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JEL Classification: P16, P26, D72, H54, O18, Q32

Keywords: political economy, democracy, infrastructure, Natural resources, Development

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Priority Roads: the Political Economy of Africa's Interior-to-Coast Roads*

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Abstract

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1 Introduction

According to the institutional view of development, inclusive political institutions are required for sustained economic growth, as only these will deliver the public goods and market-supporting infrastructure that make growth possible. In contrast, extractive political institutions will focus on enriching the ruling elite and on making it hard for other groups to emerge (e.g. Acemoglu and Robinson, 2012). Indeed, Acemoglu et al. (2019) find that democracy has a positive effect on GDP per capita. One specific type of market-supporting infrastructure is the paved road network. In this paper, we empirically investigate the link between political institutions and the construction of such network.

We focus on West Africa in the post-colonisation period (1965-2014). One striking fact about this region is that, following on a colonial pattern, paved roads have evolved mostly in an *interior-to-coast* direction (see Figures 1 and 2),[1] which makes them potentially useful to export natural resources but relatively ill-suited to promote internal and regional connectivity.[2] Although the shape of these roads has long been criticised by policy makers and development economists (e.g. Nkrumah 1964, p. 23; Rodney 1972, p. 209; Sachs et al. 2004, p. 182), a conclusive proof that they are indeed suboptimal has been hard to produce. This is because the complexity of the optimal network design problem together with limited data availability for this region make it hard to identify the optimal network that the actual networks should be compared with. Then, it is hard to rule out that the actual networks are the optimal response to the geography or comparative advantage of the West African countries.[3]

To make progress, we take an indirect approach and investigate the political circumstances under which these networks were built. Controlling for country fixed effects and thus for geography, we document that the West African paved road networks expanded in a more interior-to-coast

[1]In most countries in this region, paved roads tend to run from the interior to the coast. Very few roads run in the direction of connecting two interior-to-coast roads, two overland borders, or two neighboring countries (particularly after excluding the interior-to-coast roads of the landlocked countries).

[2]We focus on roads and not on railways because West African railways are in a derelict state due to poor maintenance, and almost none have been extended since independence, thus giving us no variation to analyze. Moreover, railways tend to have roads near them that run in parallel: based on Vmap0 (www.mapability.com), 86% of used and unused rail kilometer segments in Africa intersect with roads, while at the median the remaining 14% have a road within 1 kilometer. See also Jedwab & Moradi (2016).

[3]It is interesting to note that when one draws the *entire* road networks of these countries (that is including all unpaved roads and tracks), these do *not* look predominantly interior-to-coast. This casts doubt on the possibility that geography is the driving factor behind the interior-to-coast shape of the *paved* road network, though of course geography could matter differently for paved roads and lower quality roads.

fashion in periods of autocracy, relative to periods of democracy. In particular, autocracies relative to democracies focused more on connecting metal and mineral deposits to ports. Since deposits are typically located in a country's interior, this resulted in more interior-to-coast connections being built under autocracies. Combined with the region's history of frequent autocratic rule, this empirical result goes towards explaining the observed interior-to-coast shape of the West African networks. These findings are robust to controlling for factors that may drive changes in both political institutions and comparative advantage over time (such as time fixed effects and the world prices of exported metals and minerals) and to instrumenting for autocracy as suggested by the most recent literature (using lags in space and time as in Acemoglu et al., 2018). We also show that the deposit-to-port bias of autocracies is only there if the deposits to be connected were located on the elite's ethnic homeland.

Seen through the lens of the institutional view, these results have a natural interpretation. As is well known, African elites are typically defined along ethnic lines. Deposit-to-port roads will generate resource rents: since these will be more easily captured by the ruling elite under autocracy than under democracy, and if the deposits are located on elite's ethnic homeland, one would expect autocracy to be more biased in favor of these roads, the more so the more the deposits are located on the elite's ethnic homeland. In this interpretation, the interior-to-coast expansion of West African roads since independence was at least in part the suboptimal consequence of extractive political institutions.

Uncovering the political determinants of African infrastructure, as this paper tries to do, is important for a number of reasons. First, transport infrastructure is an important item of expenditure for many African governments and donors.[4] Second, internal trade costs remain very high in Africa,[5] suggesting that a great expansion of the African networks will be required in the future.[6] Third, there are conflicting views on the type of links that such expansion should prioritise. On the one hand, the African Development Bank, backed by Western donors, has been calling for more interior-to-interior links of the kind that Africa most sorely misses.[7] On the other

[4]Jedwab & Storeygard (2020), p. 3, report that transport accounted for 14% of World Bank lending, and 22% of African Development Bank disbursements in 2012-2015.

[5]Atkin & Donaldson (2015) find that internal trade costs in Ethiopia and Nigeria are four to five times larger than in the US. Porteous (2019) finds slightly higher values for a sample covering all 42 Sub-Saharan African countries.

[6]More infrastructure may spread growth more evenly as it facilitates the propagation of shocks (Amarasinghe et al., 2018).

[7]The Bank has been pushing for the completion of the Trans-African Highway network, a large part of which is made up of interior-to-interior links. This approach is backed up by Buys et al. (2006), who argue that to

hand, China has been funding projects that essentially reinforce the existing interior-to-coast links (Bonfatti and Poelhekke, 2017, p. 105), often in cooperation with governments of doubtful democratic credentials. Finally, these decisions are likely to have large welfare consequences. For example, it is widely accepted that high internal trade costs are a major obstacle to the modernisation of African agriculture (Blimpo et al. 2013, p. 62), whose low productivity and high labour share are found to explain most of the income gap between sub-Saharan Africa and the rest of the world (Porteous 2019).

Our analysis is based on a newly-assembled dataset of roads, metal and mineral deposits, ports and cities covering 12 West African countries over the period 1965-2012. The data on roads was obtained by digitising 18 successive editions of Michelin road maps of West Africa.[8] For each year of publication, we have information on the full network of paved and unpaved roads, so that we can track the expansion of the paved road network. Rather than working with gridded data, we take into account the links between roads and digitally recreate the complete network. The data on deposits, which we put together from several different sources, gives us the location and year of discovery of 391 metal and mineral deposits in West Africa, of which we also know the size and main metal or mineral extracted.

Our first challenge is to measure the extent to which paving in any given period displayed a *deposit-to-port bias* - defined as the extent to which it focused on connecting deposits to port, as opposed to any other pairs of locations. We proceed in two steps. In the first step, for each country in our sample, we construct a counterfactual road expansion path, inspired by the work of Burgess et al. (2015) on Kenya. The counterfactual aims to approximate the locations where roads would have been paved from period to period between 1965 and 2014, had the government only cared about connecting deposits to ports. More specifically, we take all deposit-port pairs that are not yet fully connected by paved roads, and rank them according to their market potential (sum of port size and deposit size, divided by bilateral distance). In each period, we then re-allocate the actually paved kilometers to connecting the deposit-port pairs that are at the top of the ranking. Not surprisingly, in a typical country and period, the counterfactual looks very interior-to-coast in shape.

connect the major cities of Sub-Saharan Africa through primary roads would boost trade by US$ 250bn at a relatively little cost. More recently, Porteous (2019) shows, using an estimated model of agricultural trade, that to reduce African trade costs to international standards along the Trans-African Highway network links would achieve a significant portion of the welfare gains achievable by reducing trade costs on a much larger set of links.

[8] The map scans were kindly shared by Alexander Moradi, Free University of Bozen-Bolzano.

In the second step, we calculate the extent to which real paving in any given period overlapped with the counterfactual network of that period. Using this time-varying estimate of overlap we can assess the government's deposit-to-port bias, because a high score denotes a type of paving that focused a lot on connecting deposits to ports.

We regress our measure of deposit-to-port bias in any given period on the level of autocracy at the beginning of the period, controlling for country fixed effects, time fixed effects, country-specific trends, and a rich set of controls. Our main result is that autocracies displayed a stronger deposit-to-port bias. This effect is large, with a one-standard deviation increase in autocracy resulting in a third of a standard deviation increase in deposit-to-port bias. The effect is robust to instrumenting for autocracy using a lagged measure of autocracy in a country's region, as suggested by the most recent literature (Acemoglu et al., 2018).

Having established that autocracies had a causal impact on deposit-to-port road paving, we next turn to consider possible mechanisms. Our results are not driven by autocracies paving more kilometers than democracies, or by their being more in favor of trade openness, or by the fact that deposits were close to big cities that autocracies had a bias for. In contrast, we find evidence that autocracies only had a deposit-to-port bias when the deposits were located on the elite's ethnic homeland. As explained above, this is evidence in support of the view that autocracies built more deposit-to-port roads than democracies, because they could enrich themselves more using such roads.

One noteworthy aspect of our approach is that although we have data at the level of individual locations (deposits), we then aggregate this up so that our unit of observation in the regressions is a country-period. This approach is adopted because our goal is to investigate how the *shape* of the national road networks evolved over time (specifically whether they evolved in an interior-to-coast fashion). Clearly, the shape of a national road network is best investigated at the country level.

Our paper is most related to a small but growing literature on the political determinants of transport infrastructure. Burgess et al. (2015) study ethnic favoritism in the allocation of Kenyan roads in 1963-2011. They find strong evidence of favoritism, which however attenuates under democracy. For a sample covering 39 Sub-Saharan African countries over the same period, Jedwab & Storeygard (2020) find that democracies tend to pave more roads close to big cities. They also find that democracies tend to pave more roads. Inspired by this, and to avoid any mechanical effect of extra paving on our measure of overlap, we also attempt a specification in which we control for the number of kilometers paved in each period. This is consistent with the findings by Burgess et al. (2015), since favored ethnic groups will not necessarily be located close to the big cities. However, our main contribution is to document the bias of autocracies in favor of connecting deposits to ports, and relate our findings to the debate on the interior-to-coast shape of the African networks.

Alder et al. (2018) obtain results for China which resemble those of Burgess et al. (2015). They compare the expansion of Chinese motorways in 1992-2007 to an optimal network calculated by connecting the country's largest cities through a heuristic algorithm. They find that a significant portion of the discrepancy between the two networks is due to an especially high weight being given to the birthplaces of officials in power at the time of expansion. Finally, Felbermayr & Tarasov (2015) derive optimal investment on transport infrastructure in a continuum of locations disposed along a line, and show (theoretically and empirically, using data on European roads) that if neighboring governments act non-co-operatively the equilibrium features too little investment close to borders. While this may provide an additional explanation for the scarcity of *inter*-national interior-to-interior links in Africa,[9] it cannot easily explain the scarcity of *intra*-national interior-to-interior links, nor the pattern of changes over time that we document in this paper.

This paper builds on earlier work by Bonfatti & Poelhekke (2017). They show that coastal African countries endowed with more mine-to-port roads also feature national trade costs that are strongly biased in favor of trade with overseas countries, relative to neighboring countries.[10] This effect is reversed for landlocked countries, presumably because the mine-to-port roads can also be used to trade with transit neighbors. In conjunction with those earlier results, our current results suggest that the bias in road investment decisions associated with extractive political institutions will have a large and long-lasting effect on the direction of trade in the African countries, and

[9]Note however that Porteous (2019) finds that cross-border trade costs in Africa are not correlated with differences in colonial heritage. This seems to suggest that, at least in colonial times, the scarcity of international interior-to-interior links was not driven by a lack of co-ordination between neighboring governments. In a similar vein, Bonfatti & Poelhekke (2017) find no evidence that pairs of neighboring African countries sharing the same coloniser trade more with each other than other pairs of neighboring African countries.

[10]Not observing trade costs directly, they infer this from observed import flows. Three facts suggest that what they identify is trade costs, and not simply the comparative advantage of metal- and mineral-endowed countries for trading with rich countries that tend to be located overseas. First, they show that the effect is opposite for coastal and landlocked countries. Second, they control for the difference in income between trading countries. And third, they show that metal- and mineral-endowed coastal countries also import more from overseas *African* countries.

specifically one that penalises regional integration.[11]

The paper is structured as follows. Section 2 presents the basic empirical approach, and section 3 delves into the construction of our measure of deposit-to-port bias. Section 4 lists the data sources. Section 5 presents the main results, with robustness checks being provided in Section 6 and in the Online Appendix. Section 7 concludes.

2 Empirical approach

We want to test for the hypothesis that autocracies, relative to democracies, had a greater deposit-to-port bias - which we define as a preference for connecting mineral and metal deposits to the ports, as opposed to any other pairs of location. To this purpose, we will construct a dependent variable capturing the deposit-to-port bias of the sitting government in country i and period t, and regress it on the state of autocracy in the same country and period. Full details on how the dependent variable is constructed are provided in the next section. In this section, we take the dependent variable as given, and describe the overall structure of our regressions.

Let $\textit{deposit-to-port bias}_{i,t}$ be our dependent variable. We regress it on a measure of autocracy in country i at the beginning of period t, $A_{i,t}$:

$$\textit{deposit-to-port bias}_{i,t} = \beta\, A_{i,t} + \gamma\, X_{i,t} + f_i + f_t + trend_i + \epsilon_{it}, \tag{1}$$

where we always include country fixed effects f_i, period fixed effects f_t, and country-specific trends $trend_i$, and $X_{i,t}$ denotes a vector of time-varying, country-level controls.

Our choice of periods is dictated by the years of publication of the Michelin maps of West Africa. Such maps are not published every year, although for all years in which they are published they cover all countries in our sample. We observe 23 maps published between 1965 and 2014, which results in 22 irregular time periods with an average length of 2.23 years (and a minimum and maximum length of 1 and 7 years). Since Michelin maps are published at the beginning of a year and are supposed to capture the situation on the ground shortly before publication, we

[11] Our current results are that, even controlling for kilometers paved, autocracy built more deposit-to-port roads, which by the earlier results implies national trade costs that are more biased in favor of overseas countries relative to neighbouring countries. In unreported regressions, we also find no evidence that autocracies paved more kilometers in total. This suggests that autocracies were bad for non-deposit-to-port roads (and thus potentially for the cost of trading with neighboring countries) not only in relative terms but also in absolute terms.

take our periods to start at the beginning of each publication year. For example, our first two road maps were published in 1965 and 1967: our first period then starts on January 1, 1965 and ends on December 31, 1966.

