

# The Power to Tax in Sub-Saharan Africa: LTUs, VATs, and SARAs.

CHRISTIAN EBEKE / MARIO MANSOUR / GRÉGOIRE ROTA GRAZIOSI

 CHRISTIAN EBEKE, International Monetary Fund

 MARIO MANSOUR, International Monetary Fund

 GRÉGOIRE ROTA GRAZIOSI, Ferdi and Cerdi-CNRS, University of Auvergne, France.

(Corresponding author: [gregoire.rota@gmail.com](mailto:gregoire.rota@gmail.com); [gregoire.rota-graziosi@udamail.fr](mailto:gregoire.rota-graziosi@udamail.fr))

## Abstract

In the context of achieving the new Sustainable Development Goals, revenue mobilization is a high priority in developing countries and in Sub-Saharan Africa, where governments' ability to tax remains limited. Using a unique revenue dataset spanning the period 1980-2010, we analyze three important tax reforms: the Large Taxpayers Unit (LTU), the Value Added Tax (VAT), and the Semi-Autonomous Revenue Agency (SARA). We propose an ex-post impact assessment of these tax reforms in SSA countries based on propensity-score matching methodology (PSM) and synthetic control method (SCM). VAT and SARA are found to have an unambiguously large and positive effect on non-resource taxes, while the impact of LTU is insignificant—LTU seems however an important precondition for the adoption of the first two reforms. We conclude also that VAT and SARA display some synergy, and their positive effects strengthen several years after their adoption.

**JEL Classification Numbers:** H2; O23; O55; C1

**Keywords:** tax reforms; Africa; revenue mobilization; causality

The views expressed in this paper are those of the author(s) and do not necessarily represent those of the IMF or IMF policy. They describe research in progress by the author(s) and are published to elicit comments and to further debate. We would like to thank Michael Keen, Katherine Baer, Juan Toro, Olivier Benon, and colleagues who participated in the IMF Fiscal Affairs Department's weekly seminars, for helpful comments and suggestions.



## 1. Introduction

Taxation is a core function of the state. It contributes to the social contract between the state and citizens. The history of state formation highlights the preponderant role of taxation in the relationship between rulers and citizens (Friedman, 1974, and Tilly, 1990). Recent research suggests that this relationship is important in developing countries, where state building should rely on governments' ability to raise taxes (Brautigam et al., 2008; and Besley and Persson, 2011). Beyond the academic circle, the recent United Nation Financing for Development Conference in Addis Ababa (July 2015) highlighted the crucial role of domestic revenue mobilization in developing countries to achieve the post-2015 sustainable development goals (SDGs). Domestic revenue mobilization was already a priority in the international policy debate since the Monterrey Consensus on Financing for Development in 2002 (reaffirmed in 2008), and as emphasized by several international organizations (IMF, 2011, 2014, and 2015; OECD 2010(a,b) and 2014; Mascagni et al., 2014 for the European Union).

Part of the relationship between state building and taxation finds its source in the economic theory of public enforcement of (tax) law (see Shavell and Polinsky, 2000), in particular the amount of resources devoted to apprehend violators, or equivalently here to implement tax laws.<sup>1</sup> Some of these theoretical analyses find their practical use in a number of tax policy and administration innovations or reforms. We consider three of the most important of these reforms over the past decades in Sub-Saharan Africa (SSA): setting-up Large Taxpayers Units (LTUs); introducing Value Added Taxes (VATs); and reorganizing tax administrations in Semi-Autonomous Revenue Authorities (SARAs). In 1980, two of the 41 SSA countries our data covers had a VAT: Côte d'Ivoire (1960) and Senegal (1980); none had implemented a LTU or a SARA. By 2010, 34 had adopted a VAT, 21 had an LTU, and 17 had a SARA. Over the same period, the revenue-to-GDP ratio passes on average from 17.7 percent to 18.5 percent, and from 13.3 percent to 15.3 percent if we exclude revenue from non-renewable natural resources.<sup>2</sup> These reforms have been core elements in government's efforts to improve revenue mobilization, an important factor of economic development as emphasized by Acemoglu (2005) or Besley and Persson (2013).

A common denominator of LTU, VAT, and SARA is that they all aim to increase revenue through better compliance with tax laws. The LTU consists in a rationalization of the tax administration through the segmentation of taxpayers according to some measure of size—frequently turnover. The VAT, in addition to being an important tax policy reform,<sup>3</sup> is a partial delegation of revenue collection to the private sector; its self-enforcing mechanism (through the credit-invoice method)

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<sup>1</sup> Another part of this relationship results from the political economy analysis, which links the tax system to government's accountability.

<sup>2</sup> Non-renewable natural resources include oil, gas, and minerals. Total revenue is defined as tax revenues plus revenue from extractive industries (mainly, royalties, profit sharing, and the general corporate income tax, or profit tax in countries that still use a schedular tax system).

<sup>3</sup> VAT was often introduced as a substitute to distortionary sales taxes and in the context of decreasing tariff rates.

and spreading of tax collection along the production-distribution chain are key elements of its quality as a tax.<sup>4</sup> A SARA corresponds to a strategic delegation of tax collection to an agent, more or less autonomous from the government. Two reasons are frequently advanced in favor of adopting a SARA: reduce political interferences with the collection of taxes; provide better human resource incentives (e.g. pay, bonuses, managerial flexibility) than usually provided in the general public service. Both of these reasons tend to be particularly relevant in SSA, and more generally in developing countries.

Each of these three reforms has a possible (expected positive) impact on revenue mobilization. But each may suffer from design defects that could weaken its expected impact or even yield the opposite result (a negative impact on revenue). For instance, LTUs may induce too much resources allocated to well-known and visible taxpayers, and not enough to the rest of the tax population; depending on the relative size of each group, the final impact of LTU on tax revenue may actually be negative. The effectiveness of a VAT depends on a number of policy choices, including: rate(s), registration threshold(s), exemptions, and limitations on refunds; these are particularly important in cases where the VAT replaced one or several other transaction taxes—the case of most SSA countries that adopted a VAT. Finally, SARA may not have enough autonomy and financial resources to achieve its revenue goals.

This paper presents for the first time an *ex-post* impact assessment of LTU, VAT, and SARA in SSA countries. It contributes in several ways to the debate on tax reforms and revenue mobilization in SSA. First, it provides an assessment of the effectiveness of each of the three considered reforms on domestic non-resource revenue. This complements previous work studying the efficiency of some tax reforms in developing countries (e.g., Keen and Lockwood, 2010, for the VAT<sup>5</sup>). Second, beyond the average effect of each reform, potential synergies among them are captured by considering their combined impact. Third, the dynamic effect of each reform is appreciated through their respective impact on revenue depending on a time horizon.

Our database allows us to isolate the effect of these reforms on non-resource tax revenue, which excludes royalties and corporate income tax on natural resource extractive activities (i.e. upstream mining, oil, and gas).<sup>6</sup> Since the latter is strongly affected by international commodity price volatility, and since the responsibility for its collection is frequently shared by several ministries or agencies,<sup>7</sup> excluding these revenues better captures the impact of each studied reform on domestic revenue mobilization efforts. Moreover, the taxation of natural resources raises political

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<sup>4</sup> In addition to the fact that in theory it is a tax on a broad base (i.e. final consumption), and is more efficient compared to most other taxes in the typical taxation arsenal of a country.

<sup>5</sup> To our knowledge, there is no similar empirical analysis on the effects of LTUs and SARAs on domestic non-resource revenues.

<sup>6</sup> As robustness tests, we also examine the impact of each reform and their combinations on total tax revenues. The results are unchanged.

<sup>7</sup> It is frequent in SSA that royalties and production sharing are collected sectoral ministries or specialized government agencies, such as national oil companies. Revenue mobilization in this case cannot be attributed to SARAs or LTUs.

economy issues that are fundamentally different from issues raised by taxes whose incidence fall (ultimately) on individuals.

We consider each tax reform: LTU, VAT, and SARA as a “treatment” to improve revenue mobilization. We apply the propensity-score matching methodology (PSM) as developed by Rosenbaum and Rubin (1983) and the synthetic control method (SCM) as established by Abadie and Gardeazabal (2003). The former allows us to appreciate synergy between studied tax reforms, while the latter focuses on the dynamic impact of each reform. Both methodologies present the advantage to rely on a transparent way in building counterfactual. Our PSM analysis concludes that VAT and SARA improve non-resource revenue by 2 percentage and 4 percentage points of GDP, respectively. Moreover, combining VAT and SARA may even increase non-resource revenue by 4.5 to 6.5 percentage points of GDP (compared to the scenario of no VAT and SARA), while the combination of the three reforms has an impact of about 1.2 to 2.4 percentage points of GDP. If the creation of a LTU has no significant direct impact on revenue mobilization, it appears as an important precondition for the adoption of the other two reforms, especially VAT.

We extend our analysis by considering that the impact of studied tax reforms may take several years to fully materialize.<sup>8</sup> This dynamic approach leads to the application of the SCM, as proposed by Abadie and Gardeazabal (2003) and extended by Abadie, Diamond, and Hainmueller (2010), and Cavallo et al. (2013), to apprehend average impact of each reform over time. The counterfactual here is based on similarity between treated and not treated countries before tax reforms occurred with respect to relevant covariates and past tax revenue-to GDP ratios. This analysis confirms also our previous results: VATs and SARAs have a positive and strong impact on tax revenue, while the effect of LTUs is more ambiguous. These conclusions are not only valid for the short-term impact but also for the medium term. Indeed, VAT and SARA have both a significant and stronger positive impact after 5 years of their implementation. It is worth noting that the dynamic positive effects of the VAT tend to soften in the medium term while the SARA’s effect remains strong.

The rest of the paper is structured as follows. Section II provides an overview of the reforms, with emphasis on their strengths and weaknesses in SSA countries. Section III presents the data source and variables of interest. The empirical analysis is developed in section IV. Section V concludes.

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<sup>8</sup> Moreover, the studied events may have been modified over time: governments introduce new VAT exemptions and limit refunds; they change the registration threshold for the VAT, and the definition of a large enterprise for purposes of the LTU; SARAs induce new relationships between taxpayers, tax policy makers, and tax collectors.

## 2. The Reforms: VAT, LTU, and SARA

Among other objectives, VATs, LTUs, and SARAs share the common goal of improving compliance, hence revenue. Each presents some advantages in terms of improved revenue collection, but each is also exposed to risks. This section briefly reviews the strengths and weaknesses of these reforms, which helps to form expectations about the sign of their potential impact on revenue.

### 2.1. Value Added Taxes

The VAT has been adopted by more than 150 countries, in the majority of cases replacing distortionary cascading sales taxes and/or trade taxes. The VAT is first and foremost a fundamental policy reform. Its main quality lies in that in theory, it falls on final consumption and is neutral on production decisions. In other words, it targets a large tax base, and is growth friendly. In practice, however, this quality depends largely on the design features of the VAT, such as the number of rates, the prevalence of exemptions, the level and number of registration thresholds, and the limitations on refunding excess VAT credits—to name just a few (see Bird and Gendron, 2011). Also, while the production-neutrality property of the VAT has generally been an important factor in countries' decision to adopt it, the benefits from such neutrality—in terms of improving resource allocation in an economy—are usually less important in developing countries where the number of production stages is relatively small.

