Do Electronic Filing and Payment Increase Tax Compliance? Evidence from Large Taxpayers in Senegal

Léo Czajka Awa Diouf Fabrizio Santoro

4th December, 2024

Abstract

Tax administrations in low-income countries progressively electronic filing and payment technologies with the aim of reducing both enforcement costs for the administration and compliance costs for taxpayers. This shift also holds the potential to decrease opportunities for collusion or extortion by minimizing in-person interactions with tax officials. However, the available evidence on the impact of such technology remains scarce. Using a fuzzy dynamic double-difference method, we investigate the causal impact of the adoption of e-filing and e-payment by the largest taxpayers in Senegal. Our findings indicate either no, or very limited, impact on the probability to declare or pay. Nevertheless, we observe a significant decrease in the prevalence of missing values in digitized declarations. We also find a positive, and possibly large, impact on tax payments, but essentially concentrated on the largest taxpayers. Later in time, additional evidence finally suggests that by reducing compliance costs, online services enabled previously enrolled taxpayers to continue submitting tax statements even amidst the containment measures implemented in response to the COVID pandemic.

1. Introduction

Technology and good institutions are considered pillars of economic development. In that regard, the recent acceleration in the production and worldwide diffusion of innovations in information technology could contribute to foster the efficiency and transparency of low-income countries' (LICs) administrations, thereby hastening their convergence toward higher living standards. Accordingly, recent evidence shows that governments form low- and middle-income countries have in fact largely invested in the adoption of IT infrastructure. In 2000, only 31 countries had four or more of the nine main governmental management information systems identified under the World Bank's GovTech classification,[1] but by 2022, 163 countries had at least eight of them. More recently, governments have started to increasingly develop online services, building from existing management information systems to allow remote interactions between citizens and various state departments. This includes solutions to declare (*e-filling*) and pay (*e-payment*) taxes online. As of 2016, 32% of LICs had already adopted e-filling (World Bank, 2016). In the context of LICs, where revenues are too low to satisfyingly fund various under-provided public goods (OCDE, 2022), these tools are expected to significantly contribute to increase tax revenue. The underlying assumptions behind such expectations are that e-filling and e-payment reduce both enforcement costs for the administration and compliance costs for taxpayers, and limit opportunities for collusion or extortion by minimizing in-person interactions with tax officials. Relatedly, it is also expected to increase transparency and therefore trust in the fiscal institution. However, such positive impacts for taxation need not to materialise. Indeed, saved costs may be allocated to other objectives than becoming more compliant (for the taxpayers) or strengthening tax enforcement (for the administration). Collusion/extortion related impacts are conditional on the prevalence of such practices ex-ante among taxpayers now having access to online solutions. Finally, increased trust in the tax administration is not necessarily correlated to improved believes regarding state departments in charge of *spending* the collected revenues. Considering this, the consequences of e-filling and e-payment on tax compliance remain an empirical question.

This article provides causal evidence on the impact of jointly adopting e-filling and e-payment services in the context of Senegal. After years of development, this technology (*Etax*) was made mandatory by ministerial decree in July 2017 for all taxpayers monitored by the Large Taxpayers Center. This has induced 80% of the targeted taxpayers to effectively register and use the platform by January 2018. Combining newly collected administrative data from three different monthly tax instruments (value added tax (VAT), withholding tax on wages (PAYE) and withholding on service suppliers (WTS)) with a fuzzy, dynamic, double-difference method, we exploit exogenous variations in e-filling and e-payment adoption generated by the reform to investigate its causal impact on various indicators capturing tax compliance. To improve the comparability of the two groups, we restrict our control group to the subset of its largest taxpayers. While our setting presents some challenges for the identification strategy, it has the advantage of being very representative of how tax administrations would typically deploy new IT technology: starting by the largest taxpayers and gradually expanding to smaller, more

[1] See GovTech Maturity Index (GTMI) Data Dashboard

loosely monitored ones (Ali et al., 2015). In our main specification, our sample is a balanced panel of 1,691 taxpayers observed during the 36 months of 2017-2019. We opt for a strictly balanced panel to avoid possible confusions between real and pure compositional effects.

First we document a short-lived but clear 2.5 percentage point negative impact on the probability to declare just after the ministerial decree. We interpret this as evidence of the transition costs faced both by the taxpayers and the administration to swiftly register hundreds of taxpayers into the new system and adapt previously entrenched practices. However in the following months, we do not find any significant results on the probability to declare. This is despite recovering standard errors that would have allowed detecting a minimum impact of about 2 percentage points.[2] However, the divide seems to widen overtime: point estimates become significant one year and a half after the reform. We find a similar, yet slightly more salient pattern for the probability to declare a positive tax liability. In light of this, we cannot reject that the reform may have had a lagged impact on such probabilities, but we believe this interpretation is rather speculative given the available evidence.

We do find sharp and very consistent 5-7 percentage points impact on the probability to submit a declaration containing missing values. Given that we do not find any impact on other measures of information quality (such as inconsistencies of implausible values), we would find it hard to interpret this as resulting from a change in the taxpayers behavior. Rather, we believe it most likely reflect the fact that e-filling contributes to make visible some values which data entry agents, needed before e-filling, were not taking the time to type in order to perform their task more rapidly.

Finally, the reform seems to have had no impact on average liability declared, nor on the probability to pay (the minimum detectable effect for the later being 3.3 percentage points). Nevertheless our evidence suggests that it did have a significant impact on average payment. Keeping the full sample of treated taxpayers, this impact could be as large as USD 20,000 per month on average. Yet sensitivity tests demonstrate that the magnitude of the estimates largely depend on the presence of extremely large taxpayers. Indeed, removing from the treatment group the largest 5 taxpayers alone divides the estimate by 2. As additional observations get removed, the coefficient eventually converges toward USD 5,000. Under this specification, such an average monthly impact among treated firms would account for about 1.2% of all tax revenues collected in 2018. Unfortunately, available information does not allow to determinate whether this increase in driven by a drop in collusion or an increased capacity to enforce payment recovery.

Overall, our evidence demonstrate that the adoption of e-filling has no or very modest impact

[2]Assuming a 5% significance level and a statistical power of 80%.

on taxpayers' declarative behavior in the short and mid term. This could be due to the fact that, before the reform, larger taxpayers were comparably more compliant along this dimension because they have been historically more scrutinized by the administration. In an alternative application of our dynamic diff-in-diff model, we exploit COVID related containment measures enforced in early 2020 as an exogenous source of variation in the cost of coming in person to administration. We find evidence that taxpayers who had not registered to *Etax* prior to the COVID shock were significantly less likely to submit a declaration afterwards. The timing of the most severe negative responses (- 4 percentage points) corresponds exactly to the months during which restrictions were the strictest, and full recovery takes about 18 month to materialise. By construction of our sample (a balanced panel of taxpayers active beyond our time window) and given that tax declarations can be submitted with delays, such trends cannot be explained by taxpayers permanently shutting down because of the economic downturn. This last result suggest that by lowering the cost of declaring, e-filling did contributed to sustain high rates of declaration even amidst the pandemic.

This paper contributes to the still very scarce the literature the role e-filling and e-payment technology on taxation in LICs. A few studies document the causal implications of electronic billing machines and electronic payment system on tax compliance (Bellon et al., 2022 for Peru, Mascagni, Mengistu, and Woldeyes, 2021 for Ethiopia, Brockmeyer and Sáenz Somarriba, 2022 in Uruguay and Das et al., 2023 for India). However lessons learned from that strand of the literature apply to a technology that digitizes transactions between taxpayers themselves (thus generating a third-party reported paper trail), not between taxpayers and the administration. To our knowledge, there exists only two studies exploring the causal impact e-filling and e-payment on tax compliance in a low/middle income country (Okunogbe and Pouliquen, 2022 for Tajikistan and Amine and Santoro, 2023 for Eswatini). Our study innovates from the later first by investigating the behavior of the largest taxpayers - by far the most strategic sub-sample of taxpayers when considering aggregate tax revenue (Milanez, 2017 Slemrod and Velayudhan, 2018). Second, our use of high frequency monthly data that allows us to document the evolution of treatment impact in the short and mid term. We provide new causal evidence that in the very short term, the impact can indeed be negative due to the transition costs as hypothesized by Scarpini, Okunogbe, and Santoro, 2023. This paper is the first to provide evidence on the positive impact for information quality - a result consistent with findings related to the adoption of electronic billing machines (Mascagni, Mengistu, and Woldeyes, 2021). Finally, despite null or very modest impacts on declarative compliance, our results do suggest that e-payment solutions could be instrumental to increase average effective payment by the largest taxpayers, and thus generate possibly very significant revenue gains.

The rest of the paper is structured as follows: section 2 documents the context, section 3 specifies our assumptions on taxpayers and administrators' behavior as well as the main expected channels, section 4 describes our data and empirical strategy, section 5 discusses the results and section 6 concludes.

2. Institutional Context

The Senegalese Tax Administration (TA) directly monitores about 30,000 taxpayers, one third of which are legal person (including incorporated firms, associations, regulating agencies etc.), others being self-employed individuals. These taxpayers are distributed across 20 tax centres according to business size, sector, and location. Taxpayers with a turnover exceeding USD 3,200,000 for three consecutive years are managed by the Large Taxpayer Centre (LTC), irrespective of their headquarters' location. Smaller taxpayers operating within the region of Dakar are distributed across two medium-sized taxpayers centres (MTC1, MTC2), one unit for firms operating in regulated sectors (RSC) and seven municipal tax centres.[3] The rest of the taxpayers are spread across 11 regional centres.

Since 2006, the TA has adopted and progressively deployed an integrated digital tax management system. This multi-module software allows to store tax declarations, compute tax liability, process payments and produce receipts. Under this system, tax declarations are submitted in paper format, mostly by taxpayers themselves and not by mail, to their respective tax centre, and then digitized into the system by data entry agents, and possibly corrected afterwards in case data entry mistakes had been identified. Taxpayers would also have to come to their tax centre in order to pay, mostly in cash or cheque.

Starting in early 2017, the TA began to implement and test a new software (Etax), geared to the existing IT infrastructure, allowing taxpayers to declare online (e-filling) and pay online (e-payment). In July 2017, the Senegalese Minister of Economics and Finance released a decree making mandatory for taxpayers monitored by LTC to use Etax. Most of them adopted it in early January 2018, the rest gradually joining by the end of 2018. During the Fall of 2018, firms from the upper-middle taxpayers centre (MTC1) were also embarked into the new system. About 80% joined, even though there was no legal obligation to do so. The adoption of Etax was subsequently encouraged among taxpayers from two other centres (MTC2 and RSC), but with very limited take-up. The reform had multiple objectives: i) spare taxpayers the cost of moving

[3] Regulated sectors are medical and legal professions, accountants, architects, state agencies and N.G.O.. Firms within the MTC1 (MTC2) have a turnover between USD 1,600,000 and USD 3,200,000 (USD 800,000. USD 1,600,000). Sector prevails over size for fiscal assignment. Darkar's seven municipal centres are Dakar-Plateau, Ngor-Almadies, Parcelles Assainies, Dakar Liberté, Grand Dakar, Pikine-Guédiawaye and Rufisque.

to their centre to declare and/or pay, ii) spare the administration the cost of handling all paper declarations, iii) improving the quality of the information by removing mistakes introduced by data entry agents. Tax officials report that, especially in the first years of implementation, crashes of the platform due to network saturation close to fiscal deadlines sometimes prevented taxpayers to declare and or pay on time. Nevertheless, as of 2022, the new infrastructure was considered to be robust enough to start systematically embarking more and more tax centres.

In order to strictly conform to the declarative nature of the tax law, e-filling through Etax mimics as closely as possible paper declaration: only identification information are pre-filled, inconsistencies as well as missing values are allowed and not automatically flagged. There is no automatic reminder for missing declaration. With a few minor exceptions, all type of taxes can be declared through Etax. Tax liability is then automatically computed by the system based on the information reported by the taxpayer. In case a taxpayers realises it has made a mistake, it must address a correction request which is then handled by the tax centre (taxpayers cannot correct their declaration themselves). Taxpayers can consult their data at all time. Then e-payment occurs through the banking system and as such requires taxpayers to have and share a bank account number. In that regard, tax officials report that some taxpayers of the LTU were initially reluctant to adopt Etax due to concerns about sharing their bank account details and that this was part of the motivation for releasing a ministerial decree.

3. Assumptions and Expected Impacts

The introduction of e-filling and e-payment is likely to affect tax compliance through multiple mechanisms, both positively, or negatively, with immediate effect or only in the long run. In this section we synthesize what mechanisms we believe should play a role in this study, based on our understanding of the context and the technology's specifics, theoretical assumptions on the behavior of the various agents involved as well as available evidence from the previous empirical evidence.

3.1. General behavioral assumptions

On the behavior of taxpayers: In accordance to the now classic deterrence model for tax evasion first developed by Allingham and Sandmo, 1972, we assume that taxpayers mostly act as free-riders. In other words, absent any risk associated with tax evasion (bad reputation or legal sanctions), economic actors would simply not pay taxes. This assumption is broadly aligned also with the existing empirical literature whose result mostly converge to the conclusion that, at least in the short-term, deterrent measures are significantly more effective at inducing compliance than those interventions appealing to the intrinsic value of tax morale, or the need to fund public goods (see Luttmer and Singhal, 2014 and Slemrod, 2019).

On the behavior of tax collectors: Following a now rich corpus of work that builds form the seminal work of Weber, 1922 we assume that the principal-agent model is an adequate framework to reflect on the behavior of bureaucrats. In our specific setting, the principal will typically be a higher-tier bureaucrat (or a minister up the chain), and the agents will be civil servants under her supervision, in charge of performing tasks contributing to tax collection, the final objective of the fiscal administration. The agents decide upon the level of effort they invest to perform the tasks they have been assigned to, and in most cases it is reasonable to assume that this level is not observed by the principal. The later therefore needs to set up the right incentives (promotion/relegation, bonus/sanctions) based on some measurable aspects of the agents' work, either input (e.g. number of hours worked) or output (e.g. number of reminders sent to late taxpayers) - see Besley et al., 2022 for a formal exposition of this general model. The impact of such incentives on the accomplishment of the administration's mission will vary depending on the principal's capacity to accurately measure relevant inputs/outputs and on the agents' intrinsic motivation to fulfill the tax administration's objective - and mis-designed incentive schemes can even become detrimental to the objectives (Rasul, Rogger, and Williams, 2018).

3.2. Expected impacts

Saving paper processing related costs: As detailed in section 2, before the introduction of Etax, all declarations were submitted on paper format. Every month, several agents of the LTC had to collect such declarations, digitize them into the central system and classify them into each taxpayers' physical folder. Beyond a possible transition period, the reduction in the cost to process tax declaration should inevitably free-up some time for agents of the LTC who previously participated to this activity. Unlike for minor organisational change, such gain will inevitably be observed by these agents' principal and we should expect the agents to be given replacing tasks. Such reallocation could result in increased monitoring of LTC's taxpayers, which in turn may improve tax compliance (see section 4.1 for a discussion on how to measure tax compliance). Yet this is not straightforward, in particular because reallocation could take place across tax centres, especially if the personnel that has acquired expertise in processing paper is needed in other centers where e-filling has not yet been adopted.

Improving information quality and availability: Paper based declarative system requires a lot of man power, but also carries important informational risks: declarations could be lost (before or after having been digitized) and incorrect values could be entered, intentionally or not, during the digitization process. In light of this, we should expect the introduction of e-filling to eradicate at least mistakes introduced during data entry. It may also reduce the share of seemingly missing declarations, as the later cannot be lost anymore. But maybe the most important implication of e-filling is that taxpayers and administrators share, and know that they share, the same information about what has been declared. This implies that a taxpayers knows which inputs had to be used to compute its liability. The impact of that change on observed indicators is hard to predict as it depends on whether taxpayers were previously over paying or not because of incorrect values introduced during the digitisation but which we cannot identify in the data. We would expect it to at least increase taxpayers' consent to pay on average. Another implication of the sharing of information is that tax inspectors now know that what they reads in the declaration corresponds to what the taxpayer has effectively declared. Given this, they can more confidently hold taxpayers accountable for any errors found in the declaration, or any contradictions with other sources (as third-party reported revenues for instance). In other words, it facilitates tax inspectors' job. Depending on their efforts to perform their tasks correctly (either through intrinsic motivation of though incentives provided by their principal), this could contribute to increase tax enforcement, and therefore tax compliance.

Saving transport related cost: Form the point of view of the taxpayer, the possibility to declare and pay online obviously allows to save time and transport related expenditures. This is a desirable outcome per se, it constitute one the main rationale to adopt such technology. Given our assumptions on taxpayers' motivations, we expect this could induce some taxpayers, who otherwise would have not declared, to declare - but only to the extent that i) they perceive a positive risk for not declaring, and ii) previously faced transport related costs were large enough for them to take that risk. In other words, this mechanism should only impact these previously *almost declaring* taxpayers, and is unlikely for instance to contribute to embark taxpayers who perceive very little risk associated with not declaring. In our setting, given that treated taxpayers consist of the largest firms of the country, it is reasonable to assume that such costs represent a very small share of revenue and therefore that this mechanism should play little role.

Sharing bank ID: Online payment requires taxpayers to share their bank identity to the administration, a step they were reluctant to take (see 2). Indeed, with their bank identify at hand, the tax administration can more easily activate a legal procedure (administrative seizure to third party holder) that forces banks to pay the tax administration any outstanding tax liability a given taxpayer owes - provided there is sufficient provision in the account. The availability of banking information should therefore limit taxpayers' leeway to resist tax recovery

- a phenomenon that can be pervasive in low-income countries (Best, Shah, and Waseem, 2021).[4] We expect tax payments to increase through this channel for taxpayers previously resisting tax recovery.