Our dependent variable, *deposit-to-port bias*$_{i,t}$, is constructed using all new paving that occurred in period t. We infer the period's paving by comparing maps published at the beginning and end of the period. For example, if a road shows up as paved in the 1967 edition but unpaved in the 1965 edition, then we conclude that it was paved sometime between January 1, 1965 and December 31, 1966 (athough we do not know exactly when).

Our main measure of autocracy will be the inverse of the Polity IV index of democracy, which is available yearly. Thus, our measure will be equal to 10 for the strongest autocracy, and equal to -10 for the weakest one (or the strongest democracy). Since $A_{i,t}$ should measure the state of autocracy at the beginning of period t, and the Polity IV index measures political conditions on December 31 of each year (see Marshall et al., 2017, p. 12), we construct $A_{i,t}$ using the inverse of the Polity IV index for the last year before period t. For example, for the period that goes from January 1, 1965 till December 31, 1966, $A_{i,t}$ is constructed using the inverse of the 1964 Polity IV index, which measures the state of autocracy on December 31, 1964.

The vector $X_{i,t}$ contains variables that may affect road paving decisions and correlate with autocracy. It includes stock variables measured at the beginning of each period (e.g. a country's stock of deposits) or year (openness, commodity prices) as well as flow variables measured period by period (current paving, deposit discoveries) or year-by-year (foreign aid, FDI, an indicator for civil war). All yearly-measured variables are averaged over the period. To avoid confusion, the variables' names reported in the regression tables will always clearly specify the timing of measurement of each variable.

It is worth pointing out that the specification in (1) is quite demanding. Any time invariant characteristic that may induce a country to build a more or less interior-to-coast road network - be it geography, the time-invariant determinants of comparative advantage, colonial heritage, etc - will be absorbed by the country fixed effects. Any common time process that may matter for the way in which the shape of the networks evolve - e.g. the passing of time since independence, the end of the Cold War and the subsequent wave of democratisation, technological progress, price changes that apply to all metals and minerals - will be captured by the period fixed effects. Any mechanical effect driven by the fact that our periods are of irregular length will also be captured by the period fixed effects. Finally, any country-specific, linear time process - such as those driven by long-run population or economic growth, or by price trends in the specific metals and minerals produced by each country - will be accounted for by the country-specific trends. We will, in summary, only be using the within-country variation which is left after removing all of these time processes, which we will then further discipline by adding a variety of country-specific and time-varying controls.

3 Construction of the dependent variable

To construct our dependent variable - a measure of the sitting government's preference for connecting metal and mineral deposits to ports, as opposed to any other pairs of locations - we proceed in two steps. First, for each country in our sample, we construct a counterfactual road expansion path. This counterfactual aims to approximate the way in which paved roads would have expanded in 1965-2014, had the government only cared about connecting deposits to ports. Our measure of deposit-to-port bias, which we construct in the second step, will be a measure of how close the actual expansion path was to the counterfactual. The two steps are described in sections 3.1 and 3.2 respectively.

3.1 A counterfactual road expansion path

The construction of our counterfactual road expansion path is inspired by the work of Burgess et al. (2015) for Kenya. As they do, we begin by calculating how many kilometers of roads were paved in each country in each period (the "paving quota" for that period), by comparing the observed status of roads in two subsequent map years. We then re-allocate the quota of each period to paving different roads in that period, proceeding period after period as explained below. In other words, we imagine that the government would follow a different expansion path to the one it actually followed, leading to a different, counterfactual network by 2014.

Burgess et al. (2015) construct a counterfactual network expansion path which is meant to approximate the way in which paved roads would have expanded in Kenya since 1964, had the government only cared about connecting cities to one another. They do so by ranking all possible city pairs according to their market potential (the sum of the cities' size, divided by their distance along existing roads), and by re-allocating the paving quota in each period to yet unpaved bilateral connections in the order in which city pairs show up in the ranking (continuing with unfinished pairs from the previous period before moving down the ranking).[12] To take the topography into account, they construct the counterfactual by only paving unpaved roads along the shortest route between city pairs in the initial (1964) network. They can do so since all the roads paved during their period were initially unpaved, and already in existence in 1964. The initial unpaved network, then, is a reasonable indication of the set of road segments that could potentially be paved.

We proceed in a similar way, except that our counterfactual connects deposit-port pairs instead of city pairs. For each country independently, we prepare a list of all possible deposit-port pairs. We then rank them by market potential, and re-allocate the paving quota in each period to connecting pairs in the order in which they show up in the ranking.[13] "Market potential" of a pair is defined as the sum of indices of deposit size and port size, divided by the distance along existing roads between them.[14] Large deposits are thus connected to large ports first, unless a deposit is very remote compared to smaller ones. We always start paving from the port, because the port may have multiple uses so that even unfinished connections may be useful if they start from the port. We start with deposits that had been discovered by 1965 and let additional deposits enter the ranking from the year that they were discovered, which shakes up the ranking over time. In case any unused quota remains in a period (which may happen if there are few or no unconnected deposits at the beginning of the period), we drop it. This is based on the logic that a government who only cares about connecting deposits to ports would spend any surplus money on things other than paving.[15]

Differently from Burgess et al. (2015), some of the paved roads constructed in our sample did not exist at all in the initial (1965) networks. Thus, to use the initial unpaved networks to construct our counterfactuals would mean throwing away some of our knowledge of the topography, since we would be constraining ourselves to not build roads in places where we know roads could be built (as evidenced by the fact that they were built at a later stage). To circumvent

[12] When paving a connection between a pair, they start paving from the largest city in the pair.

[13] For landlocked countries, we pair each deposit to the closest port anywhere in West Africa, and pave that connection up to the country's border.

[14] In robustness tests we experiment with Burgess et al. (2015)'s alternative ways of ranking pairs, which are based on size only or on distance only.

[15] Ades & Glaeser (1995) find that autocracies tend to have relatively larger capital cities than democracies, suggesting that more government spending is directed to the seat of government. We thus assume that unused quota is spent on the capital city rather than on road paving. In the estimation we will control for the total amount of paving in each period.

this problem, we add to our 1965 networks all roads (paved or unpaved) which were built by 2014. This means that our counterfactual-building procedure is allowed to pave not only over roads that were unpaved in 1965, but also over territories where a road did not exist in 1965, but where a road would be built by 2014.[16]

The case of Sierra Leone is exemplified in Figure 6, where deposits are denoted by green asterisks, ports (including those on navigable rivers) by blue squares, the initial (1965) network by black lines, and the deposit-to-port counterfactual in 2014 by red lines. For comparison, Figure 5 reports the actual network in 2014. As can be appreciated, the counterfactual network is quite similar to the actual one, suggesting that road paving in Sierra Leone has been placing a lot of emphasis on connecting deposits to ports. The reason why the counterfactual network is somewhat more extended than the actual network has to do with the way road deterioration is treated in the counterfactual, and is further described below.

Two issues with the construction of the counterfactual must be flagged up. These have to do with the fact that, in our sample, we observe deterioration as well as upgrading of roads. In particular, it happens in 43% of country-periods that the quality of at least one segment of road deteriorates. At the median, 3% of the road network becomes unpaved in such an event. This may be the result of lack of maintenance, or of traumatic events such as civil war.

The first issue is that, in the presence of deterioration, paving in any given period may involve roads that have never been paved before, or roads that have already been paved in the past but have since deteriorated to unpaved status. If the two types of paving carry a different cost, then one should ideally give them different weights in calculating the quota used in the construction of the counterfactual. We tackle this issue in two ways. First, we use the World Bank's 2008 ROCKS dataset (also used by Collier et al. 2016), containing more than 3,000 road construction projects in 89 low and middle-income countries, to investigate weather the per-kilometer cost of paving an entirely new road differed on average from that of repaving a previously paved road. We find that it did not, neither in the full sample nor in Africa.[17] This provides at least some

[16] When no road of any quality exists to connect a deposit and a port (not even a track) we artificially connect the deposit to the road network by taking the shortest straight-line distance from the deposit location to the nearest road.

[17] The ROCKS dataset contains many different types of road construction activities. After careful reading we identify two categories, "Upgrading to Bituminous 2L" and "Reconstruction bituminous", as the best candidates to represent the paving of a new road, and the re-paving of a previously existing road. We are thus left with 1,124 projects, 333 of which are located in Africa. We regress cost per kilometer on "first paving" (an indicator variable for the first category), an interaction between first paving and the Africa dummy, country and year fixed effects, the length of the overall road project, and the type of terrain on which the road was built. The coefficient

reassurance that we are not making a first-order mistake in constructing the counterfactual the way we do. Second, we also show that results are robust to recalculating the counterfactual assuming that to re-pave a kilometer of a previously paved road costs 50% less than to pave a kilometer of an entirely new road.

Secondly, when constructing our baseline counterfactual, we only re-allocate the resources used to pave the road network (which we have a proxy for), and not those used to maintain it (which is not observed). This means that two periods with the same number of new kilometers paved, but with different number of kilometers deteriorated, are treated in the same way. Furthermore, when reallocating the paving quota, we do not let the counterfactual planner use the resources that are in reality used to pave new roads, to instead maintain the current counterfactual network. In fact, we simply do not let the baseline counterfactual networks deteriorate over time. To tackle this issue, we show that our results are robust to recalculating the counterfactual so as to take deterioration into account. In brief, we now calculate a "deterioration quota" in each period, beside the paving quota. This equals the number of kilometers that actually deteriorated. We then let the counterfactual deteriorate by this deterioration quota, starting from the lowest-ranked already-connected deposit-port pairs. This ensures that the counterfactual expands less (or even contracts) in periods of deterioration, and that the counterfactual planner may spend part of the paving quota to maintain the existing network (since it will be used to offset the effect of the deterioration quota on the lowest-ranked pairs). Further details on the construction of this alternative counterfactual are provided in Section 6 below.

3.2 The *deposit-to-port bias*$_{i,t}$ measure

Our measure of deposit-to-port bias in country i and period t is the fraction of actual paving in country i and period t which overlaps with deposit-to-port counterfactual networks for that country, as it would have appeared at the end of period t. These overlap measures range between 0 and 1. A score of 1 denotes perfect overlap with the deposit-to-port counterfactual in that country and period, while a score of 0 denotes no overlap.

More precisely, the overlap between actual paving and the deposit-to-port counterfactual is calculated as follows. We omit the country subscript i for the rest of this section, but it should be clear that the same procedure applies to all countries in our sample. Let $A_{t,real}$ denote the real

on first paving is negative and insignificant, for either values of the Africa dummy.

road network in place by the end of period t, and $A_{t,cf}$ the counterfactual deposit-to-port network as it would have appeared by the end of period t. The actual paving that occurred in period t is represented in set notation by $A_{t,real} \setminus A_{t-1,real}$, that is the difference between the actual road networks at the end of periods t and $t-1$. Therefore, $(A_{t,real} \setminus A_{t-1,real}) \cap A_{t,cf}$ denotes the overlap between the actual paving in period t, and the counterfactual deposit-to-port network at the end of period t.[18] Dividing this overlap by $A_{t,real} \setminus A_{t-1,real}$ gives us the fraction of the actual paving in period t that overlaps with the deposit-to-port counterfactual at the end of period t, or our overlap measure:

$$\textit{deposit-to-port bias}_t = \frac{(A_{t,real} \setminus A_{t-1,real}) \cap A_{t,cf}}{A_{t,real} \setminus A_{t-1,real}}. \tag{2}$$

To illustrate the method intuitively, we use Figure 7. Let area $A_{0,real} = a$ represent the initial (1965) paved road network. Assume that by the end of period 1 (Dec 31, 1967) the real network was extended towards the south-west, to cover area $A_{1,real} = a + b + c + d$. The actual paving in period 1 is thus $b + c + d$, which provides us with the paving quota for that period. Let area $A_{1,cf} = a + d + e + f$ towards the south-east represent the deposit-to-port counterfactual network as it would have appeared at the end of period 1. The reason for the discrepancy between counterfactual and actual network might be that there are deposits located in region f whose connection was not a priority under the actual planner, but would have been a priority under the counterfactual planner. The overlap between the actual paving in period 1 and the the counterfactual at the end of the period is then d, and $\textit{deposit-to-port bias}_1$ is equal to $d/(b + c + d)$.

Suppose now that by the end of period $t = 2$ (Dec 31, 1968),[19] the real network was extended to cover area $A_{2,real} = a + b + c + d + e + g + h$. The actual paving in period 2 is thus $g + h + e$, which provides us with the paving quota for period 2. Under the deposit-to-port counterfactual, however, the additional paving in period 2 would have covered area $c + h + i$, such that the counterfactual network by the end of period 2 would have covered area $A_{2,cfp} = a + c + d + e + f + h + i$. At this point, we could proceed in two different ways.

On the one hand, if we considered as overlap the intersection of the actual and counterfactual

[18] Obviously, such overlap can only include parts of the counterfactual network that had not been already paved at the start of period t.

[19] As explained earlier, this choice of periods is dictated by the fact that the next Michelin map of West Africa was published in early 1969.

additional paving, the overlap in period 2 would be only area h, because area e should have been paved one period earlier. However, this approach betrays the fact that area e *was* paved in period 2, and it overlaps with the deposit-to-port counterfactual as it appeared in that period. The paving of e could well be the choice of a period 2 government who cares more about connecting deposits than did its predecessor in period 1, and wants to pave whatever is left unpaved of the deposit-to-port counterfactual. To avoid unreasonably low overlaps because of this issue, we allow for the possibility that governments revert back to the entire (yet unpaved) counterfactual in any period. Consequently, we count as overlap in period 2 the overlap between the actual paving in that period, and the entire counterfactual network as it appears at the end of the period. The overlap in our example is then $e + h$, and *deposit-to-port bias* $_2$ is equal to $(e + h)/(e + g + h)$.