Beyond its qualities as a policy instrument, the VAT is believed to improve compliance and reduce risks to revenue collection. First, it is collected by firms at the various stages of the value chain through the invoice-credit mechanism, which provides an incentive to report purchases (input) in order to claim a credit/refund against VAT on sales (output). Second, the credit-invoice partitions revenue collection along the value chain, and reduces the risks of revenue loss—relative, for example, to the retail sales tax, the economic-equivalent of VAT.

These compliance and administrative benefits can, however, be overstated. The incentive to underreport VAT on output increases when sales are made to final consumers or to non-registered entities; in essence the seller keeps some of VAT collections rather than remitting them to the government. The credit-invoice mechanism may even create informality chains as stressed by Paula and Scheinkman (2010) for Brazil. The possibility of claiming a VAT refund introduces also a risk of fraudulent practices that other taxes that the VAT replaced do not suffer from. These risks can be important in SSA, where VAT implementation frequently coincided with difficult budget situations, leading to the choice of high VAT rates, and too many exemptions.<sup>9</sup> These, in turn, shrink the tax base, and break the VAT chain, weakening compliance and enforcement, and providing more opportunities for fraudulent refund claims.

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<sup>9</sup> In particular, trade liberalization imposed a significant stress on public revenue in SSA, where taxes on international trade remain important. This effect was accentuated by IMF programs, which were significant in the adoption of VAT (see Keen and Lockwood, 2010). These may explain why VAT rates were relatively high in SSA (generally above 15 percent).

Another technical dimension which matters for the success of the VAT in mobilizing revenue, particularly in SSA where administrative capacity is limited, is the registration threshold. This threshold is usually determined in terms of turnover and results from a delicate trade-off. If it is too low, the tax administration is likely to be stretched and unable to monitor registered firms effectively; the probability of audit is weak and the credibility of the VAT is compromised. If it is too high, the VAT base is narrower (see Kanbur and Keen, 2014 for an analysis of the optimal threshold). More problematic, the tax is not neutral for firms below the threshold, since they pay the VAT on their inputs, and are not allowed to claim it back. These firms usually exert pressure on the VAT base in the form of lobbying for exemptions of inputs that go into the production of their outputs (agriculture is perhaps the most common example in SSA). If such lobbying efforts are unsuccessful, these firms are more likely to deal with other unregistered firms, which would reinforce structural dualism and may affect growth by making participation in the formal sector less attractive.<sup>10</sup>

## **2.2. Large Taxpayers Units**

The introduction of LTUs contributes to the effort of rationalizing a tax administration by consolidating all its functions under one umbrella for large taxpayers. The emphasis on segmentation of taxpayers according to size, of which the LTU is one organizational form, occurred initially in Latin America in the late 70s, and is considered as one of the most important features of the modernization of tax administrations in developing countries (see, for instance, Baer, 2002). It consists in establishing a self-contained office within the tax administration, acting as a single clearance window for large taxpayers for the main domestic taxes: income taxes, VAT, sector-specific taxes (e.g. resource royalties), etc.

Several advantages can be expected from a LTU. First, segmentation improves the specialization of the personnel of the tax administration by allowing a better coverage of heterogeneous taxpayers with different sets of compliance behavior and risk. This specialization may be reinforced by an internal organization of LTUs by economic sector, improving the knowledge of its personnel on sector-specific issues. The division of tasks (return processing, assessment, audit, collections, etc.) inside LTUs may also contribute to reduce corruption by reducing the number of interactions with taxpayers (McCarten, 2005).

Second, efficiency can be gained from the centralization of core functions for all taxes. For example, auditing a large taxpayer simultaneously for income taxes and VAT has obvious advantages that can be hard to obtain in different organizational settings without seamless information sharing across various units of a tax administration.

Finally, the creation of LTUs has been found to be an opportunity to induce other efficiency-improving reforms such as self-assessment, adoption of unique taxpayer identification numbers,

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<sup>10</sup> A similar argument is made in Emran and Stiglitz (2004).

electronic filing, and new computerized information system (Baer, 2002; and McCarten, 2005). Once introduced in the context of an LTU, these reforms can be rolled out across the whole tax administration, thus improving revenue collection and reducing administrative and compliance costs.

LTUs may, however, have some negative impacts on revenue. Establishing a LTU is only a first step in the segmentation and coverage of the taxpayers' population, which also requires a focus on middle and small taxpayers—this is still very much work in progress in SSA. This partial rationalization of tax administration may perturb the right balance of resource allocation between known taxpayers, and tax evaders (Terkper, 2003). LTUs do not address directly the latter. But LTUs are generally expected to address the problem of underreporting income and other forms of tax avoidance and evasion by large taxpayers, which is the key issue in sectors where economic activity is dominated by large taxpayers.

LTUs may also worsen the revenue situation by concentrating tax administration resources (particularly, most competent human resources) on a small number of large taxpayers. Such imbalances favor the emergence of a dual economy, where a large part of tax revenue comes from a few taxpayers. Over time, this can increase the concentration of revenue, lead to a distorting tax system (Auriol and Warlters, 2005), and create incentives for rent seeking (Burgess and Stern, 1993). Unless the benefits of LTUs are spread out to the rest of the tax administration fairly quickly, LTUs may not improve the revenue performance of the overall tax administration, even if they may be beneficial in collecting revenue more effectively and efficiently from large taxpayers.

LTUs may trigger strategic behavior from taxpayers. For example, taxpayers may break-up their activities in order to remain below the LTU threshold (Terkper, 2003). Dhramapala, Slemrod, and Wilson (2011) show how a differentiated treatment of small and large firms may involve a missing middle with costly tax collection.<sup>11</sup> The efficiency of LTUs may even hinder its initial purpose by motivating some large taxpayers to obtain tax advantages through derogatory regimes (something quite common in many developing countries in sectors such as telecommunications, mining, large infrastructure, and other activities dominated by multinationals). In that sense, the increase in tax incentives provided under investment codes can be viewed as a reaction to the improvement in tax-law enforcement (Keen and Mansour, 2010b).

### **2.3. Semi-Autonomous Revenue Agencies**

First implemented in Africa by Ghana in 1985, the SARA is a drastic reform consisting in delegating tax collection to an autonomous agency. This delegation of power, which occurred in other fields such as monetary policy with the independence of central banks, is a strategic device to improve the credibility of tax law enforcement. But the main reasons for the independence of SARAs is not

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<sup>11</sup> This may be true for VAT too. Onji (2009) shows that the VAT introduction in 1989 in Japan triggered a clustering of firms just below the turnover threshold.



time-inconsistency, as presented by Kydland and Prescott (1994); rather, it is to isolate revenue collection from political interferences, provide management autonomy, and merge customs revenue collection function and tax administrations into a single entity.<sup>12</sup>

The autonomy, which may differ significantly across countries, is a signal to a more credible audit policy, since control should be realized without any political interference. For Taliencio (2004), SARA is even a credible commitment of the government towards more competent, effective, and fair tax collection. Based on surveys in four Latin American countries,<sup>13</sup> the author establishes a significant and positive relationship between the perception of a higher autonomy of SARA and better performances in tax collection. This signal effect is reinforced by the greater flexibility of SARA to manage its human resources than standard public sector agencies. Indeed, recruitment, promotion, and dismissal do not have to respect civil service's rules, allowing a number of flexibilities, such as higher wages (Fjeldstad and Moore, 2009 and Moore, 2014).

The second important advantage of SARAs is the integration of revenue collections by merging tax and customs administration. The rationale for merging the two is: (i) exploiting synergies, in particular for VAT on imports (Keen, 2008); (ii) saving costs by combining operational functions in tax collections (World Bank, 2010). Given the complementary role of customs and tax administration, the lack of coordination in their operations often explains the poor performance in domestic revenue in SSA countries (e.g. a large importer/retailer may avoid VAT registration with the domestic tax administration as long as customs do not share data on imports with the domestic tax agency).

However, SARAs may raise some particular issues. First, the credibility of the commitment is far from equivalent to that of an independent Central Bank. Importantly, SARAs are not typically in charge of tax policy, a core function of macro-economic management that remains in the hands of the government and parliament. Second, SARA may be implemented partially only (see Kidd and Crandall, 2006), in particular without integrating tax and customs administrations. Third, the tasks assignment between SARA and the ministry of finance may be somewhat blurred, inducing some conflicting relationships fueled by the lack of ownership of tax reforms (and influence over them). The separation between tax collection and tax policy may even pose a threat on the consistency of policymaking (Fjeldstad and Moore, 2009). Another issue raised by Moore (2014) is the mobility of tax auditors from SARA to the private sector and the induced threat of conflicts of interests.<sup>14</sup> Finally, SARA may be particularly disruptive since current civil servants working in tax or customs

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<sup>12</sup> Fjeldstad and Moore (2009) consider a third one, which is facilitating other reforms such as introducing identification numbers.

<sup>13</sup> The author uses surveys of 200 large taxpayers and tax consultants in Bolivia, Mexico, Peru, and Venezuela in 1998 and 1999. Bolivia is characterized by a standard tax administration, while Mexico and Venezuela have a SARA, each being considered as less autonomous than the Peruvian SARA.

<sup>14</sup> A theoretical issue that SARAs induce is the standard informational asymmetry between the Principal, the government, and the Agent, the SARA (Sanchez and Sobel, 1993).

administration are not sure to be hired by the new agency. This transitory period may be particularly costly in terms of revenue mobilization.

### 3. Data and Stylized Facts

#### 3.1. Data

This paper uses a unique revenue dataset for SSA countries, constructed primarily from IMF staff documents and other sources.<sup>15</sup> It presents two advantages for the study of the reforms considered in this paper. First, it covers 41 countries over the period 1980-2010, a period that spans all the considered reforms in SSA. The revenue dataset is completed by data on the dates of adoption of LTUs (Baer, 2002, and Fossat and Bua, 2013), VAT (IMF Fiscal Affairs Department VAT database), and SARAs (Kloeden, 2011, and Fjeldstad and Moore, 2009).<sup>16</sup>

Figure 1 shows average tax revenue-to-GDP ratios with and without natural resource revenue, and linear fitted values, over 1980-2010. From 13.3 percent of GDP in 1980, non-resource revenue increased to 15.4 percent in 2010. Most of this increase occurred over 2000-2010; revenues were stagnant or slightly declining over 1980-2000. The impressive increase in revenue came from non-renewable natural resources (essentially royalties and corporate income taxes on upstream activities from oil, gas and mining). A closer examination of the data reveals that in 2010 resource revenues were highly concentrated in oil producing countries—about 75 percent in Nigeria and Angola, and 15 percent in Francophone Central Africa (Cameroon, Congo, Equatorial Guinea and Gabon). Thus, non-resource revenue mobilization is still a real challenge in SSA.