End of cheques: Before e-payment, taxpayers paid in cash or cheque, cheque being more frequent for larger amounts. In both cases the official payment receipt is provided by the tax administration (as a proof of tax payment), upon the reception of the cash or the cheque, which are then transferred to the treasury. An issue with cheques however is that they mostly do not come with a guaranty for sufficient provision in the taxpayer' bank account. According to tax officials, it is not uncommon (even though we do not have data on the exact frequency), for cheques to be returned, by the treasury to the tax administration, because they are (partially) unfunded. When this happens, the tax administration needs to invalidate the initial payment receipt, reach out to the taxpayer and have it pay again. Exploiting this weakness of the payment system is a way for taxpayers to at best defer payment, at worst resist payment to the point they may negotiate collusion-based rebate. As this cannot happen anymore with e-payment, we expect tax payments to increase through this channel for taxpayers that most used such strategy before the reform.

Limiting in-person interactions: In person interactions between bureaucrats and taxpayers may affect tax collection in several ways. In the worst case scenario, they can collude to the detriment of the government: taxpayers may reduce their effective liability in exchange of paying the bureaucrat a share of the implied gain. In that case, in-person interaction reduces tax compliance as well as tax revenue. Inversely, tax officials may also force taxpayers to pay more than they owe to the state in order to meet their own tax revenue target, and perhaps avoid a sanction or receive a bonus. In that case, tax compliance, understood as distance from what should be, is reduced as well but through over-payment. Suggestive evidence for both channels have been found by Okunogbe and Pouliquen, 2022. Considering this, it is hard a priori to predict in what direction should declared liability and payment go following the adoption of online services. However, in the context of this study, given that treated taxpayers are extremely large we expect the bargaining power to be in their favor when interacting in person with a tax official. Hence we suspect over-payment to be rare and expect that the adoption of Etax would increase tax compliance and payment among previously colluding firms.

[4] Note that this procedure is strictly codified and requires the agreement of several officials to be binding, therefore limiting opportunities for tax official to abuse their power. Similar institutional constraints are in place for fines and sanctions as well.

4. Data and Empirical Strategy

4.1. Data

We assemble four datasets, all of which are produced by the tax administration along its operational routine. In addition to the source specific information listed below, each declaration/payment comes with an identification number, the name of the centre it was recorded by, and whether it was processed through Etax or not. We use this information to identify taxpayers' fiscal centre and trace their effective use of the e-filling and e-payment. We do no have data on the date of the declaration.

> *Value added tax (VAT)*: includes information on total sales, sales subject to VAT, sales exempt from VAT, exports, gross VAT due, deductible VAT (excluding past VAT credit)
>
> *Withholding taxes on employees (PAYE)*: includes information about the number of employees, total wage bill and total liability declared.
>
> *Withholding taxes on informal service suppliers (WTS)*: includes information on the total amount spent in exchange of services from informal suppliers, and the total liability declared (which should always be 5 % of the amount).
>
> *Payment data*: this data set was extracted separately and includes information on the amount paid, the type of tax and the fiscal period it applies to, the payment date and the mode of payment.

Tax compliance is inversely proportional to the difference between what a taxpayer *should do* (according to fiscal law) and what it *actually does*. As such, it is captured by a collection of indicators: the probability to declare, the probability to pay when a liability is due, the probability to perform these acts on time, the difference between *declared* and *real* tax base/liability, and finally the difference between tax *effectively paid* and tax *really due*. Such indicators can be hard to measure directly because most of what we observe is *reported*, therefore subject to manipulation or mistakes, and not a direct measure of the reality. Considering this, the probability to declare is probably the indicator that can most unequivocally capture tax compliance, because we know a declaration is due every month. It is still imperfect however as some declaration might be missing only because they were not digitized ex-ante (e.g. submitted but lost). Assuming taxpayers prefer to not pay tax (see section 3) all observed increase in indicators implying that taxpayers pay more taxes, or could be subject to higher taxes is indicative of improvement in tax compliance as long as it does not stem from an increase in real activity or extortion by tax officials. Under these conditions, increases in declared tax base/liability, tax rate, or effective payment should reflect increase in tax compliance.

In addition, for each declaration, we can compute binary indicators encoding whether values are missing, inconsistent or implausible. We consider a declaration has a *missing* value if a positive liability is associated with the declaration but at least one variable which should be positive whenener the liability is positive (e.g. total wage bill for PAYE) is missing or equal to 0. We consider a declaration to have *inconsistent* values if some of its *positive* values are inconsistent. Last we consider a declaration to have *implausible* values, if some its *positive* values is above some variable specific extreme value.

4.2. Selection

We select taxpayers that have started declaring and/or paying before January 2017 and that have continued doing so beyond December 2019. In other words, we restrict to a set of taxpayers which we know existed before and after this time window[5]. Unobserved declarations within that period are considered missing - we therefore "fill the gaps" to impute the corresponding missing observations in our dataset. By construction, this allows to produce a balanced panel of taxpayers from the beginning of 2017 until the end of 2019.

We cannot satisfyingly exploit data declared much before 2017 because a major update of the IT system in late 2016 caused a breach in the use several fiscal ID, thus preventing us from identifying firms across the update. When studying the impact of the introduction of Etax, we also prefer to not exploit data beyond 2019, as the COVID pandemic spreads to Senegal in early 2020, affecting taxpayers differently depending on whether or not they had already adopted Etax before (a phenomenon we document in the second part of our results). We define the treated group as all firms that have consistently been monitored by the LTC during the reference period. Figure 1a and 1b show that first registrations and use took place in September 2017 and sharply increased afterwards. By February 2018, 80% of the LTC's taxpayers had used Etax at least once, and each month 85% of taxpayers submitting a monthly declarations used Etax for it, and 80% of taxpayers paying some positive of amount of taxes did so through Etax. The adoption took place quickly enough to allow for a credible impact evaluation, and by the end of the period nearly all LTC taxpayers of this selected sample were registered, effective, users of the platform. Yet, a non trivial share of the treatment group did not register in the first half

[5]Start and end date are not measured well enough by the administrations to identify *active* taxpayers in a less indirect way. This is essentially because of the prevalence of informality: economic actors can register to the tax administration years after having started being active, and inversely, they may become inactive from the point of view of the TA, and continue to do business, sometimes relocating.

of 2018. As a control group, we use all firms monitored by tax centers in the region of Dakar that did not adopt etax - including MTC2, and RSC where a small proportion of taxpayers did adopt Etax during the second half of 2018, probably on a voluntary basis (see Figure 1a and 1b). We account for such imperfect compliance to the treatment in our empirical strategy by instrumenting Etax registration on pre-reform centre affiliation (see section 4.3).

After this preliminary selection, the full sample consists 5,694 taxpayers, 831 (15.6%) of which are treated. By construction, treated and control taxpayers are very different. The main forcing variable which the administration uses to allocate firms to the large taxpayers centre is the turnover. Figure 3a, displays the empirical distribution of taxpayers in each group with respect to their turnover, indicating the threshold (XOF 1,000,000,000) above which taxpayers ought to declare to the LTC. Graphical evidence suffice to discard the possibility to apply any regression discontinuity design, threshold is not strictly binding and the density around the threshold is too low. To make our sample as comparable as possible without loosing too much statistical power, we restrict our control group to its largest 860 taxpayers (measured by average pre-reform turnover). This generates our default dataset, with a total of 1,691 taxpayers. Despite significant trimming of the initial control group, average taxpayers size still differ significantly across groups as show in figure 4. For all of our main indicators, we address this issue by running sensitivity tests on 40 different sub samples obtained by incrementally removing the largest 5 taxpayers from the treatment group.

4.3. Empirical Strategy

We estimate the impact of the reform by comparing taxpayers monitored by the Large Taxpayers Centre (LTC) to other taxpayers of Dakar, before and after the use of Etax was made legally mandatory for the LTC (see section 4.2 for a precise definition of treatment and control group). Treatment adoption is set at the centre level, therefore the first treated period is the same across all taxpayers and after this month we consider *effectively treated* any taxpayer monitored by the LTC, irrespective of whether it actually uses e-filling and or e-payment technology. Considering this, our first estimates will capture the *intention-to-treat* of adopting e-filling and or e-payment at the centre level onto its taxpayers. By construction of the sample, treatment status is also an absorbing state. Given these initial specifications, and following Roth et al., 2023 and Miller, 2023, we can estimate the impact of the reform using an event-study model (or dynamic diff-in-diff model) with this functional form :

$$y_{it} = \alpha + \sum_{k=1}^{K} \beta^k \cdot Lead_{it}^k + \sum_{j=-2}^{J} \gamma^j \cdot Lag_{it}^j + \delta_i + \lambda_t + \mu_c + \epsilon_{it} \tag{1}$$

The statistical unit is the taxpayer (i) at the month level (t), with $J \leq t \leq -1$, or $1 \leq t \leq K$. Variables Lag_{it}^{j} and $Lead_{it}^{k}$ are binary indicators equal to one for an observation of a treatment group taking place respectively j months *before* the adoption of Etax and k months *after*. $Lead_{it}^{1}$ indicates the first treated month, while Lag_{it}^{-1} (the last untreated month) is omitted from the equation 1 to serve as baseline. Each β^{k} captures the impact of the reform exactly *k-months* after its introduction compared to baseline ($t = -1$) and is equivalent to the β coefficient one can retrieve by estimating a simple diff-in-diff model with only two periods, either before treatment ($t = -1$) or after ($t = k$). The coefficient γ^{j}'s are similar but for the period *before* the reform, and act as placebo tests. The coefficients δ_i λ_t and μ_c are individual, time and centre fixed-effect, and ϵ_{it} is the unobserved error term.

With this specification, we can provide graphical evidence on the evolution of the impact by plotting γ^{j}'s and β^{k}'s coefficients across time. Then to summarize the overall impact for the entire post-reform period observed into one single coefficient, we group all observations taking place after the reform and estimate the following 2x2 diff-in-diff model with fixed effect :

$$y_{it} = \alpha + \beta \cdot Treat_{it} \cdot Post_{it} + \delta_i + \lambda_t + \mu_c + \epsilon_{it} \tag{2}$$

where $Treat_{it}$ and $Post_{it}$ are indicators for being in treated centre, and observed after the reform respectively. When estimating 2 we restrict our sample so that $-1 \leq t \leq K$, thus keeping the same baseline as in our dynamic specification. Smilarly, a backward version of model 2 allows to estimate a general γ which provides a summary test for the parallel pre-trends.

As for any diff-in-diff model, the underlying identification assumption requires that, absent the reform, indicators y_{it} would have evolved similarly in the treatment and the control group. This assumption cannot be proven and in fact taxpayers from the treatment and the control group are arguably very different (see section 4.2). To address this issue we show evidence that the parallel trend assumption is at least not refuted by placebo tests run for our main outcome of interest on the pre-reform period. Furthermore, we estimate equation 2 from multiple alternative samples of the treatment and the control group to discuss how treatment impact varies as the difference across group widens.

Unfortunately compliance to the treatment is imperfect (see section 4.2): some treated taxpayers do not adopt Etax ("never takers") and some control taxpayers from the MTC2 and RSC do adopt it ("always takers"). To account for this, we estimate fuzzy versions of model 1 and 2 above, using $Lead_{it}^{k}$ and ($Treat_{it} \cdot Post_{it}$) as instruments for effective adoption of Etax. Let m_i^0 be the first month when taxpayer i uses Etax (either by declaring or paying through Etax), we define the effective use of Etax relative to the post-period k as a binary variable $Etax_{it}^{k}$ equal to one if and only if $m_0 \leq t = k$. Similarly, we define the effective use of Etax relative to the full post-period $Etax_{it}$ as a binary variable equal to one if and only if $m_0 \leq t$. Note that these definitions do not depend anymore on centre assignation. We estimate these fuzzy diff-in-diff versions of our models using two stages least square method. By definition, lag's coefficient γ^j's in model 1 will be unaffected, while lead's coefficients β^k (model 1), or β (model 2) will become the Wald-DID estimates, capturing the local average treatment effect on the compliers.

5. Results

Figure 5a shows that just in between the release of the ministerial decree in July 2017 and the effective adoption of Etax observed in December 2017 / January 2018, the probability to submit any of the tree monthly declarations sharply drops among taxpayers of the LTC, but not for other taxpayers. Consistently with reports by tax officials working within the LTC, we interpret this short-lived negative impact as a result of the the transition cost to the new system. Taxpayers' accounts had to be created with secure login credentials and both administrators and taxpayers had to learn how to use the platform. For this reason, rather than defining our reference period (i.e $t = -1$ in model 1 and 2) as the first month *before* we observe any declaration or payment through Etax by any taxpayers (that would be November 2017), we assume September and October were already affected by the reform and therefore set August 2017 as our reference period. We refer to the last four months of 2017 as the *transition period*. Our overall results are robust to the inclusion/exlcusion of these four months (see Tables 1 and 2 for a comparison).

We do not find any significant impact during the first year on the probability to submit at least one of the tree monthly declaration (see Figure 5b). A few months do come out significant in the following year, which could suggest that the adoption of Etax increases the probability to declare later in time by about 4 percentage points - before Etax, average declaration rate in the treatment and the control group were 94 % and 96 % (see Figure 5a). In case these results are indeed the lagged consequences of the introduction of Etax, they likely do not reflect a response to saving transport costs as we would typically expect this mechanism to be effective in the short-term. Rather, we would interpret it as most likely resulting from an improved monitoring of the taxpayers by the LTC. As highlighted in section 3.2, improved monitoring may stem from better information or reallocation of time previously spent by the bureaucrats on processing paper declarations. Interestingly, we find a similar pattern for the probability to declare a positive total liability (Figure 9b): no impact in the the short-term but a significant discrepancy by the very end of the period - with orders of magnitude around 5 percentage points. Trends in monthly averages across groups show that these probabilities are roughly similar in level ex-ante, then the treatment group see its probability increase by 1-2 percentage points and plateau, while that of the control group progressively looses almost 5 percentages points. Note however that, as we cannot completely discard the possibility for an impact on the probability to submit a declaration, any average impact on outcomes whose measurement depends on information present in the declaration can become harder to interpret because of compositional effect.

We find a sharp and strong negative impact on the submission of declarations with missing values. The probability to submit at least one declaration with a missing value decreases by 5-7 percentages point (see Figure 6b). The trend in Figure 6a are consistent with what we would expect: missing values drop in the treated group (while remaining relatively stable in the control group). Relative to pre-trends level, this implies a more than 50% change. Nevertheless, we do not find any clear impact on other outcomes capturing the quality of the information available in the declaration - neither on inconsistencies (Figures 7a and 7b), nor on implausible values (Figures 8a and 8b). Had the improvement in information come from the taxpayer, we would have expected to observe an impact on other dimensions as well. Our interpretation here is that the drop in missing values results from the eradication of voluntary omissions made by data entry agents. By construction of the indicators for the missing values, most of them relate to fields which data entry agents do not have to fill to validate a given declaration as long as they enter value for the tax liability. As tax payments can still be processed as long as there is information about the tax liability, some level of tolerance for missing secondary information may have been granted to accelerate data entry. Incidentally, these results imply that any average impact whose measurement depends on information present in the declaration may become even harder to interpret as there are now two sources of compositional effect: the composition of those who declares, and, conditional on declaring, that of those who do not have missing value.[6]

Average total liability declared clearly seems unaffected by the reform (Figure 10a), estimated treatment effects vary around zero through the entire period (Figure 10b). Nevertheless, graphical evidence for the trends in average payments strongly suggest that effective payment increase (Figure 12c): average monthly payment among treated taxpayers jumps from roughly USD 40,000 on average in 2017 to at least USD 50,000 on average in 2018, and USD 60,000 in 2019, while it remains flat for the control group. Monthly average treatment impact are significant at the 5% level for 12 out of 24 months in the post-period and increase as well overtime, from approximately USD 20,000 to USD 25,000. If accurate, this impact is arguably large: a net average monthly impact of USD 20,000 across the 831 treated firms would imply

[6]This is the reason why we do not include here impacts related to tax-base or tax rates.

that about 6.5 % all tax revenues collected in 2018 resulted from the reform, i.e the equivalent of 1% of Senegal's 2018 GDP. However, the magnitude of the results appears to be largely driven by extremely large taxpayers (measured in terms of turnover declared prior the reform). Figure 12g indeed shows that the overall impact decreases from a little more than USD 20,000 per month on average to a little less than USD 10,000, just by removing the 5 largest firms. As a few more firms get removed, it converges to about USD 5,000. The payment data we exploit is a direct trace of payment effectively executed by the tax administration information software for the benefit of the treasury, this source is the cornerstone of the administration's performance results and is its most scrutinized source of information. For that matter, we have very good reasons to believe the source is accurate. Furthermore, a case by case inspection carried out by tax administrators themselves confirmed amounts measured in the database for the largest 20 taxpayers. If we can most safely assume that these extreme levels are not due to measurement errors, our identification strategy obviously does not guaranty that *for these very firms* something else than the reform might have player a role. Given their leverage on the average impact we must acknowledge that the true magnitude of impact on payments will remain uncertain. Our sensitivity tests on 40 alternative samples obtained by incrementally trimming the 5 largest taxpayers from the treatment group do nevertheless suggest that the reform had a positive impact on payment of about USD 5,000 per month on average (Figure 12g). Available information does not allow to determinate whether this increase in driven by a drop in collusion or an increased capacity to enforce payment recovery..