In summary,

$$
\begin{aligned}
A_{0,real} &= a \\
A_{1,real} &= a + b + c + d \\
A_{1,cf} &= a + d + e + f \\
A_{2,real} &= a + b + c + d + e + g + h \\
A_{2,cf} &= a + c + d + e + f + h + i \\
\textit{deposit-to-port bias}_{\ 1} &= \frac{(A_{1,real} \setminus A_{0,real}) \cap A_{1,cf}}{A_{1,real} \setminus A_{0.real}} = \frac{d}{b + c + d} \\
\textit{deposit-to-port bias}_{\ 2} &= \frac{(A_{2,real} \setminus A_{1,real}) \cap A_{2,cf}}{A_{2,real} \setminus A_{1,real}} = \frac{e + h}{e + g + h},
\end{aligned}
$$

and the same rules apply for $t > 2$.

Figure 8 and 10 provide an example for Sierra Leone. Figure 10 shows the initial (1965) paved network in black segments, and the deposit-to-port counterfactual as it appeared at the end of period 10 (1984-1985) in red segments. The deposits used to construct the counterfactual (those discovered by the beginning of the period) are denoted by green asterisks, and the ports are denoted by blue squares. Figure 8 shows the actual paved network by the end of period 10, highlighting the actual paving in the period (that is the roads paved in 1984 or 1985) with blue circles. These circled additions measure a total of 158 kilometers in length, giving us the paving quota for the period. The measure of overlap for period 8, *deposit-to-port bias* $_{10}$, compares the circled additions to the red network in Figure 10 (the deposit-to-port counterfactual as it appeared at the end of the period), and finds that only one of them (south of Freetown) has a section overlapping with the counterfactual red network. Dividing the length of this section by 158, we obtain *deposit-to-port bias* $_{10}$ = 0.09. This number means that only 9% of the roads that were paved in 1984 and 1985 would have been paved in that period or earlier, had the government only cared about connecting deposits to ports.

It should be noted that the absolute value of our measure of deposit-to-port bias has no clear meaning. Thus, all we are going to be focusing on is changes in this measure over time (and thus over different types of government) within each country.

One potential worry is that *deposit-to-port bias*$_{i,t}$ will be lower when a greater share of the counterfactual has already been paved, and this mechanical link may confound our results.[20] To rule this out, we define an additional variable, *cumulative deposit overlap*$_{i,t}$, which measures the share of the counterfactual that is actually paved at the start of period t. We will also control for this variable in our preferred specification.

3.3 Alternative dependent variable: ***city-to-city bias***$_{i,t}$

Another concern is that deposits and ports might be located close to cities, and *deposit-to-port bias*$_{i,t}$ might not be picking up a preference for connecting deposits to ports, but rather one for connecting cities to one another. To alleviate this concern, we conduct a falsification exercise in which we re-calculate our dependent variable so that it actually measures the preference for connecting cities to one another. To calculate this alternative dependent variable, we first construct a second counterfactual network expansion path, which aims to approximate the way in which paved roads would have expanded in 1965-2014, had the government only cared about connecting cities to one another. In constructing this counterfactual, we follow Burgess et al. (2015).[21] The idea of the counterfactual is that - unlike mineral deposits, which mostly need to be connected to a port for exporting purposes - cities need to be connected to one another for internal trade, as well as to a port for international trade. It is constructed ranking all possible city pairs in the country by market potential (the sum of population divided by distance), and then reallocate the paving quota starting from the top pairs in each period, using a procedure that is identical

[20] For example, if the deposit-to-port counterfactual is entirely paved at the start of period t, then *deposit-to-port bias*$_{i,t}$ must necessarily be zero.

[21] Like they do, we also include foreign cities close to a country's border (within a 50 km buffer). The government only paves roads that connect to these cities up to the border crossing.

to the one used for the deposit-to-port counterfactual. This procedure tends to connect cities to one another as well as to a port, given that in all our coastal countries except for Cote d'Ivoire, the capital city is a port city.[22] In the counterfactual network, the large size of the capital city attracts lots of connections from interior cities, thus implicitly recognising the importance of the international trade which flows through the capital.

Figures 9 and 11 show the city-to-city counterfactual alongside the deposit-to-port one, for Sierra Leone in 2014. Evidently, there is some overlap between the two, due to the fact the deposits in the East of the country are located quite close to cities. The city-to-city counterfactual also has somewhat of an interior-to-coast shape, due to the force of attraction of Free Town (which is also the country's main port). However the city-to-city counterfactual has less of an interior-to-coast shape than the deposit-to-port one, as it also connects cities in the North and contains two border crossings and several North-South connections.

To construct our alternative dependent variable (which we label $\textit{city-to-city bias}_{i,t}$) we proceed as explained in Section 3.2, but using the city-to-city counterfactual instead of the deposit-to-port counterfactual. The resulting measure approximates the extent to which paving decisions in country i and period t resembled those by a hypothetical planner who only cared about connecting cities to one another.

4 Data sources

We construct a panel covering 12 West African countries over 22 time periods between 1965 and 2014, although not all variables are available for the entire sample. The sample includes Benin, Burkina Faso, Côte d'Ivoire, Ghana, Guinea, Guinea Bissau, Liberia, Mali, Niger, Senegal, Sierra Leone and Togo. We do not include Nigeria. This is because we could only identify the year of discovery for 4 out of 22 deposits, and these were all discovered before 1965, leaving no variation within our sample period. Similarly, Gambia has one deposit with unknown discovery date and is thus also excluded.

We require data on autocracy, detailed road construction over time and space, the location, size and type of deposits, the location and size of ports and cities, the world prices of metals

[22] Dakar in Senegal, Bissau in Guinea Bissau, Conakry in Guinea, Freetown in Sierra Leone, Monrovia in Liberia, Accra in Ghana, Lome in Togo and Porto-Novo in Benin. All of these cities were already connected in 1965 (see Figure 1) and are large cities which are given an important role in the counterfactual expansion (for an illustration, see the example of Sierra Leone discussed below).

and minerals, foreign aid, FDI flows, periods of war, openness, a delineation of ethnic lands and a measure of the political influence of each ethnic group. We describe sources in detail in this section, and provide a table of summary statistics for the regression sample in Online Appendix Table OA1.

4.1 Autocracy

Our main measure of autocracy is equal to minus the polity score provided by the Polity IV Project. This is a widely used score which ranges from +10 (strongly democratic) to -10 (strongly autocratic). Consequently, our measure will range from +10 (strongly autocratic) to -10 (strongly democratic). This measure is available in all years in our sample, excluding a few in which there was either a collapse in government authority, or a period of transitions (Polity IV Project codes -77 and -88 respectively). Since the polity score measures political conditions on December 31 of each year, we measure autocracy at the start of period t as the polity score for the last year before period t.

Figure 3 shows that there is a lot of variation in the inverse polity score between countries and within countries. Indeed, for most of the countries in our sample, the within-country variation is similar in magnitude to that of Kenya, the country for which Burgess et al. (2015) find a strong effect of democratisation on the allocation of roads. Overall, most countries in our sample are relatively autocratic up to the 2000s, with some countries (e.g. Ghana, Burkina Faso and Sierra Leone) also experiencing periods of democracy shortly after decolonisation. In robustness checks, we also employ a dichotomous measure of autocracy equal to the inverse of the measure constructed by Acemoglu et al. (2019) by combining the Polity IV index with several other independent measures of democracy. This measure is available until 2012 and records 11 of our 12 countries as autocratic in 1965 and only 3 as autocratic by 2012. Our sample thus reveals the same unprecedented spread of democracy as highlighted in Acemoglu et al. (2019). The use of multiple sources for the dummy potentially reduces measurement error but may come at the cost of reduced variation because half of the countries in our sample only change status once.

4.2 Roads

We have digitised 23 Michelin West Africa maps covering the 12 West African countries listed at the start of this section covering the period 1965 to 2014.[23] This gives us 22 periods for each country, for a theoretical maximum of 264 observations.

The maps show colour- and pattern-coded roads, which we digitize using the ArcGIS platform. To raise the resolution of the maps to 1:30,000 we start with a world transport map from the ArcGIS public online database and track the status of roads forwards and backwards in time, and update the base map where necessary. To allow for network analysis (see further down), we also make sure that all road segments connect. One benefit, compared to a simpler gridded approach that would track whether a square grid contains paved roads or not, our more precise data tracks distance and geography better, such as rivers, border and river crossings, and ruggedness that increases curvature of roads.

Roads are classified along two dimensions: their regional importance in terms of major, secondary and local roads, and their quality. The road quality classification is broken down into six categories: "surfaced", "improved", "partially improved", "earth roads", "tracks", and "others". According to the Michelin legend, surfaced roads are paved with asphalt and/or concrete and are suitable for all-weather conditions and vehicles. In contrast, improved roads are unpaved even though they receive regular mechanical maintenance, and are only suitable for high speeds in certain sections. The other four types of worse-quality roads are also unpaved and not all-weather roads. In our analysis, we focus on the quality classification and use all roads (major, secondary and local) changing status between unpaved (equal to any of the five lower quality categories) and paved (equal to "surfaced" and thus all-weather roads). However, we use the location of all unpaved roads that exist as of 2014 as the location of segments of road that may or may not be paved by the government in any earlier year.

In Table 1 we track the distribution of roads in West Africa by quality, in 1965 and in 2014. In the table, the five categories of unpaved roads are grouped in the following way: Unpaved (medium quality) = "improved" and "partially improved"; Unpaved (lowest quality) = "earth

[23] The map editions are 1965, 1967, 1968, 1969, 1971, 1973, 1976, 1983, 1984, 1986, 1988, 1989, 1990, 1991, 1996, 1998, 2000, 2002, 2003, 2007, 2009, 2012 and 2014. Although we also have some earlier years, we start in 1965 because Michelin changed the legend of their maps in that year. While we do not know for certain that no map was issued for the intermittent years, the unequal timespan between years seems to suggest that a new map was only issued when enough changes to the road network were observed.

roads", "tracks" and "others". We see that roads are both upgrading and downgrading over time. For example, of the 10,665 kilometers of roads that were paved in 1965, 9,496 kilometers, or 89%, were still paved in 2014, while the remaining 11% deteriorated to unpaved status (possibly due to lack of maintenance and/or war). Of the latter, 187 kilometers deteriorated to the point of vanishing by 2014. Note that the paving of roads where no road was previously present is relatively rare: paving normally happens to existing roads or tracks.[24] Note also that compared to paved roads, unpaved roads are more prone to deterioration.

Figure 4 shows for each country the evolution of the stock of paved roads since 1965, expressed in kilometers per surface area. There is substantial heterogeneity, with some countries showing almost no activity (Liberia, Mali, Niger) and others making a lot of progress. However, in Online Appendix Tables OA5 and OA6 we show details of the distribution by country and period, which shows that some countries paved more than Figure 4 suggests even though it was low compared to their surface area. The stock of paved roads is in general increasing over time. For example, Côte d'Ivoire increased its stock almost six-fold from 900 kilometers to 5,200 km. Similarly, Senegal, Niger and Mali also paved many roads, while Ghana, Sierra Leone, Guinea-Bissau, and Liberia paved relatively few.

4.3 Deposits

Our deposit data is the combination of three sources: the proprietary data sets by MinEx Consulting and SNL Mining (formerly known as RMG), and the publicly available data from the United States Geological Survey (the Mineral Resources Data System or MRDS). Combined, these sources provide us with 391 records of West African deposits, for which we know the location, the year of discovery and the size of the deposit. We start from MinEx, which is a dataset of deposits and has the best coverage in terms of year of discovery.[25] We then update missing dates using MRDS and RMG, which are datasets of mines, the distinction being that a deposit is a concentrated occurrence of metal or mineral resources, while a mine is an industrial facility used to mine the resources. We interpret the year of discovery listed for the mine as the

[24] Note also that, of the 3,026 kilometers of roads which appear as paved in 2014 but do not appear at all in 1965, many (if not most) must have first come into existence as non-paved tracks or roads at some point between 1965 and 2014, before becoming paved roads by 2014.

[25] According to MinEx, "The discovery date refers to when the deposit was recognised as having significant value. This is usually set as the date of the first economic drill intersection".

year of discovery of the deposit in which the mine is located.[26] The size of deposits is classified by MinEx as supergiant, giant, major, moderate, and minor. We group giant and supergiant together because there is only one supergiant deposit, and construct four categories (giant=4, major=3, moderate=2, minor=1), which we use as weights in the ranking of which deposits to connect first.[27]

The database includes various kinds of deposits: bauxite, chromium, copper, gold, iron ore, manganese and others. In the baseline estimation we focus on all metals and minerals except diamond deposits, since diamonds are less likely to be transported via ports and bulk ships than to be airlifted. Three-quarters of deposits were discovered after 1965, almost half are at least of major size, and gold is most common.[28]

4.4 Ports

We use the 2016 World Port Index (WPI) to identify ports. It provides the location, characteristics, channel and cargo pier depth (in 12 categories), shipping facilities, and available services of ports in the World. According to Waters et al. (2000), Figure 4.46, dry bulk ships built from the 1950s onwards require at least 5m of draft. We therefore keep all ports (excluding marine terminals used for oil) with channel and cargo pier depth of at least 5m. This way we exclude many fishing ports that are unlikely to be used for metal or mineral exports. We then group the remaining nine categories of depth into four: a-d (the deepest ports, weight=4); e-g (the second deepest ports, weight=3); h-k (weight=2); l-n (weight=1). Deeper ports can accommodate larger ships with more draft, and will have priority to road connection in our counterfactual network. We include all deep water ports that exist in 2016, based on the logic that even if some of them did not exist in 1965, given the geography they could have been built where they are today.