The evolution of non-resource revenue differs significantly among SSA countries over the period considered. For instance, while Uganda, Chad, and Ghana tripled or doubled (for the latter) their tax revenue, Togo, Central African Republic, Madagascar, and Zambia lost respectively 42 percent, 35 percent, 34 percent, and 29 percent. Figure 2 illustrates the disparate evolution of tax revenue for four countries: Benin, Ghana, Kenya, and Senegal and shows the date of the three studied reforms in each country.

Figure 3 displays the evolution of the adoption of LTU, VAT, and SARA. Most of these reforms started in the mid-1990s, and accelerated rapidly through 2005. VAT was adopted earlier, and by more countries than the other two reforms. The speed of adoption of the three reforms suggests that they may be highly correlated. The VAT is now present in nearly all SSA countries; this is not the case of the other two reforms. Finally, a key point to note is that a significant number of these reforms were implemented under an IMF program—suggesting that both short-term revenue mobilization and efficiencies in tax collections were important objectives of the reforms.

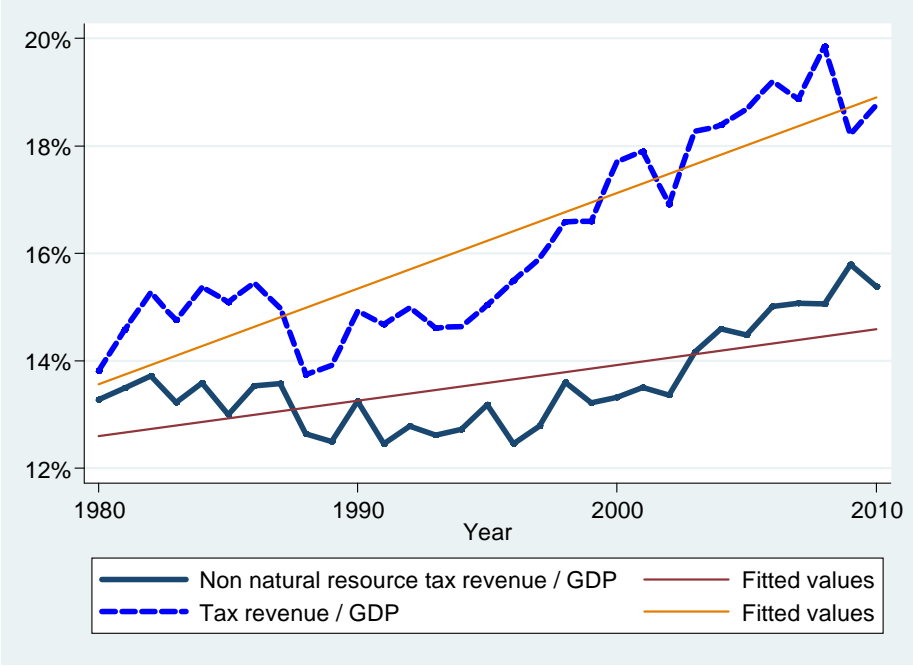
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<sup>15</sup> See Mansour (2014) for a detailed description. The dataset updates and expands on that in Keen and Mansour (2010a).

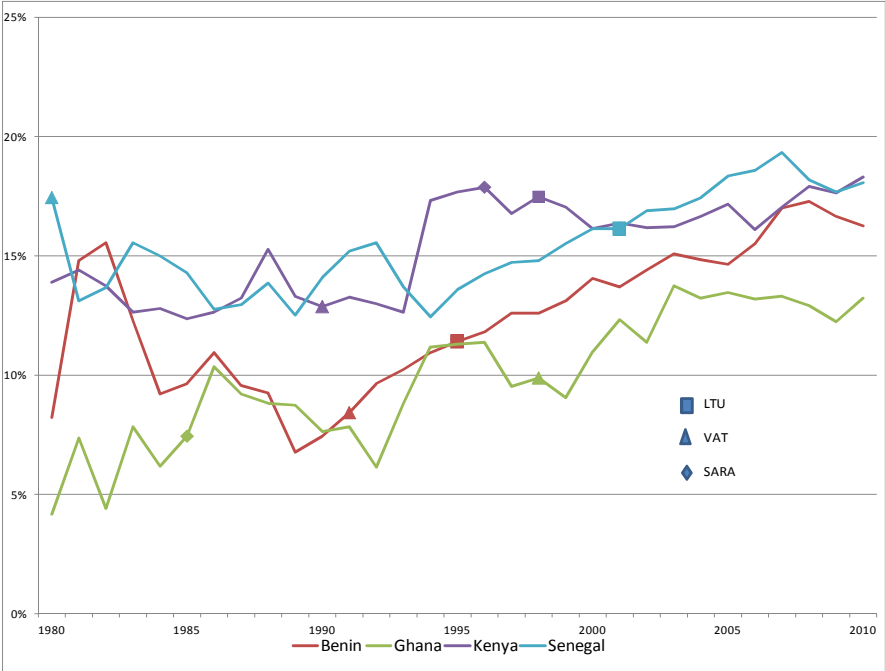
<sup>16</sup> Appendix Table A1 provides reform dates for all countries in the dataset.

Figure 4 displays a Venn diagram of the reforms in 2010. Only five countries: Burundi, Ethiopia, Kenya, Uganda, and South Africa have adopted all three reforms, while none has only LTU and SARA. It is also noticeable that one country (Sierra Leone) has SARA only without having at least one of the other two reforms, and one country only (Comoros) has LTU without one of the other two. The largest combinations of reforms are VAT-SARA and VAT-LTU.

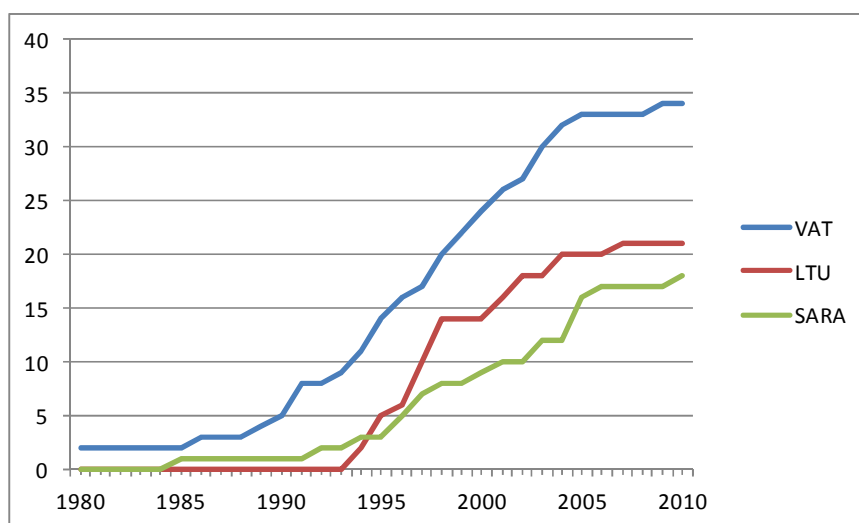
**Figure 1. Average (Unweighted) Non-resource Taxes to GDP in SSA (1980-2010)**



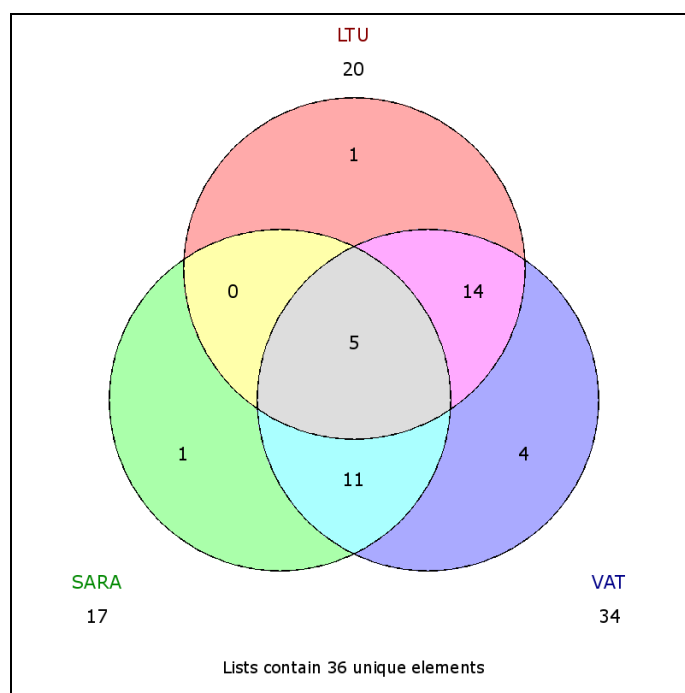
**Figure 2. Non-resource Taxes-to-GDP and Reforms Dates: Selected Countries**



**Figure 3. The Adoption of LTU, VAT and SARA in SSA (1980-2010)**



**Figure 4. Venn Diagram of Adoption Dates of VATs, LTUs and SARAs in 2010**



Source: [www.bioinformatics.lu](http://www.bioinformatics.lu)

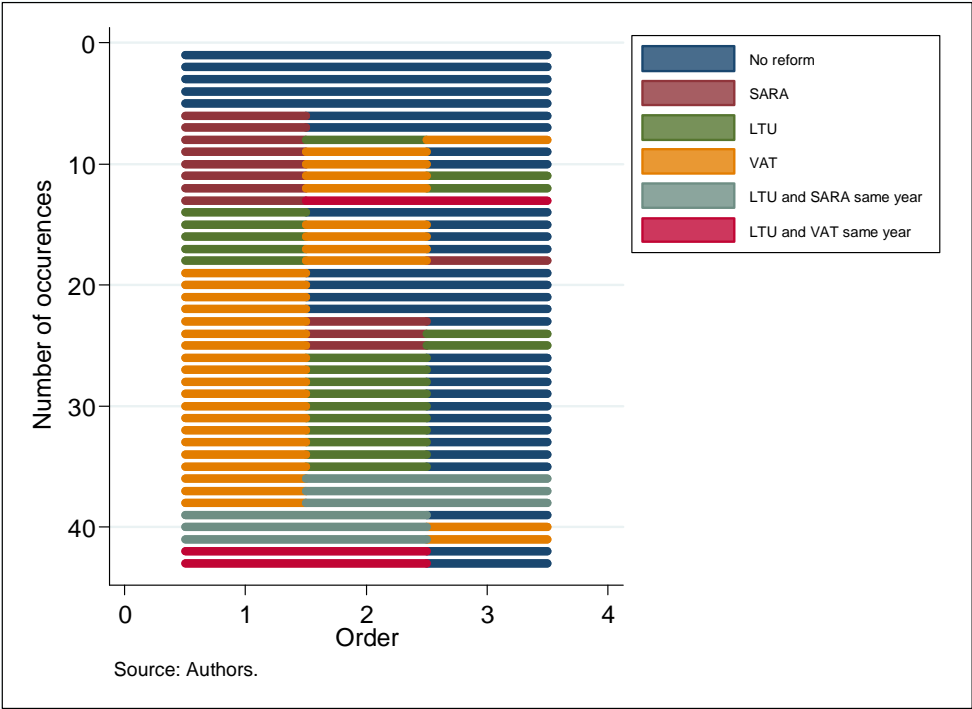
### 3.2. Reform sequencing

An important source of variation in the data is the sequencing of the tax reform adoption by countries. While the econometric investigations will provide an answer to the fundamental question of the effectiveness of the tax reforms, it is also particularly important to understand first how the reforms are packaged. For example, which of the three reforms is more likely to be implemented first, and which one would follow?