On the COVID-shock: Later in time, as the COVID pandemic spread to Senegal and containment measures were put in place, we observe that taxpayers who had not adopted Etax by then were less likely to submit a declaration. Figure 16a show that the probability to declare drops from 95% to 91% among these taxpayers, while it remains very stable for taxpayers who could declare online. Applying model 1 over 2019-2021 for a balance sample of 2,778 taxpayers, redefining treatment and control according past use of Etax and using March 2020 as the new time reference, we estimate that the COVID-shock decreased the probability to declare for about 4 percentage points for 3 consecutive months, and that it took about one year and a half for this negative shock to fully resorb. By construction of our sample, all taxpayers in the sample, including those negatively affected, do declare and or pay of beyond 2021. In that regard, missing declarations here could and should have been declared later in time, after containment measures had been lifted and thus should have filled the gap we observe.[7] In other words, the effects we estimate here are not driven by real economic downturn leading firms to close: all of them survived and delayed declaration are possible. The most plausible channel to explain

[7]As for all estimation of the paper, we always use *fiscal* time and not *real* time, as we do not have this information. For example: missing declaration for April 2020 in Figure 16a could have been declared in July 2021.

this negative shock is therefore an increase in the cost of submitting a declaration. The state of emergency was proclaimed on the 23^{rd} of March, and maintained until the 29th of June, it implied movement restrictions and a curfew. These 3 months correspond exactly to the period for which we observe the most severe impact. Yet, under this interpretation, these findings must then be reconciled with the fact the introduction of e-filling did not lead to any increase in the probability to declare among taxpayers of the LTC, in particular as ex-ante levels are very similar for treated firms in both settings. A simple explanation would be that the 2018 decrease in the declarative cost induced by the introduction of e-filling was not as large as its increase due to COVID related containment measures.

6. Conclusion

This article investigates the causal impact of the adoption of electronic filing (e-filing) and electronic payment (e-payment) technologies by the largest taxpayers in Senegal. The analysis employs a fuzzy dynamic double-difference method, focusing on the period following the mandatory implementation of the Etax system in July 2017. The findings reveal intriguing insights into the complex relationship between technology adoption and tax compliance.

The results indicate a short-lived negative impact on the probability to declare immediately after the ministerial decree mandating Etax adoption which suggest that taxpayers and the administration faced a small transition cost. However, in subsequent months, there is no significant impact on the probability to declare. Interestingly, we observes a clear decrease in the prevalence of missing values in digitized declarations, most probably reflecting the fact that under the previous system, data entry agents were omitting to type values unnecessary to compute tax liability. While there is no significant impact on the probability to pay nor on average liability declared, we find a positive and possibly large impact on tax payments, particularly concentrated among the largest taxpayers. This could be due to reduced opportunities for collusion or increased capacity to enforce payment recovery. Further research would be needed to disentangle the two.

In summary, the article provides valuable causal evidence on the impact of e-filing and e-payment adoption on tax compliance in Senegal, shedding light on both the short and medium-term effects on various indicators. Despite no or modest impacts on declarative tax compliance, the results suggest that e-payment solutions could play a crucial role in increasing average effective payments by the largest taxpayers, potentially leading to significant revenue gains. This research contributes to the limited literature on the role of e-filing and e-payment technologies in low-income countries, offering nuanced insights into their effects on tax compliance and revenue collection.

References

Ali, Merima, Abdulaziz Shifa, Abebe Shimeles, and Firew B Woldeyes (2015). "Information technology and fiscal capacity in a developing country: evidence from Ethiopia". In.

Allingham, Michael G and Agnar Sandmo (1972). "Income Tax Evasion: A Theoretical Analysis". In: *Journal of public economics* 1.3-4, pp. 323–338.

Amine, Razan and Fabrizio Santoro (2023). *Mandating Digital Tax Tools as a Response to Covid: Evidence from Eswatini*. Tech. rep.

Bellon, Matthieu, Era Dabla-Norris, Salma Khalid, and Frederico Lima (2022). "Digitalization to improve tax compliance: Evidence from VAT e-Invoicing in Peru". In: *Journal of Public Economics* 210, p. 104661.

Besley, Timothy, Robin Burgess, Adnan Khan, and Guo Xu (2022). "Bureaucracy and development". In: *Annual Review of Economics* 14, pp. 397–424.

Best, Michael, Jawad Shah, and Mazhar Waseem (2021). *The Deterrence Value of Tax Audit: Estimates from a Randomised Audit Program*. Tech. rep. mimeo.

Brockmeyer, Anne and Magaly Sáenz Somarriba (2022). "Electronic Payment Technology and Tax Compliance: Evidence from Uruguay's Financial Inclusion Reform". In.

Das, Satadru, Lucie Gadenne, Tushar Nandi, and Ross Warwick (2023). "Does going cashless make you tax-rich? Evidence from India's demonetization experiment". In: *Journal of Public Economics* 224, p. 104907.

Luttmer, Erzo FP and Monica Singhal (2014). "Tax morale". In: *Journal of economic perspectives* 28.4, pp. 149–168.

Mascagni, Giulia, Andualem T Mengistu, and Firew B Woldeyes (2021). "Can ICTs increase tax compliance? Evidence on taxpayer responses to technological innovation in Ethiopia". In: *Journal of Economic Behavior & Organization* 189, pp. 172–193.

Milanez, Anna (2017). "Legal Tax Liability, Legal Remittance Responsibility and Tax Incidence: Three Dimensions of Business Taxation". In.

Miller, Douglas L (2023). "An Introductory Guide to Event Study Models". In: *Journal of Economic Perspectives* 37.2, pp. 203–230.

OCDE (2022). "Revenue Statistics in Africa 1990-2020". In.

Okunogbe, Oyebola and Victor Pouliquen (2022). "Technology, taxation, and corruption: evidence from the introduction of electronic tax filing". In: *American Economic Journal: Economic Policy* 14.1, pp. 341–372.

Rasul, Imran, Daniel Rogger, and Martin J Williams (2018). "Management and bureaucratic effectiveness: Evidence from the ghanaian civil service". In: *World Bank Policy Research Working Paper* 8595.

Roth, Jonathan, Pedro HC Sant'Anna, Alyssa Bilinski, and John Poe (2023). "What's trending in difference-in-differences? A synthesis of the recent econometrics literature". In: *Journal of Econometrics*.

Scarpini, Celeste, Oyebola Okunogbe, and Fabrizio Santoro (2023). "The Promise and Limitations of Information Technology for Tax Mobilisation". In: *Institute of Development Studies, International Centre for Tax and Development Working Papers* 17871.

Slemrod, Joel (2019). "Tax Compliance and Enforcement". In: *Journal of Economic Literature* 57.4, pp. 904–954.

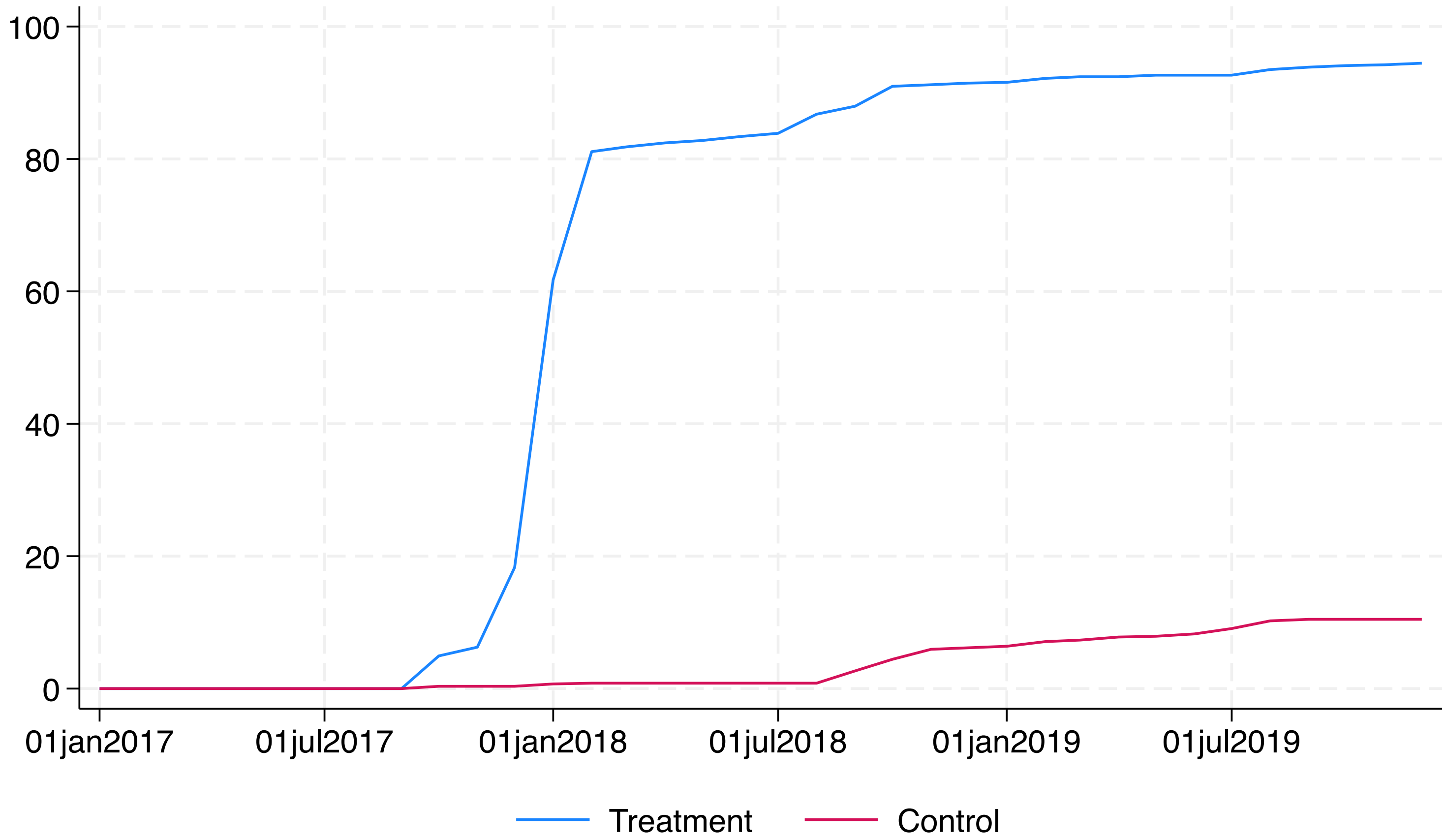
Slemrod, Joel and Tejaswi Velayudhan (2018). "Do Firms Remit at Least 85% Of Tax Everywhere? New Evidence From India". In: *Journal of Tax Administration* 4.1, pp. 24–37.

Weber, Max (1922). *Economy and Society: An Outline of Interpretive Sociology*. Vol. 1. University of California Press.

World Bank, World (2016). *World development report 2016: Digital dividends*. World Bank Publications.

Figure 1 – Adoption of Etax

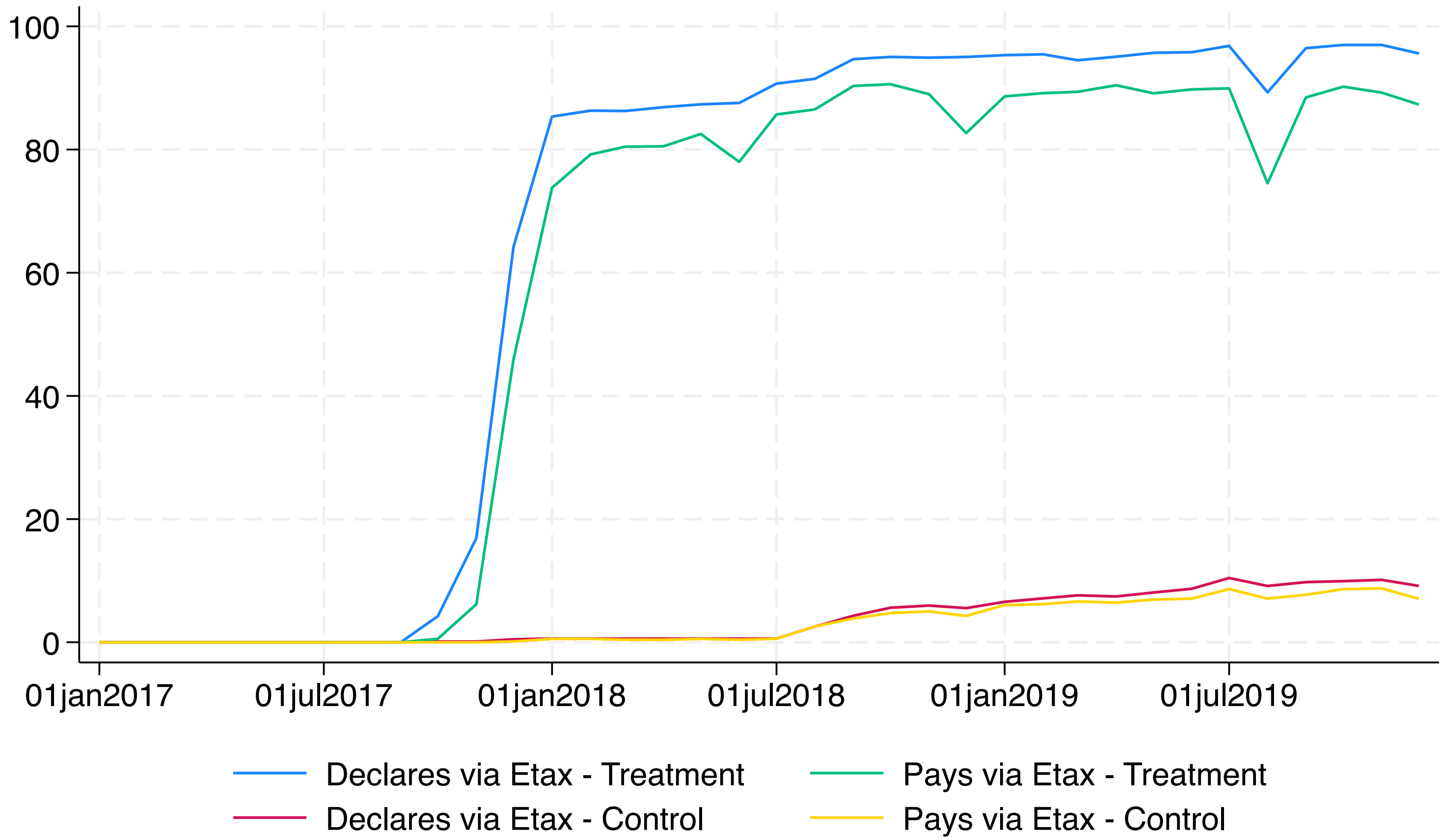
(a) Share of taxpayers having used Etax at least once



Notes. The figure displays the share of taxpayers having used Etax at least once (either through e-filling or e-payment) within each group. The sample here is the sample we use for all our main estimation, with 831 treated taxpayers and 860 taxpayers in the control group.

Figure 1 – Adoption of Etax

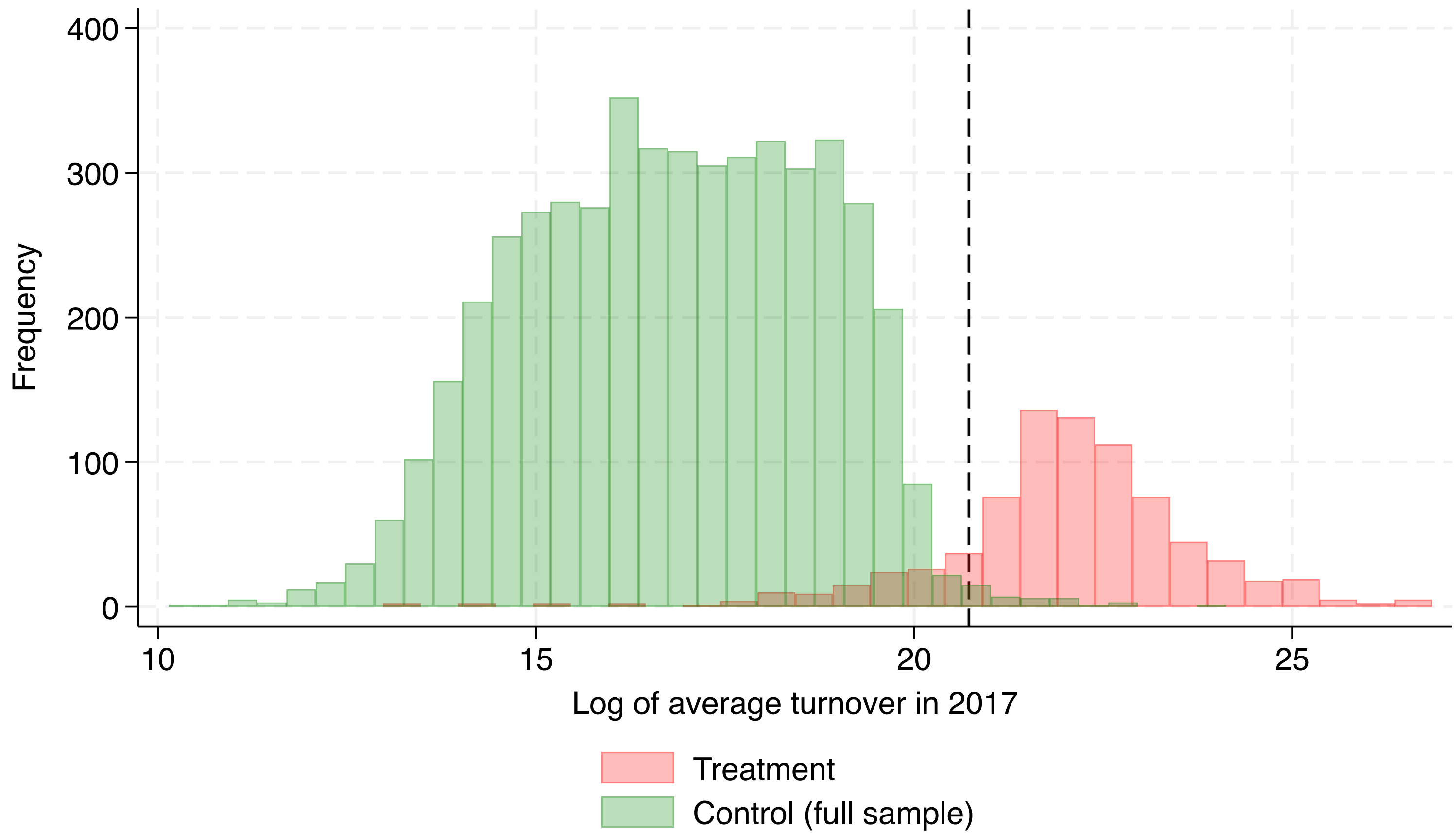
(b) Monthly share of effective users



Notes. The figure displays the share of taxpayers that effectively use Etax each month, either through e-filling or e-payment, within each group. The sample here is the sample we use for all our main estimation, with 831 treated taxpayers and 860 taxpayers in the control group.