[26]Per source, 242 records are from MinEx, 131 from RMG, and 18 from MRDS.

[27]The original delineation is not linear in the metal- and mineral-content of ore, which depends on ore grade and on the metal or mineral. For example, in MinEx a giant gold mine has > 6Moz Au and s major one > 1Moz Au (which differ by a factor six), but a giant iron ore mine contains > 1000Mt and a major one > 200Mt (which differ by a factor five). For our purpose, it is sufficient that the rank order from giant to moderate is preserved. A few deposits have other size measures which come from RMG and MRDS: for reference, we group one 'medium' mine into moderate and one 'small' mine into minor. Only a subset of deposits also list the size in terms of the estimated amount of metal- or mineral-containing ore.

[28]Online Appendix Table OA2 lists further details on the number of discoveries by country and period and the distribution of metals, minerals and size.

4.5 Cities

The data on cities was kindly shared by Hervé Gazel of the Africapolis project.[29] It contains the location and population of cities with at least 10,000 inhabitants in 2010, for 33 countries and for the years 1960, 1970, 1980, 1990, 2000 and 2010. We observe 822 cities within our sample. We use the 477 cities which exist in 1960 and use their population size to rank them.[30] We find that 1960 population explains 85% of the variation in 2010 population, and cities that exist in 1960 make up 88% of the population of cities that exist in 2010. New cities that were created after 1960 have on average only 18,000 inhabitants and never more than 96,000.[31]

4.6 Prices, aid, FDI, war and openness

We collected data for control variables on the world price of metal and mineral commodities, on aggregate inflows of foreign aid and foreign direct investment, on the occurrence of war, and on openness to trade. The data on the world price of 15 metal and mineral commodities comes from UNCTAD (downloaded June 14 2017), Plunkert & Jones (1999), and Bazzi & Blattman (2014). We constructed a country-specific, time-varying index in which each price is weighted by the share of deposits producing that metal or mineral in a given country in the middle of our sample period (1989).[32] The data on aid and FDI come from the World Bank Development Indicators. Average inflows of foreign aid and FDI in our sample is 173 and 105 million US$, respectively, with Ghana and Côte d'Ivoire typically receiving most aid and Ghana receiving most FDI. The data on war come from the Correlates of War (COW) Project. There was no war between our countries in our period, however we identify several civil wars in the Mano River Region, including Côte d'Ivoire (2002-04), Guinea (2000-01), Liberia (1989-90; 1992-95; 2002-03) and Sierra Leone (1991-00), in addition to Guinea-Bissau (1998-99). Finally, the data on openness comes from Wacziarg & Welch (2008), who constructed a dummy variable which is equal to zero if a country is "closed" to the world economy according to a set of criteria, and one if it is open. The dummy is available for all our countries up to the year 2005. For eight

[29] Université Jean Moulin Lyon 3. See: `http://www.afd.fr/lang/en/home/publications/travaux-de-recherche/archives-anciennes-collections/NotesetEtudes/Africapolis`

[30] Online Appendix Table OA3 shows further details on the population distribution of these towns.

[31] Spearman's rank correlation test clearly rejects independence of 1960 and 2010 towns, with $\rho = 0.598$

[32] In other words, the only time variation in the index comes from exogenous changes in prices. We construct the index in this way out of necessity, since we do not observe the exact size of each deposit, and thus cannot infer the way in which new discoveries will affect the overall value of the country's deposits.

countries, it is equal to zero until it turns one in a year between 1984 and 2001, and remains one afterwards. For the other four countries (Liberia, Niger, Senegal and Togo) it is zero throughout.

4.7 Ethnicity

For each deposit, we want to construct a measure of the extent to which the ethnic group on whose homeland the deposit is located is politically influential in each period. To this purpose, we combine data from Murdock's Ethnolinguistic Map of Africa (Murdock 1959) and from the Ethnic Power Relationship Core Dataset 2019 (Vogt et al. 2015; EPR from now on). The first dataset allows us to identify the ethnic group on whose homeland each deposit is located. The second dataset gives information on the political power of each ethnic group, in each year. More in detail, for each ethnic group that, in year t or in any earlier year, was politically relevant, the EPR dataset classifies the group's influence on the central government into five categories: "dominant", "senior partner", "junior partner", "powerless" and "discriminated".[33] A sixth category, "irrelevant", is reserved for groups that are not politically relevant in period t or in an earlier year, but will become so in a later year. Ethnic groups that were never politically relevant are not included. We matched ethnic groups in the two datasets, listing as irrelevant those groups which appear in the Ethnolinguistic Map, but not in the EPR dataset. We then create a dummy that is equal to one if the group's political status is dominant, senior partner, or junior partner, and zero if it is powerless, discriminated or irrelevant.[34] For each deposit, the dummy can switch on and off, as the group's influence can change over time. Since the EPR dataset measures political conditions on January 1 of each year, we measure a deposit's political influence at the start of period t with the dummy for the first year of that period.

5 Main results

We begin by providing some summary statistics on our dependent variable. Table 2 describes the distributions of paving and of $\textit{deposit-to-port bias}_{i,t}$ across country-periods in our sample, with country-by-country and period-by-period details provided in Online Appendix Tables OA5 and

[33]The dataset also has category "monopoly", which however does not apply to any ethnic group in our sample.

[34]The former three categories include, respectively, 4,566, 5,935 and 15,242 observations (deposit-year). The latter include 5,202, 775 and 11,668 observations. There are also 124 observations that are coded as "state collapse" in the EPR dataset, and for which we set the dummy equal to zero.

OA6.[35] Our full sample is made up of 12 countries which we observe for 22 periods, giving us 264 country-periods. However in country-periods in which no actual paving occurred, our dependent variable is zero by construction. To distinguish this kind of zeros from the those denoting a planner who paved roads that do not overlap with the deposit-to-port counterfactual, we drop zeros due to no actual paving from the sample. We are thus left with 174 country-periods, which is also the maximum number of observations in our regressions.[36]

In our full sample (top panel of Table 2), *deposit-to-port bias*$_{i,t}$ was on average equal to 0.17. That indicates that, in the average country-period, 17% of the kilometers that were newly paved in the period overlapped with the deposit-to-port counterfactual. The mean rises to 0.25 when we exclude periods with no paving (bottom panel). Note also that *deposit-to-port bias*$_{i,t}$ displays a substantial standard deviation and has maximum and minimum values of 1 and 0 (indicating that there are cases in which the new paving overlaps perfectly with the deposit-to-port counterfactual, and cases in which it does not overlap at all). This suggests that different governments in different country-periods took substantially different approaches to paving.

Online Appendix Table OA7 delves into the case of Sierra Leone. In 7 out of 22 periods, no actual paving occurred in Sierra Leone, necessarily leading to *deposit-to-port bias*$_{i,t} = 0$. Of the 15 periods with positive paving, 8 report a positive value for *deposit-to-port bias*$_{i,t}$. Importantly, there is substantial variation in *deposit-to-port bias*$_{i,t}$ over time, suggesting that different governments had different paving priorities. For example, for the government in charge at the beginning of the 1965-66 (a score 6 democracy according to the Polity IV index), *deposit-to-port bias*$_{i,t}$ is equal to 0.31, while for the government in charge at the beginning of 1976 period (a score -6 autocracy according to the Polity IV index) had a score of 0.66. This suggests that the latter government prioritised connecting deposits to ports (as opposed to any other pairs of locations) more than the former government.

[35] One potential worry is that in "elongated" countries such as Benin or Togo the newly paved roads may have to necessarily overlap with the deposit-to-port counterfactual, which by construction has an interior-to-coast shape. That is not the case, however. Online Appendix Table OA5 shows that while *deposit-to-port bias*$_{i,t}$ was large on average in Togo (0.43), it was low on average in Benin (0.14). More importantly, the standard deviation (not reported in the table) was high in both countries, at 0.46 and 0.30 respectively. This suggests that there were periods in which the newly paved roads overlapped a lot with the deposit-to-port counterfactual, but also periods in which they overlapped little.

[36] This restricted sample also excludes a few country-periods in which the Polity indicator is not available.

5.1 Baseline estimates

Table 3 builds up to our main estimate of the effect of autocracy on the bias towards connecting deposits to ports, as opposed to any other pairs of locations. In column (1) we simply regress deposit-to-port bias in period t on autocracy at the start of t, including country and period fixed effects and country-specific time-trends. The sample only includes country-periods when new paving took place, and we correct standard errors for possible heteroskedasticity.[37] As explained above, periods are here defined as the time between two consecutive Michelin maps of West Africa, which are the same for all our countries since they are all included in each map. We find a significant positive effect of autocracy on deposit-to-port bias. This suggests that autocracies are biased and tend to focus more on connecting deposits to ports (as opposed to any other pairs of locations), relative to democracies. The average autocracy level in our sample is 3.59 with a large standard deviation of 5.32. A one-standard deviation increase in autocracy predicts a 0.11 point (or a third of a standard deviation, c.f. Table 2) increase in $\textit{deposit-to-port bias}_{i,t}$.

In columns 2-4, we show that this effect is robust to adding additional controls. In column 2, we add three pre-determined, and thus plausibly exogenous variables. The cumulative deposit overlap variable, $\textit{cum. deposit overlap}_{i,t}$, measures the overlap of the entire stock of paved roads with the counterfactual network at the start of each period. It controls for the possibility that autocracies and democracies may have different histories in terms of road paving, which may mechanically affect their current deposit-to-port bias. Even though the coefficient is not significant, its sign suggests a process of mean reversion over time. A high score on $\textit{cum. deposit overlap}_{i,t}$ (indicating that much of the deposit-to-port counterfactual has already been paved) predicts a low value of $\textit{deposit-to-port bias}_{i,t}$ (indicating that little of the deposit-to-port counterfactual is additionally paved during the period). The stock of deposits at the start of period ($\textit{cum. deposit discoveries}_{i,t}$) and the number of discoveries in the previous period ($\textit{deposit discoveries}_{i,t-1}$) control for the possibility that countries with a greater mineral endowment may be more likely to be an autocracy, and at the same time pave more deposit-to-port roads. The coefficient on $\textit{deposit discoveries}_{i,t-1}$ seems to confirm the latter conjecture, however our coefficient of interest is not affected by adding these controls.

[37] Because the polity score is missing for some country-period, the number of observations drops from 188 to 174. We lose another 10 observations in columns 2, 3 and 5 due to $\textit{cumulative deposit overlap}_{i,t}$ not being defined in the first period, and 55 observations in column 4 due to limited availability of the additional controls there included.

Column 3, which we consider our baseline specification, adds a fourth plausibly exogenous variable. This is a country-specific, time-varying price index, capturing the current world prices of the minerals produced in a country. It is included for the same reasons why we have included the previous two variables: a greater value of a country's mineral endowment might simultaneously lead to more autocracy, and to more deposit-to-port roads being paved. However we do not find a significant effect of the index on deposit-to-port bias in paving, and our coefficient of interest is unchanged.

Column 4 includes additional controls. The amount of aid, FDI, and a dummy for the occurrence of Civil War, could be omitted variables in the previous specifications. It is possible that they are correlated with autocracies (both directions of causation seem possible), and have an independent effect on deposit-to-port bias. Results are not invalidated by including these variables. If anything, our coefficient becomes bigger and more significant. Because these additional controls might well be endogenous, and to include them reduces the sample size significantly, we decide to exclude them in our baseline regression.

Finally, in column 5 we cluster standard errors two-way by country and period to allow for serial correlation in the error term. This does not change our conclusion. Because the number of country and the number of period clusters is small, we favor specification 3 as our baseline. In Appendix Table OA4, we show that all our main results are robust to clustering standard errors in this way.

5.2 IV results

Our baseline analysis has adopted a number of strategies to rule out that the estimated relationship between autocracy and deposit-to-port bias might be driven by omitted factors. Because we control for country fixed effects, our results are not driven by structural features of a country (such as geography, comparative advantage or historical experiences) driving both political regimes and the type of roads that were paved. Similarly, because we control for period fixed effects, our results are not driven by important moments in history (such as the passing of time since decolonisation, or the end of the Cold War). We also control for evolving comparative advantage (at the regional level through the period effects, and at the country-level through the price index) and country-specific linear trends.

Nevertheless, one might still be worried that the effect of autocracy on deposit-to-port bias might not be causal, but driven by omitted factors. To further dispel this worry, we conduct an instrumental variable analysis in this section.

To identify an instrument - that is an exogenous variable that is correlated with autocracy, but not with a country's paving decisions - we follow the most recent literature on democracy and growth. Several recent papers have emphasised the fact that democratisation tends to spread across countries in the same region (Buera et al. 2011; Acemoglu et al. 2019). In particular, Acemoglu et al. (2019) instrument for democracy in country i and year t using average democracy in $t-1$ in the same World Bank region that i belongs to (but including only countries that had a similar political regime as i at the start of the sample), and including time fixed effects.[38]

In adapting this approach to our setting, we are constrained by the limited variation in our data. First, all our countries are in the same region, which pre-empts us from exploiting any time variation in a specification with time fixed effects. Second, only one of our countries was a democracy at the start of our sample, limiting our capacity to instrument for democracy using different regional averages for different countries. Given these limits, the variation left would be entirely cross sectional, and it would be largely mechanical given that average regional democracy is calculated by excluding the country under consideration.[39]

To solve this issue, we proceed in two ways. First, to exploit some of the time variation in the instrument, we drop the period effects in our IV regressions, and replace them with 5-year effects.[40] Because many such effects span over multiple periods (recall that the average period lengths is 2.23 years), this allows us to exploit the time variation in the instrument within the five-year periods. Importantly, to replace the period effects with 5-year effects does not make our results any stronger. This is shown in Table 4, where column 1 replicates the baseline, and column 2 replaces the period effects with 5-year effects. As a result, the coefficient on autocracy is almost unaffected. Second, to increase precision, we use as an instrument average autocracy

[38]Their regions are Africa, East Asia and the Pacific, Eastern Europe and Central Asia, Western Europe and other developed countries, Latin America and the Caribbean, the Middle East and the North of Africa, and South Asia.

