C.1 displays results for the full sample, which includes Djibouti, Ethiopia, Liberia, Libya and Somalia. Panel (a): real revenue levels. Growth rates below the panel report the mean annual growth of fiscal capacity across all polities. They exclude World War I (mean growth rate: 3.1%) and II (mean growth rate: 2.8%). Panel (b) averages the shares of the respective revenue category across the balanced sample. The resulting shares are-rescaled such that they sum up to 100%.

One previously under-appreciated feature of the fiscal state stands out: The 20th century was a century of growth. Both ordinary revenue and fiscal capacity per capita were more than 11 times larger in 2000 than they were in 1900. This growth has not always been stable, as the growth rates below the graph show. However, the perception of "state weakness" originates mostly from the crisis period between 1980 and 2000, neglecting periods of strong growth before and afterwards. We also note that growth in revenues is not always due to a higher share of resource and trade taxes. Since the

1990s, the share of hard-to-collect taxes has increased whereas the share of trade taxes has dropped significantly. Nonetheless, the average trend depicted in Figure 1 masks substantial heterogeneity across time and polities. This is partly illustrated by the widening difference between the mean and median since the 1960s in Panel (a). Before moving to a formal investigation of the polity-specific trajectories, the following paragraphs detail the heterogeneity across time.

Early colonial period: 1900-1914 By the start of the sample period in 1900, most colonial polities had established some degree of territorial control (Young 1994). However, as it was built on a legacy of European military conquest, colonial control was fragile, and often challenged in conflict with local rulers.[8] Especially in the British colonies, administration could only be executed by incorporating pre-colonial rulers into the structures of the colony through systems of indirect rule (Müller-Crepon 2020). Initial fiscal capacity was correspondingly low, with an average tax burden equivalent to about 2.2 labor days in 1900. Nonetheless, metropolitan treasuries were generally unwilling to subsidize their colonies. Credit markets were only an option for the few polities who had a history of borrowing as sovereign states, such as Egypt, Morocco, and Madagascar.

Fiscal autonomy and low revenues gave rise to the "revenue imperative" as the overriding aim of the colonial polity (Gardner 2012). As evidenced by high growth rates in fiscal capacity, colonial states invested heavily in tax collection, often through the introduction of poll taxes ("hut tax") on the native population. Frequent tax rebellions, such as the Maji-Maji rising against German rule in present-day Tanzania and the Aba Women's War in South-Eastern Nigeria, show how Africans contested this fiscal expansion (Naanen 2006). Two alternatives were available to the early colonial polity: trade taxes and forced labor. Forced labor added on average between 2 and 12 labor days per capita to the state's tax take, and more in the Portuguese colonies (see Appendix Figure C.2). Trade taxes, easily administered at ports, were a way of monetizing the suitability of the colonial territories for growing cash crops, especially vegetable oils, rubber, and cotton, the cultivation of which was expanded by colonial governments (Frankema and van Waijenburg 2014).[9]

War and interwar period: 1915-1945 The interwar period saw tentative metropolitan efforts to "develop" their African holdings economically, leading to an increase in grants (Havinden and Meredith 1996). Capital market access was also eased through systems of imperial trusteeship (Sunderland 2007). However, the deteriorating economic situation in Europe quickly stymied these efforts. Cash crops continued to dominate (Roberts 1996), and total revenues were high during the commodity price boom of the late 1920s, but fell precipitously during the Great Depression. Faced with volatile trade taxes and grants, colonial polities continued measures to increase direct tax collection. Attempts to

[8] For example, the Wadai Empire thwarted French dominance of the Chad region, tenuously established in 1900, for six years starting in 1903. We note that written state records are scarce for the precolonial period, limiting the usefulness of quantitative investigation. In Appendix A, we point to some important case studies of African states for this period.

[9] Initially, most trade taxes were import taxes, but import demand and hence tariff revenue was ultimately determined by the export proceeds that could be realized from selling cash crops on world markets.

introduce income taxes were made, especially in areas such as Kenya where an influx of white settlers provided a taxable base. However, settlers could also use local consultative assemblies to delay such measures (Gardner 2012). Now firmly entrenched, colonial governments were rarely challenged through armed rebellions and hence much of the growth in real tax income we observe came from expanding the taxation of African populations.

The world wars themselves had a profound fiscal effect on African polities. Loans became hard to obtain as the London market was closed for colonial issues, while colonies in the French Maghreb continued to be able to issue bonds (Havinden and Meredith 1996). Conversely, British colonies continued to receive some financial support from the metropolis, while France, facing the threat of military annihilation at home, mandated funds from its colonies (Huillery 2014). Resource prices oscillated dramatically, with non-military commodities experiencing a serious glut. Meanwhile, those African polities on whose territory fighting took place had to shoulder the costs of equipping expeditions, recruiting soldiers, and hiring laborers (Young 1994). In many polities, direct tax collection thus increased strongly as other potential revenue sources ceased to yield enough revenue for financing such expenditures. The humps of the share in direct taxes during the world wars in Figure 1b reflect this compositional effect. In terms of its absolute rather than relative importance, the average annual growth rates of "hard-to-collect taxes" remained high, accruing to 3.1% in WWI and 2.8% in WWII. Clearly, these growth rates mask the substantial heterogeneity across countries and the highly erratic nature of fiscal capacity growth during the world wars.[10]

Late colonial period: 1946-1959 Our results show the lowest growth in fiscal capacity during this period. Real tax and total revenues even declined as decolonization became imminent. Apart from a possible anticipation of political change, several factors may have conditioned this. Resource prices were generally buoyant in the mid-1950s. Colonial states introduced export taxes, often through marketing boards for cash crops, thus prolonging the notorious reliance of the African fiscal state on trade taxes displayed in Figure 1. Credit market access became near universal, bolstered by investment programs from multilateral agencies such as the IBRD. Metropolitan transfers picked up in tandem, as colonizers at last expanded the range and reach of public goods supplied beyond administration and security. In this, they followed ideas of "development planning" that became increasingly popular after the war (Havinden and Meredith 1996).

Early independence period: 1960-1979 At independence, African governments may have taken over the developmental ethos of the state from the former colonizers (Ake 2001), but our data suggests that it was them who significantly increased its reach. As Figure 1 shows, real tax revenues grew rapidly from 1960 until at least the mid-1970s. Over the first two decades of independence, African states doubled their real tax take from the equivalent of 9 days of labor to 18.[11] Total rev-

[10] See Appendix Table E.3 for the corresponding growth rates and standard deviations.

[11] The post-colonial increase is even more pronounced in Appendix Figure C.2a when factoring in the use of forced labor by colonial states.

enues rose even more strongly, largely reflecting the strong growth in resource revenues. Yet resource revenues were quite compatible with increases in fiscal capacity.[12] We also see that the expansion of direct tax revenue was accompanied by a continued reliance on trade taxes, a characteristic feature of developmental states using export taxes to finance investment in industry (Austin et al. 2016).

Clearly then, sovereign African polities were able to grow fiscally when the conditions were right. What were those conditions? Independence was accompanied by a sharp, if short-lived, surge in popular participation, before many polities settled into the relative stasis of one-party-rule (Young 2012). Often, there was significant institutional continuity between metropolitan transfers and the aid provided by former colonizers, but the amounts offered declined strongly (Pacquement 2010). Aid was forthcoming from new players, in particular the US and USSR, but was tied to significant political concessions. Polities did make use of their now de jure complete access to credit markets, but global interest rates generally increased over the period, limiting the attractiveness of this option.

Crisis: 1980-1999 Our results corroborate the common view of the 1980s as a crisis period for the African state. Growth in real tax revenues averaged close to 0% across polities, and collapsed in many. Growth in total revenues was also low, affected by the reduction in trade taxes through trade liberalization programs (Cagé and Gadenne 2018) and the reduction in non-tax revenues in the wake of privatization. Although tax revenues had, on average, recovered slightly in the 1990s, the median tax level in Figure 1 suggests the majority of polities did not exit this crisis until the turn of the century. The causes of this crisis have been analyzed elsewhere (Frimpong-Ansah 1992; Bates 2001). For present purposes we note that this period combined frequent, and often irregular, changes in leadership with generally undemocratic governments lacking legitimacy to tax. Sometimes, changes in government were associated with civil war. These factors likely eroded incentives for investment into fiscal capacity. In terms of external finance, African states made increased use of multilateral loans: The IMF granted access to its financial facilities from the 1970s onwards, in particular to Structural Adjustment loans from 1979 (Van de Walle 2001).

Recovery: 2000-2015 We detect a strong recovery in real tax revenue at the start of the new century. The data in Figure 1b suggests that much of this expansion was driven by an increase in indirect taxes proper (VAT), which casts new light on a literature that has hitherto been skeptical as to the effect of this tax (Ahlerup et al. 2015; Moore 2014). We do also detect an increase in direct tax revenue. The strong increase in tax revenues is concurrent with several waves of democratization, which have transformed the political landscape of the continent (Young 2012). We also find that even during this period of fiscal expansion, resource revenues have remained buoyant. However, not all countries have participated in this pattern of growth. The large gap between median and mean fiscal capacity has remained constant in the past two decades (Figure 1) and no significant convergence in fiscal capacity among African polities has taken place.

[12] Some of the polities with the largest increases in capacity during this period are actually mineral exporters, including Botswana (diamonds), Zambia (copper) and Gabon (oil).

4 Empirical strategy

The heterogeneous trajectories of fiscal states in Africa call for a conceptual explanation. We develop four hypotheses explaining changes in fiscal capacity, drawing on canonical theories of state-building, especially the framework by Besley and Persson (2009, 2010). This framework offers three advantages. Its microfoundations add discipline to our analysis and guard against multiple hypothesis testing. Nonetheless, the theory is broad enough to allow for the inclusion of a range of variables and interactions. Most importantly, its predictions are in line with the factors identified in the historical overview, and with the literature on state-building. After developing this framework, we discuss its empirical implementation.

4.1 Theory

Following Besley and Persson (2009) and Besley et al. (2013), we define a government as being comprised of political decision makers with similar preferences. We start from the premise that increasing fiscal capacity entails short-term expenses, as censuses are conducted, wealth and incomes are assessed, and institutional infrastructure needs to be built up. These investments may yield a positive return in the future, as permanently higher levels of taxation are unlocked. Tax revenues yield utility to the government through direct consumption, or through spending on public goods that are closely aligned to the government's preferences.[13]

However, returns to investments in fiscal capacity are *ex ante* uncertain to the government. Uncertainty arises from government turnover. For example, the current government may not be in power anymore by the time higher tax revenues materialize. This is likely to reduce the expected pay-off from the investments, either because they cannot be consumed directly, or because the spending preferences of the new government are likely to differ. A precarious government that is likely to lose power quickly will therefore not commit to costly uncertain investments, and may prefer to fund itself through easily obtainable means, such as trade taxes. Therefore we have:

H1 (Government turnover): *Investment in fiscal capacity will decrease in the likelihood of the current government losing power.*

If it has lost power, the extent to which the former government enjoys the fruits of its investment will depend on the institutional environment. If the *new* government faces institutional constraints on its spending decisions, such as a powerful legislature or a rule-bound bureaucracy, its capability to use the newly materialized tax revenues exclusively to its own advantage will be limited. If such cohesive institutions exist, the former government may still reap the returns of their prior investment,

[13] We are therefore agnostic about the government's spending patterns. Colonial governments in Africa largely consumed their revenues by paying high wages to European bureaucrats stationed in Africa (Gardner 2012). Today, African governments may raise taxes to fund public goods. While the welfare implications will clearly differ, what matters for our analysis is that in both cases, the government faces an imperative to increase revenues (Levi 1988).

either because it can influence the allocation of revenues through the legislature, or because it can rest assured that the revenues will not be redistributed solely to the benefit of the new government. Both considerations will increase the attractiveness of fiscal investments:

H2 (Cohesive institutions): *Investment in fiscal capacity will increase if purely redistributive spending by the executive is constrained.*

As a related mechanism, democracies could see a greater degree of tax compliance from citizens who have a stake in the future of their polity. This relationship between democratization, broadly conceived, and fiscal capacity is corroborated by empirical evidence for 19th century Europe (Aidt and Jensen 2009) as well as for contemporary developing countries (Profeta and Scabrosetti 2010).

The combination of H1 and H2 allows a variety of useful equilibria to emerge for our study. We would clearly expect the largest increases in fiscal capacity in democracies with low rates of government turnover, such as Botswana after independence. Nonetheless, colonial autocracies could have sustained prolonged periods of state-building, given the potentially long time horizon of colonial rulers. However, in less institutionalized polities with high turnover rates we would predict very low investments in fiscal capacity.

Government and opposition may also be bound together in their spending preferences by common interests, rather than restrictive rules. This is most likely to be the case if exogenous shocks, such as war, threaten the survival of both. In such an emergency, government and opposition may assent to programs increasing tax collection:

H3 (Common interest shocks): *Shocks demanding a unified response from all interest groups within a polity will lead to an increase in tax collection.*

It is here that the framework by Besley and Persson most clearly overlaps with established literature from other fields. In what is now known as the bellicose theory of fiscal capacity, the fiscal history of Europe is often explained by competing states constructing tax systems to fund their military exploits.[14] However, scholars working on the history of African conflict have been sceptical of the bellicose theory, and much of the literature points to the relative dearth of interstate conflict in Africa[15]. However, as we discussed in section 3, African polities were strongly affected by a range of conflicts initiated by their colonial rulers, most notably the two world wars, and our data show

[14]The hypothesis goes back at least to Schumpeter, and is now usually associated with the works of Tilly (1992) and Brewer (1990). For recent empirical work, see Dincecco and Prado (2012), for a historiographical overiew see Yun-Casalilla et al. (2012)

[15]Herbst (2000) argued that the interstate environment was not a threat to the colonial or sovereign African polity, a view recently reinforced by Dincecco et al. (2019). Bates (2001) focuses on the negative effect of civil wars on the African state. Besley and Reynal-Querol (2014) also find historical conflict in Africa is negatively correlated with multiple present-day outcomes.

increases in real tax revenues during these periods. We therefore do not discount the bellicose theory at the outset.

The three variables outlined above affect the pay-off to a current government from investing in hard-to-collect taxes. However, this calculus must be conditional on the opportunity cost of the investment, i.e. the pay-off from tapping into other revenue sources. This has been the focus of a burgeoning literature on state building in developing countries, which has investigated how the availability of windfall revenues might have stymied state-building outside of Europe (Gadenne 2017; Paler 2013; Bates 2001). Natural resource incomes feature prominently in this analysis (Collier and Hoeffler 2005; McGuirk 2013; Besley and Persson 2010). Other analysts have focused on external aid flows, arguing these to be "strategic rents" acquired through political maneuvering rather than fiscal prudence (Moore 2004; Moss et al. 2006; Djankov et al. 2008). It has also been pointed out how public debt helped governments in the Global South to ease their revenue constraints, especially in case of military emergencies (Centeno 1997; Queralt 2019). In line with much of our historical analysis, we expect the availability of alternative revenues to decrease investment in domestic fiscal capacity.