Figure 2 – Probability distribution with respect to taxpayers size

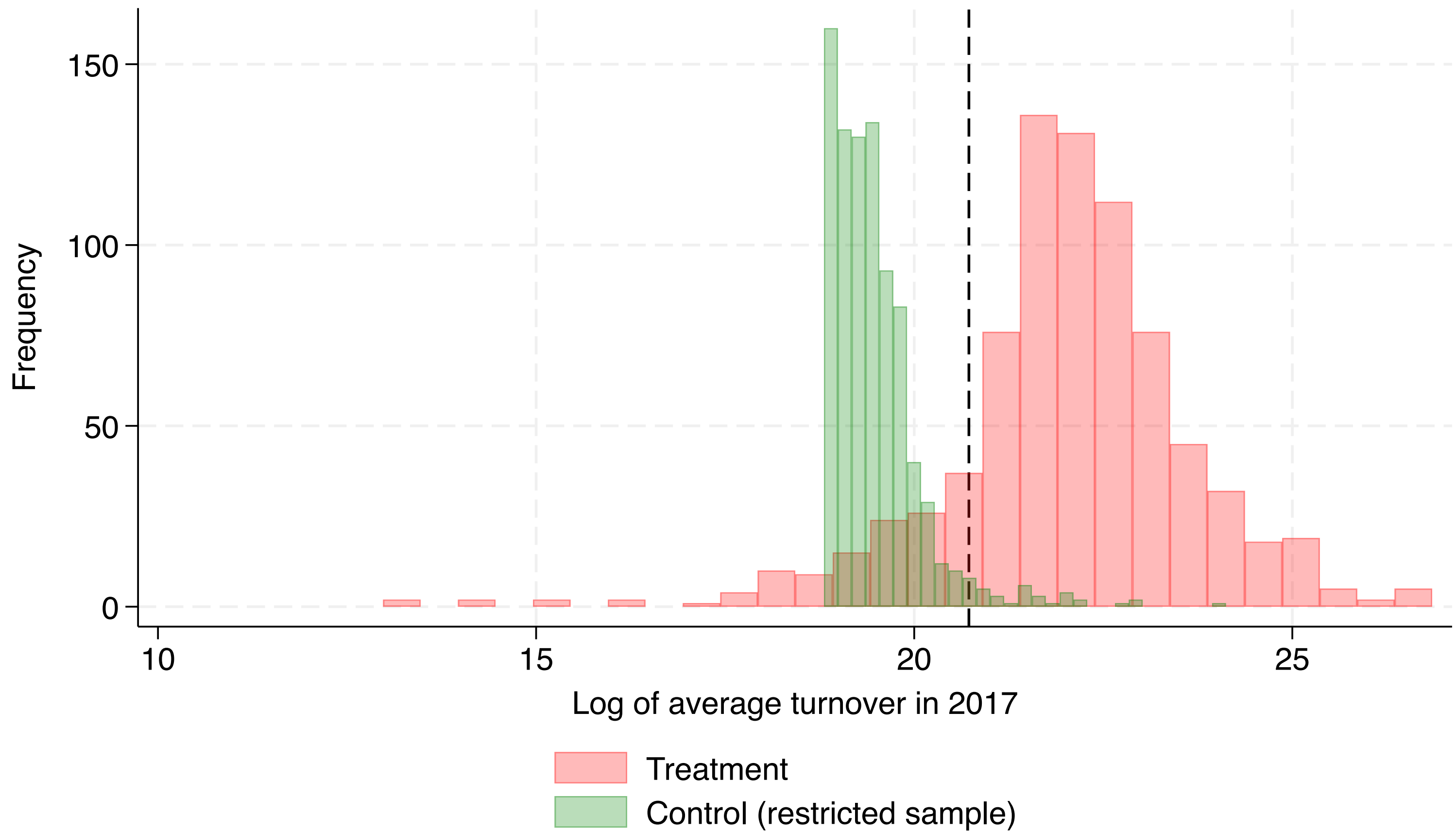
(a) Full (initial) Sample



Notes. The figure displays histograms of the density distribution of the treatment and control group by size, measured as average turnover in 2017 before any trimming of the control group. The 831 treated taxpayers are all those monitored by the Large Taxpayers Center. The dashed line indicates the threshold above which a firms becomes eligible to the LTC.

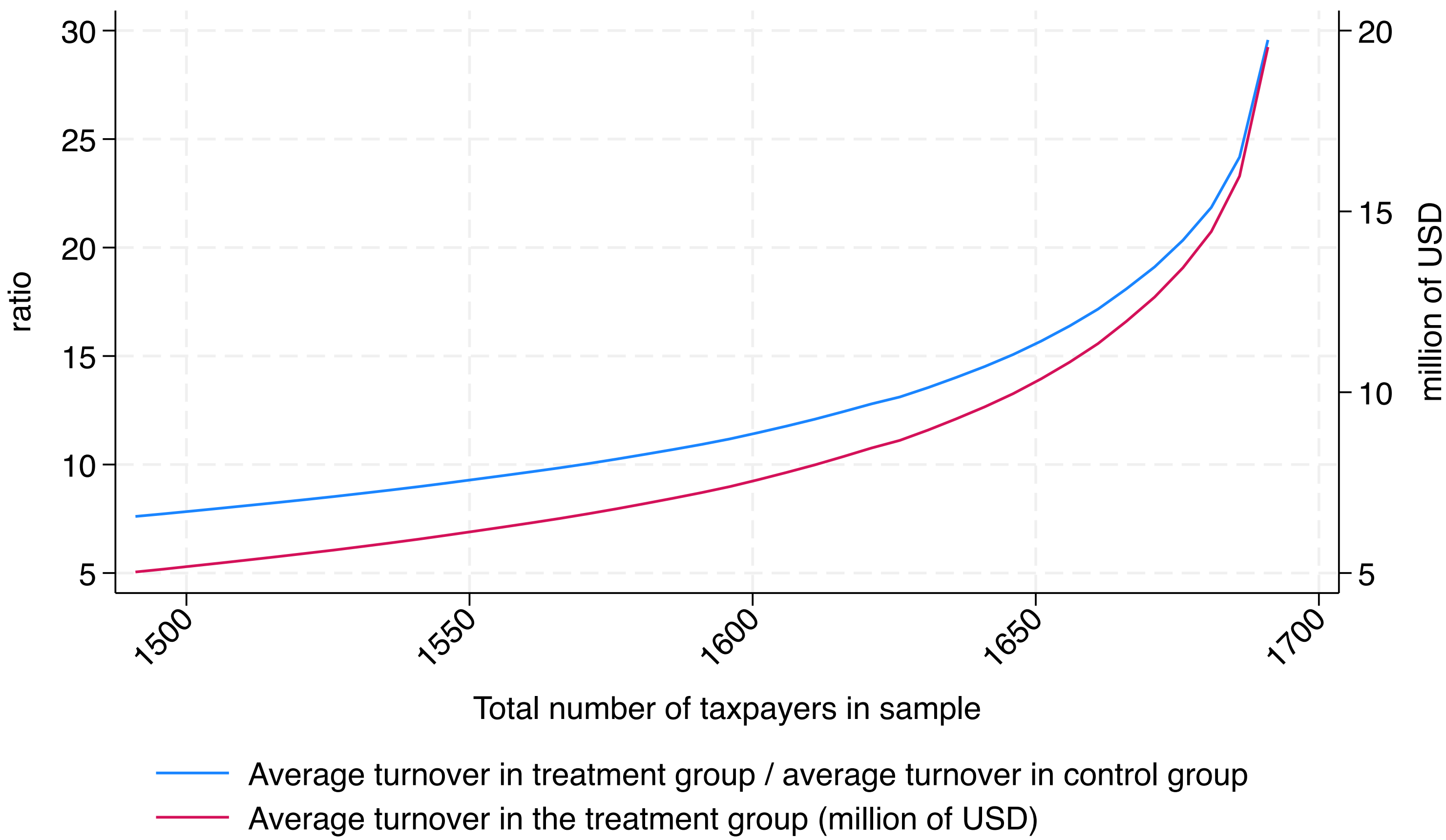
Figure 3 – Probability distribution with respect to taxpayers size

(a) Restricted (main) Sample



Notes. The figure displays histograms of the density distribution of the treatment and control group by size, measured as average turnover in 2017, after restricting the control group to its 860 largest taxpayers. The 831 treated taxpayers are all those monitored by the Large Taxpayers Center. The dashed line indicates the threshold above which a firms becomes eligible to the LTC.

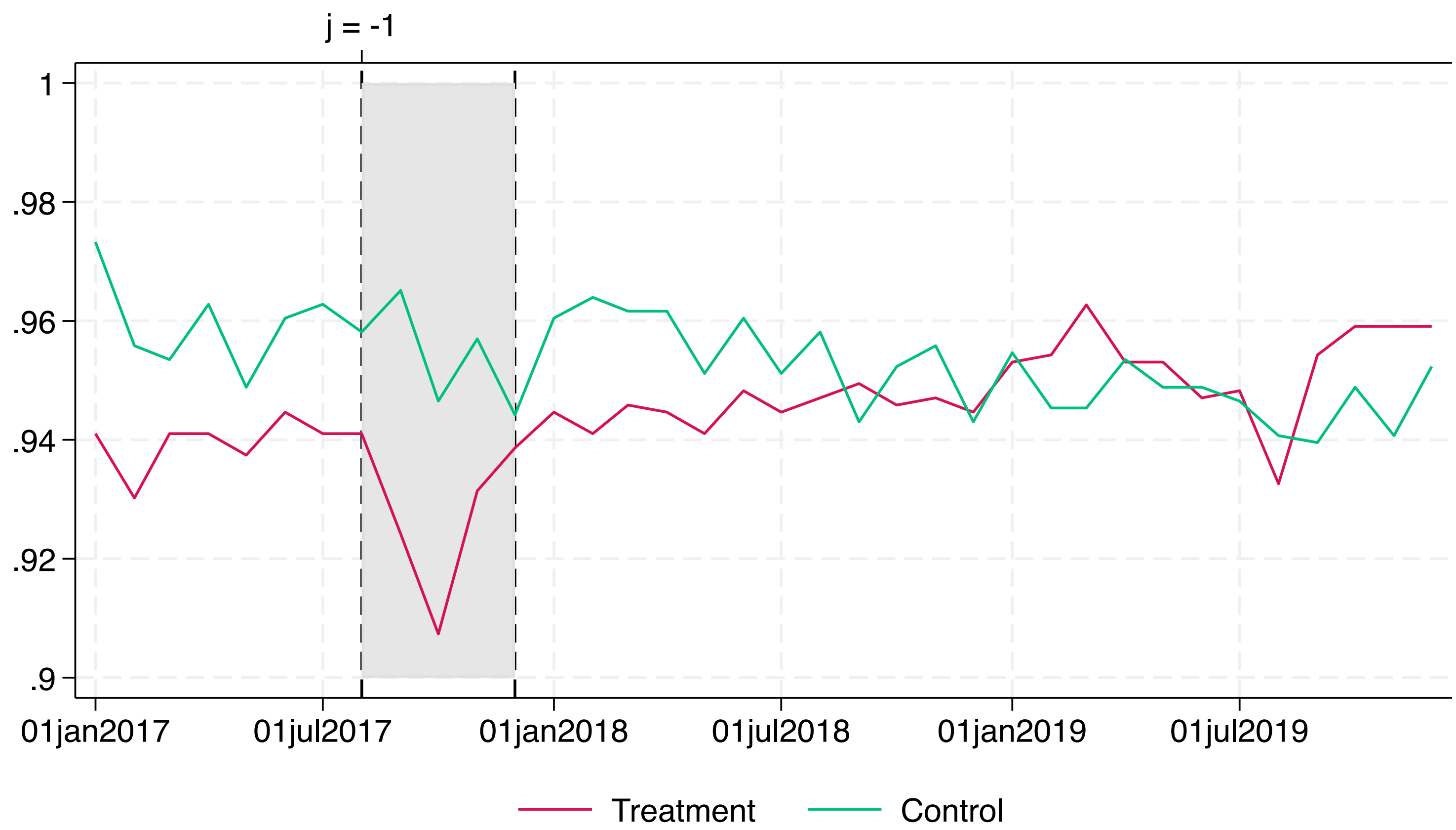
Figure 4 – Sensitivity to sample selection



Notes. The figure displays variation is in the average turnover among treated taxpayers, and in the ratio between this average and the average turnover among control taxpayers, as we incrementally remove the 5 largest taxpayers from the treatment group. When keeping all 831 treated firms, their average turnover in the pre-reform period is about USD 20 million, which is about 30 times more than in the control group. Trimming the largest 200 taxpayers from the treatment group (top 24%) would bring down this ratio to 7.5.

Figure 5 – Submits at least one declaration

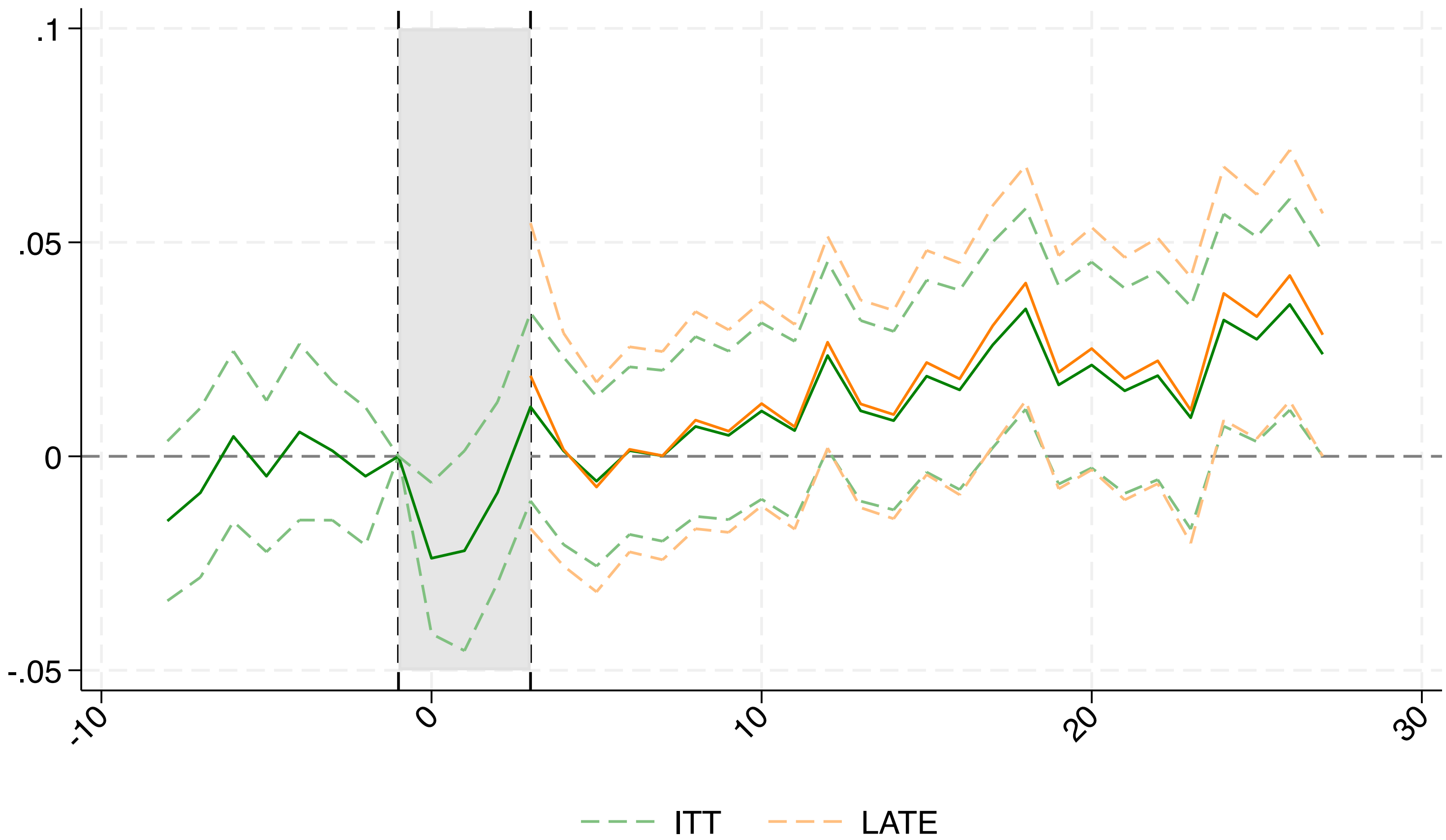
(a) Time trends



The figure displays average values of the outcome by group and across time. Treated taxpayers taxpayers monitored by the Large Taxpayers Center (LTC), where the adoption of Etax was made mandatory by a ministerial decree issued in July 2017. Controlled taxpayers are the largest 860 taxpayers monitored by other centers of Dakar. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876.