[39]This generates a spurious, *negative* cross sectional correlation between own and average regional democracy, since countries with below-average own democracy increase the average when they are excluded, and countries with above-average democracy increase it.

[40]We create ten dummies for the years 1965-1969; 1970-1974; 1975-1979; etc. For each period, the dummy equals one when the year of the Michelin map at the end of the period falls within the 5-year period. For example, the second dummy is one only for the periods 1 Jan 1969 - 31 Dec 1970 and 1 Jan 1971 - 31 Dec 1972, and zero otherwise. This is because these are the only two periods for which the year of the map at the end of the period (respectively 1971 and 1973) falls within the 5-year period of the second dummy.

in West Africa, and not in Africa as a whole as in Acemoglu et al. (2019).[41] More precisely, our region is composed of our sample of countries and their direct neighbors.[42]

The first and second stage are presented in columns 4 and 5 of Table 4. The instrument is strong enough, with an F-test above 10: lagged average regional autocracy is a strong predictor of a country's own-autocracy.[43] Our central result, namely that autocracies results in a greater deposit-to-port bias in road paving, is confirmed. In fact, once we instrument for own autocracy using average regional autocracy, our coefficient becomes more than four times bigger. This is in line with results in column 4 of Table 3, where we showed that to include some potentially omitted variables had the effect of increasing the coefficient. All in all, results in this section support the view that autocracies had a causal impact on the type of roads being paved in West Africa over our sample period, making them more deposit-to-port (and thus interior-to-coast) in nature.

5.3 Mechanisms

Having established that autocracies had a causal impact on deposit-to-port bias in road paving, we now turn to consider mechanisms in Table 5.

In column 1, we add the amount of current paving to our baseline (column 3 of Table 3). This variable is included to rule out a mechanical channel through which autocracy might affect overlap with the deposit-to-port counterfactual (other than the intrinsic deposit-to-port bias of autocracies). If autocracies pave more kilometers than democracies,[44] and more paving mechanically leads to more overlap with the counterfactual, then our results might overestimate the deposit-to-port bias of autocracies. The coefficient on current paving is positive but insignificant, while the coefficient on autocracy is unchanged. This suggests that this mechanical channel is not important.

[41] We also experimented with including all African countries in the regional average, and with splitting our countries along the median democracy score in 1965 to construct two separate reference groups. These instruments were much weaker with F-test scores below 6 and thus not suitable.

[42] Direct neighbors to our sample of countries are Algeria, Cameroon, Cape Verde, Chad, Libya, Mauritania, and Nigeria.

[43] We do not add more than one lag of democracy, as Acemoglu et al. (2019) do, because adding a second or third lag weakens the joint significance of the instruments, while reducing sample size. The second stage in that case is still significant but 0.080** and 0.075**, respectively.

[44] Note however that in an (unreported) regression of current paving on autocracy (with country and period fixed effects and country-specific trends) we find a positive but marginally insignificant relationship (p-value: 0.17). These results differ from Jedwab et al. (2019), who in a broader sample of 43 sub-Saharan African in 1960-2015 find that democracy was positively correlated with investment in paved roads.

Column 2 adds the degree of openness to trade to our baseline. This is done to rule out an alternative explanation for the deposit-to-port bias of autocracies (other than their greater interest in appropriating resource rents). If autocracies are more in favor of an open economy, they might pave more deposit-to-port connections in order to exploit the country's comparative advantage. By controlling for a broad measure of openness, we can shut off this channel. Again, the channel does not appear to be important: if anything, to control for it makes the coefficient on autocracy bigger.

We now turn to our central hypothesis, namely that autocratic regimes have a greater deposit-to-port bias because they can realise greater private gains from exploiting the country's natural resources. According to the institutional view of development, politicians operating under extractive political institutions tend to adopt policies designed to benefit the ruling elite. In the case of the African countries, whose wealth typically lies in natural resources, such policies will typically focus on the embezzlement of resource rents. There is no obvious reason why infrastructure policy should be an exception. Faced with a choice between a road investment that would benefit the economy at large, and one that would maximise the country's resource rents, a government operating under extractive political institutions will tend to favor the latter over the former. This provides a possible explanation for the estimated positive relationship between autocracy and deposit-to-port bias.

To attempt to prove or disprove this hypothesis, we can take advantage of the fact that African elites have traditionally been defined along ethnic lines. For example, Padró i Miquel (2007) has constructed a theory of how autocrats in ethnically divided societies can survive for a long time while extracting large rents, by offering patronage to their own ethnic group and excluding the others. Hodler & Raschky (2014) and Burgess et al. (2015) both provide evidence (respectively for a large sample of countries and within Kenya) that regions that were ethnically linked to the current leader received greater benefits, and this was more true under autocracies than under democracies. Under the proposed hypothesis, our results should have an ethnic dimension, too. In particular, we might expect that autocracies should have a particularly strong deposit-to-port bias when the deposit are located on the elite's ethnic homeland, since the resource rents will be easier to embezzle in this case.

To further study the geography of our effect in terms of ethnic homelands, we construct a new variable which captures the extent to which the deposit-to-port counterfactual is located on the elite's ethnic homeland. We begin by counting the total number of yet unconnected deposits located on the deposit-to-port counterfactual at the start of period t. These are the deposits that are not yet connected by a paved road at the beginning of t, but should be connected according to the deposit-to-port counterfactual. The variable $ethnic\ deposit_{i,t}$ measures the weighted (by deposit size) share of these deposits that are located on the ethnic homeland of the ruling elite at the start of period t.[45] As explained in section 4.7, a deposit is coded to be on the elite's homeland if the group on whose homeland it is located is classified by the Ethnic Power Relationship (EPR) dataset as being "dominant", "senior partner" or "junior partner" in the national government, while it is not coded as such if the group is "powerless", "discriminated" or "irrelevant".

In column 3 of Table 5 we add $ethnic\ deposit_{i,t}$ to our baseline regression. While the coefficient is positive and significant, the coefficient on autocracy is very similar to the baseline. This suggests that even after controlling for the extent to which the possible deposit-to-port connections are located on the elite's homeland, autocracies still tend to pave more deposit-to-port connections than democracies. Evidently, our results are not spuriously driven by the fact that autocracies like to give roads to their ethnic homeland more than democracies (as shown by Burgess et al. 2015) and deposits happen to be located on those homelands.

In column 4, we interact autocracy with $ethnic\ deposit_{i,t}$. Now, the coefficient on autocracy turns insignificant, while the one on the interaction term is positive and significant. Moreover, the sum of the two coefficients is estimated to be positive and significant at mean and median values of $ethnic\ deposit_{i,t}$, as shown at the bottom of the table.[46] This suggests that autocracies did not have a deposit-to-port bias when none of the deposits were located on their ethnic homelands, however they did have one when a sufficiently high share of deposits were. This is *prima facie* evidence in support of the hypothesis that autocracies built more deposit-to-port roads than democracies, because they could enrich themselves more using such roads.

One final concern is that autocracies may appear to connect deposits, not because they had a deposit-to-port bias, but because deposits happened to be located closer to other geographical units that autocracies want to connect. The most obvious candidate is cities. To the extent that deposits tend to be located close to cities (either because exploration is easier, or because cities

[45] Results are robust to using the simple share.

[46] The minimum value of $ethnic\ deposit_{i,t}$ for which autocracies are estimated to have a deposit-to-port bias (with 95% confidence) is 0.25. About 56% of our observations have a value of $ethnic\ deposit_{i,t}$ above this threshold.

are born in the proximity of deposits), then our results may partly driven by autocracies wanting to connect cities to one another more than democracies. To test for this hypothesis, we replace out dependent variable with *city-to-city* $bias_{i,t}$, the alternative dependent variable described in Section 3.3. Such variable approximates the extent to which paving decisions in country i and period t resembled those by a hypothetical planner who only cared about connecting cities to one another.

Results are presented in columns 5 and 6 of Table 5. For symmetry with our baseline regression, we also include a variable named *cumulative city overlap*, which measures the share of the city-to-city counterfactual that is actually paved at the start of period t. If our results were driven by a bias of autocracies towards connecting cities, then we should expect the coefficient on autocracy to be positive and highly significant in column 5. Instead, we find a positive but insignificant coefficient, at least at standard levels of confidence. A similar picture emerges when we include our two ethnic variables in column 6, which are also not significant.[47] All in all, these results alleviate the concern that our main results may be driven by the proximity of deposits to other geographical units that autocracies might want to connect.

6 Robustness

We now investigate whether our main finding holds up to a few important robustness checks. First, we follow Acemoglu et al. (2019) and replace the inverse of the Polity IV score that is defined between -10 and 10, by their dummy value of autocracy equal to one if a country is non-democratic overall and zero otherwise. More precisely, the dummy considers a country as democratic if during a given year after 1972 Freedom House codes it as "Free" or "Partially Free", and Polity IV assigns it a positive score. If one of these two sources is not available, then the dummy is constructed using a variety of additional sources. Figure 3 shows the difference between the Polity IV score and the dummy. In our sample the dummy attains a value of 1 whenever the Polity IV score is below zero (except for the years 1968-1969 in Sierra Leone).

In Table 6, we repeat the baseline result in column 1 for comparison and replace the policy measure of autocracy with the dummy version in column 2. To compare magnitudes, we can use

[47]The *ethnic* $cities_{i,t}$ variable is constructed by analogy with *ethnic* $deposits_{i,t}$: it measures the weighted (by population size) share of yet unconnected cities located on the city-to-city counterfactual that are located on the ethnic homeland of the ruling elite at the start of period t.

the difference between the average inverse polity score among democratic countries (which is -6) and the average autocracy score among autocratic countries (which is +6). We thus find that moving 12 units on the inverse polity score towards autocracy increases deposit-to-port bias by 0.26, while we find that the Acemoglu et al. dummy suggests that becoming autocratic increases deposit-to-port bias by a very similar 0.28.

In columns 3 to 8 we introduce different definitions of the counterfactual network that connects deposits to ports.[48] First, in column 3, we include deposits that contain diamonds, even though our prior is that these have such a high ratio of value to weight that they would not be transported via roads and bulk ships. Only 6% of deposits in our sample contain diamonds and including them does not change our results.

In columns 4 and 5 we introduce deterioration of roads explicitly. We observe that roads can also change status from paved to unpaved. On average, 43km of roads deteriorate to unpaved status each year and country, even though the paving average is 125km (see Table 2). For some countries civil wars may be to blame, such as in Sierra Leone between 1991 and 2002, which coincides with a peak in deterioration, but it is also possible that natural disasters and lack of budget for maintenance are the underlying cause. While we observe the status change for every kilometer of road, our data does not allow us to distinguish between the possible reasons for the change directly, although we do control for civil wars. In the case of a limited budget for road paving and maintenance, one could envisage that changing circumstances that feed into a government's decision to allocate that budget to paving and/or maintenance, may lead to governments favoring the paving of some roads while simultaneously cutting back on maintenance of other existing roads. Even in years when no paving takes place and roads only deteriorate, the government will still have to decide which roads to maintain and which to let deteriorate. Table OA7 suggests indeed that paving and deterioration can happen simultaneously but also that some years see only deterioration.

We incorporate deterioration into our counterfactual paving program by first creating two budgets: a 'paving budget' based on observed paving (as in the baseline), and a 'deterioration budget' based on observed deterioration, which captures the idea that governments can willingly cut back on maintenance or at least fail to implement repairs between years for which we observe road maps. Using these budgets, we apply the paving budget to the top-ranked connections with

[48]Summary statistics are provided in Table 2.

unpaved road segments (as in the baseline), and apply the deterioration budget to the *lowest* ranked connections with *paved* road segments.

Moreover, we create two versions and distinguish never-paved roads from previously-paved roads. In the first version, repaving a road that was paved in the past but deteriorated over time costs only half of the budget of paving a road for the first time (labeled '0.5 $*$ *paving*' in column 4). More specifically, we allocate the paving budget in the counterfactual to unpaved roads, where paving 2 kilometers of unpaved road costs only 1 kilometer of budget if those roads had paved status in the past but do not have that status today, while it still costs 2 kilometers of budget if those roads have never been paved before. In the second version, repaving costs the same as paving a road for the first time (labeled '1.0 $*$ *paving*' in column 5). Since we argued using road construction data that repaving roads is not cheaper than paving roads for the first time, the second version follows our preferred baseline specification in terms of how repaving is treated.

The results show that our baseline estimate is very robust to these two much more elaborate definitions of the counterfactual.

In column 6 we double the weight of the port that is in or near a country's largest coastal city, or, in the case of landlocked countries, the port of the nearest largest coastal city in the transit country. In practice, this does not materially change the ranking of deposit-port pairs and thus yields the same estimate of autocracy on deposit-to-port bias.

Finally, in columns 7 and 8 we change the ranking of deposit-to-port connections by ranking only based on the distance between them in column 7, or only by the sum of their size in column 8.

7 Conclusions

In this paper, we have used a newly-constructed dataset to establish a new fact about the way in which the West African road networks have expanded since independence. In a sample of twelve countries and spanning almost five decades, more autocratic governments have displayed a stronger preference for paving interior-to-coast roads, and particularly deposit-to-port roads. We also found evidence that autocracies only had a deposit-to-port bias when the deposits were located on the elite's ethnic homeland.

One possible interpretation of this finding is that autocracies attach a greater weight than democracies to rent extraction, which has led them to bias their paving decisions in favor of the extractive industries and against economic development more in general. For example, weaker electoral concerns may make it less required of autocracies to seek consensus through a balanced road investment program, while at the same time weaker media and civil societies control may make it easier for them to hide a biased program. This interpretation is consistent with earlier results by Burgess et al. (2015) on Kenya, showing that democracies reduced ethnic favoritism in the allocation of roads. It is also consistent with the institutional view of economic development, according to which it is only inclusive political institutions that will deliver the market-supporting policies required to generate sustained economic growth.