H4 (Alternative revenues): *Investment in fiscal capacity will decrease in:*

1. *The value of resource incomes*
2. *The availability of intergovernmental transfers, such as aid*
3. *Access to external credit markets*

4.2 Covariates

We have combined novel primary sources with a variety of established datasets to test hypotheses H1-H4 empirically. Most of our covariates span the period 1890-2015 such that we can lag explanatory variables without losing observations.

While gathering data on cohesive institutions, government turnover and conflict is relatively straightforward, measuring the importance of alternative revenue sources (H4) is more difficult. Firstly, intergovernmental transfers and debt are not always recorded consistently in our fiscal sources. More importantly, the use of alternative revenue sources is likely to be a response to existing fiscal pressures, and will be endogenous to tax revenues. We therefore model exogenous *access* to aid, credit, and resource prices, rather than actual use of aid, resource revenues and sovereign debt. Our estimates can be interpreted as a reduced form estimation of the causal effect of access to these alternative revenue streams on fiscal capacity—in terms of methodology much in the spirit of Acemoglu et al. (2005). We also employ these access variables as instruments for the endogenous variables aid, resource revenues and debt when data availability permits us to do so. Table 1 summarizes the covariates, whereas Appendix D provides more details.

Table 1: Empirical implementation

Hypothesis	Explanatory variable(s)	Source
Government turnover (-)	Change in party providing chief executive or change in autocratic regime	Varieties of democracy (VDEM) database (Coppedge et al. 2020)
	Number of leader changes	Archigos project (Goemans et al. 2009); Own coding for colonial period by using historical encyclopedias
Cohesive institutions (+)	liberal democracy index (measuring constraints on executive and minority rights)	Varieties of democracy (VDEM) database (Coppedge et al. 2020).
Common interest shocks (+)	Incidence of armed conflicts, civil or international	UCDP/PRIO database (Gleditsch et al. 2002) & own coding for colonial polities based on Brecke (1999)
	Conflict magnitude (1-11 scale), civil or international	MEPV database (Marshall 2019) & own coding for colonial polities based on Brecke (1999)
	Number of battle deaths, civil or international	Correlates of War (Sarkees and Wayman 2010)
Alternative revenues (-)		
- Access to aid	Alliance similarity (Signorino and Ritter 1999) with permanent members of the UN Security Council, interacted with budget balance of UNSC members (for sovereign polities); metropolitan budget balance (for colonies)	Own calculation based on political alliance similarity from Häge and Hug (2016) and budget data for donors from Mitchell (2003).
- Access to credit markets	Ability of polity to issue debt interacted with inverse of global interest rate for colonial polities; inverse of global interest rate for sovereign polities	Own coding based on secondary and primary sources. Global interest rate: Bank of England rate
- Resources	Resource prices weighted by polity's export basket following Bazzi and Blattman (2014)	Own calculation until 1956 based on colonial trade statistics and world market prices; Bazzi and Blattman (2014) from 1957 onwards

Note: More details on the variable construction can be found in appendix D.

4.3 Specification

We have collected our fiscal data and covariates at an annual frequency. For the formal analysis, we average this data over 5-year periods. Although this reduces the number of observations, it has two distinct advantages. Conceptually, our interest is in fiscal capacity as the product of structural forces, rather than cyclical annual variations in tax collection. Empirically, this step reduces idiosyncratic volatility in the data, which could lead to imprecise estimates.

The dependent variable is our measure of fiscal capacity, real tax collection per capita net of resource and trade taxes. As our theoretical framework makes predictions about the incentives to

invest in fiscal capacity, we analyze changes in this variable, rather than levels.[16] Our benchmark specification is:

$$\Delta y_{i,t} = \beta_0 + \beta_1 \text{government turnover}_{i,t-1} + \beta_2 \text{democratic constraints}_{i,t} + \beta_3 \text{conflict}_{i,t} + \beta_4 \text{exposure to aid}_{i,t} + \beta_5 \text{resource prices}_{i,t} + \beta_6 \text{access to credit}_{i,t} + Z_{i,t} + \mu_i + \gamma_t + \epsilon_{i,t} \quad (2)$$

where i is the polity, t is the 5-year period, and μ_i , γ_t are polity and period fixed effects respectively. Taken together with the differencing of the dependent variable, the inclusion of the polity fixed effects ensures that we control both for polity-specific time-invariant factors, as well as for polity-specific time trends in tax collection.[17] In addition, $Z_{i,t}$ is a vector of time-varying controls, comprising dummies for sovereign polities, territorial changes, hyperinflation episodes, socialist economic systems, and continuous variables for drought magnitude, real GDP growth, and years in sovereign default.[18]

We aim to identify causal rather than correlational relationships. As argued above, the variables measuring access to alternative revenues are plausibly exogenous. We also treat international conflicts as exogenous if imposed by the metropolis under colonial rule. For cohesive institutions, we employ an instrumental variable approach, while we enter government turnover with a one period lag to exclude simultaneity bias.

5 Results

Table 2 presents our benchmark results for the correlates of growth in fiscal capacity, using our full panel of 46 African polities from 1900-2015. In column (1) we show the results for the variables determining investments in fiscal capacity (H1-H3 from the theoretical framework in section 4.1). We gradually add controls and the variables modeling access to alternative sources of finance (H4). The results generally point in the direction predicted by theory and our historical analysis. Polities with higher democracy scores experience a more rapid rise in real tax revenues, whereas frequent changes in government in the past 5-year period are associated with a reduction in fiscal capacity. Similarly, the results suggest that governments of polities with greater access to external credit or aid face weaker incentives to invest in raising tax revenue. The standardized beta-coefficients suggest that

[16] After averaging and differencing the dependent variable, we are left with 22 five-year periods and 46 polities in the full sample, yielding 1,012 observations, 873 (86%) of which we have real tax data for. We use this as our benchmark sample, but also report results for the balanced sample.

[17] For example, this procedure controls for different degrees of fiscal centralization, as well as for countries experiencing a trend towards fiscal decentralization.

[18] The inclusion of GDP growth could be problematic if the effect of our variables on fiscal capacity works principally through increased incomes. While the existence of such an effect is possible, it is likely to be a long-term one (Besley and Persson 2010). We thus prefer to net-out income effects, analyzing the impact of our variables on fiscal capacity through channels other than income.

the magnitude of these effects is substantial. For example, a one standard deviation increase in access to credit decreases tax revenues by 0.22 standard deviations.

Table 2: Determinants of African fiscal capacity, 1900-2015: **Benchmark**

	Dependent Variable: Change in real tax collection per capita, excluding trade & resource taxes						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Domestic variables only	Including controls	Including resource prices	Including Aid Exposure	Including capital market access	Full specification	Full specification (standardized β)
Liberal democracy score (VDEM)	12.700*	9.393	12.625*	12.169*	9.886	9.206	0.141
	(6.689)	(6.703)	(6.845)	(6.418)	(6.625)	(6.475)	
Conflict incidence (PRIO)	-0.129	0.295	-0.122	-0.174	0.334	0.294	0.015
	(0.840)	(0.864)	(0.843)	(0.854)	(0.868)	(0.882)	
Change in government (lagged)	-0.683*	-0.681*	-0.670*	-0.638*	-0.661*	-0.634*	-0.052
	(0.364)	(0.351)	(0.370)	(0.354)	(0.348)	(0.356)	
Real resource prices			-0.010			0.003	0.006
			(0.034)			(0.028)	
Exposure to foreign aid				-4.698**		-4.598**	-0.174
				(2.145)		(2.058)	
Credit market access					-9.853**	-8.091*	-0.217
					(4.607)	(4.659)	
Polity fixed effects	✓	✓	✓	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.186	0.202	0.185	0.188	0.204	0.205	0.205
Observations	873	873	873	873	873	873	873

Note: Sample: African polities, 1900-2015 (5 year averages). See Table E.1 and Table E.2 in the online appendix for summary statistics, and section 4.2 for variable definitions. Controls for droughts, independent statehood, socialist economic systems, territorial changes, hyperinflation episodes, real GDP growth, and sovereign debt default included but not shown (see Appendix Table F.2 for full results). All regressions are OLS. Standard errors are clustered at polity level and shown in parentheses; * p<0.1, ** p<0.05, *** p<0.01.

However, we note that some coefficients, especially those on democratic institutions, are only marginally significant. Conflict incidence (summarizing both domestic and interstate conflicts) is not statistically significant at all. Neither do resources seem to affect incentives to invest in fiscal capacity. Clearly, average coefficients may not present a meaningful picture given the vast heterogeneity in local experiences discussed in our historical analysis. This interpretation is supported when we vary the sample composition: while some variables, such as aid exposure, exert a relatively constant effect across sub-samples, this is not true for others, such as government turnover or conflict.[19] We analyze this heterogeneity in the following sections, where we also explore the causal interpretation of these estimates in more detail.

The above conclusions are robust against (i) the omission of potentially "bad" controls such as GDP growth or sovereign default; (ii) employing annual data rather than 5-year-averages (even though the

[19]See Appendix Table F.1. We find, however, that the results do not differ greatly when using the balanced sample instead of the full sample.

precision of the estimates decreases); and (iii) incorporating forced labor into our dependent variable.[20] Finally, we construct a quasi-placebo test. A potential concern is that our covariates might generally correlate with factors affecting government revenues rather than fiscal capacity per se. For example, frequent changes in government could undermine a government's ability to raise any type of revenue effectively. If this was true, we would expect total government revenue and fiscal capacity to react in the same way. This, however, is not the case. When using total ordinary revenues rather than fiscal capacity, the magnitudes of our coefficients of interest shrink substantially and all of them turn insignificant. Instead, total revenues are driven primarily by GDP. This suggests that our benchmark measure captures fiscal capacity as intended.

5.1 Cohesive institutions

The benchmark results in Table 2 suggest a positive, albeit weak, association between democratic institutions and tax collection. Appreciating the heterogeneity of historical experiences is key to understanding this result. In the context of the democracy-fiscal capacity nexus, four historical legacies appear potentially relevant: precolonial political centralization (Michalopoulos and Papaioannou 2013), population density at the start of colonization (Herbst 2000), colonizer identity (Ali et al. 2019), and ethnic fractionalization—itself a consequence of European imperialism (Ndegwa 1997).

Exploratory analysis suggests little role for pre-colonial centralisation in determining the importance of democracy for fiscal capacity growth, but a positive and significant role for low initial population density.[21] Furthermore, Column (1) of Table 3 suggests that former British colonies drive a positive democracy effect. These two characteristics, low population density and British occupation, were emblematic of the key destinations for European settlers in colonial times: South Africa, Zimbabwe, and to a lesser extent Kenya and Botswana. Indeed, an influential literature argues that European settlers determined the degree of democratization in the colonial period, which then persisted into the post-colonial era (Acemoglu et al. 2001; Hariri 2012). In model (2), we apply this insight by weighting the VDEM democracy score with the share of European settlers in 1900. The variable becomes strongly significant, lending support to the hypothesis that democratic (former) settler colonies taxed more.

This positive relationship, however, is driven by the peculiar nature of democratization and the fiscal apparatus in settler colonies. The law generally discriminated natives and favoured non-natives (Mamdani 2001). Voting rights were no exception, being granted to the settler minority only. The fiscal systems mirrored this segregationist nature. Colonial governments introduced separate taxes for the settler minority and the native majority. Column (3) illustrates this insight empirically by

[20] See Appendix Table F.2 for the corresponding regressions. When including estimates for forced labor in the dependent variable, we find that the coefficient on exposure to aid is reduced, which suggests colonial states did not decrease the amount of forced labor recruited as a result of receiving more (metropolitan) transfers. However, the opposite holds for credit market access. This is in line with the historical literature, as loans and forced labor were both used in infrastructure projects, thus rendering them substitutes from the point of view of the colonial state (van Waijenburg 2018).

[21] See Appendix Table F.3 for the corresponding sample splits.

Table 3: Determinants of African fiscal capacity: **Cohesive institutions**

	Dependent Variable: Change in real tax collection per capita, excluding trade & resource taxes							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Interaction with colonial status	European settlers	Controlling for suffrage	Interaction-ethnic fractionalization		Instrument institutions with		Executive constraints
				(Fearon & Laitin)	(Montalvo & Querol)	Lag	Settlers & Neighbours	
Lib. democracy	-2.01			46.85**	43.39**	65.61***	75.34***	
	(5.44)			(19.66)	(17.73)	(24.98)	(27.86)	
Lib. democracy × (former) British colony	17.41*							
	(9.96)							
Lib. democracy (settler share weighted)		87.95***	85.67***					
		(28.27)	(27.59)					
Population granted suffrage			0.63					
			(1.30)					
Lib. democracy × Ethnic frac.				-55.87**		-79.83**		
				(26.70)		(33.71)		
Lib. democracy × Ethnic frac.					-51.01**		-124.11**	
					(22.59)		(63.09)	
Executive constraints								3.31**
								(1.56)
Executive constraints × Ethnic frac.								-4.66**
								(2.15)
Polity fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.217	0.203	0.203	0.230	0.227	0.225	0.118	0.246
N	873	873	873	873	873	873	696	490

Note: Sample: African polities, 1900-2015. See Table E.1 and Table E.2 in the online appendix for summary statistics, and section 4.2 for variable definitions. The same controls and main covariates as in column (6) in Table 2 are included, but not shown. Standard errors are clustered at polity level and shown in parentheses; * p<0.1, ** p<0.05, *** p<0.01.

controlling for the share of the population enjoying suffrage. The coefficient on the settler-weighted democracy variable is not affected at all, suggesting these "democratic" polities were able to tax more without extending the franchise. As Mkandawire (2010, p. 1654) stresses, it was precisely the fact that democratic voice and public goods were not extended to the African majority that lay at the root of the ability of states such as segregationist South Africa and Rhodesia to tax their white minority populations heavily: "Whites paid income taxes while natives were confined to 'poll taxes' or to service user charges. In such an order it was important to ensure that none of the tax collected from whites 'leaked' to other sections of the population. The segregation of local authorities in these economies ensured that there was no transfer of revenue among the various racial groups." More democratic former settler colonies left a legacy of relatively strong fiscal states, but they also left a fiscal system built on discrimination.