Figure 5 – Submits at least one declaration

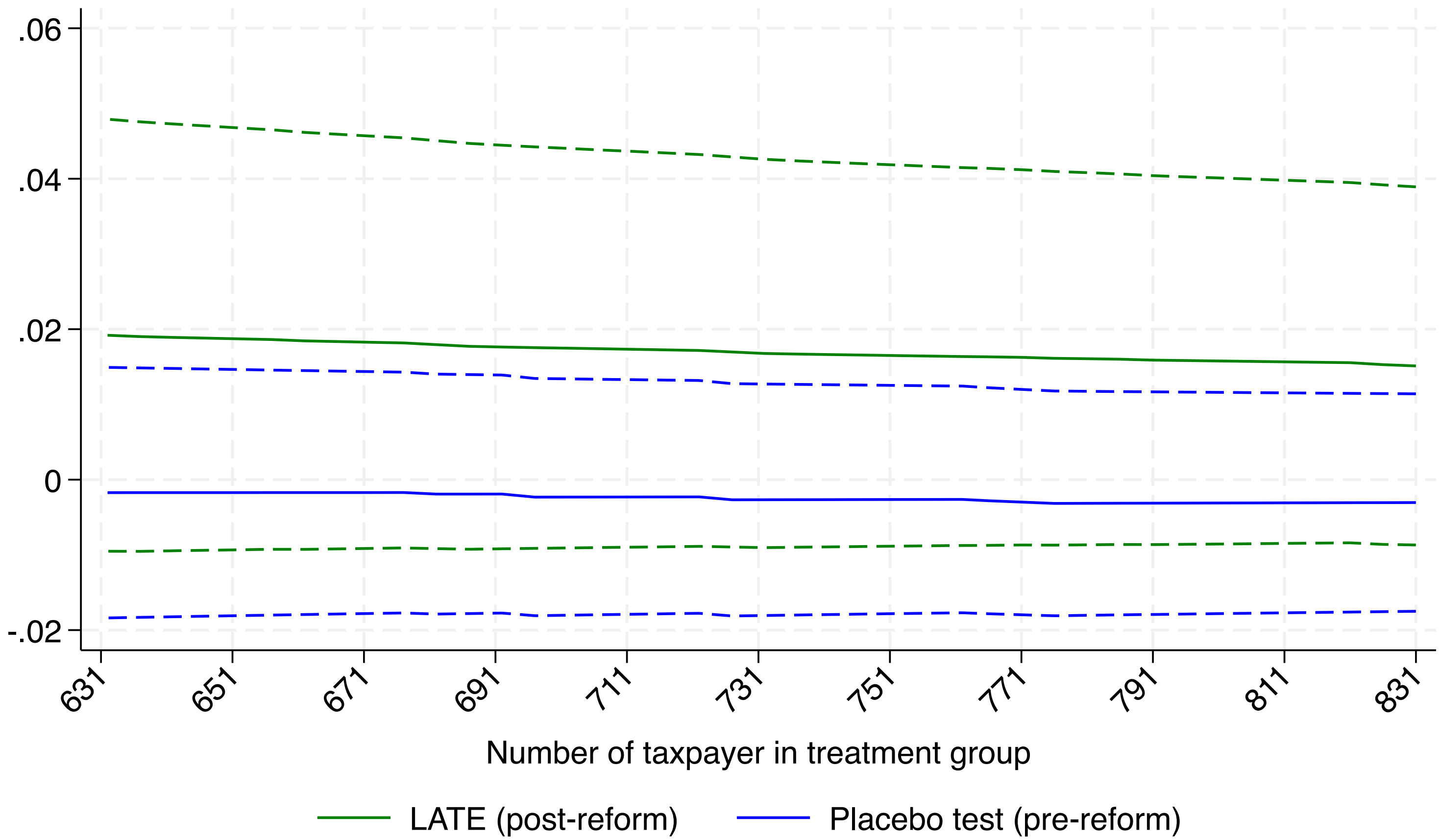
(b) Time-varying impact



Notes. The figure displays coefficients of lags, and leads (ITT or LATE) indicators of model 1 using the mandatory adoption of Etax for the Largest Taxpayers Center, set by ministerial decree in July 2017, as a source of exogenous variation. The reference period ($t = -1$) is August 2017. A transition period is highlighted in grey, during which adoption progressively took place. In the LATE specification, we instrument each leads of model 1 by an indicator $Etax_{it}^{k}$ equal to one if and only if $t = k$ and taxpayer i has effectively adopted etax by t. All estimations use time and taxpayer fixed-effects. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Figure 5 – Submits at least one declaration

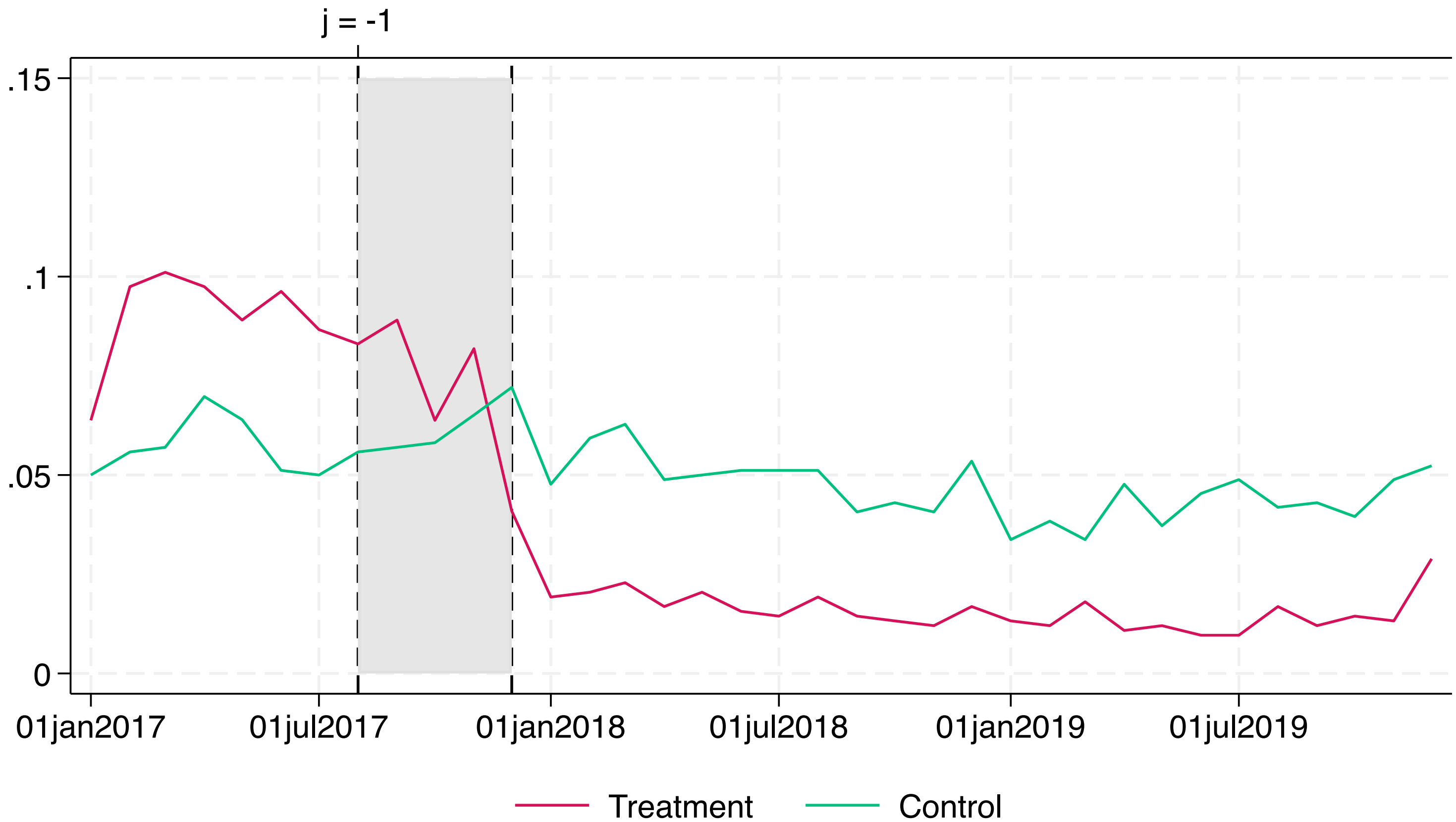
(c) Sensitivity to extremely large taxpayers



Notes. The Figure displays variations in treatment impact (post-reform) and placebo test (pre-reform) as we trim the treated group (taxpayers monitored be the LTC) by incrementally removing the 5 largest taxpayers. LATE coefficients (green line) are obtained by estimating model 2 for the post-reform period, using two stages least square method and instrumenting $(Treat_{it} \cdot Post_{it})$ by an indicator $Etax_{it}$ equal to one if and only if taxpayer i has started using Etax by t. Coefficients of placebo tests (blue line) are obtained by estimating model 2 for the pre-reform period replacing $(Treat_{it} \cdot Post_{it})$ by $(Treat_{it} \cdot Pre_{it})$ and using Ordinary Least Square method. The reference period is always August 2017. All samples are balanced, and all estimations use time and taxpayer fixed-effect. The axis indicates the number of taxpayers in the treatment group after trimming. The initial sample size of the treatment group is 831. The size of the control group is 860 and remains constant through all specifications.

Figure 6 – Some declaration has missing information

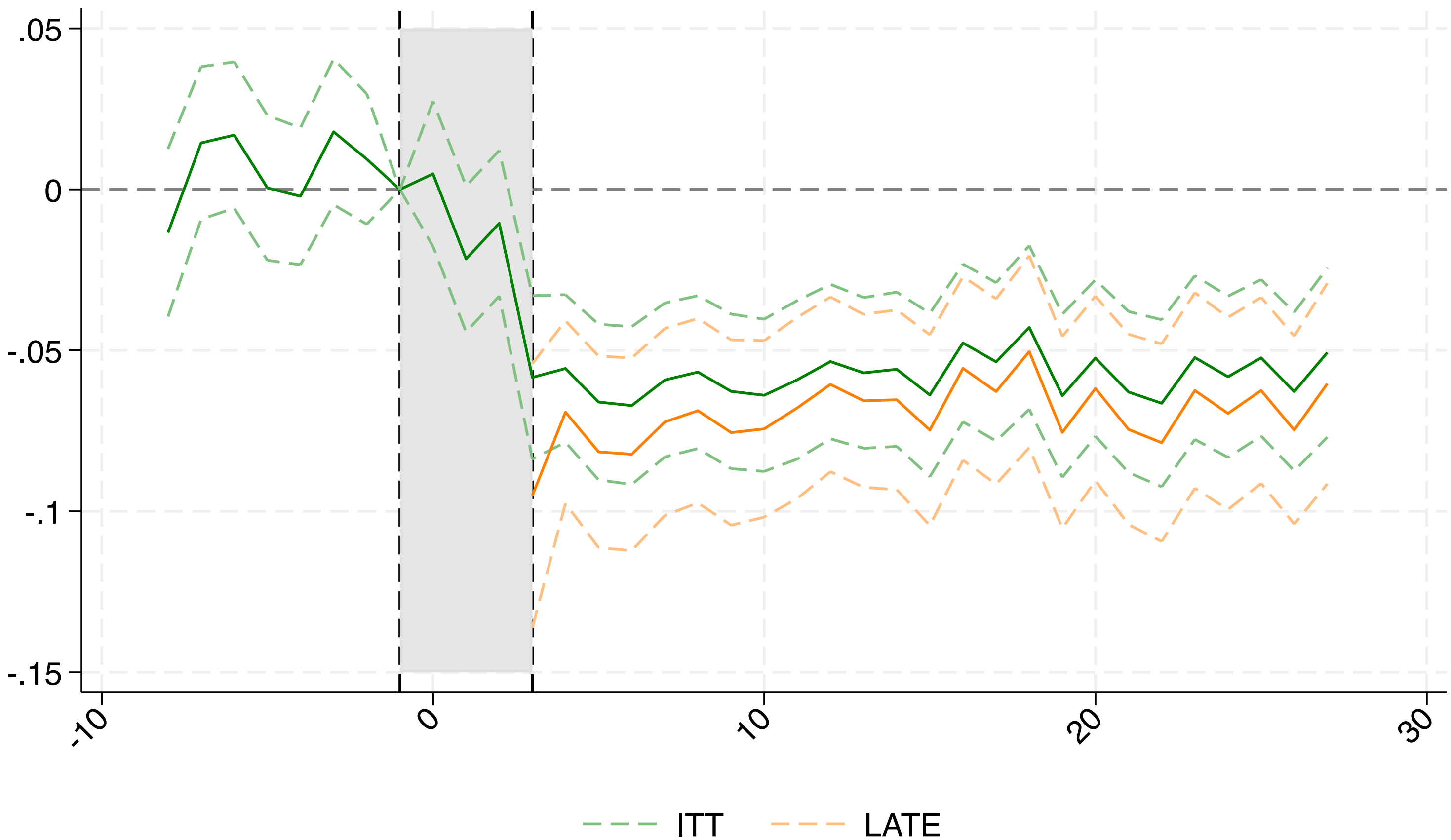
(a) Time trends



The figure displays average values of the outcome by group and across time. Treated taxpayers taxpayers monitored by the Large Taxpayers Center (LTC), where the adoption of Etax was made mandatory by a ministerial decree issued in July 2017. Controlled taxpayers are the largest 860 taxpayers monitored by other centers of Dakar. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876.

Figure 6 – Some declaration has missing information

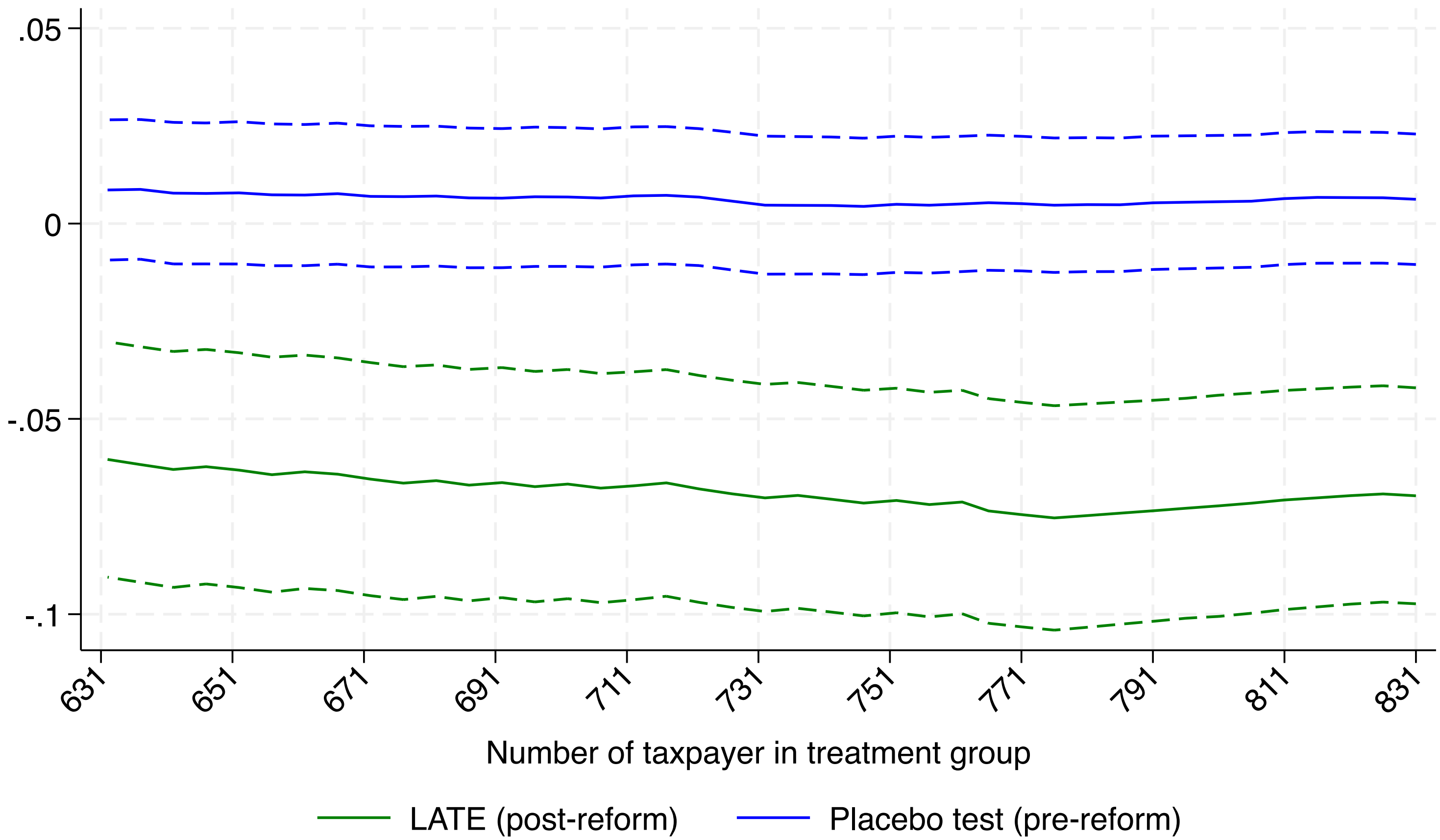
(b) Time-varying impact



Notes. The figure displays coefficients of lags, and leads (ITT or LATE) indicators of model 1 using the mandatory adoption of Etax for the Largest Taxpayers Center, set by ministerial decree in July 2017, as a source of exogenous variation. The reference period ($t = -1$) is August 2017. A transition period is highlighted in grey, during which adoption progressively took place. In the LATE specification, we instrument each leads of model 1 by an indicator $Etax_{it}^{k}$ equal to one if and only if $t = k$ and taxpayer i has effectively adopted etax by t. All estimations use time and taxpayer fixed-effects. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Figure 6 – Some declaration has missing information