These questions have critical implications on the way the econometric estimates (the average treatment effects) will be interpreted. Let us assume for instance that an LTU tends to be implemented last. This would suggest that when running a regression of the effect of LTU on revenue, one runs the risk of associating the point estimate with LTU itself instead of recognizing that some of those effects may be multiplicative and complementary by involving the effects of the other reforms. Finding a positive and significant association between LTU and revenue may simply be the result of a confounding bias associated with the effect of other reforms already in place (VAT and SARA in this context).

Figure 5 allows an appreciation of the dynamic aspect of the three tax reforms. There are 41 sequences corresponding to the 41 SSA countries in the database. The number of occurrences (countries) is shown on the y-axis, and is associated with each reform combination. For example, the first-five blue bars indicate that 5 countries over the period of the study (1980—2010) have not implemented any of the three reforms we discuss: Angola, Eritrea, Guinea-Bissau, Sao Tome and Principe, and Swaziland.<sup>17</sup> Note the prominent role played by the VAT, as the majority of countries adopted the VAT first. SARAs have also been adopted earlier than LTUs—although there are three cases where LTU and SARA were adopted simultaneously. After VAT adoption, the most immediate reform to follow is the LTU,<sup>18</sup> but there are cases in which LTU adoption precedes VAT.

**Figure 5. The Sequence of Reforms adoption in SSA (1980-2010)**



Source: authors.

<sup>17</sup> This case is not presented in our Venn diagram.

<sup>18</sup> This goes against standard advice to set up LTUs before proceeding with VAT implementation.

## 4. Empirical Analysis

This section provides an assessment of the effects of each of the three tax reforms on revenue. Measuring the effects of the implementation of tax reforms (including combinations) is an empirical exercise similar to the investigation of the effects of specific programs on socio-economic outcomes (using micro data). Methods borrowed from the impact evaluation literature can be applied in our context. We propose two complementing empirical approaches: propensity score matching (PCM) estimates and the synthetic control method (SCM), which is a generalization of the difference-in-difference technique to quantify the causal effects of VAT, LTU and SARA on the tax revenue-to-GDP ratio.<sup>19</sup>

Ideally, the causal effects of the tax reforms would be estimated from the implementation of a random assignment of the allocation of LTUs, VATs and SARAs between countries. This would ensure that the control group (countries that randomly have not received the treatment) is the relevant counterfactual group (i.e. what would have happened to the tax-to-GDP ratio in a situation where the treated country did not implement the reform). In the real world, conducting such experiments at the cross-country level and in a large-scale is not feasible. The econometric investigations should therefore find ways to bypass the fundamental problem of finding an acceptable counterfactual group to estimate an unbiased effect of the reforms.<sup>20</sup>

Since it is widely thought that mobilizing additional revenue has been a primary objective of the three reforms considered,<sup>19</sup> we would expect them to have a positive effect on the non-resource revenue-to-GDP ratio. However, because other objectives have also been pursued (e.g. improving tax administration efficiency and taxpayer services; replacing tariff revenues and cascading sales taxes with VAT; etc), and for reasons discussed above on possible poor or incomplete design of the reforms, the effect may be nil or negative. These expectations hold for the individual and combined effects of the reforms.

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<sup>19</sup> In an appendix available upon request we estimate fixed-effects and dynamic panel estimates with fixed-effects, which capture the heterogeneity in the date of tax reform adoption among countries. We observe that VATs and SARAs improve non-resource revenue by 1 to 2 percent of GDP, while the impact of LTUs is much lower and only significant at 5 percent. The latter vanishes when the three reforms are considered simultaneously.

<sup>20</sup> The selection-bias associated with the decision to adopt the tax reform could be large. Indeed, countries tend to embark into reforms under certain specific macroeconomic or socio-political circumstances that may well be correlated with fiscal performance. Put differently, the adoption of a specific reform is conditional on the expected gains/benefits associated with the reform. For example, countries with poor performance in revenue are more likely to embark into ambitious reforms. Ignoring this simple regularity in the data would lead to underestimate the impact of the reform on revenue mobilization. On the other hand, some reforms are more likely to be adopted in good rather than bad times. Ignoring this bias could result in an over-estimation of the true effect of the reform on the outcome variable. The econometric framework we use in this paper aims at tackling the estimation biases to better capture the reform's impacts.

## 4.1. Propensity Score Matching Estimates and Reforms Interaction

### **Framework.**

We first use the propensity score matching technique (PSM) to investigate the impact of each tax reform on the non-resource taxes-to-GDP.<sup>21</sup> This involves comparing country groups following a two-step approach:

- First, the probability of observing VAT, LTU or SARA in a given country in year  $t$ , is estimated conditional on observable economic conditions and country characteristics (selection model). We follow Keen and Lockwood (2010) and model the probability of a given tax reform as a function of lagged macroeconomic variables, including the level of past revenue, trade openness, resource rents, value added of the agriculture sector, and a dummy variable identifying IMF arrangements; we also control for some demographic factors, namely the age demographic dependency ratio and total population. In some alternative specifications, we perform estimations aimed at gauging the spillovers from one reform into the likelihood of implementing another. More precisely, we investigate whether the initial adoption of a given tax reform determines the probability of adopting a subsequent one. By doing so, we formally test the sequencing of the reforms. We make use of the propensity score matching framework to investigate the effects of reform packages on revenue. Does it pay off to adopt two or three of the tax reforms? What is the additional tax revenue ratio generated by the adoption of various sets of reforms? Each tax reform, or package, has its own selection equation using the same battery of control variables to extract predicted probabilities of observing a particular tax reform in each country.
- Second, these probabilities, or propensity scores, are used to match reform countries to non-reform countries, and thereby construct a statistical control group.

The matching based on the likelihood of adopting a tax reform ensures similarity of initial macroeconomic conditions and country characteristics in the control group. The control provides a proxy for the counterfactual—i.e., the tax ratio if a country had not implemented the reform. The effect of the tax reform is then calculated as the mean difference in the non-resource tax -to-GDP ratio across the two groups. Inference is made via bootstrapping techniques. We present results using nearest-neighbor, radius, and Kernel matching.<sup>22</sup>

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<sup>21</sup> The use of the PSM technique in the macroeconomic literature has been popularized by recent empirical papers focusing on the effects of the inflation-targeting arrangement on macroeconomic performances (Lin and Ye, 2007), on the effects of fiscal rules on fiscal behavior in developing countries (Tapsoba, 2012), and on the economic effects of foreign capital flows (Chari, Chen, and Dominguez, 2012), and on the effects of IMF programs on various macroeconomic outcomes (Bal-Gunduz et al., 2013).

<sup>22</sup> The nearest-neighbor matching estimator sorts all records by the estimated propensity score and then searches forward and backward for the closest control units. We make use of the three, four, and five nearest neighbors. Radius matching uses all comparison observations within a predefined distance around the propensity score. Kernel matching

(continued)

## **Results.**

Tables 1 and 2 present the first step of the PSM approach. They report probit estimations of the adoption of one or a combination of the studied reforms. This extends Keen and Lockwood (2010) in two ways: First, we estimate the probability of adopting LTU or SARA in addition to VAT; second, we are able to appreciate the spillover effect of one reform on the probability of adopting another; for instance, we can assess if having already in place a LTU or a SARA would favor the adoption of VAT, while controlling for other determinants of the reforms.

Income per capita, the age dependency ratio, and the size of the agricultural sector have the expected impact on the adoption probability of each reform. Moreover, as in Keen and Lockwood (2010), participation in an IMF program has a significant and positive impact on adopting any of the reforms.<sup>23</sup> In contrast, however, trade openness does not reduce significantly the probability of adopting a VAT or LTU, but improves the probability of adopting a SARA.

Table 1 displays some interactions between the three studied tax reforms. A striking result is the huge and significant impact of having a LTU in place on the adoption of VAT. Indeed, the predicted probability of adopting VAT is 0.77 greater for a country, which has a LTU than for a country, which does not. We remark also that having a LTU has a positive impact on adopting a SARA: The marginal effect of LTU on VAT is lower (0.09), but significant at 1 percent. Symmetrically, having a VAT or a SARA improves respectively by 0.558 and 0.22 the probability of adopting a LTU. Finally, we notice that there is no direct interaction between VAT and SARA.

Table 3 shows the final results of the PSM estimations for each reform separately. There is a robust and significant effect of the presence of the VAT and SARA but no statistical effect is found regarding the LTU. Having a VAT in place leads to a gain of about 2 percentage points of GDP in additional tax revenue compared to the counterfactual scenario of no VAT. For the SARA, the effect is much larger: 2.6 to 4 percentage points of GDP, depending on the matching algorithms. The absence of results from the LTU echoes the results found above in the fixed-effects specifications.

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entails a weighted average of the outcome of all non-treated units, where the weights are related to their proximity to the treated unit.

<sup>23</sup> The issue of the endogeneity of an IMF program is discussed in Keen and Lockwood (2010, page 144).