The notion of discriminatory fiscal systems extents into the present. Experiments and case studies suggest that ethnic fractionalization can restrict tax contributions if the propensity to share revenue with other ethnic groups is limited (Miguel 2003; Habyarimana et al. 2007). A natural conjecture from

this literature is that the success of democracy in increasing fiscal capacity is conditional on moderate levels of ethnic fractionalization. Columns (4) and (5) test this conditionality by interacting the VDEM democracy score with indices of ethnic fractionalization. The main effect of democratic institutions now becomes very large and statistically significant, while the interaction itself is strongly negative. This provides evidence that in ethnically divided societies, democratization may not generate fiscal capacity, as tax payers may worry about revenues being redistributed to other ethnic groups. However, it may still be that democracy is endogenous to changes in fiscal capacity, or correlated with omitted variables determining both.[22] Columns (6) and (7) provide causal evidence. In model (6), we exploit the temporal structure of our data and instrument democracy with its one period lagged value. In model (7), we instrument democracy with the interaction of settler mortality (Acemoglu et al. 2001) and lagged democracy scores of neighboring polities (Persson and Tabellini 2009). In both cases, we find that our results hold, and that democratic, but ethnically divided polities, see lower growth in fiscal capacity. A final important question pertains to the channel through which democracy matters. The results in column (2) rule out that suffrage plays a major role. In line with our theoretical expectations, column (8) suggests that executive constraints are decisive.[23]

To summarize, democratization exerted positive effects on fiscal capacity growth in the past, whenever the voting population could ensure the excludability of unwanted beneficiaries. In the colonial period, discriminatory political and fiscal system fulfilled this function. Since independence, the positive effect of democracy, and constrained executives in particular, prevails mostly in polities with limited ethnic fractionalization.

5.2 Conflict

The theoretical framework postulates that common interest shocks, wars in particular, could increase fiscal capacity. Yet, the historical analysis cautions to treat all wars the same way. It suggests that interstate wars coincided with periods of fiscal capacity growth during the colonial period, but that the continuance of this relationship is uncertain for sovereign polities. Moreover, the experiences of the 1980s lead to the impression that civil conflicts differ in their consequences for fiscal capacity from interstate wars. Table 4 translates these historic qualifications into our empirical framework.

Testing the bellicose theory of state capacity building, columns (1)-(6) show a marked difference in the effect of interstate wars in the colonial and independent periods. While the coefficients for colonial interstate wars are positive, this does not hold true for sovereign African polities. Two factors may explain this difference in the explanatory power of the bellicose theory. Firstly, the extent and externalities of interstate conflicts differed. Even though the COW and MEPV data aim to capture the size of conflict, not all effects of war can be measured directly through the extent of fighting. As discussed in the historical section, the global nature of the world wars severely compromised the ability of the colonial state to finance itself through debt, trade, and metropolitan transfers. Due to this lack

[22] In fact, a large literature is concerned precisely with investigating the effect of taxes on pressures towards popular representation (see Prichard 2015, for an application to four African polities).

[23] We use the Polity V index of executive constraints, which is not available for the colonial period.

Table 4: Determinants of African fiscal capacity: **Conflict**

	Dependent Variable: Change in real tax collection per capita, excluding trade & resource taxes					
	(1)	(2)	(3)	(4)	(5)	(6)
	Interstate wars (PRIO)		Interstate wars (COW)		Interstate wars (MEPV)	
	colonial polities	sovereign polities	colonial polities	sovereign polities	colonial polities	sovereign polities
Interstate war incidence	1.012**	-0.937				
	(0.421)	(1.374)				
Interstate war battle deaths			0.118	-0.239**		
			(0.362)	(0.103)		
Interstate war magnitude (lagged)					0.131**	0.162
					(0.059)	(0.349)
Polity fixed effects	✓	✓	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.171	0.234	0.163	0.235	0.170	0.234
N	384	489	384	489	384	489
	(7)	(8)	(9)	(10)	(11)	(12)
	Civil wars (PRIO)		Civil wars (COW)		Civil wars (MEPV)	
	sovereign polities	resource prices	sovereign polities	resource prices	sovereign polities	resource prices
Civil war incidence	-0.124	2.200				
	(1.343)	(1.601)				
Incidence × real resource prices		-0.149*				
		(0.079)				
Civil war battle deaths			-0.012	0.085		
			(0.027)	(0.056)		
Battle deaths × real resource prices				-0.014**		
				(0.006)		
Civil war magnitude					-0.088	0.101
					(0.057)	(0.101)
Magnitude× real resource prices						-0.014**
						(0.006)
Polity fixed effects	✓	✓	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.233	0.249	0.233	0.249	0.236	0.250
N	489	489	489	489	489	489

Note: Sample: African polities, 1900-2015 for models (1)-(6), year of independence-2015 for models (7)-(12). See Table E.1 and Table E.2 in the online appendix for summary statistics, and section 4.2 for variable definitions. The same controls and main covariates as in column (6) in Table 2 are included, but not shown. All regressions are OLS. Standard errors are clustered at polity level and shown in parentheses; * p<0.1, ** p<0.05, *** p<0.01.

of alternative finance, investment in tax collection became widespread. By contrast, African interstate wars since independence were relatively rare and did not exhibit such global externalities. Secondly, metropolitan taxpayers were notoriously unwilling to fund expensive military ventures. Postcolonial

governments, on the other hand, have been able to turn to other revenue sources, which we can only imperfectly capture.[24] The magnitude of the wars and the availability of alternative finance are likely candidates for explaining why the bellicose theory holds in the period of colonial occupation, but not since independence.

The lower panel of Table 4 shifts the focus towards civil wars and thus limits the sample to the post-independence period. The correlation between civil conflicts and investments in fiscal capacity in columns (7), (9), and (11) is negative, but far from conventional levels of statistical significance. Given the conventional narrative maintaining that civil wars erode the capacity to tax, this result is surprising. However, it may be that higher tax revenues encourage civil uprisings (in a variant of the greed hypothesis), or it may be that the negative effect of destruction is counteracted by the need to raise revenues to fund the fighting. We offer support for this hypothesis in columns (8), (10), and (12) where we interact civil wars with our measure of real resource prices. Although the effect is not necessarily causal, we find that civil wars are associated with lower growth in tax revenues only when a country's natural resources commanded a high price on world markets.[25] In times of crisis, resource incomes may provide an alternative revenue stream that can be controlled and exploited at relatively low costs. Indeed, resources played a major role in many conflicts in Sub-Saharan Africa in the 1980s and 1990s—the diamonds in Sierra Leone's civil war being the case in point (Silberfein 2004).

In the African context, the effects of war on fiscal capacity have been heterogenous throughout the last century. Interstate wars increased fiscal capacity only during the colonial period—in some sense, war made the colonial state. Yet, it is hard to think of wars initiated by the colonial occupier as common interest shocks due to the prevailing power relations. In the post independence period, the bellicose theory fails us entirely: civil wars did not make state. They likely even had detrimental effects on fiscal capacity if resources were available as an alternative revenue stream.

5.3 Executive turnover

Beyond war and cohesive institutions, theory predicts a negative association between past government turnover and current growth in tax revenues. The benchmark regressions (Table 2) confirmed this. The century of African fiscal data and the continent's critical historical junctures allow us to elucidate the proposed mechanism, namely that anticipated political change discourages rulers to invest in fiscal capacity.

The process of decolonization is well-suited for exploring this mechanism. For many observers, Ghana's landmark declaration of independence in 1957 ushered in the age of independence (Young

[24] For example, Prichard (2015, 172-174) recounts how the Ethiopian government faced little incentives to increase tax rates during the Ethiopian-Eritrean war (1998-2000), one of the few large post-colonial international confrontations in Africa. Apart from aid flows and credit market access resuming quickly, the government was able to utilize one-off privatization receipts to fund the war.

[25] Appendix Table F.4 shows that the negative effect of resource prices in times of civil conflict persists if we use alternative measures of fiscal capacity, such as tax share in GDP. The same table also shows that we do not find participation in military interventions abroad to be associated with any detectable tax pressure.

Table 5: Determinants of African fiscal capacity: **Government and leader turnover**

	Dependent Variable: Change in real tax collection per capita, excluding trade & resource taxes				
	(1)	(2)	(3)	(4)	(5)
	Controlling for decolonization	Low liberal democracy score	High liberal democracy score	Interaction with resource prices	Leader turnover
Change in government (lagged)	-0.713*	-0.915**	-0.524	-1.097**	
	(0.374)	(0.400)	(0.923)	(0.463)	
Decolonisation	-2.473*	-2.200	-11.811*	-2.508*	-2.019*
	(1.311)	(1.463)	(6.668)	(1.292)	(1.180)
Change in government (lagged) × Real resource prices				0.025	
				(0.023)	
Change in chief executive (lagged)					0.047
					(0.189)
Polity fixed effects	✓	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓	✓
Adjusted R^2	0.206	0.163	0.187	0.206	0.203
N	873	601	272	873	873

Note: Sample: African polities, 1900-2015. See Table E.1 and Table E.2 in the online appendix for summary statistics, and section 4.2 for variable definitions. The same controls and main covariates as in column (6) in Table 2 are included, but not shown. Change in government refers to a change in the party or regime holding power. Change in chief executive refers to a change in the individual holding power. Models (1)-(5) lag these variables by one period (5 years). Decolonization is defined as a dummy variable taking the value of 1 for any colonial polity after Ghana's declaration of independence in 1957; model (2) and (3) split the sample at the median level of the VDEM liberal democracy score of all independent polities; Resource prices are measured using real world market prices evaluated at fixed export shares and weighted by a polity's trade share. All regressions are OLS. Standard errors are clustered at polity level and shown in parentheses; * p<0.1, ** p<0.05, *** p<0.01.

1994). From this declaration onwards, it became increasingly unlikely that colonial governments could sustain their power for long. Correspondingly, they were unlikely to invest in fiscal capacity. We thus introduce a "decolonization" dummy taking the value of 1 for any polity that is still a colony after Ghana's declaration of independence. Column (1) of Table 5 indeed suggests that investments in fiscal capacity were decreased substantially. This result mirrors the slow growth in tax revenues in the late 1950s (Figure 1). In accordance with theory, colonial governments invested in tax collection only as long as their rule was expected to last.

Beyond this mechanism, we expect the effect of government turnover to be conditional on the institutional setup. Leaders in democratic polities are more likely to reap the fruits of investments in fiscal capacity even after their rule has ended. Therefore, the effect of government turnover in democracies should be smaller relative to that in autocratic polities. Hence, columns (2) and (3) split the sample according to the polity's VDEM liberal democracy score. As expected, we find that the negative effect of changes in government on fiscal capacity only exists in polities with a low democracy score, suggesting that rulers in polities with weak institutions are unlikely to invest in tax collection if their rule is not expected to last. Contrary to the theoretical model by Oechslin (2010), column (4) indicates that this effect is not stronger if those leaders can avail themselves of resource rents.

Our theoretical argument pertains to government turnover as a change in the group in power, rather than the individual in power. In columns (1)-(4) we counted as changes in government only

those instances where the ruling party or regime changed. In column (5), we instead measure changes in the individual holding executive power. This includes changes in colonial governors and changes in the president for sovereign polities, even if both presidents are from the same party. The results are markedly different: the latter coefficient is positive (albeit not statistically significant).[26] This confirms our theoretical expectation: it is the possibility of revenues being redistributed to members of another group, rather than leadership change per se, that deteriorates the incentive to invest in fiscal capacity.

On balance, the empirical assessment supports the proposed mechanism. Groups, not individuals, matter. They are less likely to invest in fiscal capacity if they expect their rule to be short and cannot expect to reap the benefits of their investments. This disincentive is most prevalent in autocracies, as the return on investment is most insecure in these polities in the face of political change.

5.4 Alternative revenue sources

The final hypothesis of our conceptual framework highlights the importance of alternative revenue sources. As tapping into aid, debt, and resources generates revenues at lower cost than hard-to-collect taxes, their availability may disincentivize investment in fiscal capacity. Our benchmark specification (Table 2) suggests that access to external finance (aid and debt) indeed acts as a deterrent to fiscal capacity growth, but that the same does not hold for resource income. This section confirms the validity of our measures of access to alternative finance by relating them to direct measures of actual aid flows, debt uptake, and resource rents. Moreover, it explores the heterogeneity of the effects of resource availability.

5.4.1 Aid and debt as sources of external finance

Our measure of exposure to aid, based on political alliances and donor budgets, consistently implies that better political access to aid decreases incentives to invest in fiscal capacity.[27] Models (1) and (2) in Table 6 demonstrate that this effect is particularly pronounced in polities with high political volatility. The models interact the exposure to foreign aid with changes in government over the past period. The main effects on aid and change in government remain statistically significant and large, but so does the negative coefficient for the interaction term. This implies that governments of unstable polities are particularly prone to using aid as an alternative to taxation, presumably because their shortened time horizon compounds the attractiveness of relatively easily accessible revenue sources. This effect is not driven by the occurrence of natural disasters and droughts as we show in auxiliary regressions: an interaction of these shocks with the access to aid yields insignificant coefficients.[28]

[26] We experiment with different lag structures as well as controlling for civil wars and attempted coups in Appendix Table F.5, but the results remain unchanged.

[27] This result is not dependent on a particular definition of this measure. In Appendix Table F.6, we show that results are robust to using voting patterns in the UN General Assembly rather than alliance structures.

[28] See column (1) and (2) of Appendix Table F.7. Similarly, the table shows that there is no evidence that natural disasters can act as a catalyst to improve the likelihood of common-interest taxation. These shocks seem to undermine tax revenue collection, although this may be due to the direct effect of destruction rather than a strategic substitution away from taxes.

Nor does the inclusion of further controls affect the coefficient on the exposure to aid in a substantial manner (column 3). Exposure to foreign aid appears to be an important deterrent for investment in fiscal capacity.