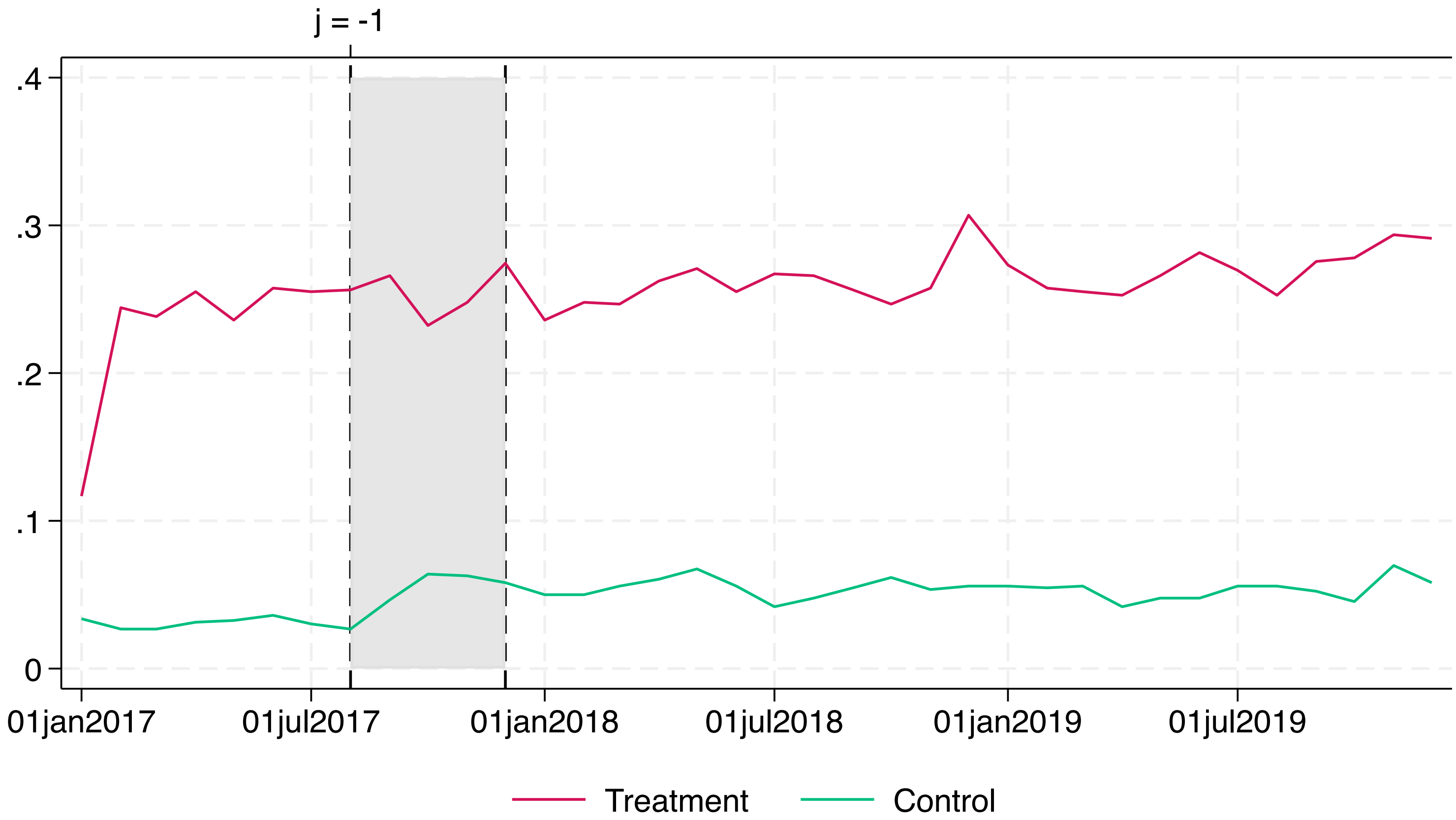
(c) Sensitivity to extremely large taxpayers



Notes. The Figure displays variations in treatment impact (post-reform) and placebo test (pre-reform) as we trim the treated group (taxpayers monitored be the LTC) by incrementally removing the 5 largest taxpayers. LATE coefficients (green line) are obtained by estimating model 2 for the post-reform period, using two stages least square method and instrumenting $(Treat_{it} \cdot Post_{it})$ by an indicator $Etax_{it}$ equal to one if and only if taxpayer i has started using Etax by t. Coefficients of placebo tests (blue line) are obtained by estimating model 2 for the pre-reform period replacing $(Treat_{it} \cdot Post_{it})$ by $(Treat_{it} \cdot Pre_{it})$ and using Ordinary Least Square method. The reference period is always August 2017. All samples are balanced, and all estimations use time and taxpayer fixed-effect. The axis indicates the number of taxpayers in the treatment group after trimming. The initial sample size of the treatment group is 831. The size of the control group is 860 and remains constant through all specifications.

Figure 7 – Some declaration has inconsistent information

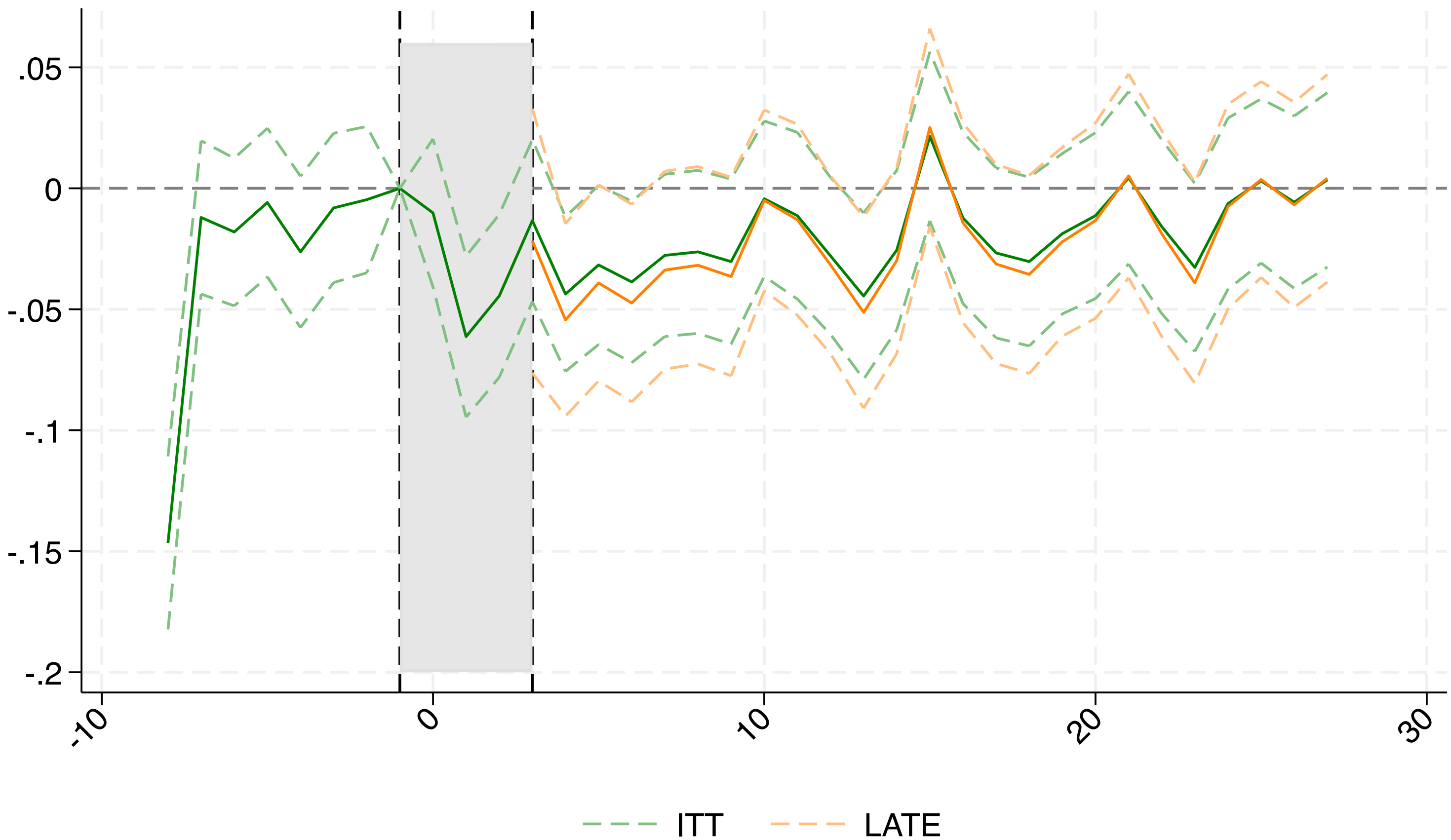
(a) Time trends



The figure displays average values of the outcome by group and across time. Treated taxpayers taxpayers monitored by the Large Taxpayers Center (LTC), where the adoption of Etax was made mandatory by a ministerial decree issued in July 2017. Controlled taxpayers are the largest 860 taxpayers monitored by other centers of Dakar. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876.

Figure 7 – Some declaration has inconsistent information

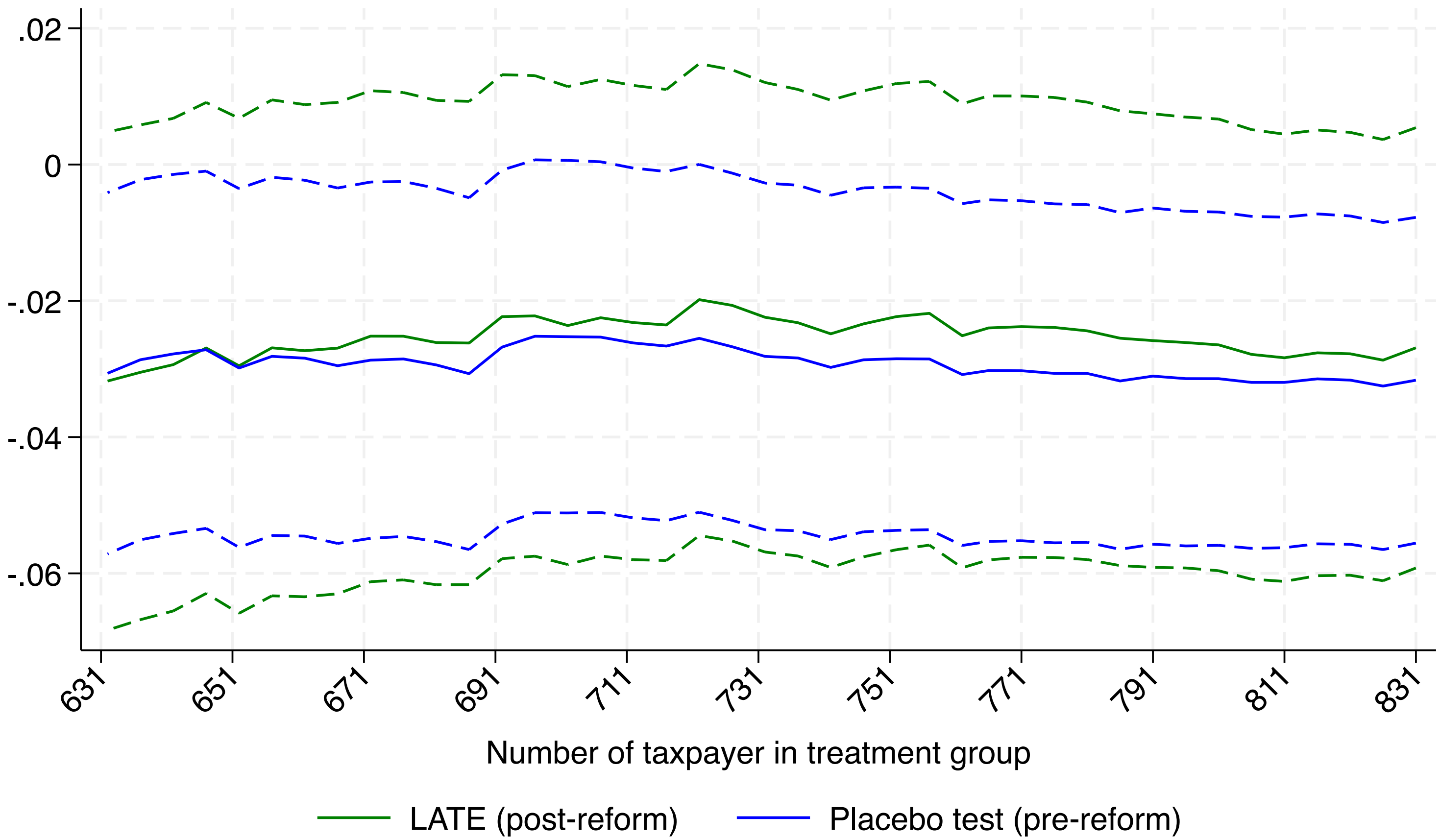
(b) Time-varying impact



Notes. The figure displays coefficients of lags, and leads (ITT or LATE) indicators of model 1 using the mandatory adoption of Etax for the Largest Taxpayers Center, set by ministerial decree in July 2017, as a source of exogenous variation. The reference period ($t = -1$) is August 2017. A transition period is highlighted in grey, during which adoption progressively took place. In the LATE specification, we instrument each leads of model 1 by an indicator $Etax_{it}^{k}$ equal to one if and only if $t = k$ and taxpayer i has effectively adopted etax by t. All estimations use time and taxpayer fixed-effects. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Figure 7 – Some declaration has implausible information

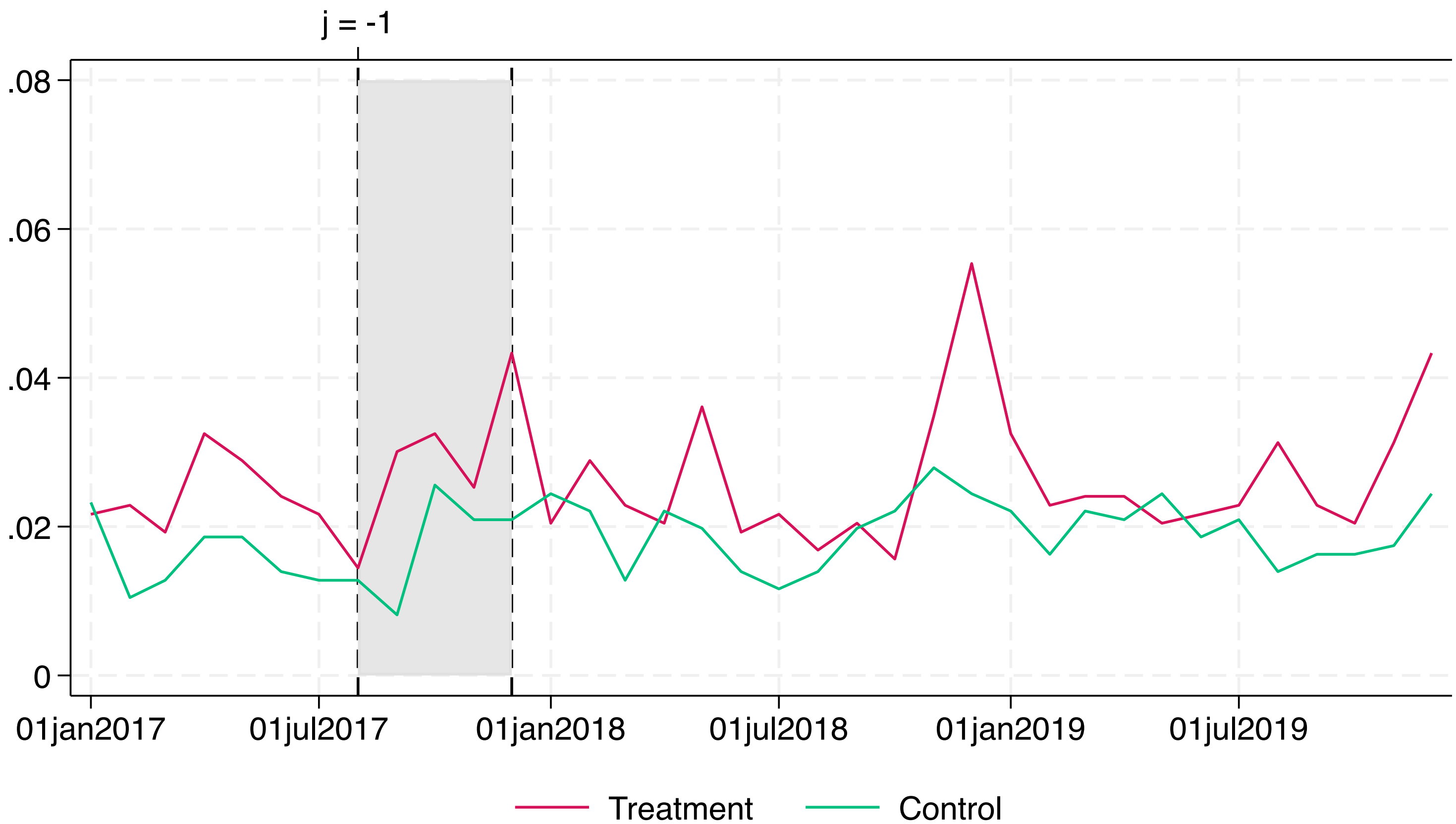
(c) Sensitivity to extremely large taxpayers



Notes. The Figure displays variations in treatment impact (post-reform) and placebo test (pre-reform) as we trim the treated group (taxpayers monitored be the LTC) by incrementally removing the 5 largest taxpayers. LATE coefficients (green line) are obtained by estimating model 2 for the post-reform period, using two stages least square method and instrumenting $(Treat_{it} \cdot Post_{it})$ by an indicator $Etax_{it}$ equal to one if and only if taxpayer i has started using Etax by t. Coefficients of placebo tests (blue line) are obtained by estimating model 2 for the pre-reform period replacing $(Treat_{it} \cdot Post_{it})$ by $(Treat_{it} \cdot Pre_{it})$ and using Ordinary Least Square method. The reference period is always August 2017. All samples are balanced, and all estimations use time and taxpayer fixed-effect. The axis indicates the number of taxpayers in the treatment group after trimming. The initial sample size of the treatment group is 831. The size of the control group is 860 and remains constant through all specifications.