**Table 1. Correlates of Reforms: Probit Estimates and Marginal Effects**

	(1) VAT	(2) LTU	(3) SARA	(4) VAT	(5) LTU	(6) SARA
Non-resource taxes-to-GDP, lagged	-0.00608** (0.00303)	-0.00711*** (0.00243)	-0.000436 (0.00180)	-0.00393 (0.00345)	-0.00703** (0.00289)	0.000661 (0.00180)
Age dependency ratio	-0.0169*** (0.00202)	-0.0120*** (0.00146)	-0.00844*** (0.00106)	-0.0124*** (0.00224)	-0.00613*** (0.00168)	-0.00745*** (0.00108)
Agriculture value added-to-GDP	-0.0167*** (0.00196)	-0.00183 (0.00144)	0.000149 (0.00100)	-0.0207*** (0.00209)	0.00564*** (0.00154)	0.00156 (0.00097)
Trade openness	-0.000205 (0.001000)	0.000876 (0.000762)	0.00224*** (0.000557)	-0.00143 (0.001080)	2.39E-05 (0.000944)	0.00208*** (0.000589)
IMF arrangement	0.299*** (0.0424)	0.115*** (0.0349)	0.0717*** (0.0240)	0.299*** (0.0495)	0.0201 (0.0368)	0.0542** (0.0238)
Population, log	0.281*** (0.0190)	0.162*** (0.0166)	0.139*** (0.0118)	0.230*** (0.0221)	0.0274 (0.0188)	0.114*** (0.0121)
SARA dummy, lagged				0.0664 (0.0724)	0.220*** (0.0421)	
VAT dummy, lagged					0.558*** (0.0402)	0.0332 (0.0276)
LTU dummy, lagged				0.772*** (0.0642)		0.0894*** (0.0270)
Constant	-2.467*** (0.322)	-1.558*** (0.267)	-1.755*** (0.184)	-2.148*** (0.326)	-0.445 (0.291)	-1.540*** (0.182)
Number of observations	904	904	904	904	904	904

Standard errors in parentheses. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1

**Table 2. Correlates of Reforms Combinations: Probit Estimates and Marginal Effects**

	(1) VAT-LTU	(2) VAT-SARA	(3) LTU-SARA	(4) All Three
Total tax revenue-to-GDP, lagged	-0.00112 (0.001760)	0.000860** (0.000397)	-9.19E-05 (0.000129)	-0.00516*** (0.001200)
Age dependency ratio	-0.00432*** (0.000951)	-0.000682*** (0.000244)	4.52E-05 (0.000064)	-0.00308*** (0.000670)
Agriculture value added-to-GDP	-0.00251** (0.001100)	-0.000144 (0.000119)	-1.56E-05 (0.000078)	-0.00335*** (0.000685)
Trade openness	0.000116 (0.000612)	5.98E-05 (0.000076)	-9.50E-05 (0.000086)	0.00126*** (0.000314)
IMF arrangement	0.113*** (0.026700)	0.00876** (0.004340)	0.00141 (0.001410)	0.0202 (0.015100)
Population, log	0.0784*** (0.01040)	0.00614** (0.00272)	0.00128 (0.00113)	0.0833*** (0.00887)
Constant	-1.046*** (0.1610)	-0.0941** (0.0471)	-0.0335 (0.0261)	-1.065*** (0.1260)
Observations	904	904	904	904

Standard errors in parentheses. Number of observations for which dependent variable takes the value 1. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

**Table 3. Estimates of Treatment Effect on Non-resource Taxes-to-GDP: Stand-Alone Reforms**

	(1)	(2)	(3)	(4)
	Nearest-neighbor matching	Three nearest-neighbor matching	Radius	Kernel
<b>Panel A. Treatment effect of LTU adoption</b>				
Average Treatment Effect	0.353 (0.729)	0.758 (0.640)	0.517 (0.408)	0.545 (0.407)
No. of treated	268	268	268	268
No. of controls	603	603	603	603
Observations	871	871	871	871
<b>Panel B. Treatment effect of VAT adoption</b>				
Average Treatment Effect	1.855*** (0.629)	2.043*** (0.548)	1.814*** (0.360)	1.813*** (0.360)
No. of treated	364	364	364	364
No. of controls	475	475	475	475
Observations	839	839	839	839
<b>Panel C. Treatment effect of SARA adoption</b>				
Average Treatment Effect	4.529*** (0.807)	3.585*** (0.659)	2.634*** (0.535)	2.805*** (0.531)
No. of treated	169	169	169	169
No. of controls	735	735	735	735
Observations	904	904	904	904

Note: Observations are matched on the 'common support'. An Epanechnikov kernel is used for kernel matching. Bootstrapped standard errors are reported in parentheses. They are based on 100 replications of the data. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

In Table 4, we investigate further the impact of tax reforms by focusing on the interactions among them. We build various feasible combinations of reforms in place and trace the effects on tax revenue. For example, we ask whether having both the VAT and the LTU makes a difference compared to the alternative group of only one of those two reforms, no reform at all, or only a SARA. Again, the control group in such a specification is not the pure case of any reform at all, but observations where a maximum of one reform is put in place.

**Table 4. Estimates of Treatment Effect on Non-resource Taxes-to-GDP: Reforms Combinations**

	(1) Nearest- neighbor matching	(2) Three nearest- neighbor matching	(3) Radius	(4) Kernel
<b>Panel D. Treatment effect of LTU-VAT adoptions</b>				
Average Treatment Effect	-1.912** (0.831)	-1.533** (0.737)	-1.645*** (0.465)	-1.616*** (0.469)
No. of treated	159	159	159	159
No. of controls	745	745	745	745
Observations	904	904	904	904
<b>Panel E. Treatment effect of VAT-SARA adoptions</b>				
Average Treatment Effect	5.702 (3.499)	4.608 (3.003)	6.410*** (2.243)	6.538*** (2.209)
No. of treated	19	19	19	19
No. of controls	881	881	881	881
Observations	900	900	900	900
<b>Panel F. Treatment effect of SARA-LTU adoptions</b>				
Average Treatment Effect	1.434 (2.200)	1.154 (1.914)	-0.88 (1.312)	-0.934 (1.317)
No. of treated	6	6	6	6
No. of controls	898	898	898	898
Observations	904	904	904	904
<b>Panel G. Treatment effect of all reforms</b>				
Average Treatment Effect	0.329 (0.927)	1.202* (0.711)	2.394*** (0.412)	2.403*** (0.410)
No. of treated	94	94	94	94
No. of controls	797	797	797	797
Observations	891	891	891	891

Note: Observations are matched on the 'common support'. An Epanechnikov kernel is used for kernel matching. Bootstrapped standard errors are reported in parentheses. They are based on 100 replications of the data. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1.

The econometric results suggest that while the VAT alone has a significant and positive average impact, the effect of VAT-LTU combination is negative with respect to no reform or to SARA alone. Two interpretations are possible. First, SARAs are so successful that their inclusion in the control group outweighs the positive impact of any other reform or combinations. The second interpretation is that combining VAT and LTU is counter-productive when LTU is poorly designed; together, they may promote dualism in the economy instead of favoring formalization: large taxpayers deal among themselves and so do non-large ones. Indeed, we would have expected

some synergies from combining VAT and LTU since the LTU should reinforce the audit of large taxpayers, and the invoice credit mechanism should spread into the rest of the economy through large taxpayers' purchases and sales, allowing the tax administration to collect information on suppliers and their customers. The econometric results do not show the effect of this virtuous circle.

In contrast, the results suggest that any combination that includes the SARA delivers a higher revenue-to-GDP ratio compared to the alternative options, except in Panel C, where we investigate the effect of the couple LTU-SARA. This estimation is less reliable as the number of occurrences in the data where it exists is very limited (only 6 observations). Adopting all 3 reforms is also found to significantly increase revenue (Panel D).

#### **4.2. Synthetic Control Method for Comparative Case Studies**

We apply here the synthetic control method (SCM) developed by Abadie and Gardeazabal (2003) and extended in Abadie, Diamond, and Hainmueller (2010) to investigate the effect over time of studied tax reforms. We construct a weighted combination of potential control countries, the synthetic control, to approximate the most relevant characteristics of the country affected by the intervention (the tax reform). After the tax reform takes place in a specific country, the SCM can be used to estimate the counterfactual situation of this country by looking at the outcome trend of the synthetic control. To get a sense of the average treatment effect, averages of deviations between each treated country and its corresponding synthetic control are computed.

The SCM is relatively recent and has become an interesting alternative tool to assess the causal effects of various large scale events or programs. Cavallo et al. (2013) have implemented the SCM to investigate the effects of natural disasters on per capita income. Billmeier and Nannicini (2013) applied the technique to measure the effects of trade liberalization on output. Gathani et al. (2013) use similar methodology to investigate the impact of the introduction of a one-stop shop for business registration on new firm creation.

Within the SCM framework, we ask whether the adoption of the VAT, LTU or SARA in a given economy in year  $T$  leads to higher tax-to-GDP ratio in years  $T+i$  (with  $i \in [1, 10]$ ) compared to similar countries that have not implemented such reforms. The advantage of this approach lies in the transparent estimation of the counterfactual outcome of the treated country, namely, a linear combination of untreated countries. The countries that form the synthetic control unit are selected by an algorithm based on their similarity to the treated country before the treatment with respect to relevant covariates and past realizations of the outcome variable (non-resource tax-to-GDP ratio).

Another important advantage of the SCM is that unlike most of the previous estimators used in the literature, it can deal with endogeneity from omitted variables bias by accounting for the presence of time-varying unobservable confounders. This feature improves on panel models such as fixed effects or difference-in-differences, which can account for only time-invariant unobservable

confounders. A remaining limitation, however, is that economic reforms might be triggered by the anticipation of tax revenue, thus leading to endogeneity from reverse causation.<sup>24</sup>

Appendix tables A2-A4 present the weights associated with the constructed synthetic control country for each country-reform pair. There are a few data limitations. First, the sample used in the synthetic control discards all countries lacking data on all the variables. Second, control countries must not have experienced the studied tax reform at any point of time over the sample period. This restriction seems particularly strict in the case of the LTU or the VAT. As a consequence, the small size of the “donor pool” implies that the dynamic treatment effects could be particularly sensitive to this group of countries and may bias the estimates.<sup>25</sup> In the case of SARAs, the size of the “donor pool” is larger since eight countries adopted this reform over the period 1980-2010. A detailed analysis of the impact of SARA in each country is then possible, but is beyond the scope of our analysis.

Previous data constraints lead us to appreciate the average treatment effect of each reform. We follow the approach of Cavallo et al. (2013), which consists of including overall probability of significance based on permutation techniques among all treated countries. This allows a larger “donor pool” and consequently dampen the sensitivity bias, which arises from country-specific results.

### **Framework.**

The procedure for implementing the SCM is discussed in Appendix B. The mathematical formulation and the generalization of the inference technique are borrowed from Cavallo et al. (2013). The usual statistical significance, based on regression-based standard errors, is not relevant in our case since the uncertainty regarding the estimate of the causal effect of tax reform does not come from uncertainty about the aggregate data. We then have to combine the placebo effects to account for the fact that we are interested in doing inference about the average (normalized) effect estimated across the country-specific comparative case studies of each tax reform.

Following Abadie and Gardeazabal (2003), Abadie et al. (2010), and Cavallo et al. (2013), we use exact inference techniques, similar to permutation tests, to conduct inference in comparative case studies. The important contribution by Cavallo et al. (2013) over previous studies is to extend the idea in Abadie et al. (2010) by generalizing the placebo approach to produce quantitative inference in comparative case studies for the average treatment effect. We follow the same procedure discussed in their study. It consists in using permutation tests to examine the statistical significance of the results: we separately assume that every other country in the control sample for a given

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<sup>24</sup> Another drawback of the SCM, which could be extended to all non-experimental reduced-form causal analysis, needs to be acknowledged (Cavallo et al., 2013): it might well be that other big events that occur simultaneously or after the tax reform took place end up driving the results. This can seriously bias the results upward or downward.