In this interpretation, the notorious "interior-to-coast" nature of the African road networks is not (or not only) the natural result of geography or the natural choice of countries whose comparative advantage lies in exporting natural resources, but also the result of political distortions. The immediate policy implication is that, as advocated by many, the African countries should indeed focus on re-balancing their networks: this seems important to know, particularly at a time in which China's investments in Africa are, if anything, helping to exacerbate the traditional interior-to-coast pattern.

In order to corroborate this interpretation, one would need to construct and estimate a quantitative model of road building, trade and growth in one or more African countries, which one could then use to make counterfactual welfare calculations for a case in which the country had been a democracy ever since independence. We leave this for future research.

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Figures

Figure 1: Paved roads (black) and all other roads (green) in 1965

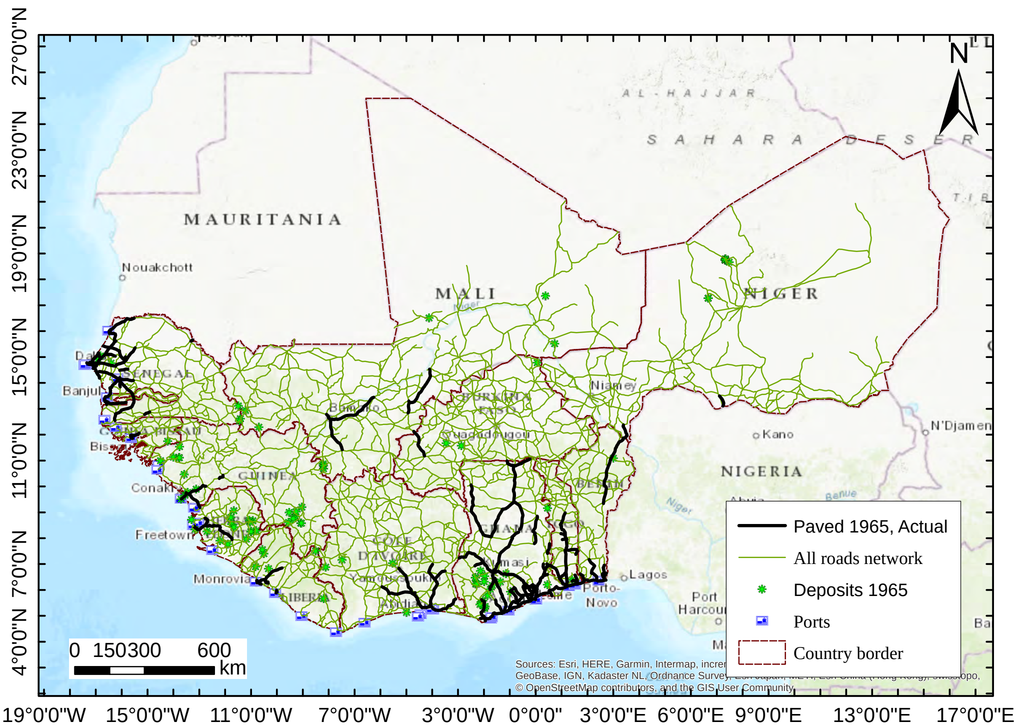
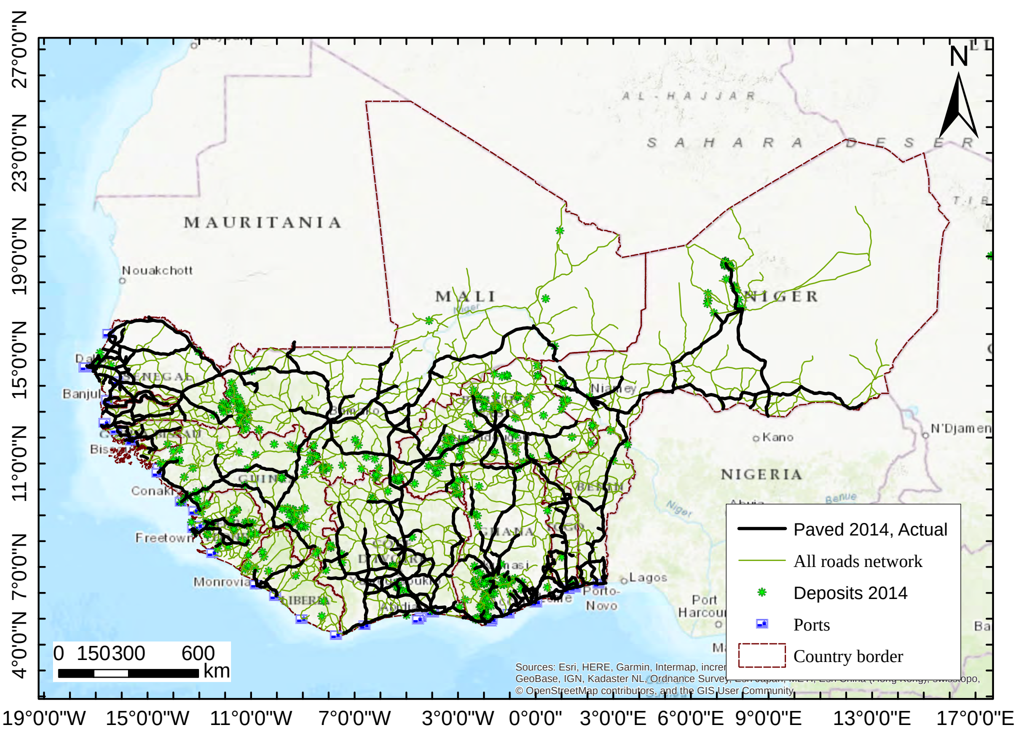


Figure 2: Paved roads (black) and all other roads (green) in 2014



Source: Michelin road maps of West Africa. See Section 4.2 for a full discussion of sources.

Figure 3: Autocracy scores

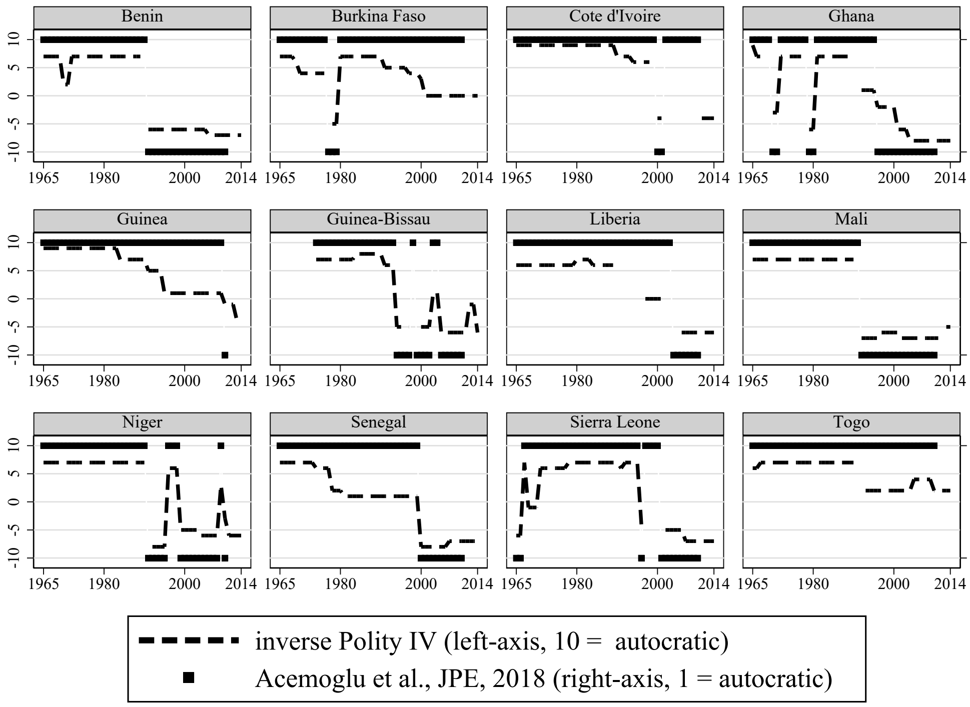
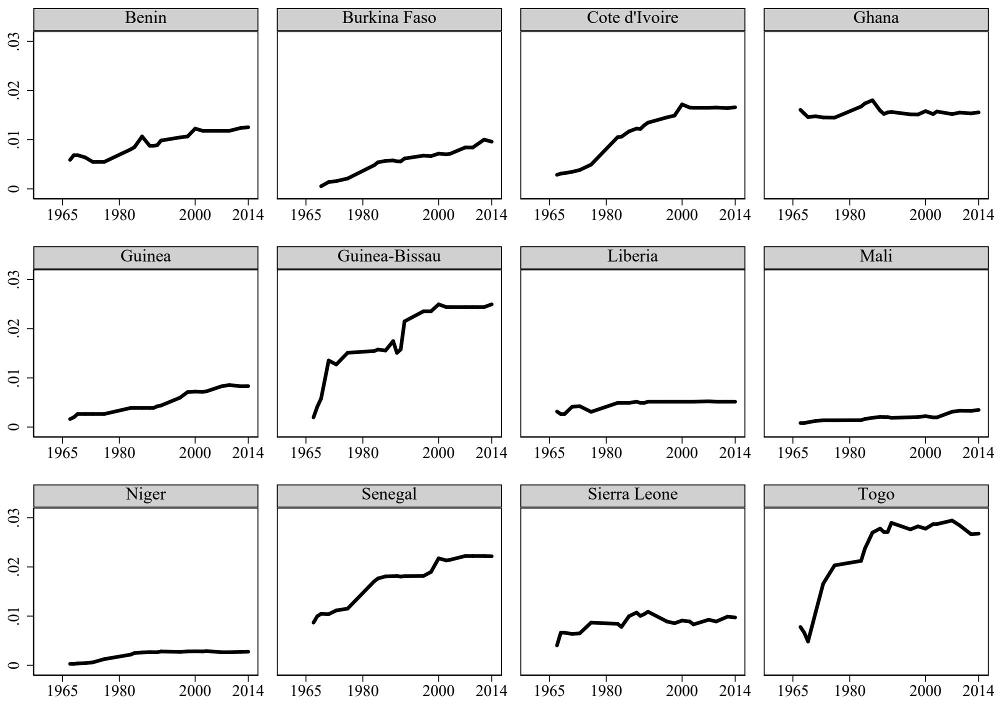


Figure 4: Cumulative paving since 1965, in km per square km of land area



Note: Paved roads as appearing in subsequent Michelin road maps of West Africa. For a full discussion of sources, see Section 4.2.