Figure 8 – Some declaration has implausible information

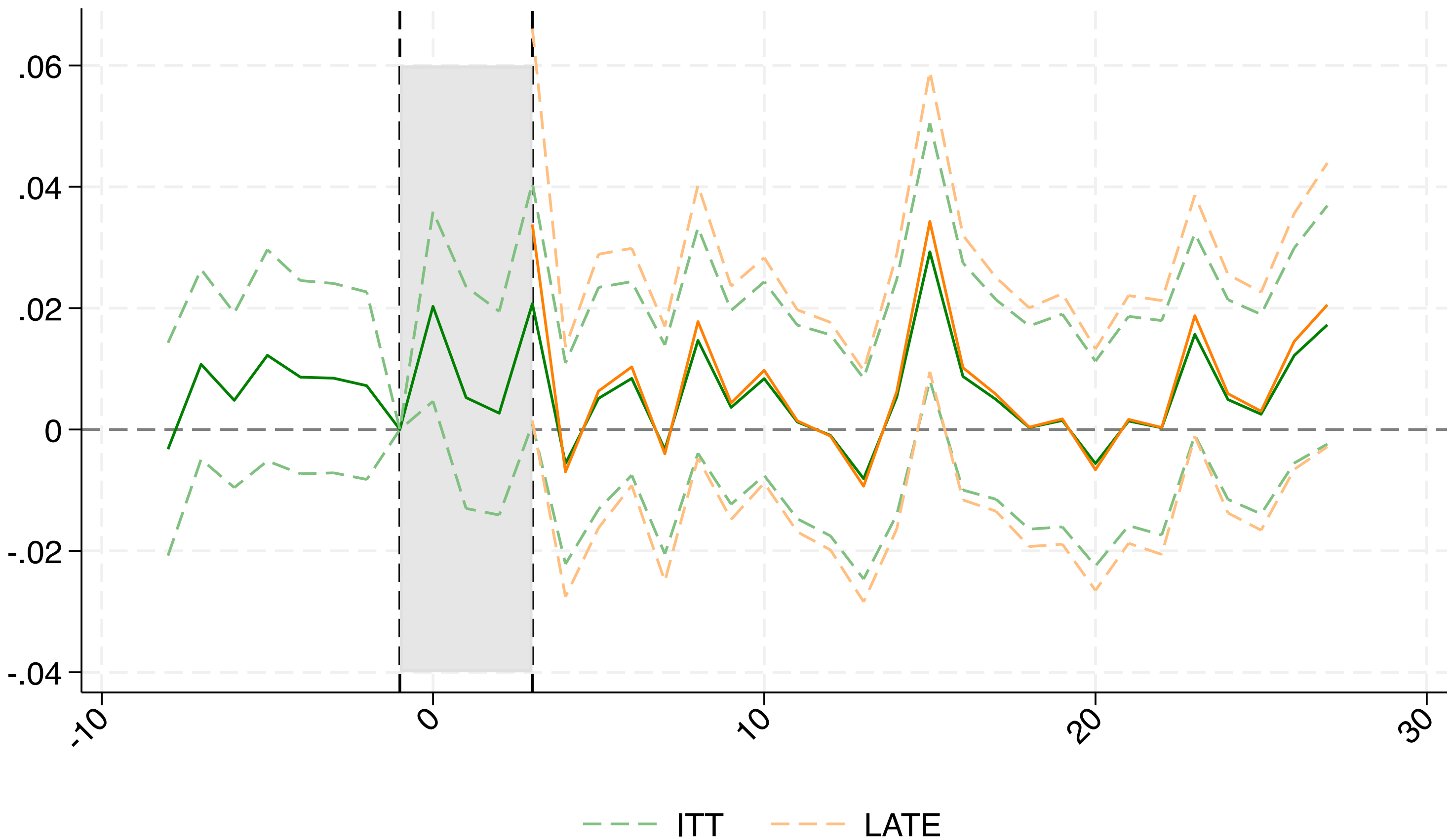
(a) Time trends



The figure displays average values of the outcome by group and across time. Treated taxpayers taxpayers monitored by the Large Taxpayers Center (LTC), where the adoption of Etax was made mandatory by a ministerial decree issued in July 2017. Controlled taxpayers are the largest 860 taxpayers monitored by other centers of Dakar. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876.

Figure 8 – Some declaration has implausible information

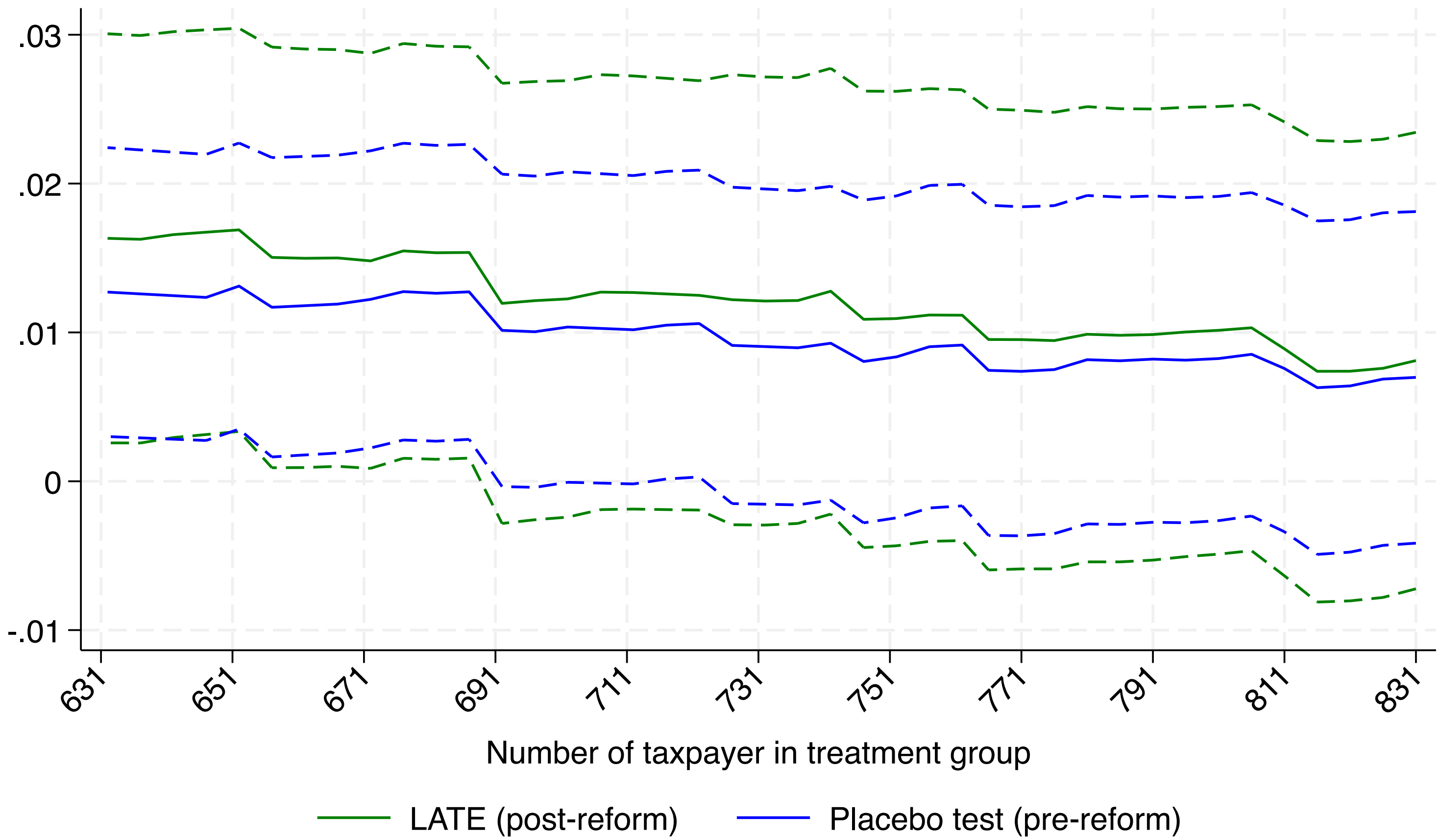
(b) Time-varying impact



Notes. The figure displays coefficients of lags, and leads (ITT or LATE) indicators of model 1 using the mandatory adoption of Etax for the Largest Taxpayers Center, set by ministerial decree in July 2017, as a source of exogenous variation. The reference period ($t = -1$) is August 2017. A transition period is highlighted in grey, during which adoption progressively took place. In the LATE specification, we instrument each leads of model 1 by an indicator $Etax_{it}^{k}$ equal to one if and only if $t = k$ and taxpayer i has effectively adopted etax by t. All estimations use time and taxpayer fixed-effects. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Figure 8 – Some declaration has implausible information

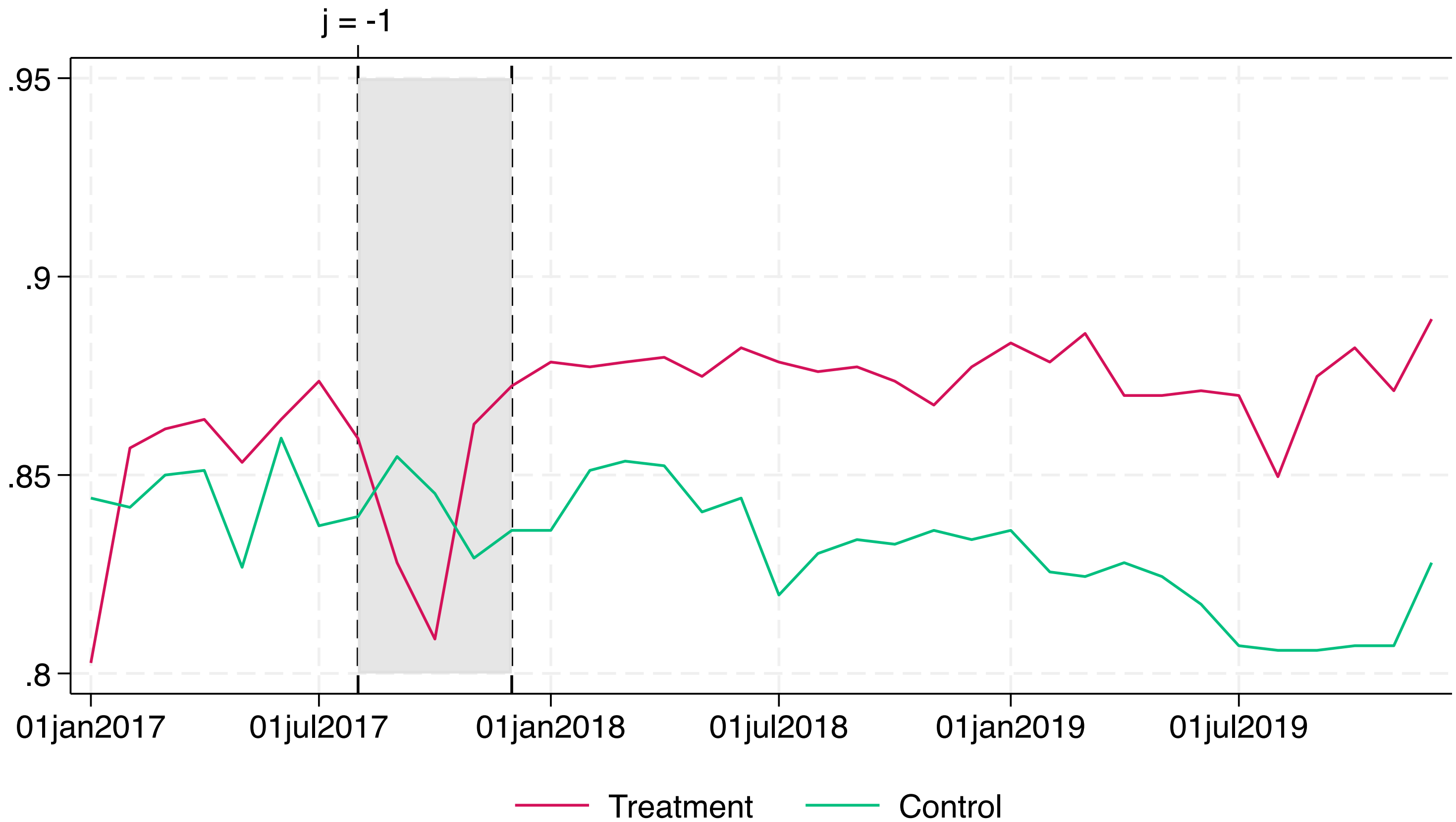
(c) Sensitivity to extremely large taxpayers



Notes. The Figure displays variations in treatment impact (post-reform) and placebo test (pre-reform) as we trim the treated group (taxpayers monitored be the LTC) by incrementally removing the 5 largest taxpayers. LATE coefficients (green line) are obtained by estimating model 2 for the post-reform period, using two stages least square method and instrumenting $(Treat_{it} \cdot Post_{it})$ by an indicator $Etax_{it}$ equal to one if and only if taxpayer i has started using Etax by t. Coefficients of placebo tests (blue line) are obtained by estimating model 2 for the pre-reform period replacing $(Treat_{it} \cdot Post_{it})$ by $(Treat_{it} \cdot Pre_{it})$ and using Ordinary Least Square method. The reference period is always August 2017. All samples are balanced, and all estimations use time and taxpayer fixed-effect. The axis indicates the number of taxpayers in the treatment group after trimming. The initial sample size of the treatment group is 831. The size of the control group is 860 and remains constant through all specifications.

Figure 9 – Liability declared is positive

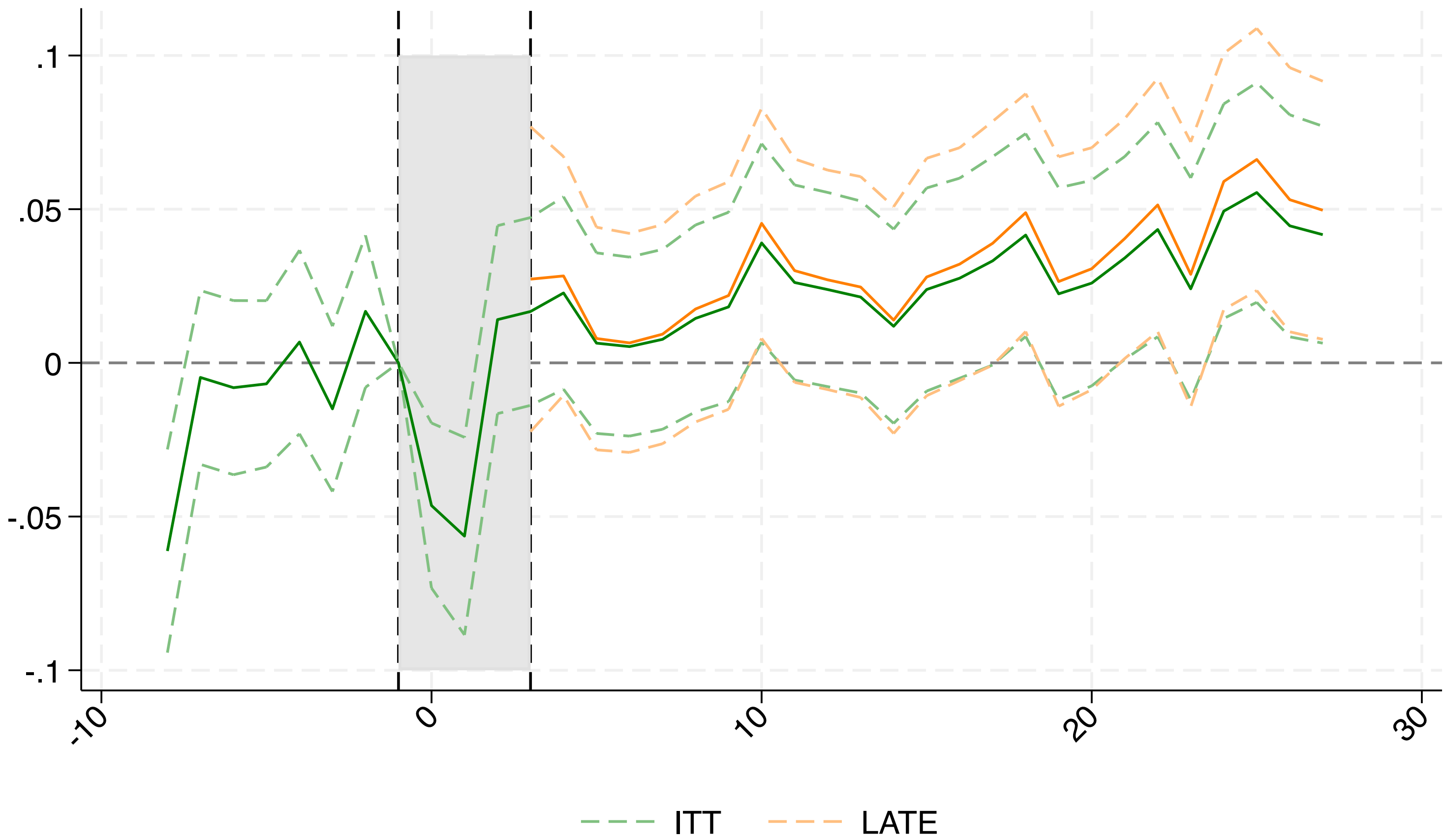
(a) Time trends



The figure displays average values of the outcome by group and across time. Treated taxpayers taxpayers monitored by the Large Taxpayers Center (LTC), where the adoption of Etax was made mandatory by a ministerial decree issued in July 2017. Controlled taxpayers are the largest 860 taxpayers monitored by other centers of Dakar. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876.