<sup>25</sup> However, the values of the root mean square prediction error are not very large and suggest that the “donor pools” associated with country-reform pairs perform relatively well in aligning the pre-reforms trajectories of the tax-to-GDP ratios.

country exposed to the reform implements a similar (and imaginary) tax reform in the same year. We then produce counterfactual synthetic control for each “placebo control”. These synthetic counterfactuals are then used to calculate the impact of the placebo tax reform in every year following its (non)-occurrence. This allows for assessing whether the effect estimated by the synthetic control for the country exposed to a particular tax reform is large relative to the effect estimated for a country randomly chosen (which was not exposed to the tax reform at all).

### **Results.**

The econometric results are shown in figures 6 to 11. They confirm earlier findings that both VAT and SARA have significant positive effects on non-resource tax revenue (Figures 8 and 10). For example, 5 years after the adoption of the VAT, the non-resource tax revenue-to-GDP is larger than it was at the time of the VAT adoption, whereas it is roughly unchanged in the counterfactual scenario of no VAT adoption. For the SARAs, tax revenue is also larger few years after their implementation compared to both the beginning of the reform and the counterfactual scenario by about 2 percentage points of GDP. These effects are statistically significant as shown in Figures 9 and 11. The LTU reform is not found to exert any particularly significant effect on revenue (Figures 6-7), consistent with earlier findings from the propensity score matching techniques and panel fixed effects<sup>26</sup>.

### **Statistical interferences.**

All previous estimation methods are based on the assumption of no interferences between units (countries).<sup>27</sup> As it is called in the literature, the stable-unit treatment value assumption (SUTVA) requires that the observation on one country is unaffected by the particular assignment of treatments to the other countries. When the SUTVA holds, the effect of the treatment (the tax reform) compares two potential tax revenue-to-GDP responses that the country would exhibit under treatment and under control. If there is interference, then the country has not two, but many potential responses depending on the treatments assigned to other units.

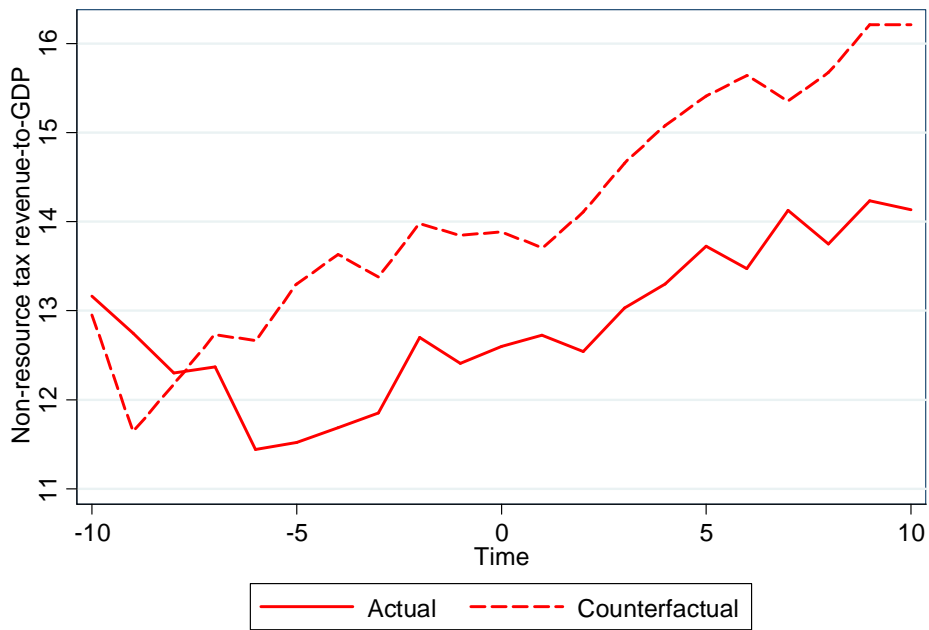
The existence of interferences among neighboring countries does not invalidate our results, since the estimates in the context of the reforms selected are potentially biased downward. Indeed, if one of the studied tax reforms is effective in improving revenue in one country, it is equivalent to an increase in the effective tax rate, and may trigger a positive tax base effect in neighboring countries (by inducing an outflow of mobile factors, such as capital and skilled labor, or cross-border shopping). Hence, neighboring countries may see their tax revenue increase without having engaged in any reforms.

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<sup>26</sup> See appendix available upon request.

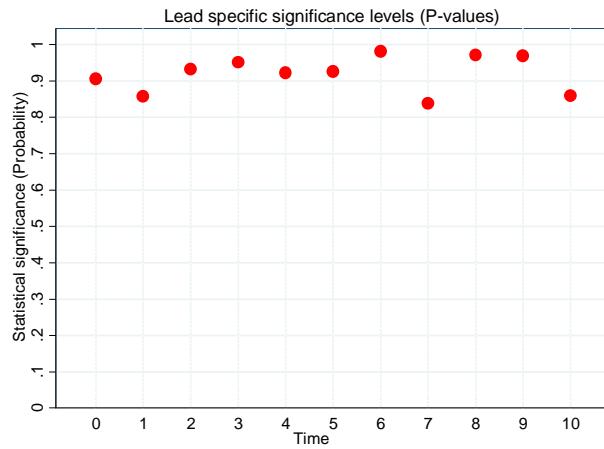
<sup>27</sup> One important type of interference between countries is tax spillovers. IMF (2014) highlights a significant and positive base spillover effect by analyzing the corporate income tax base for 173 countries over 1980-2013. This effect was particularly strong for developing countries.

**Figure 6. Average non-Resource Taxes-to-GDP: LTU Adoption Effect**

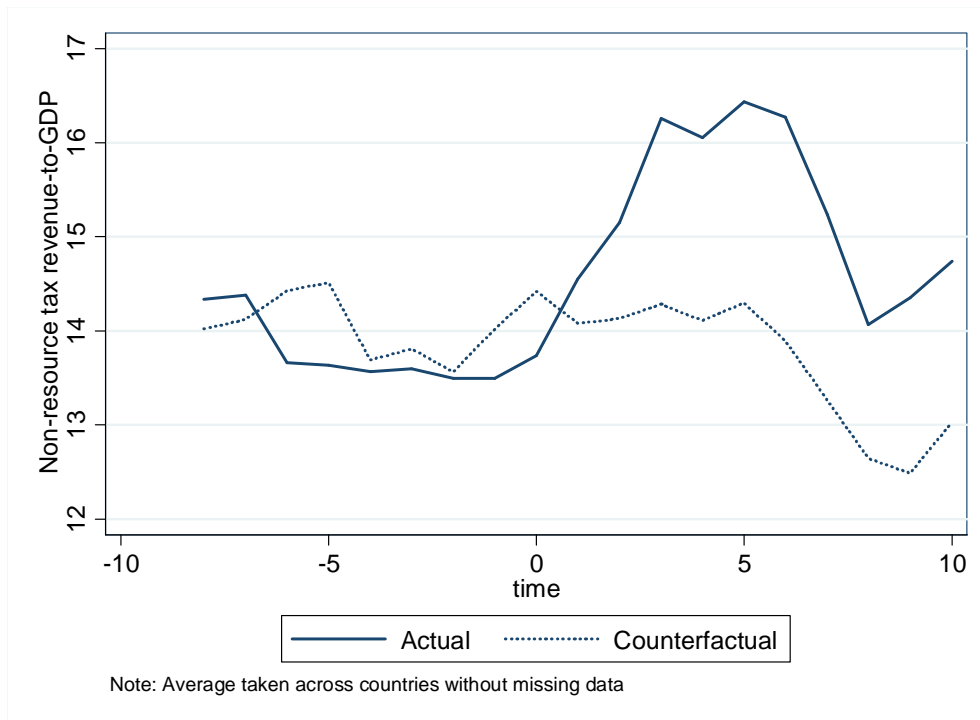


Note: Average taken across countries without missing data.

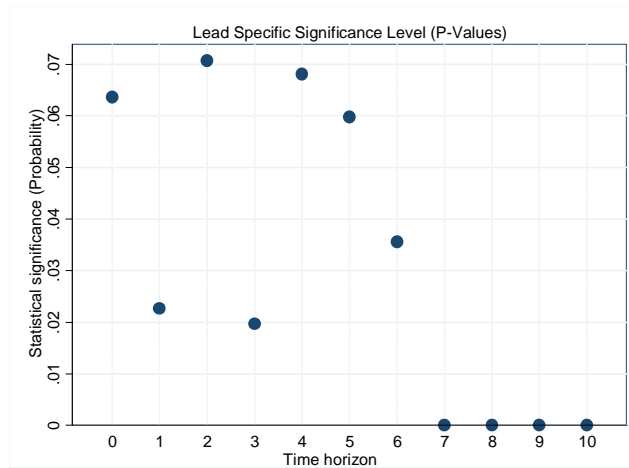
**Figure 7. Significance Levels for LTU**