Table 6: Determinants of African fiscal capacity: **Access to aid and credit**

	Dep. Var.: Change in real tax collection				Dep. Var.: New debt issues	
	(1)	(2)	(3)	(4)	(5)	(6)
	Aid w/o controlling for debt	Aid interacted with government turnover		Debt w/o controlling for aid	Explaining debt issuance	
			Additional controls		Dummy	Loan Size
Exposure to foreign aid	-5.4537***	-4.3753**	-4.4174**		-0.0877	0.0232
	(1.9166)	(2.0811)	(2.0003)		(0.2855)	(0.1006)
Change in government (lagged) × exposure to aid		-1.8572**	-1.8801**			
		(0.9165)	(0.9219)			
Change in government (lagged)	-0.6374*	-0.8131**	-0.8178**	-0.6661*	-0.0251	-0.0157**
	(0.3544)	(0.3802)	(0.3851)	(0.3586)	(0.0234)	(0.0073)
Civil war incidence (PRIO)			0.1129			
			(1.1601)			
Coups d'etat, 5yr period (lagged)			0.0397			
			(0.3621)			
Credit market access		-7.8236*	-7.7859*	-9.9615**	0.6418***	
		(4.5341)	(4.4137)	(4.6224)	(0.1861)	
Credit market access (lagged)						0.1810**
						(0.0875)
Polity fixed effects	✓	✓	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.204	0.206	0.205	0.204	0.539	0.194
N	873	873	873	873	585	645

Note: Sample: African polities, 1900-2015. Debt issuance for colonial polities is defined for French colonies only. See Table E.1 and Table E.2 in the online appendix for summary statistics, and section 4.2 for variable definitions. The same controls and main covariates as in column (6) in Table 2 are included, but not shown. Capital market access refers to a dummy taking the value of 1 for polities with institutional access to interstate credit markets (either through imperial guarantee or attained sovereignty) interacted with inverted global interest rates. Debt issuance in model (5) is a dummy for a polity issuing debt in a given period, in model (6) debt issuance is expressed as a share of GDP. All regressions are OLS. Standard errors are clustered at polity level and shown in parentheses; * p<0.1, ** p<0.05, *** p<0.01.

We measure access to aid through political alliances, rather than actual aid flows. Even if it were possible to gather comprehensive data on transfers, measuring exogenous access is still preferable given endogeneity concerns: polities with low tax revenues are more likely to receive aid. Nonetheless, this raises the question whether our measure corresponds to actual aid flows. Auxiliary regressions in the appendix show that this is indeed the case. We approximate aid flows from the (former) metropolis with aid data published by the OECD for the post-colonial period and extraordinary revenues for the colonial period. Once we substitute our measure of access with these actual flows, we also obtain significant coefficients.[29] Yet, they cannot be interpreted as causal. We thus prefer to base

[29]See Appendix Table F.7. We do this for the French colonies only, as we are able to ascertain from the literature that extraordinary budgetary revenues broadly correspond to French colonial transfers (Huillery 2014). Naturally, this truncates the sample considerably. Results also hold if we exclude the colonial period itself such that any effects stem from aid extended by the former colonizer in the spirit of De Mesquita and Smith (2009).

our conclusion that the availability of aid hampers the build-up of fiscal capacity on the coefficients for plausibly exogenous aid access in Table 6.

Debt uptake constitutes a second potential source of external finance. The benchmark estimate suggests that access to credit markets, measured by institutional access rules interacted with global interest rates, attenuates the incentive to invest in domestic taxation. Unsurprisingly, this effect is slightly stronger if we exclude our measure for access to aid, as the ability to access aid and debt may to some degree overlap (column 4 in Table 6). Nonetheless, both are strong individual predictors of fiscal capacity. An important question is whether our measure of access to credit is correlated with actual debt uptake. As with aid flows, data on debt issues by African polities is difficult to collect systematically, because revenues raised by debt were often assigned to special auxiliary budgets whose relationship with the main budgeting process is not always clear (Huillery 2014; Sunderland 2007). We have, however, compiled indicative data on debt issues from French colonial archives (pre-1960), IBRD documentation (1960s), and the Global Debt Database (from 1970 onward). Given the measurement error involved in converting debt stocks into debt issues, our preferred measure is a simple dummy taking the value of 1 if we detect an increase in debt. Credit market access turns out to be very strong predictor of both, debt issuance and loan size (columns 5 and 6). In times of low interest rates, and when their institutional structure allowed them to do so, polities supplemented their revenues by issuing debt. When they did so, a reduction in domestic tax collection followed.[30]

5.4.2 Resource income

The benchmark results suggest that the availability of natural resources, such as valuable agricultural commodities and minerals, does not decrease investments in fiscal capacity (Table 2). The variable capturing the importance of natural resources employs real world market prices for commodities, interacted with fixed commodity export shares. We thus rely on variation that is largely exogenous to the individual polity. Our null-result is not tied to a specific definition of the price index and is robust to alternative formulations.[31]

Yet, following this strategy may raise the concern that resource prices do not appropriately capture resource revenues. Many resource-producing countries, such as oil-producing Nigeria, sign long-term agreements with multinational resource companies. Hence, the annual payments received by the government may not depend fully on current prices. Our data collection method allows for a consistent coding of budgetary resource revenues such that we are in a position to test the fit of our resource price measure. In columns (1) and (2) of Table 7 we show that resource prices strongly determine budgetary

[30] As our measure of debt issue is not available for the whole sample, we confine our main analysis to the reduced form. A 2SLS approach, however, can be insightful with respect to magnitudes. In Appendix Table F.8, we instrument endogenous debt issue with credit market access and find the same substitution effect as in the reduced form. The magnitude of the effect is substantial: issuing any debt causes a reduction of tax revenues of 19 labor days, more than one third of the 2010 mean. As these insights are based on French colonial data only, we investigate the stability of the result by excluding polities with characteristics that could be correlated with credit market access (sovereign defaults, capital controls, Central Bank lending restrictions) from the sample.

[31] See Appendix Table F.9.

resource income: governments of resource-exporting polities can expect considerably higher resource incomes during price booms.[32] Clearly, resource prices matter in determining resource revenues.

Table 7: Determinants of African fiscal capacity: **Resource income**

	Dep. Var.: Resource income		Dep. Var.: Change in real tax collection per capita, excl. trade & resource					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Effect of prices on resource income		Effect of resource income on fiscal capacity					
	LCU	US dollars	LCU	US dollars	LCU (2 SLS)	US dollars (2 SLS)	Mineral exports only	Oil exporters only
Real resource prices	6.233***	0.898**						
	(1.504)	(0.343)						
L.Real resource prices								-0.120**
								(0.048)
Real resource revenue (LCU)			0.010*		0.0005			
			(0.006)		(0.004)			
Real resource revenues (USD)				0.0005		0.003		
				(0.013)		(0.029)		
Real mineral prices							-0.00035	
							(0.00059)	
Polity fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.578	0.446	0.240	0.205	0.208	0.205	0.200	0.155
N	873	873	873	873	873	873	770	142

Sample: African polities, 1900-2015. See Table E.1 and Table E.2 in the online appendix for summary statistics, and section 4.2 for variable definitions. The same controls and main covariates as in column (6) in Table 2 are included, but not shown. Resource prices are measured using real world market prices interacted with polity-specific fixed export shares and weighted by a polity's trade share. Mineral prices are measured using real world market prices for mining and mineral commodities only, interacted with polity-specific variable export shares and weighted by a polity's trade share. Resource revenues in local currency units (LCU) are those collected from polity-level budget data, deflated by local wages and normalized by population. Resource revenues in USD are converted to US$ using the nominal exchange rate and deflated by the US consumer price index. Columns (7) uses a price index for minerals only (and hence drops observations without any mineral exports in the base period). Column (8) restricts to countries, which have a share of oil above 10% in the base period. Models (5) and (6) are 2SLS, all others OLS regressions. Standard errors are clustered at polity level and shown in parentheses; * p<0.1, ** p<0.05, *** p<0.01.

However, these resource revenues are not associated with lower growth in fiscal capacity, as shown in columns (3) and (4). The coefficients on resource revenues in these specifications is likely to be endogenous to tax revenues, but if substitution between resource and non-resource revenues were pervasive, we would expect the coefficient to be biased *away* from 0, that is strongly negative. Instead, it is even marginally positive. Columns (5) and (6) confirm that no negative effect of the availability of resource rents on fiscal capacity exists by employing resource prices as an instrument for resource income. In both cases, the identified causal effect is a precisely estimated 0.[33] The last two columns focus on the nature of commodities. When only mineral prices are employed (and hence countries without any minerals in their export basket drop out), the results remain unchanged. Only among

[32] When using standardized β-coefficients, we find a increase in real commodity prices of one standard deviation leads to an increase in resource revenues of 0.55 standard deviation.

[33] We obtain similar results when using alternative measures of resource prices as instruments and when splitting the sample into pre- and post-independence eras (Appendix Table F.10). All measures of resource prices strongly predict resource revenues in the first stage, but instrumented resource revenues do not determine fiscal capacity.

oil-producing countries (column 8), there is evidence of a substitution between resource and fiscal capacity building. While this shows that accounts of a "resource curse" may be appropriate for subsets of countries and commodities (Jensen 2011), our results caution against taking the experience of oil-producing countries as representative for a broader class of commodity producers.

How surprising is this result? To some extent, the perceived abundance of natural resource rents in Africa and the generalized perception of weakness of African polities, especially of post-colonial states, may have erroneously been conflated. As we stressed in the historical overview, our data show that commodity exporters did often experience high growth in fiscal capacity.[34] This suggests that, during boom periods, resource revenues can be used for investments in fiscal capacity (Deaton 1999). Moreover, it is sometimes neglected that effective taxation of the resource sector often requires an elaborate institutional setup (Botlhole et al. 2012; Moore et al. 2018). The factors determining this institutional setup may well be similar to those determining the capacity to tax other sectors. For example, studies of gold mining in Africa elicit how taxation of this sector has been affected by the interplay of colonial histories and polity-specific institutions (Charlet et al. 2013). Furthermore, the resource sector in many African countries possesses its own powerful interest groups that may at times belie the characterization of this sector as easily taxable—a circumstance that may actually lead to a strengthening of fiscal capacity. Gwaindepi and Siebrits (2020a,b) make this case for South Africa. The power of the diamond cartel prevented the mining sector from being taxed heavily in the Cape Colony, a circumstance that hastened the introduction of an income tax by a cash-strapped government in 1904. It was only after the creation of the Union of South Africa, with a strong central government, that the diamond sector was subjected to a diamond export levy in 1916, as well as temporary excess profit taxes in 1917. In other words, direct taxes and strong institutions preceded, and were in many ways a precondition for, effective resource taxation.

6 Conclusion

Employing comprehensive long-run panel data, our empirical analysis shows that the African experience adds important nuance to established theoretical and empirical analyses of fiscal capacity. The fact that state building is a process implies that one has to put its observed modern levels into historical perspective. We have argued that, on the revenue side, the recent trajectory of fiscal capacity building in many African polities is one of growth, rather than failure. Notwithstanding this general picture, substantial cross-country heterogeneity exists. Democracy increases investment in fiscal capacity, but it does so foremost in polities with low or moderate ethnic fractionalization. Shortened time horizons of ruling groups (not individuals) lead to less investment, especially in weakly institutionalized poli-

[34] The argument holds for tax levels as well: The top quartile of countries by level of tax collection (excluding trade and resources) in our dataset for the period 2010-15 includes (in that order) Botswana (diamonds), South Africa (diamonds, gold), Namibia (diamonds), Gabon (oil), Ghana (cocoa, gold, oil). And while some oil exporters such as Nigeria and Libya rank in the bottom quartile, other notable hydrocarbon exporters, such as Algeria are placed comfortably above the median.

ties. Both the historical and the empirical analyses illuminate the importance of external finance as a deterrent to fiscal capacity investments—a factor that has less bearing in a European context. Neither do we find evidence for the power of the bellicose theory since independence, nor a universal curse of resources on fiscal capacity, except for oil exporters.

As many African polities turn out "stronger" on the revenue side than much of the literature would lead us to expect, future research should take the lessons of this analysis to the expenditure side. If most African states do not lack the capacity to fund public good provision, why has its extension remained elusive in many? Perhaps by bringing new data to the table and by cutting loose the Western European yardstick, one may well find a trajectory of growth here, too.

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Appendix to
'The Fiscal State in Africa: Evidence from a century of growth'

Contents

A Existing work on African tax data

In this appendix, we briefly survey existing data work on taxation in African polities.

Precolonial taxation: The conventional periodization usually divides African history into the pre-colonial, colonial and post-colonial period.[1] Each period has its own distinct data problems, but only for the pre-colonial period do these preclude integration into our dataset.

There is an abundance of states to study in the long pre-colonial period. However, written (and kept) state records are extremely scarce for this period and thus the creation of annual comparable time series for a range of states is likely to be impossible. Certainly, the pre-colonial period deserves more attention in future research (Reid, 2011). There is a number of centralized kingdoms that collected taxes in a mix of in-kind, tribute, and cash (see Goody, 1969, for a review). For some pre-colonial centralized states, there is information about types and rates of taxation that may well be used for comparative studies. These include work on the Mahdist State (present-day Sudan) by Abu Shouk and Bjørkelo (1996) and the Merino Kingdom in current day Madagascar (Sanchez, 2013). Clearly, this list is not exhaustive. Even so, the rise of early colonial taxation—and the extent to which it replaced or supplemented taxation of the preceding precolonial period—-most likely will remain a subject that requires detailed historical case studies rather than efforts to build a comprehensive panel dataset.

The matter for the colonial and post-colonial periods is quite different. One cannot blame a dearth of data in archives. Here the records do indeed exist, but they are not easily standardized. They have therefore not been compiled in continent-wide, systematic, and comparable fashion.

Colonial taxation: Our understanding of colonial taxation is improving along with the literature on the impact of colonial institutions. Relatively little comparative work across the colonial empires exists. Most research focuses on the British and, to a slightly lesser extent, French colonies. There are also studies focusing on single countries, or parts thereof. Unfortunately, the data are not always published in a fashion that makes them comparable to other polities. Moreover, various languages and locations of the archives mean that creating a harmonized database is resource-intensive. This may explain the lack of a continent-wide dataset. Table A.1 reports some of the main papers that have appeared in the past decade focusing on colonial taxation in Africa.

This progress notwithstanding, large gaps in the coverage and concerns about data comparability remain. Revenue collection and reporting differed substantially between colonial empires. These differences are often, though not always, reflected in country- or group-specific studies (for example in the classification of revenue items). Finally, for a sensible comparison across countries, it is important to develop a metric that converts the nominal data into real data. Unlike for the postcolonial period, where GDP deflators become available, GDP estimates for colonial times are extremely scarce.

Taxation in sovereign polities: As a general rule, most countries did publish official statistics in a regular fashion in the post-colonial period. Moreover, efforts to accumulate data in larger databases through the UN and related agencies have increased (Ward, 2004). While these databases have decent coverage for the last decade, this does not apply to earlier years. The impact on statistical reporting of the difficulties most African economies found themselves in between the late 1970s and early 1990s has been well noted—the 1980s and the 1990s have been referred to as "lost decades" not only in terms of economic growth, but also statistical record-keeping (Jerven, 2013). The emergence of international datasets has improved data access, but problems of data coverage in the official databases remain.

Figure A.1 underscores the validity of this point by comparing the coverage of two prominent databases. The International Monetary Fund's Government Finance Statistics (IMF GFS) is a dataset of revenue and expenditure statistics from countries around the world. The GFS provides four main tax measures: total taxes, trade taxes, direct taxes, and indirect taxes in nominal terms. Yet for the African continent, the coverage is extremely sparse. Data are

[1] These are of course overlapping and vary from polity to polity.