Figure 5: Actual network in 2014

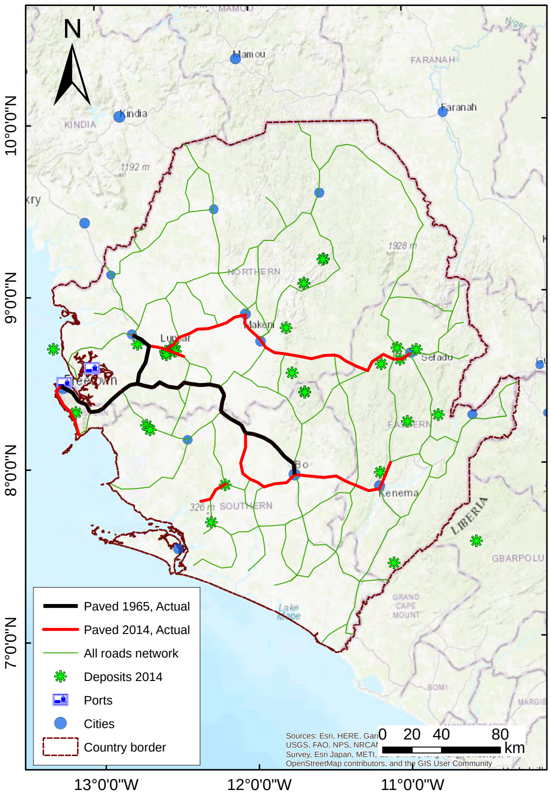


Figure 6: Deposit-to-port CF network in 2014

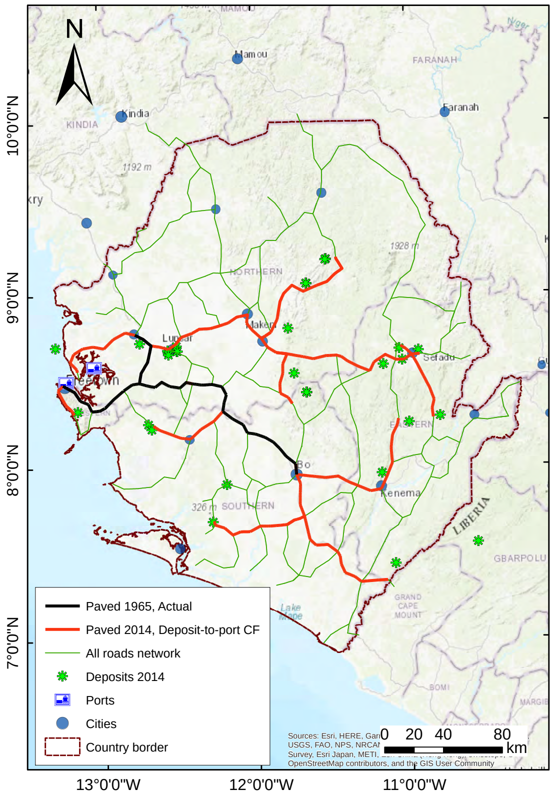


Figure 7: Overlap diagram

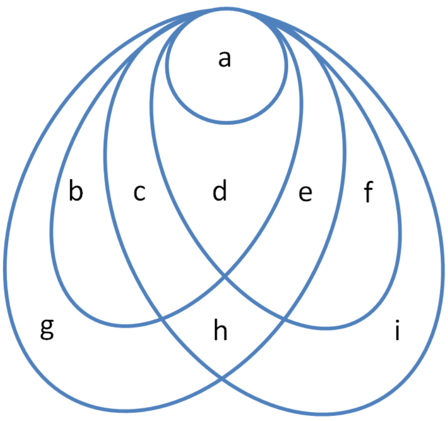


Figure 8: Actual network in 1986

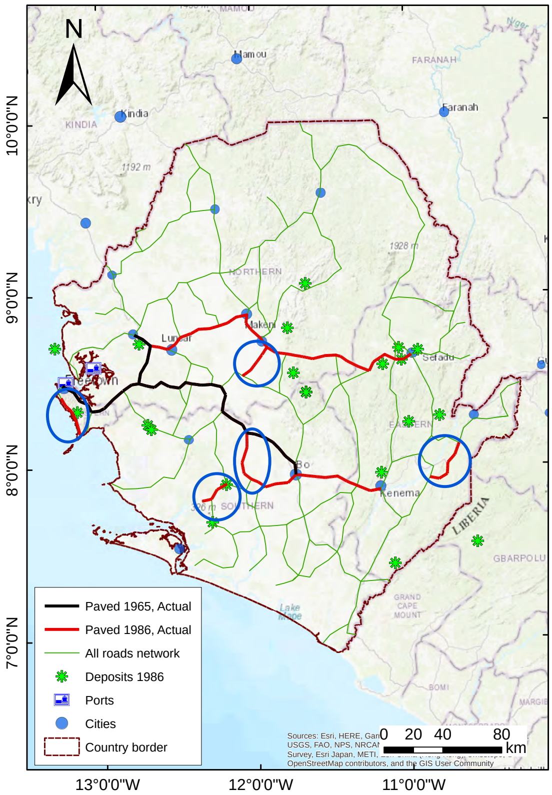


Figure 9: City-to-city CF network in 2014

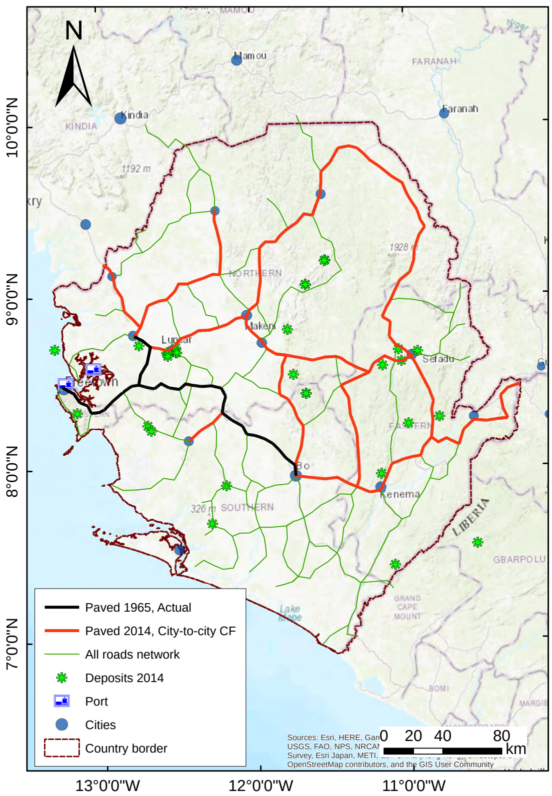


Figure 10: Deposit-to-port CF network in 1986

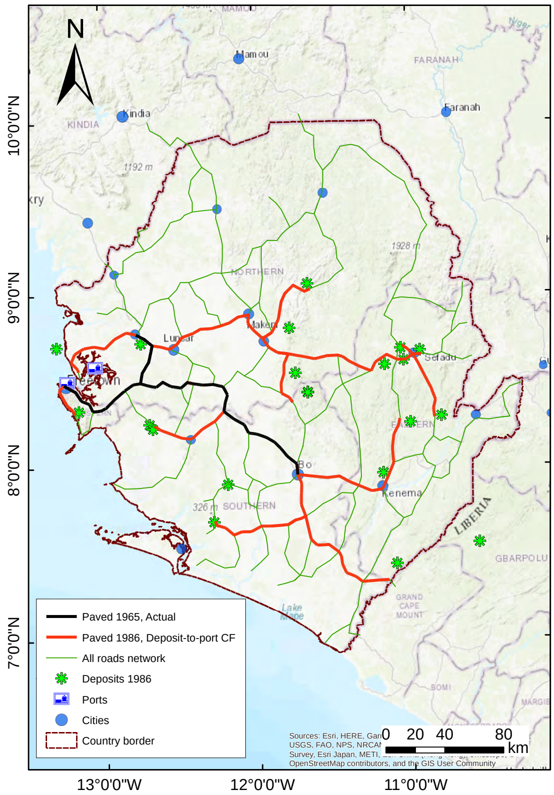
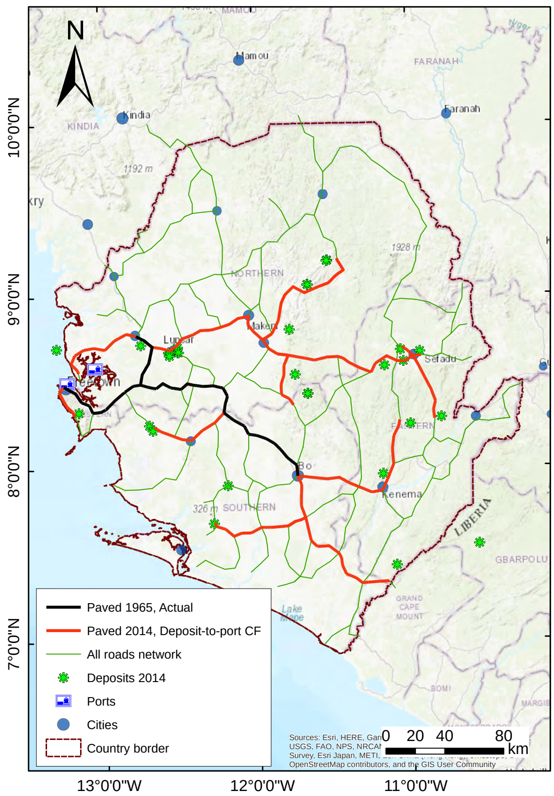


Figure 11: Deposit-to-port CF network in 2014



Tables

Table 1: Road quality changes between 1965 and 2014 in km

Status in 1965			Status by 2014			
			No road	Unpaved (lowest quality)	Unpaved (medium quality)	Paved (good quality)
No road	21,241	→	2,096	7,668	10,547	3,026
Unpaved (lowest quality)	27,729	→	*1,482*	15,812	7,607	4,310
Unpaved (medium quality)	57,423	→	*1,679*	*8,697*	30,620	18,124
Paved (good quality)	10,665	→	*187*	*235*	*934*	9,496

Note: The table lists changes in road quality across 13 West African countries as inferred from comparing the digitised Michelin maps of West Africa for 1965 and 2014. The three quality categories used in the table match to the six quality categories used by Michelin in the following way: Paved (good quality) = "surfaced"; Unpaved (medium quality) = "improved" and "partially improved"; Unpaved (lowest quality) = "earth roads", "tracks" and "others". "No road" are sections where a road is observed in at least one year after 1965. For example, there are 2,096 km which were recorded as track or roads at some point between 1965 and 2014, but these no longer exist by 2014. Only paved (surfaced) roads can be used year-round. Road quality deteriorations are denoted in italics. For example, 187 km of paved roads disappeared by 2014, and 235 deteriorated to the "unpaved (track)" quality. For sources, see Section 4.2.

Table 2: Road paving characteristics

All country-periods	N	mean	s.d.	min	max
Current paving (km)	264	124.82	208.59	0.00	1794.12
Current deterioration (km)	264	-43.20	80.41	-622.60	0.00
Deposit-to-port bias	264	0.17	0.30	0.00	1.00
City-to-city bias	264	0.32	0.37	0.00	1.00
Country-periods with new paving	**N**	**mean**	**s.d.**	**min**	**max**
Current paving (km)	174	185.00	234.66	6.29	1794.12
Current deterioration (km)	174	-47.78	89.15	-622.60	0.00
Deposit-to-port bias	174	0.25	0.34	0.00	1.00
City-to-city bias	174	0.46	0.37	0.00	1.00

Note: This table shows summary statistics for the main variable of interest for a sample of country-years in which new paving occurs (bottom panel) and all country-years (top panel).

Table 3: Deposit-to-port bias increases in autocracy

Dependent variable →	*Deposit-to-port bias*				
	(1)	(2)	(3)	(4)	(5)
Autocracy, start of t	0.021**	0.022**	0.022**	0.029**	0.022**
	(0.009)	(0.010)	(0.010)	(0.013)	(0.010)
Cum. deposit overlap, start of t		-0.190	-0.155	-0.404	-0.155
		(0.332)	(0.330)	(0.448)	(0.428)
Cum. deposit discoveries, start of t		-0.004	-0.005	-0.003	-0.005
		(0.007)	(0.007)	(0.010)	(0.006)
Deposit discoveries, t-1		0.023*	0.022	0.018	0.022
		(0.014)	(0.014)	(0.014)	(0.014)
Price index, average t			-0.000	-0.000	-0.000**
			(0.000)	(0.000)	(0.000)
Log aid, average t				-0.005	
				(0.126)	
Log FDI, average t				-0.018	
				(0.043)	
Civil war, average t				0.237	
				(0.153)	
Country FE	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes
Country-specific trends	Yes	Yes	Yes	Yes	Yes
Clustering on country and period	No	No	No	No	Yes
Observations	174	164	164	137	164
R-squared	0.431	0.439	0.447	0.500	0.447

Note: This table shows OLS regressions of the effect of autocracy on the degree to which countries' actual paving overlaps with the counterfactual network that connects deposits to ports. All specifications include country and period fixed effects and country-specific linear trends. Robust standard errors in parenthesis: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Online Appendix Table OA1 contains summary statistics. See Section 4 for variable definitions and sources.

Table 4: Instrumenting for autocracy

Dependent variable →	Deposit-to-port bias	Deposit-to-port bias	Autocracy	Deposit-to-port bias
	(1)	(2)	(3)	(4)
Autocracy, start of t	0.022**	0.026***		0.094**
	(0.010)	(0.010)		(0.039)
Cum. deposit overlap, start of t	-0.155	-0.242	8.426***	-0.714
	(0.330)	(0.342)	(2.533)	(0.480)
Cum. deposit discoveries, start of t	-0.005	-0.004	-0.132**	0.004
	(0.007)	(0.008)	(0.056)	(0.009)
Deposit discoveries, t-1	0.022	0.016	0.084	0.015
	(0.014)	(0.014)	(0.122)	(0.015)
Price index, average t	-0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Lagged regional average of autocracy			1.035***	
			(0.351)	
Observations	164	164	164	164
R-squared	0.447	0.395		-0.245
Country FE	Yes	Yes	Yes	Yes
Period FE	Yes	No	No	No
Country-specific trends	Yes	Yes	Yes	Yes
Cluster	No	No	No	No
5-Year FE		Yes	Yes	Yes
1st stage F-test				10.878

Note: This table shows OLS (in columns 1 and 2) and IV regressions (in columns 3 and 4) of the effect of autocracy on the degree to which countries' actual paving overlaps with the counterfactual network that connects deposits to ports. Column (3) is the first stage counterpart of column 4. Robust standard errors in parenthesis: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Online Appendix Table OA1 contains summary statistics. See Section 4 for variable definitions and sources.

Table 5: Mechanisms

Dependent variable →	Deposit-to-port bias				City-to-city bias	
	(1)	(2)	(3)	(4)	(5)	(6)
Autocracy, start of t	0.022**	0.031***	0.031***	0.012	0.016	0.031
	(0.010)	(0.011)	(0.009)	(0.012)	(0.014)	(0.020)
Cum. deposit overlap, start of t	-0.201	-0.331	-0.176	-0.123	0.245	0.338
	(0.340)	(0.364)	(0.310)	(0.462)	(0.469)	(0.487)
Cum. deposit discoveries, start of t	-0.005	-0.004	-0.005	-0.003	-0.002	-0.001
	(0.007)	(0.009)	(0.007)	(0.006)	(0.009)	(0.009)
Deposit discoveries, t-1	0.023*	0.026*	0.022*	0.023	0.037**	0.036**
	(0.014)	(0.014)	(0.012)	(0.017)	(0.017)	(0.016)
Price index, average t	-0.000	-0.000**	-0.000**	-0.000***	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Current paving, t	0.000					
	(0.000)					
Openness to trade, average t		-0.065				
		(0.148)				
Ethnic deposits, t-1			0.536***	0.396**		
			(0.163)	(0.146)		
Ethnic deposits * Autocracy				0.028*		
				(0.015)		
Cum. city overlap, start of t					-1.104**	-1.169**
					(0.448)	(0.461)
Ethnic cities, t-1						0.322
						(0.239)
Ethnic deposits * Autocracy						-0.019
						(0.020)
Observations	164	146	164	164	164	164
R-squared	0.450	0.465	0.506	0.518	0.488	0.498
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific trends	Yes	Yes	Yes	Yes	Yes	Yes
Marginal effects of autocracy for						
Median ethnicity				0.022***		0.022
				(0.010)		(0.014)
Mean ethnicity				0.025**		0.022
				(0.009)		(0.014)
75% percentile ethnicity				0.039***		0.013
				(0.012)		(0.014)

Note: This table shows OLS regressions of the effect of autocracy on the degree to which countries' actual paving overlaps with the counterfactual network that connects deposits to ports (columns 1-4), or with the counterfactual network which connects cities to cities (columns 5-6). All specifications include country and period fixed effects, and country-specific linear trends. Robust standard errors in parenthesis: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Online Appendix Table OA1 contains summary statistics. See Section 4 for variable definitions and sources.