**Figure 8. Average Non-resource Taxes-to-GDP: VAT Adoption Effect**

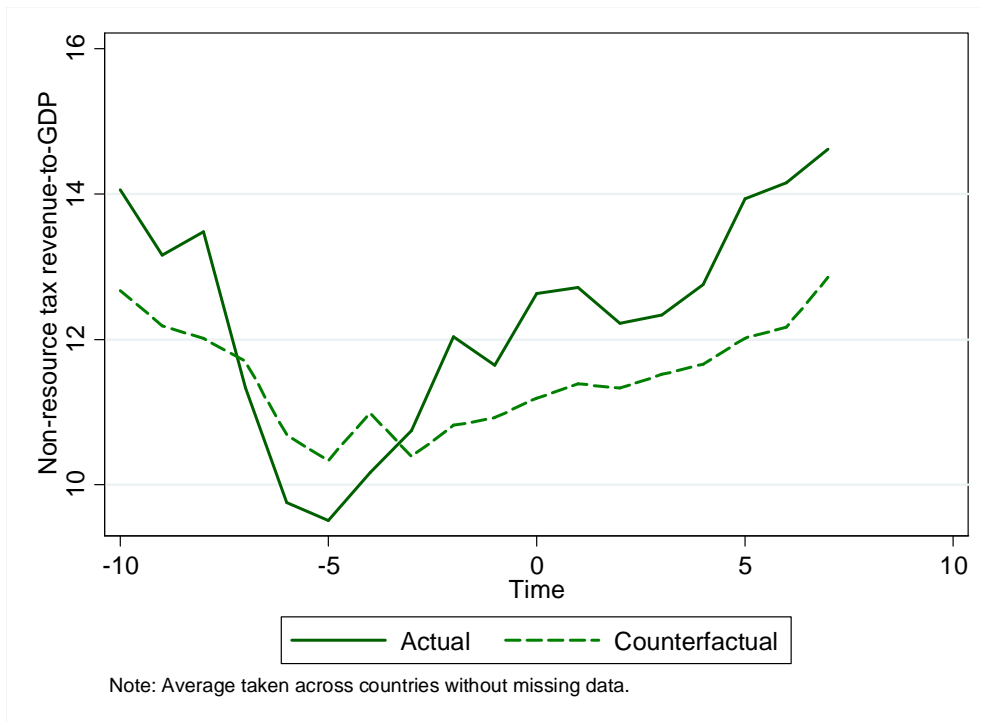


**Figure 9. Significance Levels for VAT**

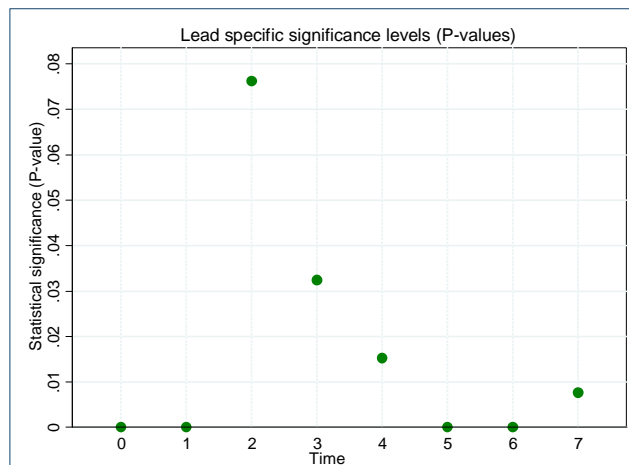




**Figure 10. Average Non-resource Taxes-to-GDP: SARA Adoption Effect**



**Figure 11. Significance Levels for SARA**



## 5. Conclusions

The results in this paper suggest that the adoption of VAT and SARA have a significant and positive impact on the non-resource taxes -to-GDP ratio in SSA, while LTU has a very low and insignificant impact. Moreover, the combination of VAT and SARA reinforces their individual effects on non-resource taxes, which takes several years to fully materialize. These results contrast with Keen and Lockwood (2010), who find that VAT does not increase total tax revenue (including resource revenue) in SSA countries.<sup>28</sup> The exclusion of resource revenue in this paper isolates the volatility of commodity prices and institutional factors, which reflect less the potential impact of the three studied reforms on domestic revenue mobilization.

The absence of a direct impact from LTU adoption on non-resource taxes may seem surprising since size segmentation is a key feature of modern tax administrations—although as noted in the analysis of the probability of adoption of studied reforms, the indirect role of the LTU as a prerequisite for the adoption of VAT or SARA should not be neglected. Two additional reasons may explain the absence of a direct impact of LTUs on non-resource taxes. One relates to the concentration of revenue among large taxpayers in a few economic sectors in SSA countries. This concentration implies that tax administrations focused on large taxpayers even before LTUs were implemented; LTUs simply centralized services to large taxpayers, which may have had an impact on their compliance costs, but limited impact on revenue. A second reason is the use of information in risk management and audit, areas where LTUs may bring little, particularly when accompanying measures are missing, such as better access to information and effective information technology. Anecdotal evidence in SSA suggests that partial LTU reforms still occupy tax administrations today. Simply put, LTUs need to be better designed, including by implementing accompanying reforms simultaneously and not years after LTU is introduced.

Several caveats to the analysis are worth noting. First, the benefits of tax reforms should be weighed against their costs, which differ across the various reforms, but can be significant—e.g, setting up and running SARA,<sup>29</sup> and administration and compliance costs of a VAT.<sup>30</sup> Second, a number of SSA countries recently revised their GDP figures upward, and by a significant margin. These revisions were made after 2010, but may affect published GDP figures for years prior to 2010. To the extent that such revisions impact GDP differently across countries, they could affect the estimates of the impact of the three tax reforms. Third, the reforms considered may yield benefits other than revenue that cannot be captured in our analysis. The economic efficiency gains that can

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<sup>28</sup> Keen and Lockwood (2010) study the probability of VAT adoption and its impact on revenue mobilization over 143 countries for 25 years. They find on average a positive but modest effect, which becomes ambiguous when countries' heterogeneity is taken into account.

<sup>29</sup> SARAs in SSA have been largely supported by donors; their operational costs are estimated at about two percent of revenues collected.

<sup>30</sup> PWC (2010) notes that VAT compliance consumes 125 hours on average (on a worldwide basis) while CIT needs 74 hours. With 135 hours, the African continent is slightly above the worldwide average and below Latin America and the Caribbean, where 192 hours are necessary for VAT compliance (see Barbone et al., 2012 for a review of the cost of VAT).

be obtained from replacing cascading sales taxes by VATs played a key role in their adoption in developed countries, but are generally neglected in SSA, where the majority of countries impose significant limitations on VAT refunds. LTU and SARA may reduce compliance costs to taxpayers by improving the efficiency of their services. They may also improve the perception of equity in the treatment of taxpayers. These potential gains are important and deserve further analysis.

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## **Appendix A. Data Sources and Tables**

The data sources are:

Non-resource tax revenue: Mansour (2014).

Date of adoption of the studied reforms:

LTU: Baer (2002), Fossat and Bua (2013).

VAT: Tax Policy Division tax databases, Fiscal Affairs Department, IMF.

SARA: Kloeden (2011), Fjeldstad and Moore (2009).

GDP and GDP per capita: World Economic Outlook, IMF.

Aid in percent of GNI, Age dependency ratio, Agriculture value added in percent of GDP, Resource rent in percent of GDP, and Trade openness (exports and imports as percentage of GDP): World Development Indicators.



## Appendix B. Theoretical Formulation of the Impact of Tax Reform

We observe  $J + 1$  countries. Without loss of generality, let the first country be the one exposed to a certain type of tax reform (VAT, LTU or SARA), so that we have  $J$  remaining countries that serve as potential controls for each reform case. In comparative case studies, it is assumed that the treated unit (the tax reforming country) is uninterruptedly exposed to treatment after some initial intervention period. In our case, this is indeed the case as there are no cases reform reversals in our sample of countries.

Following Abadie et al. (2010) and Cavallo et al. (2013), let  $R_{it}^N$  be the non-resource tax-to-GDP ratio that would be observed for country  $i$  at time  $t$  in the absence of the tax reform, for countries  $i = 1, \dots, J+1$  and time periods  $t = 1, \dots, T$ . Let  $T_0$  be the number of periods before the disaster, with  $1 \leq T_0 < T$ . Let  $R_{it}^I$  be the tax-to-GDP ratio that would be observed for country  $i$  at time  $t$  if country  $i$  is exposed to a given tax reform its aftermath from period  $T_0 + 1$  to  $T$ . Let  $\alpha_{it} = R_{it}^I - R_{it}^N$  be the effect of the tax reform for country  $i$  at time  $t$  if country  $i$  is exposed to the reform in periods  $T_0 + 1, T_0 + 2, \dots, T$  (where  $1 \leq T_0 < T$ ). We therefore allow this effect to potentially vary over time. More formally, we have:

$$R_{it}^I = R_{it}^N + \alpha_{it}D_{it},$$

where  $D_{it}$  is an indicator that takes the value 1 if country  $i$  is exposed to the intervention (tax reform) at time  $t$  and value 0 otherwise. Let's assume for now that only the country 1 has implemented the tax reform. Our parameters of interest are therefore  $(\alpha_{1,T_0+1}, \dots, \alpha_{1,T})$ , the lead-specific causal effect of the tax reform event on the non-resource tax-to-GDP ratio. Note that  $R_{1t}^I$  is observed and equals to  $R_{1t}$ . Therefore, to estimate  $\alpha_{1t}$ , we need only to come up with an estimate for  $R_{1t}^N$ .

Suppose that there exists a set of weights  $(w_2^*, \dots, w_{J+1}^*)$  satisfying  $\sum_{j=2}^{J+1} w_j^* = 1$  such that:

$\sum_{j=2}^{J+1} w_j^* R_{j1} = Y_{1,1}, \dots, \sum_{j=2}^{J+1} w_j^* R_{jT_0} = R_{1,T_0}$ , and  $\sum_{j=2}^{J+1} w_j^* Z_j = Z_1$  with  $Z_i$  the  $(r \times 1)$  vector of observed predictors for the tax-to-GDP ratio (dated before the reform takes place).<sup>31</sup>

Abadie et al. (2010) suggest using:  $\hat{\alpha}_{1t} = R_{1t} - \sum_{j=2}^{J+1} w_j^* R_{jt}$ , for  $t \in \{T_0 + 1, \dots, T\}$  as an estimator of  $\alpha_{1t}$ . The set of weights  $w$  is obtained by minimizing the distance between the observations of the treated unit  $Z_1$  and the observations for the group of control  $Z_0 w$  during the pre-treatment period.

Recall our lead specific estimates of the tax reform the country of interest (say for now, country 1) are denoted by  $(\hat{\alpha}_{1,T_0+1}, \dots, \hat{\alpha}_{1,T})$  for leads  $1, 2, \dots, T - T_0$ . Now consider taking the average tax reform effect across all  $M$  tax reformers (all countries that have adopted a given tax reform in the sample). Assume for simplicity that for all these  $M$  cases, we are able to compute the  $T - T_0$  lead

<sup>31</sup> This set of covariates is similar to the ones discussed in the previous estimations techniques.

specific estimates of the tax reform impact. Following Cavallo et al. (2013), the estimated average effect for the  $R$  reforms is given by:

$$\bar{\alpha} = (\bar{\alpha}_{T_0+1}, \dots, \bar{\alpha}_T) = \frac{1}{M} \sum_{r=1}^M (\hat{\alpha}_{r,T_0+1}, \dots, \hat{\alpha}_{r,T})$$

The usual statistical significance, based on regression-based standard errors, is not relevant in this case since the uncertainty regarding the estimate of  $\hat{\alpha}_{it}$  does not come from uncertainty about the aggregate data. Uncertainty in comparative case studies with synthetic control is derived from uncertainty regarding the ability of the post-treatment synthetic control to replicate the counterfactual post-treatment in the treated observations. We now discuss how to combine the placebo effects to account for the fact that we will be interested in doing inference about the average (normalized) effect estimated across the country-specific comparative case studies of each tax reform.

We follow Cavallo et al. (2013), who extend the idea in Abadie et al. (2010) by generalizing the placebo approach to produce quantitative inference in comparative case studies for the average treatment effect. We compute the lead-specific significance level ( $p$ ) for the estimated tax reform impact as follows:

$$p_l = \text{Prob}(\hat{\alpha}_{1,l}^{PL} < \hat{\alpha}_{1,l}) = \frac{\sum_{j=2}^{J+1} I(\hat{\alpha}_{1,l}^{PL(j)} < \hat{\alpha}_{1,l})}{\# \text{ of control countries}} = \frac{\sum_{j=2}^{J+1} I(\hat{\alpha}_{1,l}^{PL(j)} < \hat{\alpha}_{1,l})}{J},$$

where  $\hat{\alpha}_{1,l}^{PL(j)}$  is the lead  $l$ -specific effect of a tax reform when control country  $j$  is assigned a placebo tax reform at the same time as the treated country 1.  $\hat{\alpha}_{1,l}^{PL(j)}$  is computed following the same procedure outlined for  $\hat{\alpha}_{1,l}$ .