Table A.1: Comparison of coverage in existing data sets on African colonial taxation

Countries/Area:	Time period:	Authors:
Francophone West Africa: Benin, Côte d'Ivoire, Niger and Senegal	1850-2010	Andersson (2018)
The French Empire: covers almost the entire second French colonial empire. Except for the Indochinese Union, most colonies are in Africa: Algeria, Tunisia Mauritania, and Morocco, the federations of French West Africa (Afrique Occidentale Francaise, AOF) and French Equatorial Africa (Afrique Equatoriale Francaise, AEF), Togo, Cameroon, and Madagascar. The total coverage is 21 countries.	1830-1962	Cogneau et al. (2018)
Portuguese Africa: Mozambique and Angola	1850s-1970s	Alexopoulou (2018)
Portuguese Africa: Mozambique, Angola, and Guinea	Late 1800s-1950s	Havik (2013)
British Africa: Gambia, Sierra Leone, Gold Coast, Nigeria, Nyasaland, Kenya, Uganda, and Mauritius	1880-1940	Frankema (2011)
British Africa/Empire: Mainly focusing on: East & Southern Africa: Bechuanaland, Northern Rhodesia, Nyasaland Protectorate, Kenya and Tanganyika Territory. West Africa: Gambia, Sierra Leone, Gold Coast, Nigeria, Mauritius (and India). Also includes some data on United Kingdom and the Dominions: Australia, New Zealand, Canada and Union of South Africa	1870-1940	Frankema (2010)
French and British Africa: French West Africa (AOF): Côte d'Ivoire, Dahomey/Benin, Guinée, Haute Volta/Burkina Faso, Mauritanie, Niger, Sénégal and Soudan. French Equatorial Africa (AEF): Congo, Gabon, Oubangui-Chari and Tchad. French Africa Other: Cameroun, Madagascar, Somaliland, Togo and Réunion. British West Africa (BWA): Gambia, Gold Coast, Nigeria and Sierra Leone. British East Africa (BEA): Bechunaland, Kenya, Northern Rhodesia, Nyasaland, Uganda and Tanganyika. British Africa other: Mauritius	c. 1880-1940: The majority of data is from early 20^{th} century with: 1911, 1920, 1925, 1929, 1934, and 1937 as key years.	Frankema and van Waijenburg (2014)

missing for 80 to 90 percent of African polities in the 1970s and 1980s, and more than half in the 1990s. Only in recent years does coverage improve to around 70 percent. It is important to note that a "missing" observation does not imply that the revenue data itself has never been compiled or recorded. In fact, the potential catalogue of data available to the IMF is much wider, but IMF regulations on data dissemination have prevented these data from being standardized and added to the official databases (Jerven, 2016).

Many scholars have attempted to fill these gaps, often by taking the GFS data as a starting point and supplementing this database with data collected through other sources. Baskaran and Bigsten (2013) construct a dataset for 31 countries for 1990-2005 by combining data from the OECD's African Economic Outlook dataset with the World Bank's African Development Indicators. Justified by a high correlation (0.9) between these two datasets, the authors use the OECD statistics to fill in the gaps in the World Bank dataset. Prichard and Leonard (2010) similarly merge existing datasets into a new dataset on taxation in Africa. Their dataset covers 45 countries from 1972-2005, though roughly 7-20% of observations remain missing, depending on the criteria employed. Mansour (2014) augments the GFS dataset with data from IMF staff reports and statistical appendices. By doing so, the author increases coverage for 1980-2010 for a sample of 41 SSA countries. Moreover, the dataset provides a disaggregation building on the GFS's main cate-

Figure A.1: Comparison of coverage in existing data sets on African taxation, independent polities

gories (total, trade, direct, and indirect taxes) with seven different constituent series (the original four, plus corporate, individual, and resource taxes). The coverage across these series is commendable, as relatively few gaps exist in the actual data.

The post-colonial dataset with the best coverage is housed at the International Centre for Tax and Development: the Government Revenue Dataset (Prichard, 2016). By the author's own admissions, the dataset is still plagued by methodological concerns, and in particular, the contributors worry about the lack of a suitable deflator (Prichard et al., 2014, 17-29). The dataset is comprehensive, albeit for a brief time, covering 40 countries from 1980 to 2015.

Summary assessment: Figure A.2 summarizes the patchy picture for the 20th century. For the period 1940-1980, the available material is so scarce that it forbids generalizations. And yet, even for the other periods, the figure overstates the usefulness of the available material as it only refers to nominal data. Beyond the patchiness and the problems in harmonizing the existing datasets, the lack of a suitable deflator often precludes comparative research. The challenges to overcome for a new dataset are thus (i) exhaustive coverage in the time and space dimensions, (ii) making the data comparable across countries and historical periods, and (iii) finding a suitable deflator to convert nominal into real data.

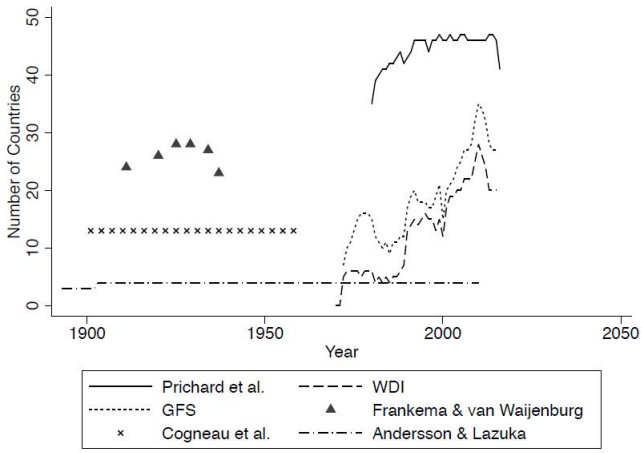


Figure A.2: Comparison of coverage in existing data sets on African taxation, 20th century

B The new data set

B.1 Fiscal data

Given the insufficiency of the available material and concerns about the comparability across time and space, we started our data collection from primary sources.[2] For each year and each polity, we aimed to gather data on revenues actually received. Where these data do not exist, we rely on estimates of the revenue - or in rare cases - on budgeted revenue. The challenge in the data collection was not missing data as such, but to locate the existing data. For the post-colonial period, this was relatively straightforward. With few exceptions, we drew on reports by the IMF that had recently been de-classified. Typically, these reports also make it possible to estimate the part of the revenues that can be attributed to the oil or mining sector.[3] For the colonial period, we have visited seven archives[4] and have drawn on recently digitized sources where possible.[5] We end up with a total of more than 4,700 such annual revenue statements (or country-year combinations).

The level of detail of these budgets differs greatly, meaning they can contain from as little as 10 items per year up to more than 100 items per year. In order to condense these data to our fiscal capacity variables of interest, we converted these budgets to spreadsheet data at the highest resolution possible. We transcribed more than 130,000 disaggregated items, of which over 25,000 are unique items. The unique items were manually coded according to the IMF government finance statistics manual in the same way revenues are classified today.[6] We then took this classification dictionary to the data to differentiate ordinary revenues according to the type of taxation: direct taxation, indirect taxation proper, trade taxation, other ordinary revenue, and resource revenue. We also introduce a category "extraordinary revenue", in which we include revenues that were classified as ordinary by the polity of the time, but are extraordinary by current IMF definitions (such as certain colonial transfers from the metropolis). Where revenues from trade taxation are pooled across federations, we take great care in separating them sensibly with auxiliary primary data. For example, the French federation (AOF and AEF) budgets contain information about the distribution of such revenues in certain years.

A natural advantage of this strategy is that, by definition, we make the data comparable across space and time as our coding is irrespective of the country the tax originates in. In applying this strategy, our database thus produces comparable disaggregated revenue data for 41 African polities in the balanced sample (46 in the full sample). Once a polity enters our balanced sample (the average entry date being 1903), the average country-year coverage is over 97 % and thus almost complete (see table B.1).[7] We document all the sources for each year, for each country, with accompanying graphs in a separate appendix available from our websites.

B.2 Deflator: Wage data

The wages we use reflect the daily remuneration of unskilled non-agricultural laborers in nominal local currency. Data before 1960 are from the unaltered series on urban labourer compensation by Frankema and van Waijenburg (2012) for the British colonies, with the exception of Egypt, Sudan and South Africa, where various sources were used. For the other colonies, a wide variety of primary and secondary sources were used. We interpolated gaps and checked the plausibility of doing so. Crucially, inflation was not a widespread problem before World War II in African polities, underscoring the validity of this procedure. Between 1960 and the early 1970s, the unskilled wage is usually taken to be the statutory minimum wage in industry as reported in the IMF country reports. Where a regionally differentiated minimum wage existed, the simple average of all regions was taken. In cases where a minimum wage did not exist, the remuneration of worker categories that were predominantly low-skilled were used. In cases where the public sector (often through a large parastatal sector) was the dominant employer in the formal sector, public sector wages of the

[2] The only exception is the data on Portuguese colonies before 1945, for which we mostly rely on Philip Havik's data (Havik, 2013).

[3] For some countries, we consult country-specific sources. From the 2000s onwards, the OECD also provides harmonized data for a selection of countries.

[4] Namely, collections in Paris, Aix-en-Provence, Rome, Porto, Berlin, and London (LSE and British Library).

[5] In particular, British (Blue Books) and French (Gallica colection) colonial records are comparatively well digitized, though large gaps remain.

[6] In addition, we introduce a category that covers resource revenues.

[7] Some polities do not exist in 1900 and thus enter later.

Table B.1: Data coverage (in %) and starting year by country

Country	Nominal Data		Real Data	
	First Year	Coverage	First Year	Coverage
Algeria	1900	100%	1900	100%
Angola	1900	96%	1900	95%
Benin	1900	99%	1909	99%
Botswana	1900	100%	1900	100%
Burkina Faso	1920	81%	1920	81%
Burundi	1924	91%	1924	91%
Cameroon	1900	94%	1900	94%
Central African Republic	1905	97%	1915	99%
Chad	1904	92%	1915	93%
Democratic Republic of the Congo	1900	100%	1905	100%
Egypt	1900	100%	1900	100%
Gabon	1904	98%	1915	98%
Gambia	1900	100%	1900	100%
Ghana	1900	99%	1900	99%
Guinea	1900	97%	1900	97%
Guinea-Bissau	1902	96%	1960	95%
Ivory Coast	1901	100%	1903	100%
Kenya	1900	99%	1904	99%
Lesotho	1900	100%	1911	100%
Madagascar	1900	100%	1901	100%
Malawi	1904	98%	1904	98%
Mali	1900	97%	1900	97%
Mauritania	1905	97%	1909	97%
Morocco	1920	100%	1920	100%
Mozambique	1900	97%	1913	94%
Namibia	1900	97%	1921	99%
Niger	1908	98%	1908	98%
Nigeria	1900	99%	1900	99%
Republic of Congo	1900	99%	1915	99%
Rwanda	1924	90%	1924	90%
Senegal	1900	98%	1900	98%
Sierra Leone	1900	100%	1900	100%
South Africa	1900	99%	1900	99%
Sudan	1906	100%	1906	100%
Swaziland	1904	100%	1911	100%
Tanzania	1900	97%	1900	95%
Togolese Republic	1900	95%	1900	95%
Tunisia	1900	100%	1900	100%
Uganda	1901	100%	1906	100%
Zambia	1907	86%	1926	98%
Zimbabwe	1900	92%	1900	88%
Average	1903	97%	1908	97%

Notes: Coverage is measured from first year in sample until 2015. Burkina Faso dissolved between 1933-1947. All sources for each country-year documentation are documented in the appendix.

lowest pay scale were used. Where possible, supplemental payments in kinds and monetary bonuses were taken into account. In cases where gaps in coverage between years existed, interpolation was used. We converted hourly or monthly wages to daily wages using an eight hour working day, and 23 (for French ex-colonies) or 25 (for most British ex-colonies) working days per month, as was often indicated in the sources.

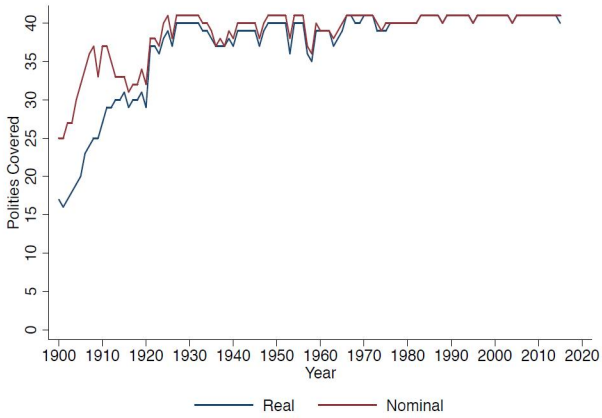
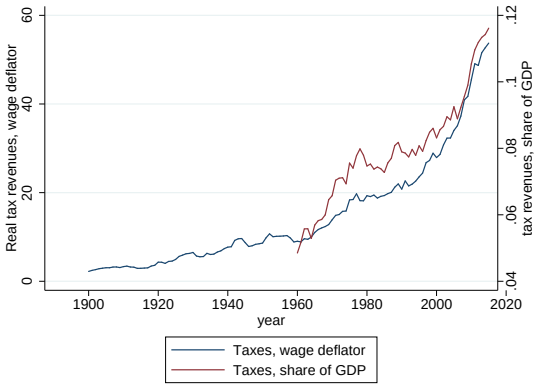


Figure B.1: Coverage of nominal and real fiscal data in our new data set, balanced sample, 1900-2015

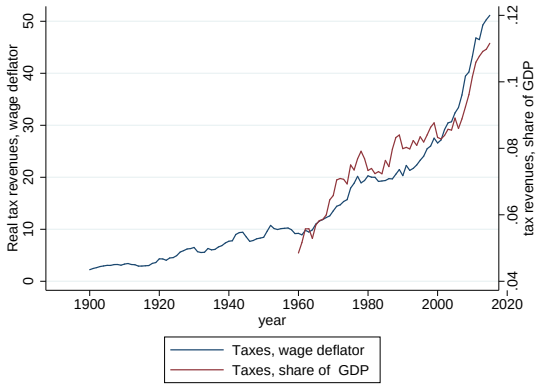
Between the mid-1970s and late 1990s, many countries stopped adjusting their minimum wage scales, and the legal minimum wage can therefore in many cases not be considered economically meaningful. From the early 2000s onwards, legislation has in many countries (partially) caught up with inflation, and minimum wage rates (as reported by the ILO) can often be considered meaningful again. The Kaitz ratio (minimum to mean wage) as reported in Bhorat et al. (2017) was used to determine when this was the case. Periods for which minimum wages cannot be considered economically binding are bridged where appropriate by extrapolating between the early 1970 and 2000 benchmarks using the dynamics of (1) mean wages in low-skill intensive sectors (food processing, textiles) (2) mean wages in total manufacturing industry (3) consumer price indices (4) mean public sector wages (rarely). (1) and (2) are supplied by UNIDO, (3) and (4) in most cases by the IMF country reports. The use of (3) is justified by empirical evidence based on a sample of African countries showing that on average 70% of increases in inflation were passed through to nominal wages (Mazumdar and Mazaheri, 2000).