Table 6: Robustness: Acemoglu et al. definition of autocracy and changing counterfactuals

Dependent variable →	Deposit-to-port bias							
Counterfactual includes →			Diamonds	Deterioration of roads, with repaving costs equal to: 0.5 * paving	1.0 * paving	Main ports double weight	Ranked by distance only	Ranked by size only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Autocracy, start of t	0.022**		0.022**	0.023**	0.020**	0.022**	0.027***	0.025**
	(0.010)		(0.010)	(0.009)	(0.008)	(0.010)	(0.010)	(0.010)
Autocracy, start of t (Acemoglu)		0.278***						
		(0.100)						
Cum. deposit overlap, start of t	-0.155	-0.065	-0.633*	-0.325	-0.107	-0.155	-0.316	-0.381
	(0.330)	(0.320)	(0.353)	(0.354)	(0.372)	(0.330)	(0.303)	(0.294)
Cum. deposit discoveries, start of t	-0.005	0.002	-0.002	-0.005	-0.005	-0.005	0.000	-0.003
	(0.007)	(0.008)	(0.008)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
Deposit discoveries, t-1	0.022	0.041***	0.022	0.022*	0.018*	0.022	0.018	0.022
	(0.014)	(0.015)	(0.013)	(0.011)	(0.010)	(0.014)	(0.013)	(0.014)
Price index, average t	-0.000	-0.000	-0.000*	-0.000	-0.000	-0.000	-0.000	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country-specific trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	164	162	164	164	164	164	165	165
R-squared	0.447	0.463	0.444	0.494	0.493	0.447	0.465	0.449

Note: This table shows OLS regressions of the effect of autocracy on the degree to which countries' actual paving overlaps with the counterfactual network that connects deposits to ports. The cumulative deposit overlap variable also adopts the different counterfactual definitions across columns. All specifications include country and period fixed effects and country-specific linear trends. Robust standard errors in parenthesis: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Online Appendix Table OA1 contains summary statistics. See Section 4 for variable definitions and sources.

Online Appendix

Priority Roads: the Political Economy of Africa's Interior-to-Coast Roads

Roberto Bonfatti, Yuan Gu, and Steven Poelhekke

October 2, 2020

Table OA1: Summary statistics

Variable	N	mean	s.d.	min	max
Deposit-to-port bias	174	0.25	0.34	0.00	1.00
Deposit-to-port bias, with diamonds	174	0.26	0.34	0.00	1.00
Deposit-to-port bias, allowing for deterioration (0.5)	174	0.23	0.32	0.00	1.00
Deposit-to-port bias, allowing for deterioration (1.0)	174	0.22	0.31	0.00	1.00
Deposit-to-port bias, main ports double weight	174	0.25	0.34	0.00	1.00
City-to-city bias	174	0.46	0.37	0.00	1.00
Cumulative deposit overlap	174	0.52	0.21	0.16	0.93
Cumulative city overlap	174	0.74	0.16	0.32	0.97
Autocracy, start of t	174	3.59	5.32	-8.00	9.00
Autocracy, start of t (Acemoglu)	164	-0.15	0.36	-1.00	0.00
Cum. deposit discoveries, start of t	174	15.99	16.61	1.00	81.00
Deposit discoveries, t-1	174	1.63	3.40	0.00	22.00
Price index, average t	174	6.0e+05	1.4e+06	11.12	6.3e+06
Log aid, average t	174	18.52	1.24	15.36	21.17
Log FDI, average t	137	2.96	2.12	-4.61	8.09
Civil war, average t	174	0.03	0.16	0.00	1.00
Openness to trade, average t	156	0.26	0.43	0.00	1.00
Ethnicity of deposits, count, scaled	174	0.46	0.42	0.00	1.00
Ethnicity of deposits, size, scaled	174	0.44	0.42	0.00	1.00
Ethnicity of cities, count, scaled	174	0.49	0.43	0.00	1.00
Lagged regional average of autocracy	165	3.79	3.10	-2.83	6.59

Note: This table provides summary statistics for all variables used in the analysis. See Section 4 for variable definitions and sources.

Table OA2: Discoveries of deposits and their characteristics

Country	Discoveries		Metal/mineral		Size	
	before 1965	after 1965				
Benin	1	2	Gold	261	Super Giant	1
Burkina Faso	3	85	Iron ore	34	Giant	38
Côte d'Ivoire	4	20	Uranium	29	Major	120
Ghana	21	54	Diamonds	23	Moderate	167
Guinea	16	34	Bauxite	22	Minor	70
Guinea-Bissau	1	0	Nickel	7		
Liberia	5	5	Manganese	6		
Mali	8	48	Phosphate	6		
Niger	9	27	Titanium, Zirconium	3		
Senegal	3	14	Silver	1		
Sierra Leone	15	19	Chromium	1		
Togo	1	1	Copper	1		
			Platinum	1		
			Zinc	1		
Total	87	309	Total	396	Total	396

Note: The table describes the discoveries of deposits in 12 West African countries through 2014, the distribution of the main metal or mineral contained in them, and the distribution of deposit size. For sources, see Section 4.3.

Table OA3: Distribution of cities

Country	Cities		Year	Population			
	in 1960	in 2010		mean	s.d.	min	max
Benin	24	81	1960	10,227	27,761	126	337,800
Burkina Faso	18	82	1970	18,149	55,951	249	624,091
Côte d'Ivoire	115	166	1980	29,878	98,508	1,067	1,251,272
Ghana	141	173	1990	45,168	157,047	1,560	2,107,460
Guinea	24	39	2000	64,498	230,042	5,610	3,043,326
Guinea-Bissau	2	6	2010	94,285	347,402	10,012	3,966,553
Liberia	5	23					
Mali	27	71					
Niger	25	51					
Senegal	31	57					
Sierra Leone	14	19					
Togo	51	54					
Total	477	822					

Note: This table shows the distribution of cities and population across countries and time. Between 2010 and 1960, 345 new cities have formed that have a population of at least 10,000 in 2010. For the years before these new cities existed, the population statistics in the right columns treat their population as missing. We use the location and population of cities in 1960 to generate the counterfactual paving schedules. Source: Africapolis project.

Table OA4: Clustering on country and period for main specifications

Dependent variable →	Deposit-to-port bias					City-to-city bias
	(1)	(2)	(3)	(4)	(5)	(6)
Autocracy, start of t	0.022**	0.022**	0.031**	0.094***	0.012	0.031+
	(0.010)	(0.009)	(0.012)	(0.010)	(0.011)	(0.020)
Current paving, t		0.000				
		(0.000)				
Openness to trade, average t			-0.065			
			(0.107)			
Ethnicity of deposits, size, scaled, start of t					0.396**	
					(0.137)	
Ethnicity * Autocracy, start of t					0.028**	
					(0.012)	
Ethnicity of cities, size, scaled, start of t						0.322
						(0.239)
Ethnicity * Autocracy, start of t						-0.019
						(0.020)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	No	Yes	Yes
5-Year FE	No	No	No	Yes	No	No
Country-specific trends	Yes	Yes	Yes	Yes	Yes	Yes
Cluster by Country and Period	Yes	Yes	Yes	Yes	Yes	Yes
1st stage F-test				19.677		
Observations	164	164	146	164	164	164
R-squared	0.447	0.450	0.465	-0.245	0.518	0.498
Marginal effects of autocracy for						
Median ethnicity					0.023**	0.023
					(0.008)	(0.014)
Mean ethnicity					0.025***	0.021
					(0.008)	(0.014)
75% percentile ethnicity					0.039***	0.014
					(0.009)	(0.014)

Note: This table repeats the main specifications in the paper by clustering standard errors by country and period. Robust standard errors in parenthesis: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. Online Appendix Table OA1 contains summary statistics. See Section 4 for variable definitions and sources.

Table OA5: Road paving characteristics by country, averages across periods

Counterfactual includes → / Country ↓	Current paving	Current deterioration	Deposit-to-port bias				
				Diamonds	Deterioration of roads, repaving costs equal to: 0.5 ∗ paving	Deterioration of roads, repaving costs equal to: 1.0 ∗ paving	Main ports double weight
Benin	99.95	−4.98	0.43	0.43	0.43	0.41	0.43
Burkina Faso	189.94	−18.88	0.16	0.16	0.06	0.06	0.16
Cote d'Ivoire	266.40	−52.09	0.12	0.12	0.12	0.12	0.12
Ghana	177.10	−139.45	0.52	0.52	0.52	0.51	0.52
Guinea	138.26	−4.40	0.39	0.45	0.39	0.39	0.39
Guinea Bissau	63.60	−16.02	0.16	0.16	0.14	0.14	0.16
Liberia	63.24	−9.43	0.37	0.37	0.37	0.37	0.37
Mali	293.87	−67.74	0.16	0.16	0.15	0.15	0.16
Niger	265.99	−33.68	0.11	0.11	0.11	0.11	0.11
Senegal	206.19	−62.49	0.24	0.24	0.24	0.22	0.24
Sierra Leone	86.49	−37.36	0.26	0.32	0.22	0.10	0.26
Togo	121.82	−65.26	0.14	0.14	0.11	0.11	0.14
Sample average	174.57	−46.55	0.25	0.25	0.23	0.21	0.25

Note: This table shows means by country, for country-periods with positive paving. Paving and deterioration in km. See Section 4 for variable definitions and sources.

Table OA6: Road paving characteristics by period, averages across countries

	Paving since last map	Deterioration since last map	Deposit-to-port bias				
Counterfactual includes →				Diamonds	Deterioration of roads, repaving costs equal to:		Main ports double weight
↓ Period ending Jan 1:					0.5 ∗ paving	1.0 ∗ paving	
1967	158.03	−57.91	0.05	0.05	0.03	0.03	0.05
1968	137.70	−82.93	0.14	0.14	0.14	0.14	0.14
1969	129.94	−27.69	0.14	0.14	0.14	0.14	0.14
1971	211.79	−69.32	0.23	0.23	0.23	0.22	0.23
1973	135.92	−42.13	0.21	0.21	0.20	0.10	0.21
1976	258.05	−30.24	0.40	0.39	0.34	0.31	0.40
1983	542.69	−19.49	0.33	0.35	0.33	0.31	0.33
1984	183.26	−34.00	0.12	0.12	0.12	0.12	0.12
1986	210.85	−43.74	0.23	0.26	0.22	0.22	0.23
1988	113.74	−91.65	0.37	0.37	0.25	0.23	0.37
1989	22.83	−44.29	0.50	0.50	0.50	0.50	0.50
1990	74.06	−28.57	0.21	0.31	0.21	0.21	0.21
1991	93.78	0.00	0.19	0.19	0.19	0.19	0.19
1996	182.62	−74.40	0.44	0.43	0.34	0.34	0.44
1998	141.91	−42.21	0.27	0.23	0.27	0.27	0.27
2000	212.86	−26.75	0.21	0.21	0.21	0.21	0.21
2002	77.37	−161.33	0.32	0.32	0.32	0.28	0.32
2003	47.06	−16.50	0.19	0.19	0.19	0.19	0.19
2007	251.24	−19.47	0.12	0.20	0.11	0.11	0.12
2009	109.22	−10.54	0.33	0.33	0.27	0.24	0.33
2012	197.37	−74.48	0.47	0.47	0.37	0.28	0.47
2014	81.72	−50.41	0.24	0.24	0.24	0.24	0.24
Sample average	174.57	−46.55	0.25	0.25	0.23	0.21	0.25

Note: This table shows means by period, for country-periods with positive paving. Paving and deterioration in km. See Section 4 for variable definitions and sources.

Table OA7: Road paving characteristics by period, Sierra Leone

	Paving since last map	Deterioration since last map	Deposit-to-port bias				
Counterfactual includes →				Diamonds	Deterioration of roads, repaving costs equal to:		Main ports double weight
↓ Period ending Jan 1:					0.5 ∗ paving	1.0 ∗ paving	
1967	81.16	−60.62	0.31	0.31	0.08	0.08	0.31
1968	229.14	−42.38	0.06	0.06	0.06	0.06	0.06
1969	0.00	0.00	0.00	0.00	0.00	0.00	0.00
1971	110.52	−128.48	0.45	0.45	0.45	0.45	0.45
1973	106.66	−97.95	1.00	1.00	0.87	0.01	1.00
1976	193.56	−36.07	0.66	0.57	0.48	0.24	0.66
1983	0.00	−17.73	0.00	0.00	0.00	0.00	0.00
1984	0.00	−45.41	0.00	0.00	0.00	0.00	0.00
1986	157.59	0.00	0.09	0.38	0.09	0.09	0.09
1988	52.62	0.00	0.56	0.56	0.41	0.20	0.56
1989	0.00	−48.80	0.00	0.00	0.00	0.00	0.00
1990	25.82	0.00	0.00	0.00	0.00	0.00	0.00
1991	35.71	0.00	0.00	0.00	0.00	0.00	0.00
1996	0.00	−143.18	0.00	0.00	0.00	0.00	0.00
1998	0.00	−25.82	0.00	0.00	0.00	0.00	0.00
2000	38.55	0.00	0.00	0.00	0.00	0.00	0.00
2002	25.58	−38.55	0.00	0.00	0.00	0.00	0.00
2003	25.82	−68.87	0.00	0.00	0.00	0.00	0.00
2007	68.87	0.00	0.00	0.66	0.00	0.00	0.00
2009	0.00	−25.58	0.00	0.00	0.00	0.00	0.00
2012	133.05	−61.89	0.81	0.81	0.81	0.36	0.81
2014	12.73	−25.58	0.00	0.00	0.00	0.00	0.00
Sample average	58.97	−39.41	0.18	0.22	0.15	0.07	0.18

Note: This table shows means by period, for Sierre Leone. Paving and deterioration in km. See Section 4 for variable definitions and sources.