To conduct valid inference for  $\bar{\alpha}$  we follow Cavallo et al. (2013) and we use the following steps:

- (i) For each case of specific tax reform  $r$  of interest, we compute all the placebo effects using the available controls  $j_r = 2, \dots, J_M + 1$  corresponding to the specific tax reform  $r$ .<sup>32</sup>
- (ii) At each lead, we compute every possible placebo average effect by picking a single placebo estimate corresponding to each case of a particular reform  $r$  and then taking the average across the  $R$  placebos. There are many possible placebo averages:

---

<sup>32</sup> We were not able to construct valid counterfactuals for all the countries that have implemented a given tax reform in our sample (there are cases of tax reform for which we could not match the pre-event tax-to-GDP trajectory to that of a synthetic control group due to the large value taken by the root mean square prediction error, RMSPE). These cases are simply discarded from the analysis. Thus, discarding from the analysis the unmatched events is similar to confining the analysis to the "common support" when using matching estimators.

$$N_{\overline{PL}} = \text{Number of possible placebo averages} = \prod_{r=1}^M J_r.$$

We index all these possible placebo averages by  $np = 1, \dots, N_{\overline{PL}}$ . This number grows very quickly in  $R$  and the typical  $J_r$ .

- (iii) We rank the actual lead-specific average tax reform effect  $\bar{\alpha}_l$  in the distribution of  $N_{\overline{PL}}$  average placebo effects (this involves  $N_{\overline{PL}}$  comparisons).

We compute the lead  $l$  specific p-value for the average effect as:

$$\begin{aligned} p_l &= \text{Prob}\left(\frac{1}{M} \sum_{r=1}^M \hat{\alpha}_{r,l}^{PL} < \bar{\alpha}_l\right) = \text{Prob}(\bar{\alpha}_l^{PL} < \bar{\alpha}_l) \\ &= \frac{\sum_{np=1}^{N_{\overline{PL}}} I(\bar{\alpha}_l^{PL(np)} < \bar{\alpha}_l)}{\# \text{ of possible placebo averages}} \\ &= \frac{\sum_{np=1}^{N_{\overline{PL}}} I(\bar{\alpha}_l^{PL(np)} < \bar{\alpha}_l)}{N_{\overline{PL}}}. \end{aligned}$$

## Appendix C. Tables. Selected Statistics and Synthetic Control Weights

**Table A1. Non-resource Taxes-to-GDP and Adoption Dates of Studied Reforms**

Country	Non-resource taxes-to-GDP (%)		Change 1980-2010	Date Reform Adopted		
	1980	2010	(%)	LTU	VAT	SARA
Angola 1/	5.0	8.8	75.0			
Benin	8.2	16.3	98.1	1995	1991	
Botswana	13.4	18.2	36.4		2002	2003
Burkina Faso	8.4	13.0	54.1	2004	1993	
Burundi	12.6	18.0	42.9	2003	2009	2010
Cameroon	10.9	11.5	6.1	2004	1999	
Cape Verde	11.0	19.1	73.5		2004	
Central African Republic	12.4	8.1	-34.7	1998	2001	
Chad 1/	2.4	9.9	307.4	1997	2000	
Comoros	8.8	11.1	26.2	1994		
Congo, Republic of	9.6	7.5	-21.7	1997	1997	
Cote d' Ivoire	21.7	17.4	-19.9	1997	1960	
Equatorial Guinea	16.7	1.8	-89.0		2005	
Ethiopia	8.6	12.7	47.4	2001	2003	1997
Gabon	12.6	12.3	-2.4	2007	1995	
Gambia	13.0	12.0	-7.5		2003	2005
Ghana	4.2	13.2	217.8		1998	1985
Guinea	5.5	10.9	96.9	1995	1996	
Guinea-Bissau	6.8	8.3	21.8			
Kenya	13.9	18.3	31.8	1998	1990	1996
Lesotho	27.2	36.8	35.0		2003	2001
Madagascar	16.1	10.7	-33.6	1996	1994	
Malawi	14.3	17.4	21.9		1989	2000
Mali	9.2	12.4	34.7	1994	1991	
Mauritius	17.3	18.8	8.2		1998	2005
Mozambique	7.5	18.1	142.2		1999	2006
Namibia 1/	18.5	25.1	36.3		2000	
Niger	12.3	12.3	-0.3	1998	1986	
Nigeria	5.0	8.9	76.2		1994	
Rwanda	9.6	12.0	25.0		2001	1998
São Tomé and Príncipe	15.1	16.6	9.8			
Senegal	17.4	18.1	3.7	2001	1980	
Seychelles	30.7	31.2	1.7			
Sierra Leone	12.9	11.7	-9.3			2003
South Africa	12.6	21.9	73.3	1997	1991	1997
Swaziland	15.7	24.4	56.0			
Tanzania	14.2	15.8	12.0		1998	1996
Togo	26.9	15.7	-41.7	1995	1995	
Uganda	2.7	11.8	340.5	1998	1996	1992
Zambia	23.0	16.4	-28.8		1995	1994
Zimbabwe	16.3	29.6	81.5		2004	2001

1/ First year of the database: Amgola (1996); Chad (1982); Namibia (1982).

**Table A2. Synthetic Control Weights When Assessing LTU Impact**

	Benin (1995)	Burkina Faso (2004)	Cameroon (2004)	Comoros (1994)	Congo, Republic of (1997)	Cote d' Ivoire (1997)	Ghana (1998)	Kenya (1998)	Madagascar (1997)	Malawi (2000)	Mali (1994)	Mauritius (2005)	Rwanda (1998)	Senegal (2001)	Tanzania (2001)	Togo (1995)	Zimbabwe (1994)
Botswana	0.513	0.047	0	0	0	0	0	0	0.068	0	0	0.534	0	0.118	0	0	0
Burundi	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Cape Verde	0	0	0	0.775	0	0	0.125	0	0	0	0.407	0	0	0.343	0	0	0
CAR	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Chad	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Equatorial Guinea	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Gabon	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Gambia	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Guinea	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Lesotho	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Mozambique	0.283	0.564	0.517	0.108	0	0.386	0.385	0.200	0.744	0.677	0.593	0	0.794	0.353	0.979	0.617	0.295
Namibia	0	0.023	0	0	0.398	0.614	0	0.416	0	0.309	0	0.466	0	0.186	0.021	0.383	0.705
Niger	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Sierra Leone	0.203	0.366	0.483	0.117	0.602	0	0.490	0.384	0.188	0.015	0	0	0.206	0	0	0	0
Swaziland	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Uganda	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
RMSPE	1.955	2.105	2.546	1.355	4.806	3.417	2.343	2.281	2.433	2.890	2.261	3.142	3.110	1.732	2.751	6.454	3.708

Notes: CAR: Central African Republic. Countries with missing data are excluded to allow the convergence of the estimation process, and compute the weights.

**Table A3. Synthetic Control Weights When Assessing VAT Impact**

	Benin (1991)	Botswana (2002)	Burkina Faso (1993)	Cameroon (1999)	Cape Verde (2004)	Central African Republic (2001)	Ghana (1998)	Guinea (1996)	Kenya (1990)	Lesotho (2003)	Malawi (1989)	Mali (1991)	Mauritius (1998)	Mozambique (1999)	Namibia (2000)	Tanzania (1998)	Zimbabwe (1995)
Burundi	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Chad	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Comoros	0.720	0.703	0	0.513	0.847	0.236	0	0.128	0.445	0.128	0.857	0.975	0.760	0.727	0	0.701	0.717
Congo, Republic of	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Cote d' Ivoire	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Equatorial Guinea	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Ethiopia	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Gambia	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Madagascar	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Malawi	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Niger	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Rwanda	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Senegal	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Seychelles	0	0.031	0.097	0	0.153	0	0.061	0	0.178	0.872	0.143	0	0.240	0	0.543	0.009	0.283
Sierra Leone	0.280	0.266	0.903	0.487	0	0.764	0.939	0.872	0.376	0	0	0.025	0	0.273	0.457	0.29	0
Swaziland	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Togo	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Uganda	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
RMSPE	1.692	2.422	2.074	2.496	3.346	3.202	2.866	4.295	0.902	4.818	1.318	1.720	2.036	2.428	2.805	2.98	2.993

**Table A4. Synthetic Control Weights When Assessing SARA Impact**

	Botswana (2003)	Ethiopia (1997)	Kenya (1996)	Malawi (2000)	Sierra Leone (2003)	Tanzania (1996)	Uganda (1992)	Zimbabwe (1994)
Benin	0	0	0	0	0	0	0	0
Botswana	0	0	0	0	0	0	0	0
Burkina Faso	0	0.266	0	0	0	0	0	0
Burundi	0	0	0	0	0	0	0	0
Cameroon	0	0	0.219	0	0.305	0	0.580	0
Cape Verde	0.126	0	0	0	0	0	0	0
CAR	0	0	0	0	0	0	0	0
Chad	0	0	0	0	0.428	0	0.420	0
Comoros	0.510	0	0	0	0	0.089	0	0
Congo, Republic of	0	0	0	0	0	0	0	0.103
Cote d' Ivoire	0	0	0.436	0	0	0	0	0
Equatorial Guinea	0	0	0	0.263	0	0	0	0
Gabon	0	0	0	0	0	0	0	0
Ethiopia	0	0	0	0	0	0	0	0
Gambia	0	0	0	0	0	0	0	0
Ghana	0	0	0	0	0	0	0	0
Guinea	0	0	0.030	0	0	0	0	0
Kenya	0	0	0	0	0	0	0	0
Lesotho	0	0	0	0	0	0	0	0
Madagascar	0.365	0.153	0	0	0	0	0	0
Malawi	0	0	0	0	0	0	0	0
Mali	0	0	0	0.489	0	0.705	0	0
Mauritius	0	0	0	0	0	0	0	0
Mozambique	0	0	0.316	0	0.267	0	0	0
Namibia	0	0	0	0.152	0	0	0	0.468
Niger	0	0.581	0	0	0	0	0	0
Rwanda	0	0	0	0	0	0	0	0
Senegal	0	0	0	0	0	0	0	0.046
Seychelles	0	0	0	0	0	0	0	0
Sierra Leone	0	0	0	0	0	0	0	0
Swaziland	0	0	0	0	0	0	0	0
Tanzania	0	0	0	0	0	0	0	0
Togo	0	0	0	0.096	0	0.206	0	0.382
Uganda	0	0	0	0	0	0	0	0
Zimbabwe	0	0	0	0	0	0	0	0
RMSPE	2.618	0.987	2.418	1.951	2.591	1.708	1.224	1.662

Notes: CAR: Central African Republic. Countries with missing data are excluded to allow the convergence of the estimation process, and compute the weights.

*“Sur quoi la fondera-t-il l'économie du monde qu'il veut gouverner? Sera-ce sur le caprice de chaque particulier? Quelle confusion! Sera-ce sur la justice? Il l'ignore.”*

Pascal



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+33 (0)4 73 17 75 30