We document the sources of the wage data in a separate data appendix on our website. Putting nominal tax and wage date together provides us with a comprehensive coverage, as shown in Figure B.1. Figure B.2 suggests that for the periods where we have both GDP data and our deflator, the correlation between our measure of fiscal capacity (in wage-days of an urban laborer per capita) and an alternative measure (revenue as a share of GDP) is extremely high.

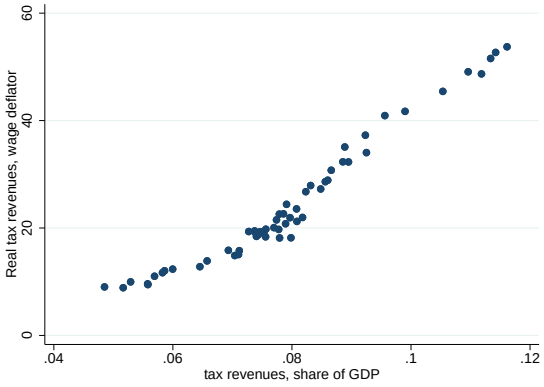
Figure B.2: Comparison of deflators: Taxes deflated by wages and taxes as share of GDP



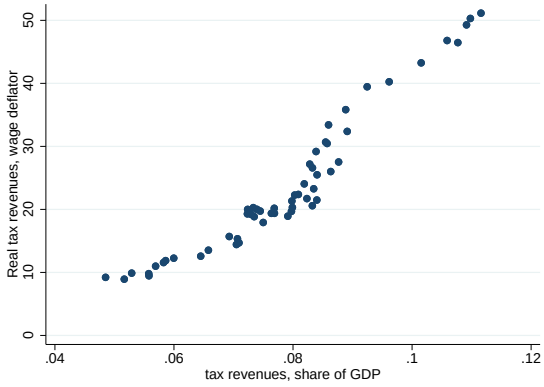
(a) Evolution over time, balanced sample



(b) Evolution over time, full sample



(c) Correlation, annual data, balanced sample



(d) Correlation, annual data, full sample

C Additional figures

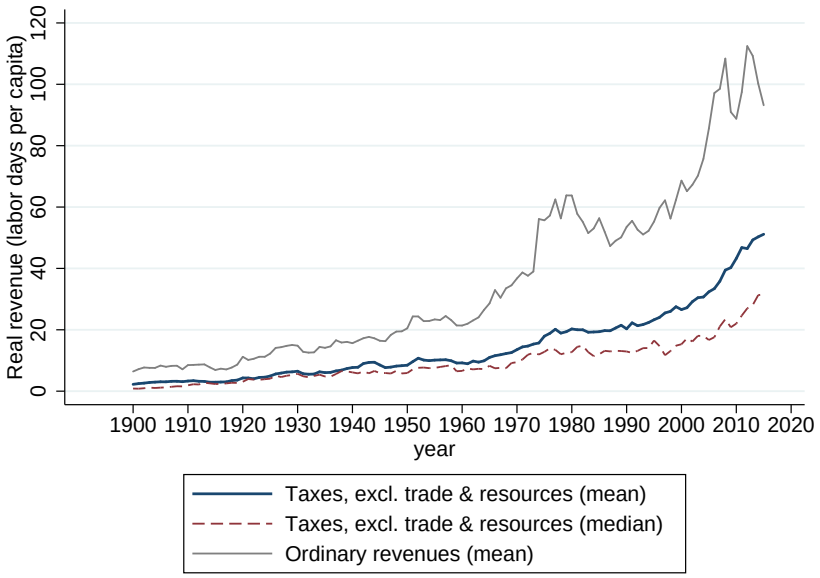
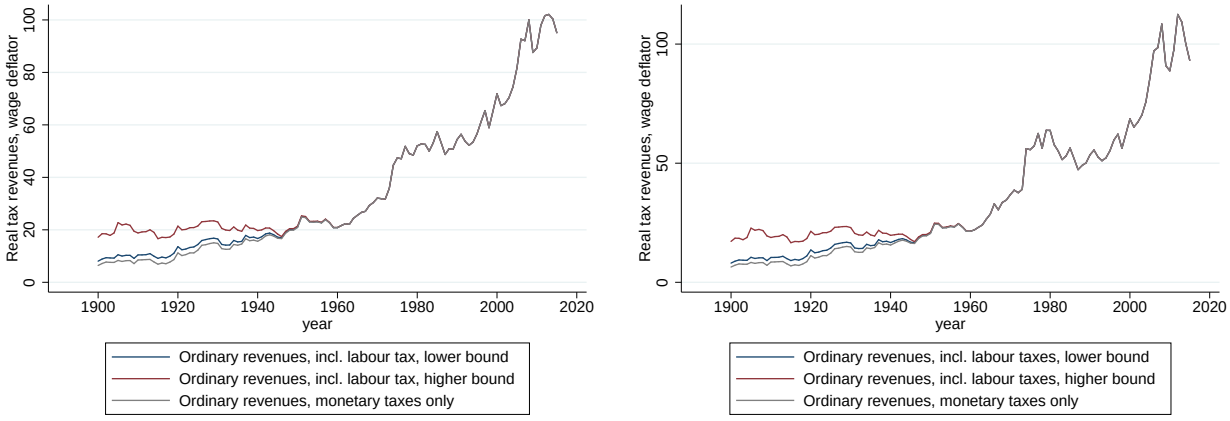


Figure C.1: Real taxation and total revenues, full sample, annual means and median



(a) Balanced sample

(b) Full sample

Figure C.2: Ordinary revenues, including forced labour estimates

D Sources and description of covariates

D.1 Covariates: Executive turnover

According to the theoretical framework in section 4.1, turnover should reflect changes in the group holding executive power. We therefore code *government turnover* as a dummy representing either a) a change in the party providing the chief executive after an election in the case of democracies, or b) a change in the ruling regime (through whatever means) in the case of autocracies. For this, we primarily rely on the coding of electoral change and regime ends provided by VDEM.[8]

As an alternative measure, we also compute *leader turnover* as the number of times the individual in power changes. In this, we largely draw on the Archigos project, which provides the names of each sovereign polity's chief executive. We extend Archigos back to 1890 by coding the names of colonial governors from historical encyclopedias.

D.2 Covariates: Cohesive institutions

We capture the concept of cohesive institutions through the democracy scores provided by the Varieties of Democracy (VDEM) project for each polity.[9] The VDEM-project codes 5 top-level democracy indices on a range from 0 to 1 based on expert assessment. These 5 indices measure constraints on the executive and legal protection for citizens ("liberal democracy"), conduct of elections ("polyarchy"), popular participation in politics ("participative democracy"), political culture ("deliberative democracy") and economic and social equality ("egalitarian democracy"). In line with the importance of executive constraints outlined in section 4.1, we focus on the liberal democracy score.

D.3 Covariates: Conflict

In our baseline specification, we proxy shifts in the demand for common-interest public goods through armed conflict. We exploit three widely used datasets on conflict: UCDP/PRIO, which measures the incidence of armed conflict, Correlates of War (COW), which measures the size of conflict by the number of battle-deaths, and MEPV, which provides a 11-point [0-10] additive scale measuring conflict magnitude. As the coverage of the PRIO and MEPV datasets starts with the end of World War II only, we extend both datasets backwards to 1890 utilizing the list of historical conflicts provided by Brecke (1999).

In many applications, we distinguish between interstate and domestic conflict. While interstate conflict could conceivably have the effect of mobilizing fiscal resources, civil conflict may have the opposite effect by eroding the government's tax base, and accentuating social divisions.[10] Throughout, we concentrate on conflicts taking place on the territory of an African polity. We code participation of an African polity in a conflict beyond its borders (such as the foreign military expeditions in the Second Congo War of 1998-2002) separately as an "armed intervention".

D.4 Covariates: External aid

There is currently no comprehensive data set on metropolitan transfers and post-colonial aid flows. In any case, the receipt of inter-governmental aid may partially reflect domestic fiscal pressures, raising serious endogeneity concerns.

[8] As an example, Tanzania is coded as experiencing a change in government in 1916, when effective control of the colony passed from Germany to Britain, and again in 1961 and 1962 (independence and establishment of the republic, respectively). Thereafter, no change is recorded until 2015, as all of the country's chief executives have hailed from the ruling TANU/CCM party. In neighboring Burundi, on the other hand, marked by ethnic conflict and successful coups, we record 10 changes in government in the post-independence period.

[9] We also utilize other indicators, such as Polity V scores, but only VDEM provides historical democracy values for the colonial polities, which constitute 46% of our observations.

[10] Our definition of interstate conflict extends beyond the narrower definition of interstate conflict used in these databases. For example, COW does not define the wars of colonial conquest fought between European imperial powers and indigenous African states in the late 19th and early 20th century as "inter-state", as most African states are not deemed to have been members of the Westphalian state system. While this may be useful for some applications, it does not quite do justice to the complex historical state-system of West Africa (Smith, 1989), and we therefore code these conflicts as interstate.

Instead, we construct a variable modeling political access to aid. This builds on a large literature emphasizing the role of political alliances in determining aid flows (De Mesquita and Smith, 2009; Alesina and Dollar, 2000; Dreher et al., 2009). This fits the historical context of independent African states in the Cold War period particularly well, when aid was determined by the maintenance of close ties to the principal former colonial powers Britain and France, or by alignment with the competing interests of the USA and Soviet Union. In more recent times, China has forged close links with African governments.[11]

We therefore focus on the political alliances of African polities with the 5 permanent members of the UN Security Council, and weigh those alliances with the budgetary resources available to that great power. For any sovereign African polity i and a UNSC permanent member j in year t we have:

$$A_{i,t} = \sum_{j} (S_{i,j,t} \times B_{j,t}) \tag{1}$$

where S is the Signorino and Ritter (1999) index of similarity between the alliances of the African polity and the UNSC power, and $B_{j,t}$ reflects the budget balance of the UNSC power. We treat all colonial polities as being perfectly and exclusively aligned with their imperial metropolis ($S_{i,j,t} = 1$) so that transfers received during the colonial period are solely determined by the budgetary situation in the metropolis.

D.5 Covariates: Resources

We are interested in evaluating whether resource availability influences incentives to invest in fiscal capacity. We recognize that the *value* of resources, however, is not fixed. It depends both on a polity's resource endowment as well as on fluctuating world market prices. We have therefore compiled a new dataset of commodity exports for all African polities, as well as world market prices for these commodities. We extend the commodity dataset by Bazzi and Blattman (2014), which commences in 1957, to the early colonial period using British trade statistics and a variety of colonial records.[12] We interact world market prices for each commodity $P_{t,k}$ with the share of that commodity in a polity's export basket $s_{i,T,k}$ to produce an index of resource prices:

$$R_{i,t} = \frac{\sum_k (s_{i,T,k} \times P_{t,k})}{\Pi_t} \times X_{i,T} \tag{2}$$

where i refers to the polity, t to the year, and k to a commodity. T refers to the period before or after 1957. As nominal prices P are denoted in British Pounds (pre-1957) and US Dollars (post-1957) respectively, we deflate nominal prices with the British and US price indices Π to produce a real index. As in Bazzi and Blattman (2014), we weight the final index for each polity by the share of its GDP accounted for by primary commodity exports $X_{i,T}$. This trade weighting increases the precision of the estimates, and reflects the idea that a given change in commodity prices should have a larger effect on a polity heavily focused on primary exports such as Libya ($X = 0.28$) compared to less commodity-trade dependent Egypt ($X = 0.09$).[13]

We depart from Bazzi and Blattman (2014) in that we fix export shares s and trade weights X within each period T. While variable shares and weights could increase precision, they could also introduce endogeneity concerns as export shares could react to changing world market prices.[14] However, we show that our results do not change when using variable shares.

[11] An example illustrates the mechanism at work here: While still a colony, Guinea's access to transfers depended ultimately on the priorities of the French Treasury. Around the time of independence in 1960, many former French colonies continued to align themselves closely with France, and a significant share of their aid came from France. Guinea's leader Sékou Touré, however, opted to distance himself from French plans for continued cooperation in 1958. The financial repercussions from the French government where swift. Touré consequently aligned himself with the Soviet Union and China, which provided increasing aid, before pivoting to the USA as the socialist block began its economic decline in the 1980s (Young, 2012)

[12] Before 1957, our dataset features 50 commodities, many of which are agricultural, such as cocoa, palm oil and groundnuts. After 1957, the list features 65 commodities, with a larger proportion of metals and minerals, and, of course, oil.

[13] We estimate primary export shares based on the data in Federico and Tena Junguito (2016).

[14] This latter point is made by Ciccone (2018), who argues for the use of fixed shares.

D.6 Covariates: Capital market access

A polity issuing debt may do so because it faces fiscal pressures, so that debt uptake is endogenous to tax revenues. As a remedy, we exploit the historical fact that the public borrowing of many non-sovereign polities was tightly regulated by the imperial metropolis (Accominotti et al., 2010). We therefore code a time-varying dummy $D_{i,t}$ from historical sources taking the value of 1 if a colony was institutionally able to issue debt. As an illustration, consider the case of the British colonies in Africa. The willingness of investors to purchase their stock depended crucially on their designation as "trustee stocks", which provided bondholders with additional protection in case of default (Sunderland, 2007). The 1900 Colonial Stocks Act accorded this privilege to Crown Colonies (Gardner, 2017). This effectively granted Gambia (Crown Colony since 1888) credit market access, while Kenya was excluded until it was granted Crown Colony status in 1920 (Sunderland, 2007). Variation in the market access variable therefore stems both from laws issued in the metropolis affecting all colonies, as well as from change in the legal status of individual colonies.

We interact this dummy for institutional access with the inverse of global market interest rates r_t, to reflect the idea that credit market access is more important in times of low interest rates:

$$C_{i,t} = D_{i,t} \times \frac{1}{r_t} \tag{3}$$

where i is the polity and t the year. All polities are coded as enjoying full credit market access after attaining sovereignty, so that any variation thereafter comes from changes in global interest rates.[15]

D.7 Covariates: Controls

- **Droughts** : Share of population affected by a drought event, calculated using data from EM-DAT (Guha-Sapir, 2020) and Spinoni et al. (2019).
- **Disasters** : Share of population affected by natural disaster, excluding drought, from EM-DAT (Guha-Sapir, 2020), supplemented by data from United Nations Office for Disaster Risk Reduction (2020) and NOAA (2020).
- **Hyperinflation episodes** : Dummy taking value of 1 if consumer price inflation exceeds 40%. Data and categorization from Reinhart and Rogoff (2009), missing data interpolated using GDP deflator or colonial consumer baskets from Frankema and van Waijenburg (2012).
- **Sovereign debt default** : Dummy taking value of 1 if polity is in external default. Data and categorization from Reinhart and Rogoff (2009), missing data interpolated using share of all debt in default from Beers and Mavalwalla (2018)
- **Independent statehood** : Dummy taking value of 1 if polity is sovereign state, defined as state system membership following Correlates of War Project (2016), some modification based on historical records, such as continuous sovereignty for Liberia.
- **Socialist economic systems** : Dummy taking value of 1 if attempt to control prices and central allocation of inputs under socialist ideology took place. Source: various.
- **Territorial changes** : Dummy taking value of 1 if a polity looses territory; Source: various, including Tir et al. (1998)
- **Real GDP growth** : Interpolated between benchmarks for colonial times from Jerven et al. (2019) (British colonies) and Prados de La Escosura (2012) (other colonies), for sovereign era from Inklaar et al. (2018).
- **Central bank lending** : Index [0,1] on statutory rules limiting lending from the central bank to the government, source: Romelli (2018).

[15] We proxy global interest rates with the Bank of England rate.

- **Capital liberalization** : Dummy taking value of 1 if polity has liberalized capital flows through either the capital or current account, data from Quinn and Toyoda (2008).
- **Settler data:** Share of population of European origin in 1900 and mortality of European population from Acemoglu et al. (2001).

E Summary Statistics

Table E.1: Summary statistics I (1900- 2015 panel, 5 year periods)

Variable	Obs	Mean	Std. Dev.	Min	Max
Fiscal variables					
Real pc tax revenues, excl. trade and resources	926	15.215	22.692	.004	253.602
Real pc tax revenues, excl. trade and resources, change over period	873	2.245	7.888	-47.598	117.508
Real pc tax revenues, incl. forced labour, lower bound	926	15.823	22.493	.436	253.602
Real pc tax revenues, incl. forced labour, upper bound	926	18.051	22.211	.436	253.602
Real pc ordinary revenues	926	28.647	37.606	.712	452.608
Tax revenues, share of GDP	435	.08	.047	.006	.256
Covariates					
real GDP growth, average over period	1047	.03	.034	-.138	.289
hyperinflation episode, dummy	1058	.368	.482	0	1
years in sovereign debt default	1058	.535	1.404	0	6
territorial change	1058	.002	.043	0	1
socialist economic system	1058	.056	.23	0	1
independent state	1058	.497	.5	0	1
(former) British colony	1058	.37	.483	0	1
(former) French colony	1058	.413	.493	0	1
(former) Portuguese colony	1058	.065	.247	0	1
(former) Belgian colony	1058	.065	.247	0	1
(former) Italian colony	1058	.043	.204	0	1
German colony	1150	.031	.174	0	1
Precolonial centralisation	1058	.532	.342	0	1
Population density	1054	25.488	42.331	.244	422.758
Cohesive institutions					
liberal democracy score (VDEM)	1052	.118	.115	.008	.668
European settlers, share of pop.	1058	.015	.041	0	.22
Population granted suffrage, share	1058	.484	.476	0	1
Ethnic fractionalisation (Fearon & Laitin)	1058	.663	.244	.039	.953
Ethnic fractionalisation (Montalvo & Reynal-Querol)	1058	.626	.257	.05	.959
Settler Mortality (Acemoglu, Johnson & Robinson)	828	428.105	572.991	15.5	2940
liberal democracy score, neighbors (VDEM)	1058	.121	.1	.02	.668
deliberative democracy score (VDEM)	1052	.109	.132	.002	.712
egalitarian democracy score (VDEM)	1052	.116	.108	.01	.554
participatory democracy score (VDEM)	1052	.087	.092	.003	.561
polyarchy score (VDEM)	1058	.162	.159	.007	.799
Constraints on Executive (Polity V)	526	3.092	1.837	1	7
Competitiveness of executive recruitment (Polity V)	526	1.121	.906	0	3
Openness of executive recruitment (Polity V)	526	2.7	1.716	0	4
Polity 2 index	535	-2.579	5.304	-9.8	9
Executive turnover					
change in government, count over 5yr period	1058	.343	.659	0	4
decolonisation	1058	.069	.254	0	1
change in chief executive, count over 5yr period	1058	1.096	1.223	0	8
change in chief executive, count over 10yr period	1058	2.217	1.979	0	13
coups d'etat, count over 10yr period	1058	.375	.91	0	7
coups d'etat, count over 5yr period	1058	.22	.599	0	5

...continued on following page

Table E.2: Summary statistics II (1900- 2015 panel, 5 year periods)

Variable	Obs	Mean	Std. Dev.	Min	Max
Armed conflict					
conflict incidence (PRIO)	1058	.265	.441	0	1
military interventions incidence (PRIO)	1058	.121	.326	0	1
international war battle deaths, '000 (COW)	1058	.211	1.329	0	23.333
international war magnitude, sum over period (MEPV)	1058	.716	3.004	0	25
civil war incidence (PRIO)	1058	.159	.366	0	1
civil war battle deaths, '000 (COW)	1058	.911	6.166	0	117.75
civil war magnitude, sum over period (MEPV)	1058	1.934	5.856	0	36
military interventions incidence (PRIO)	1058	.071	.257	0	1
military intervention incidence (COW)	1058	.051	.22	0	1
military intervention force size (COW)	1058	1.629	19.654	0	600
disaster-affected population, million	1058	.023	.105	0	1.476
drought-affected population, million	1058	.363	1.094	0	15
External aid					
Exposure to foreign aid, unweighted alliances	1050	-.14	.291	-1.127	.653
Exposure to foreign aid, weighted alliances	1050	-.118	.25	-.831	.653
Exposure to metropolitan aid, unweighted alliances	1050	-.113	.197	-.826	.748
Exposure to foreign aid, UN voting, sq. distances	1039	-.086	.274	-.949	.653
Exposure to foreign aid, UN voting, abs. distances	1039	-.062	.242	-.826	.653
K-index, UN voting, sq. distances	1039	-.053	.208	-.826	.653
P-index, UN voting, sq. distances	1039	-.008	.249	-.826	.653
Aid received from UNSC veto powers, US Dollar, mil	509	121.889	232.109	-.8	2350.6
Aid received from former metropolis, share of GDP	432	.022	.027	-.003	.212
Real aid received from (former) metropolis, local currency, mil.	655	24.01	44.177	-2.098	597.851
Resource prices and revenue					
Real resource prices, fixed export shares, trade weighted index	1058	13.042	16.548	.988	203.891
Real resource prices, variable export shares, trade weighted index	1051	11.586	14.251	.661	189.792
Real resource prices, fixed export shares, no trade weighting	1058	96.484	51.25	21.259	614.768
Real resource prices, fixed export shares, no trade weighting	1051	88.019	51.798	15.609	572.256
Nominal resource prices, variable export shares, trade weighted index	1051	30.736	78.843	.23	1548.702
Real resource revenues, US Dollars	1150	3.741	23.601	0	318.463
Real resource revenues, local currency	1058	40.153	178.176	0	2365.349
Share of oil in exports	1051	.071	.209	0	.997
Real mineral prices, variable export shares, trade weighted index	868	118.548	635.667	0	9464.534
Capital markets and debt					
External credit market access, normalized	1058	.109	.201	0	1
Debt issuance, share of GDP	654	.01	.087	-.72	.877
Debt issuance, dummy	654	.372	.342	0	1
Liberalized capital flows, dummy	191	.539	.5	0	1
Structural Adjustment Program (IMF), dummy	1058	.151	.313	0	1
Structural Adjustment Program (IMF), size (SDR)	1058	8.455	44.243	0	1045.7
Structural Adjustment Program reform intensity	1058	.391	.488	0	1
Central Bank lending restriction	435	.472	.158	0	1

Table E.3: Growth rates of real tax revenues (in %), excluding trade and resource taxes

	Average	Standard deviation
1900-1914	6.0	15.8
1919-1939	4.3	3.4
1946-1959	0.8	4.5
1960-1979	4.5	3.9
1980-1999	2.2	4.0
2000-2015	5.0	4.4
World War I	3.1	15.8
World War II	2.8	11.0

Notes: Average: takes the average of the mean growth rates of the countries in the balanced sample. Standard deviation: takes the average of the standard deviations of growth rates of the countries in the balanced sample.

F Additional results

Table F.1: **Benchmark – Sample composition**

	Dependent Variable: Change in real tax collection per capita, excluding trade & resource taxes							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Excluding....					
	Balanced sample	Sub- Saharan Africa only	never colonized	socialists	high inflation phases	civil conflicts	severe natural disasters	severe natural droughts
lib. democracy score (VDEM)	10.091	10.430	9.499	9.108	7.030	9.878	10.758	5.762
	(6.506)	(6.702)	(6.545)	(6.716)	(7.236)	(6.944)	(7.181)	(7.928)
conflict incidence (PRIO)	0.588	0.616	0.327	0.437	-0.171	0.200	0.412	-0.099
	(0.876)	(0.913)	(0.919)	(0.914)	(1.020)	(0.852)	(0.974)	(0.749)
L.change in government	-0.607*	-0.591	-0.610*	-0.499	-0.936**	-0.778*	-0.696**	-0.772*
	(0.351)	(0.393)	(0.355)	(0.397)	(0.435)	(0.422)	(0.340)	(0.404)
Real resource prices	0.022	0.054	0.003	-0.003	0.001	0.037	0.004	-0.007
	(0.040)	(0.067)	(0.028)	(0.032)	(0.031)	(0.036)	(0.028)	(0.029)
Exposure to foreign aid	-4.513**	-4.552**	-4.593**	-4.404**	-6.400**	-4.111**	-4.478*	-5.821***
	(1.982)	(1.936)	(2.044)	(2.014)	(2.487)	(1.962)	(2.232)	(1.772)
Credit market access	-9.112*	-5.704	-8.443*	-8.098*	-5.711	-9.406*	-9.420*	-6.530
	(4.887)	(5.374)	(4.777)	(4.786)	(5.304)	(4.895)	(4.798)	(4.336)
Polity fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.211	0.215	0.203	0.203	0.217	0.211	0.216	0.201
N	828	777	849	824	713	796	815	771

Note: See Table 2 in main paper for description of controls (all included, but not shown in these regressions). Additional comments: Model (1) drops Djibouti, Ethiopia, Liberia, Libya and Somalia; (2) drops Algeria, Egypt, Libya, Morocco and Tunisia; (3) drops Ethiopia and Liberia, (4)-(8) drop all observations with the characteristic indicated. Socialist economic system is a time varying dummy. High inflation is defined as exceeding 20% per year, civil conflict defined as by COW, severe natural disasters defined as affecting more than 100,000 inhabitants, severe droughts exceeding 1,000,000. All regressions are OLS with standard errors clustered at polity level. Standard errors in parentheses: * p<0.1, ** p<0.05, *** p<0.01

Table F.2: **Benchmark – Controls**

	Dependent Variable: Change in real tax collection per capita, excluding trade & resource taxes					
	(1)	(2)	(3)	(4)	(5)	(6)
	Benchmark Controls shown	Potential badcontrols omitted	Including labor tax, lower bound	Including labour tax, upper bound	Annual data	Total Revenues as dep. variable
lib. democracy score (VDEM)	9.2064	11.9011*	9.0513	8.9467	1.2932	1.8152
	(6.4747)	(6.5272)	(6.4997)	(6.9103)	(1.1527)	(6.9003)
conflict incidence (PRIO)	0.2936	0.0085	0.3257	0.1384	0.0142	-0.7835
	(0.8817)	(0.8680)	(0.8880)	(0.9389)	(0.1504)	(0.8422)
L.change in government	-0.6343*	-0.6165*	-0.6558*	-0.8116**	-0.0800	0.3723
	(0.3558)	(0.3499)	(0.3544)	(0.3849)	(0.0829)	(0.4516)
Real resource prices	0.0030	-0.0025	0.0000	-0.0255	0.0018	-0.0238
	(0.0278)	(0.0331)	(0.0276)	(0.0346)	(0.0049)	(0.0367)
Exposure to foreign aid	-4.5983**	-4.3789*	-4.8836**	-3.3763	-0.6706**	-4.0994
	(2.0579)	(2.2751)	(2.0985)	(2.6785)	(0.2866)	(3.3779)
Credit market access	-8.0909*	-8.3230*	-7.7485	-11.9305**	-1.0101**	-0.9890
	(4.6594)	(4.9126)	(4.9462)	(5.5141)	(0.3996)	(5.7637)
L.drought-affected pop.	-0.0662	-0.0407	-0.0748	-0.1114	-0.0887***	-0.7724
	(0.6442)	(0.6133)	(0.6468)	(0.6521)	(0.0238)	(0.4773)
socialist economic system	0.3604	-0.0099	0.3954	0.3123	-0.0179	-0.1058
	(0.8167)	(0.8567)	(0.8145)	(0.8497)	(0.1484)	(1.1863)
territorial change	-4.4240**	-6.4693***	-4.5192**	-4.4982*	-2.1149	0.2536
	(2.0580)	(2.0783)	(2.0887)	(2.3066)	(1.9887)	(2.6703)
hyperinflation episode	-1.0393	-1.1764*	-0.9450	-0.5277	-0.0211	-0.0677
	(0.6479)	(0.6617)	(0.6548)	(0.6686)	(0.2223)	(1.0230)
real GDP, y-o-y change	26.3123**		25.9849**	25.0559*	5.8837***	73.4325***
	(12.7649)		(12.8243)	(12.5978)	(1.3551)	(15.9158)
sovereign debt default	-0.5520***		-0.5472**	-0.5013**	-0.1393***	-0.8408**
	(0.2027)		(0.2047)	(0.2099)	(0.0493)	(0.3444)
independent state	0.7338	0.2032	0.8649	0.8933	0.1337	1.5911
	(1.0488)	(1.02880	(1.0275)	(1.0118)	(0.2233)	(2.2595)
Polity fixed effects	✓	✓	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.205	0.188	0.208	0.215	0.039	0.174
N	873	873	873	873	4428	829

Note: See Table 2 in main paper for description of controls. Additional comments: All regressions are OLS with standard errors clustered at polity level (46 polities). Regression (5) uses annual data, all other regressions condense data into 5-year-period. Standard errors in parentheses: * p<0.1, ** p<0.05, *** p<0.01

Table F.3: **Cohesive institutions – Heterogeneity**

	Dependent Variable: Change in real tax collection per capita, excluding trade & resource taxes							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Status of polity		Precolonial centralization		Population density		(former) British colony	
	Colonial	Sovereign	Low	High	Low	High	No	Yes
lib. democracy score (VDEM)	19.96	1.41	5.47	8.60	20.17**	-5.96	-8.93	21.08***
	(15.47)	(6.41)	(4.07)	(8.72)	(7.77)	(7.02)	(5.63)	(6.03)
Polity fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.172	0.233	0.169	0.196	0.190	0.258	0.287	0.189
N	384	489	421	452	422	451	516	357

Note: See Table 2 in main paper for description of controls (all included, but not shown in these regressions). Additional comments: Model (1) and (2) split the sample according to colonial status; model (3) and (4) split the sample at the median level of precolonial political centralization (Gennaioli and Rainer, 2007); model (5) and (6) split the sample at the median level of population density in 1900; model (7) and (8) split the sample by a dummy representing British colonial history. All regressions are OLS; standard errors clustered at polity level and shown in parentheses: * p<0.1, ** p<0.05, *** p<0.01

Table F.4: **Conflict – Civil wars, resources and military interventions abroad**

	Dep. var.: Change in tax as share of GDP			Dep. var.: Change in real tax collection		
	(1)	(2)	(3)	(4)	(5)	(6)
	Civil wars and resource prices			Armed interventions		
	PRIO	COW	MEPV	PRIO	COW	COW
Civil war incidence	4.7379*					
	(2.6135)					
Civil war incidence × real resource prices	-0.1757*					
	(0.1036)					
Civil war battle deaths		0.2793**				
		(0.1187)				
Civil war battle deaths × real resource prices		-0.0256***				
		(0.0066)				
Civil war magnitude			0.2952*			
			(0.1643)			
Civil war magnitude × real resource prices			-0.0267***			
			(0.0082)			
Military interventions incidence				-1.1720		
				(2.0452)		
Military interventions incidence					0.1480	
					(1.1004)	
Military intervention force size						-0.0012
						(0.006)
Polity fixed effects	✓	✓	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.100	0.108	0.105	0.235	0.233	0.233
N	388	388	388	489	489	489

Note: See Table 2 in main paper for description of controls (all included, but not shown in these regressions). Additional comments: Resource prices are measured using real world market prices evaluated at fixed export shares and weighted by a polity's trade share. Military interventions refer to a sovereign African polity taking part in an armed intervention in another African polity. Coefficients in models (1)-(3) are multiplied by 1,000 for purposes of presentation. All regressions are OLS with standard errors clustered at polity level with standard errors in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

Table F.5: **Government and leader turnover – Robustness**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable: Change in real tax collection per capita, excluding trade & resource taxes							
	Controlling for		Liberal democracy score		Leader turnover & liberal democracy score		Leader turnover	
	Civil war	Coups d'etat	Low	High	Low	High	Colonial polities	Sovereign polities
Change in government (lagged)	-0.705*	-0.717*	-0.710**	0.834				
	(0.371)	(0.372)	(0.325)	(1.432)				
Civil war incidence (PRIO)	0.080							
	(1.165)							
Decolonisation	-2.450*	-2.472*	-1.195	-7.181				
	(1.385)	(1.315)	(0.996)	(7.721)				
Change in chief executive (lagged, 10y)					0.026	1.455	-0.153	0.145
					(0.128)	(1.202)	(0.105)	(0.205)
Coups d'etat (lagged, 5y)		0.018						
		(0.355)						
Polity fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.206	0.205	0.149	0.372	0.146	0.384	0.173	0.233
N	873	873	742	131	742	131	384	489

Note: See Table 2 in main paper for description of controls (all included, but not shown in these regressions). Change in government refers to a change in the party or regime holding power. Change in chief executive or leader refers to a change in the individual holding power. Models (1)-(4) lag these variables by 1 period (5 years), (5)-(8) lag by 2 periods (10 years). Decolonization is defined as a dummy variable taking the value of 1 for any colonial polity after Ghana's declaration of independence in 1957; model (3)-(6) split the sample at the median level of the VDEM liberal democracy score of all polities; Coup d'etat refers to an attempted or planned coup. All regressions are OLS; standard errors clustered at polity level (46 polities) and shown in parentheses: * p<0.1, ** p<0.05, *** p<0.01

Table F.6: **Exposure to aid – Different indicators**

	Dependent Variable: Change in real tax collection per capita, excluding trade & resource taxes						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline exposure measure	Weighted with material capabilities	Exposure to metropolitan aid	UNGA voting patterns			
				distance weighting		Index	
				squared	absolute	κ	π
Exposure to foreign aid	-4.60**						
	(2.06)						
Exposure to foreign aid, weighted alliances		-4.75**					
		(1.96)					
Exposure to metropolitan aid, weighted alliances			-4.41*				
			(2.54)				
Exposure to foreign aid, UN voting, sq. distances				-5.38**			
				(2.07)			
Exposure to foreign aid, UN voting, abs. distances					-6.27***		
					(2.20)		
K-index, UN voting, sq. distances						-5.53**	
						(2.51)	
P-index, UN voting, sq. distances							-4.72*
							(2.53)
Polity fixed effects	✓	✓	✓	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.205	0.205	0.205	0.205	0.206	0.205	0.205
N	873	873	873	870	870	870	870

Note: See Table 2 in main paper for description of controls (all included, but not shown in these regressions). Exposure to foreign aid is a Signorino and Ritter (1999) similarity index of alignment between an African polity's alliance system and that of the permanent members of the UN Security Council, interacted with the fiscal position of the UNSC member. Model (2) weights this index by material capabilities. Exposure to metropolitan aid in model (3) is a similarity index of alignment between an African polity's alliance system and that of the colonial metropolis, interacted with the fiscal position of the metropolis. Model (4) and (5) use a Signorino and Ritter (1999) similarity index between the votes cast by an African polity in the UN General Assembly and those of the permanent members of the UN Security Council, interacted with the fiscal position of the UNSC member. Model (4) weighs this index using geographical distances, model (5) uses squared distances. Model (6) uses each African polity's UNGA voting patterns to compute Cohen (1968)'s κ; model (7) uses these votes to compute Scott (1955)'s π. Similarity indices as computed by Häge and Hug (2016). All regressions are OLS; standard errors clustered at polity level (46 polities) and shown in parentheses: * p<0.1, ** p<0.05, *** p<0.01

Table F.7: **Exposure to aid – Natural disasters and metropolitan transfers**

	Dep. Var.: Change in real tax collection per capita				Dep. Var.: Metropolitan aid received			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Interaction with		ODA from...		Incl. aid metropolitan transfers		Excl. aid metropolitan transfers	
	disasters	droughts	former metropolis	UNSC veto powers	incl. never colonized		incl. never colonized	
					yes	no	yes	no
Disaster-affected population	-9.93***							
	(3.68)							
Aid exposure	-4.37**	-4.71**						
	(2.11)	(2.06)						
Disaster-affected population × aid exposure	-7.75							
	(5.37)							
Drought-affected population (lagged)		0.056						
		(0.81)						
Drought-affected pop. (lagged) × aid exposure		0.626						
		(1.43)						
Aid exposure (lagged)-(former) metropolis, % of GDP			-84.71**					
			(37.96)					
Aid exposure (lagged)-UNSC veto powers (USD)				-0.004*				
				(0.002)				
Aid exposure - (former) metropolis					36.72**	36.05**	0.063***	0.062***
					(15.32)	(15.41)	(0.018)	(0.018)
Polity fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.215	0.206	0.239	0.236	0.389	0.384	0.461	0.453
N	873	873	380	448	625	621	422	418

Note: See Table 2 in main paper for description of controls (all included, but not shown in these regressions). Exposure to foreign aid is a Signorino and Ritter (1999) similarity index of alignment between an African polity's alliance system and that of the permanent members of the UN Security Council, interacted with the fiscal position of the UNSC member. Exposure to metropolitan aid is a Signorino and Ritter (1999) similarity index of alignment between an African polity's alliance system and that of the colonial metropolis, interacted with the fiscal position of the metropolis. Similarity indices as computed by Häge and Hug (2016). Model (1) interacts exposure to foreign aid with the number of inhabitants affected by natural disasters (EMDAT), model (2) interacts exposure to foreign aid with the number of inhabitants affected by drought events. Model (3) substitutes exposure to metropolitan aid with actual ODA received from the former metropolis model (4) uses ODA received from the permanent UNSC members. Model (5) and (6) explain ODA received from the (former) colonial metropolis, including transfers during the colonial period. Model (7) and (8) explain ODA received from the former colonial metropolis, excluding transfers during the colonial period. Model (6) and (8) exclude Ethiopia and Liberia. All regressions are OLS; standard errors clustered at polity level (46 polities) and shown in parentheses: * p<0.1, ** p<0.05, *** p<0.01

Table F.8: **Exposure to debt – Instrumental variables**

	Dep. Var.: Change in real tax collection per capita, excl. trade & resource							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Benchmark, reduced form	Benchmark, 2SLS	No sovereign defaults, reduced form	No sovereign defaults, 2SLS	No capital controls, reduced form	No capital controls, 2SLS	Low Central Bank lending, reduced form	Low Central Bank lending, 2SLS
Credit market access	-12.43**		-13.42*		-12.91*		-19.17**	
	(5.63)		(7.78)		(7.29)		(7.99)	
Debt issuance, dummy		-19.37**		-16.87**		-22.96**		-33.37***
		(8.14)		(7.83)		(11.30)		(7.48)
Polity fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.243	0.088	0.268	0.156	0.253	0.057	0.435	0.006
N	585	585	439	439	509	509	412	412
	First stage (Dependent variable: debt issuance, dummy)							
		(2f)		(4f)		(6f)		(8f)
Credit market access		0.64***		0.80***		0.56***		0.57***
		(0.19)		(0.21)		(0.17)		(0.12)
Polity fixed effects		✓		✓		✓		✓
Period fixed effects		✓		✓		✓		✓
Adjusted R^2	-	0.539	-	0.616	-	0.576	-	0.562
N	-	585	-	439	-	509	-	412

Note: See Table 2 in main paper for description of controls (all included, but not shown in these regressions). Capital market access refers to a dummy taking the value of 1 for polities with institutional access to international credit markets (either through imperial guarantee or attained sovereignty) interacted with inverted global interest rates. Debt issuance is a dummy for a polity issuing debt in a given period. Models (2), (4), (6) and (8) are 2SLS instrumenting debt issues with capital market access. Models (1), (3), (5) and (7) are OLS. Standard errors clustered at polity level and shown in parentheses: * p<0.1, ** p<0.05, *** p<0.01

Table F.9: **Resource income – Alternative indicators**

	(1) Variable shares, trade weighting	(2) Fixed shares, no trade weighting	(3) Variable shares, no trade weighting	(4) Nominal index, trade weighting	(5) Fixed shares, trade weighting with lag	(6) No large exporters
	Dep. Var.: Change in real tax collection per capita, excl. trade & resource					
Real resource prices	0.0100					
(variable export shares, trade weighted index)	(0.0373)					
Real resource prices		-0.0029				
(fixed export shares, no trade weighting)		(0.0077)				
Real resource prices			0.0010			
(fixed export shares, no trade weighting)			(0.0092)			
Nominal resource prices				0.0060		
(variable export shares, trade weighted index)				(0.0120)		
Real resource prices (lagged)					-0.0318	
(fixed export shares, trade weighted index)					(0.0405)	
Real resource prices						0.0244
(fixed export shares, trade weighted index)						(0.0358)
Polity fixed effects	✓	✓	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.205	0.205	0.205	0.207	0.207	0.198
N	873	873	873	873	873	721

Note: See Table 2 in main paper for description of controls (all included, but not shown in these regressions). Real resource prices are real world market prices for commodities, interacted with polity-specific fixed export shares and weighted by a polity's trade share, unless otherwise noted in the table. Export shares refer to a commodity's share in a polity's export basket. Trade share refers to the aggregate weight of primary commodities in a polity's export basket. Model (6) excludes large exporters, defined as those polities exporting more than 10% of global trade value for any commodity. All regressions are OLS; standard errors clustered at polity level (46 polities) and shown in parentheses: * p<0.1, ** p<0.05, *** p<0.01

Table F.10: **Resource income – Instrumental variables**

	(1) Prior to 1957 only	(2) Post 1957 only	(3) Oil exporters only	(4) 2SLS - Effect of resource income on taxes lagged	(5) variable shares	(6) unweighted
	Dep. Var.: Change in real tax collection per capita, excl. trade & resource					
Real resource prices	0.0187	0.0364				
	(0.0384)	(0.0610)				
Real resource prices (lagged)			-0.1064*			
(variable export shares, trade weighted index)			(0.0528)			
Real resource revenues, LCU				-0.0101	0.0017	-0.0016
				(0.0141)	(0.0061)	(0.0038)
Polity fixed effects	✓	✓	✓	✓	✓	✓
Period fixed effects	✓	✓	✓	✓	✓	✓
Adjusted R^2	0.066	0.221	0.079	0.100	0.216	0.194
N	387	486	82	873	873	873
				First stage (Dep. var.: Real resource revenues)		
				(4f)	(5f)	(6f)
Real resource prices (lagged)				3.1362***		
(fixed export shares, trade weighted index)				(1.1408)		
Real resource prices					5.7581***	
(variable export shares, trade weighted index)					(1.9100)	
Real resource prices						1.8595***
(fixed export shares, unweighted)						(0.4392)
Polity fixed effects				✓	✓	✓
Period fixed effects				✓	✓	✓
Adjusted R^2				0.472	0.572	0.555
N				873	873	873

Note: See Table 2 in main paper for description of controls (all included, but not shown in these regressions). Real resource prices are real world market prices for commodities, interacted with polity-specific fixed export shares and weighted by a polity's trade share, unless otherwise noted in the table. Export shares refer to a commodity's share in a polity's export basket. Trade weighting refers to the aggregate weight of primary commodities in a polity's export basket. Model (1) uses only commodity data from British import statistics (available prior to 1957); model 2 uses only data as assembled by Bazzi & Blattman (2014) available from 1957 onward. Models (1)-(3) are OLS, models (4)-(6) are 2SLS using real resource prices as an instrument for resource revenues. Resource revenues in local currency units (LCU) are as collected from polity-level budget data, deflated by local wages and normalized by population. Standard errors clustered at polity level (46 polities) and shown in parentheses: * p<0.1, ** p<0.05, *** p<0.01

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