Figure 9 – Liability declared is positive

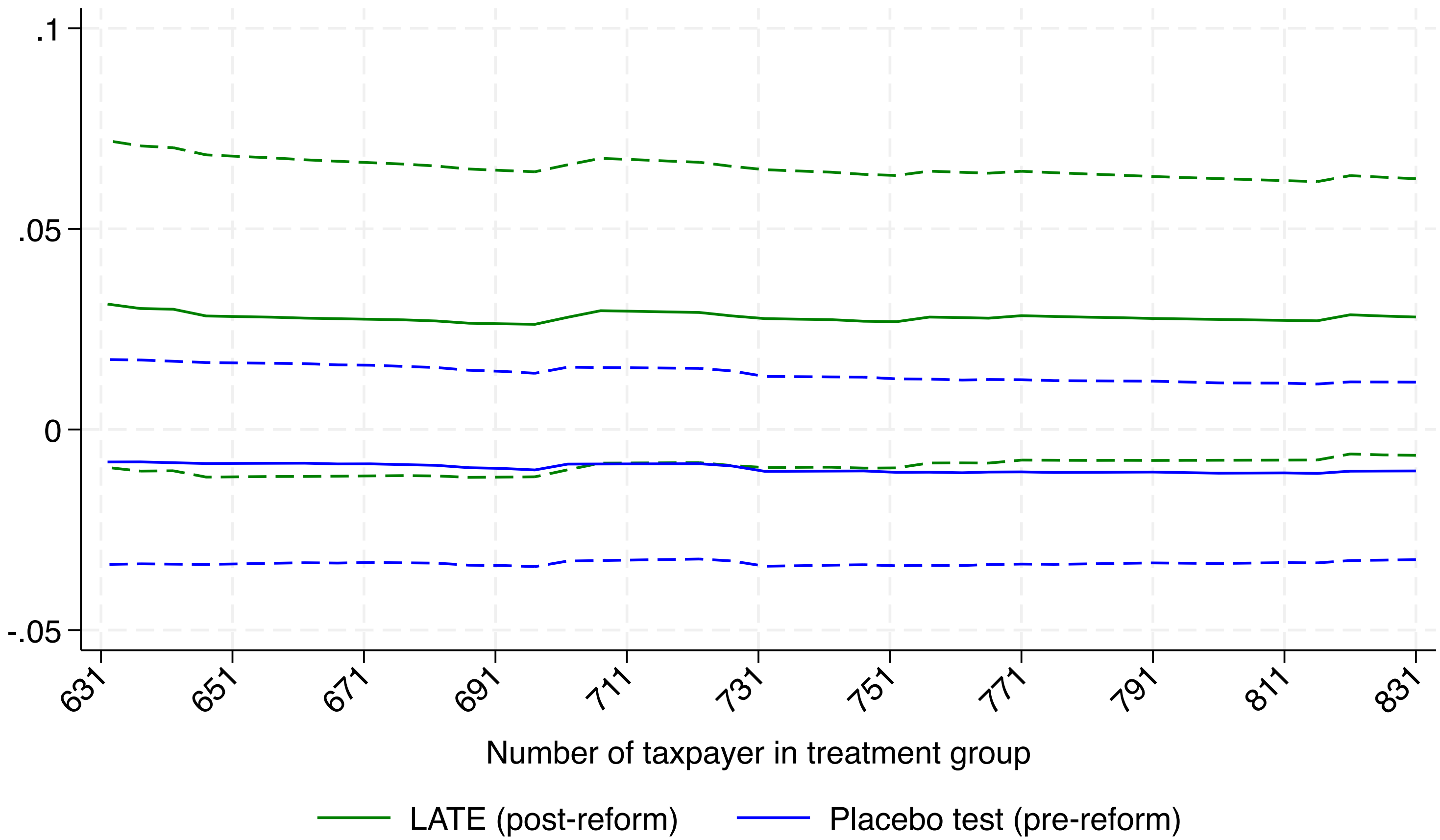
(b) Time-varying impact



Notes. The figure displays coefficients of lags, and leads (ITT or LATE) indicators of model 1 using the mandatory adoption of Etax for the Largest Taxpayers Center, set by ministerial decree in July 2017, as a source of exogenous variation. The reference period ($t = -1$) is August 2017. A transition period is highlighted in grey, during which adoption progressively took place. In the LATE specification, we instrument each leads of model 1 by an indicator $Etax_{it}^{k}$ equal to one if and only if $t = k$ and taxpayer i has effectively adopted etax by t. All estimations use time and taxpayer fixed-effects. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Figure 9 – Liability declared is positive

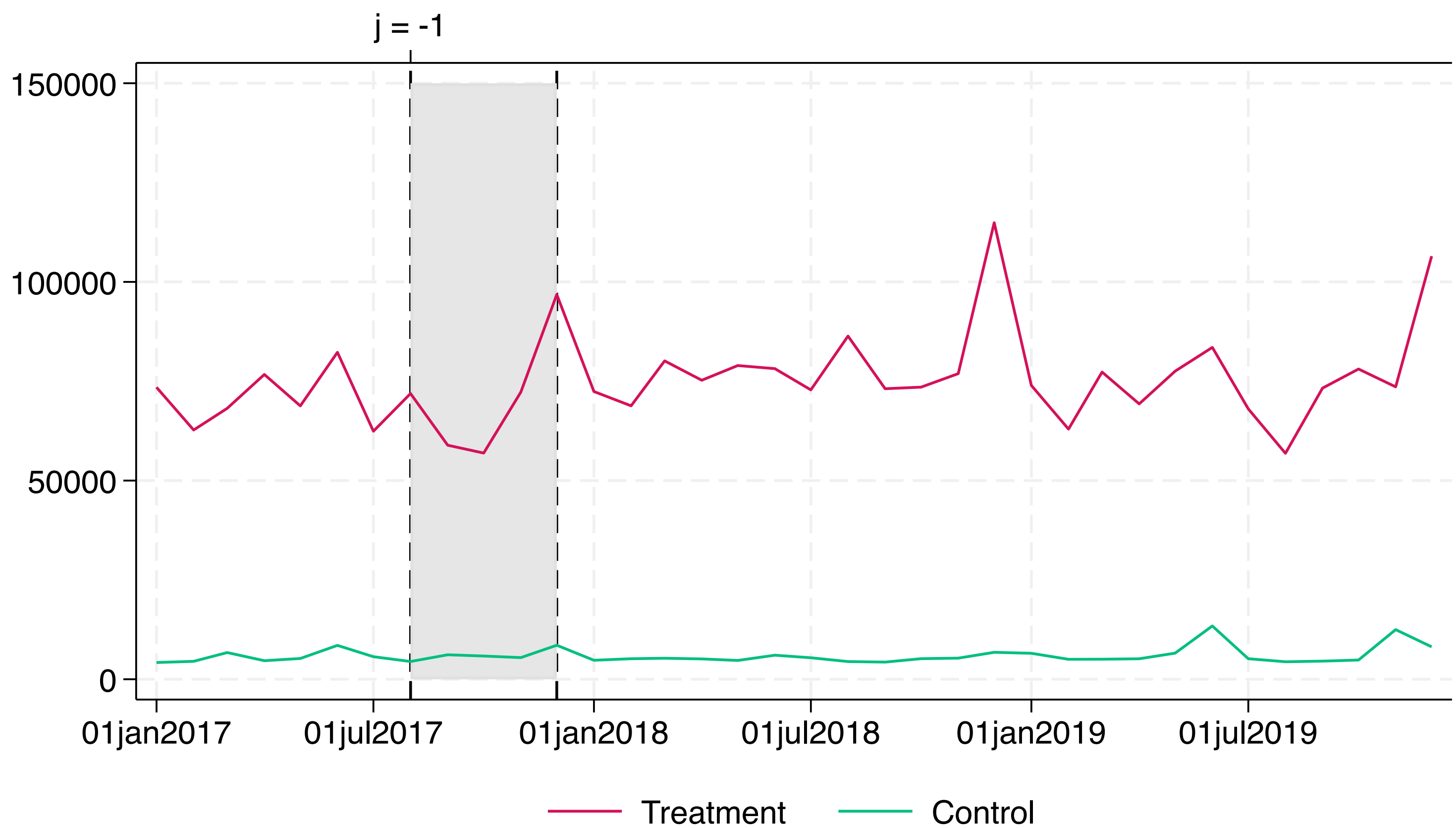
(c) Sensitivity to extremely large taxpayers



Notes. The Figure displays variations in treatment impact (post-reform) and placebo test (pre-reform) as we trim the treated group (taxpayers monitored be the LTC) by incrementally removing the 5 largest taxpayers. LATE coefficients (green line) are obtained by estimating model 2 for the post-reform period, using two stages least square method and instrumenting $(Treat_{it} \cdot Post_{it})$ by an indicator $Etax_{it}$ equal to one if and only if taxpayer i has started using Etax by t. Coefficients of placebo tests (blue line) are obtained by estimating model 2 for the pre-reform period replacing $(Treat_{it} \cdot Post_{it})$ by $(Treat_{it} \cdot Pre_{it})$ and using Ordinary Least Square method. The reference period is always August 2017. All samples are balanced, and all estimations use time and taxpayer fixed-effect. The axis indicates the number of taxpayers in the treatment group after trimming. The initial sample size of the treatment group is 831. The size of the control group is 860 and remains constant through all specifications.

Figure 10 – Liability declared (USD)

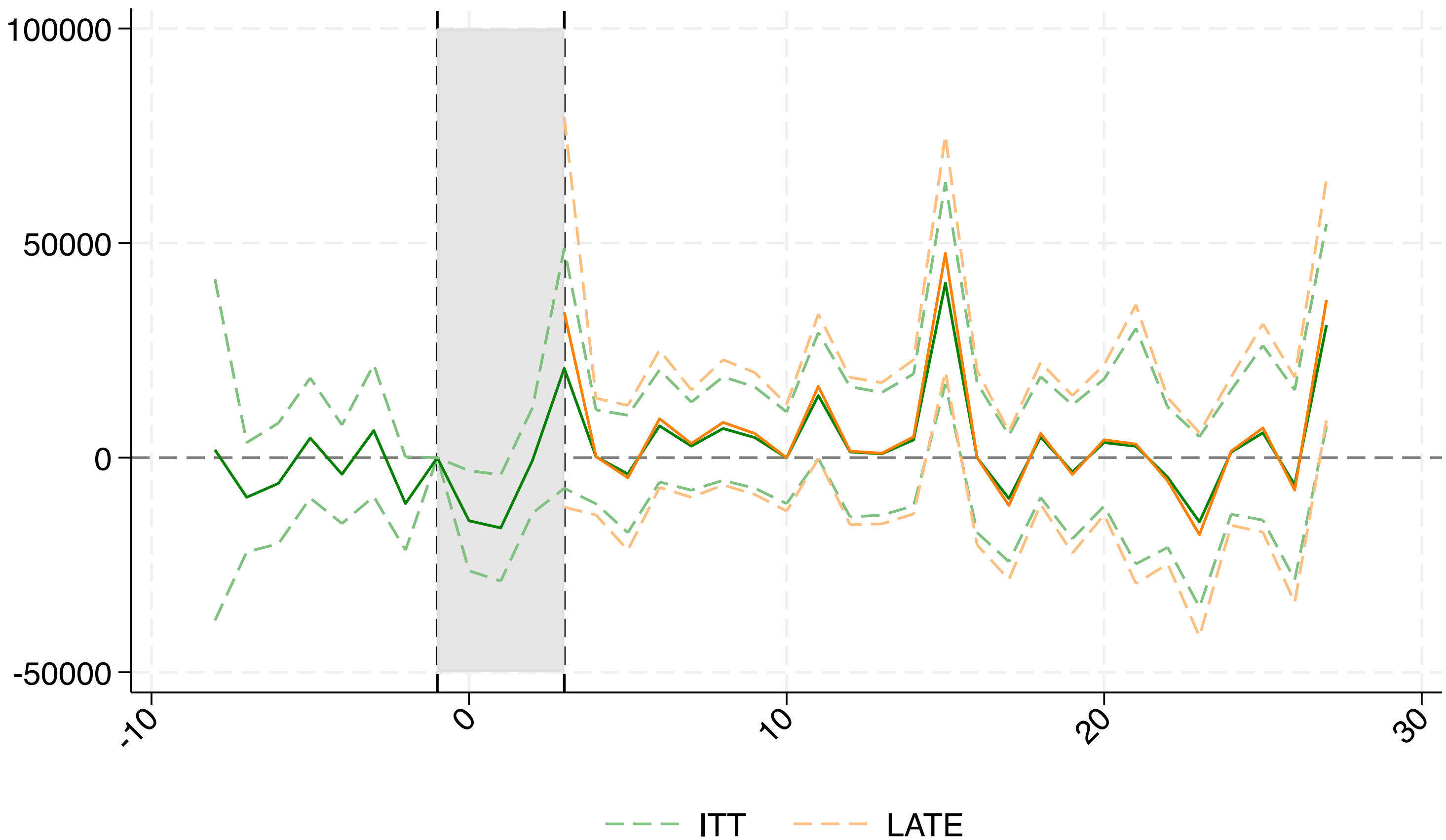
(a) Time trends



The figure displays average values of the outcome by group and across time. Treated taxpayers taxpayers monitored by the Large Taxpayers Center (LTC), where the adoption of Etax was made mandatory by a ministerial decree issued in July 2017. Controlled taxpayers are the largest 860 taxpayers monitored by other centers of Dakar. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876.

Figure 10 – Liability declared (USD)

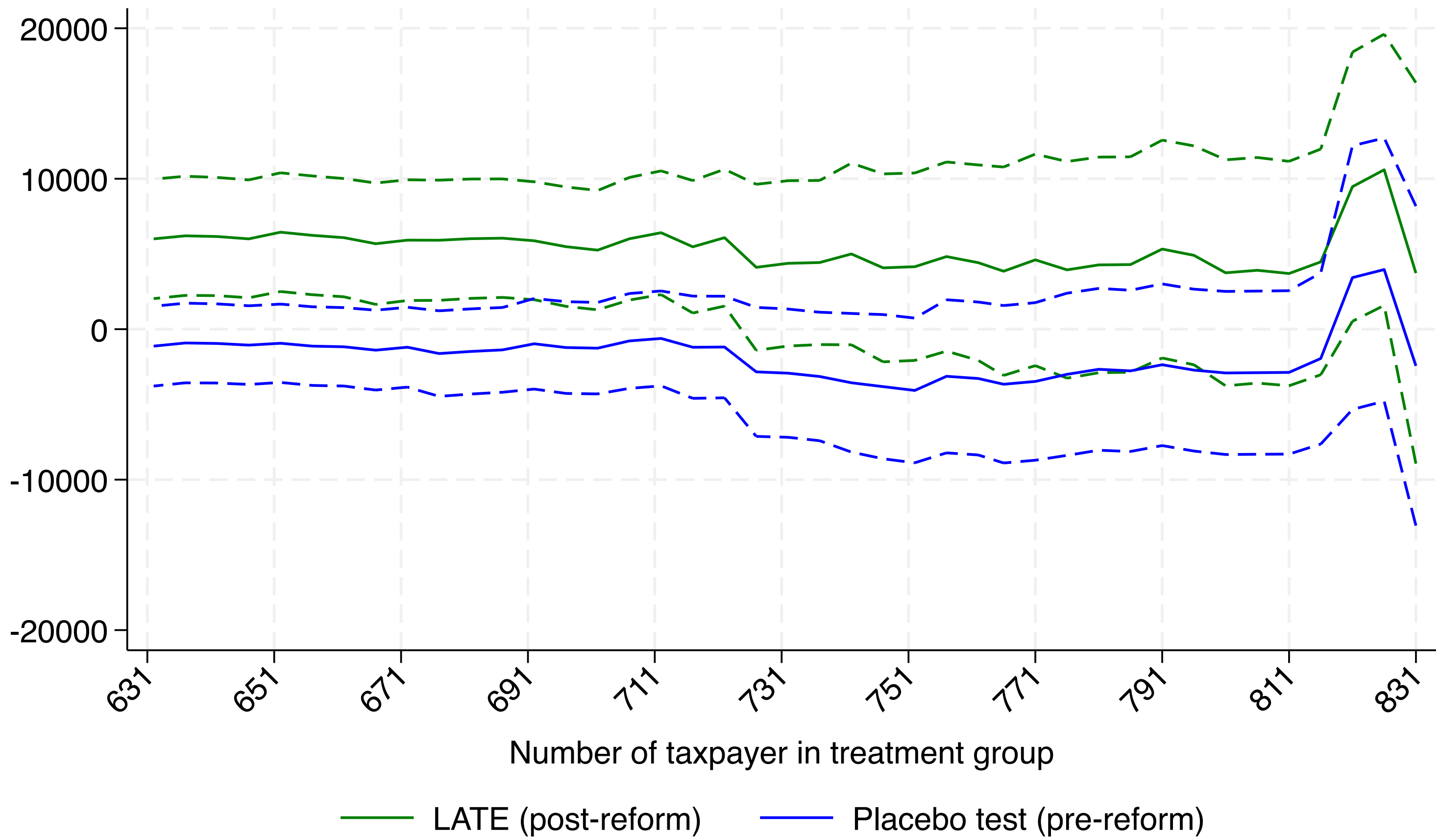
(b) Time-varying impact



Notes. The figure displays coefficients of lags, and leads (ITT or LATE) indicators of model 1 using the mandatory adoption of Etax for the Largest Taxpayers Center, set by ministerial decree in July 2017, as a source of exogenous variation. The reference period ($t = -1$) is August 2017. A transition period is highlighted in grey, during which adoption progressively took place. In the LATE specification, we instrument each leads of model 1 by an indicator $Etax_{it}^{k}$ equal to one if and only if $t = k$ and taxpayer i has effectively adopted etax by t. All estimations use time and taxpayer fixed-effects. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Figure 10 – Liability declared (USD)

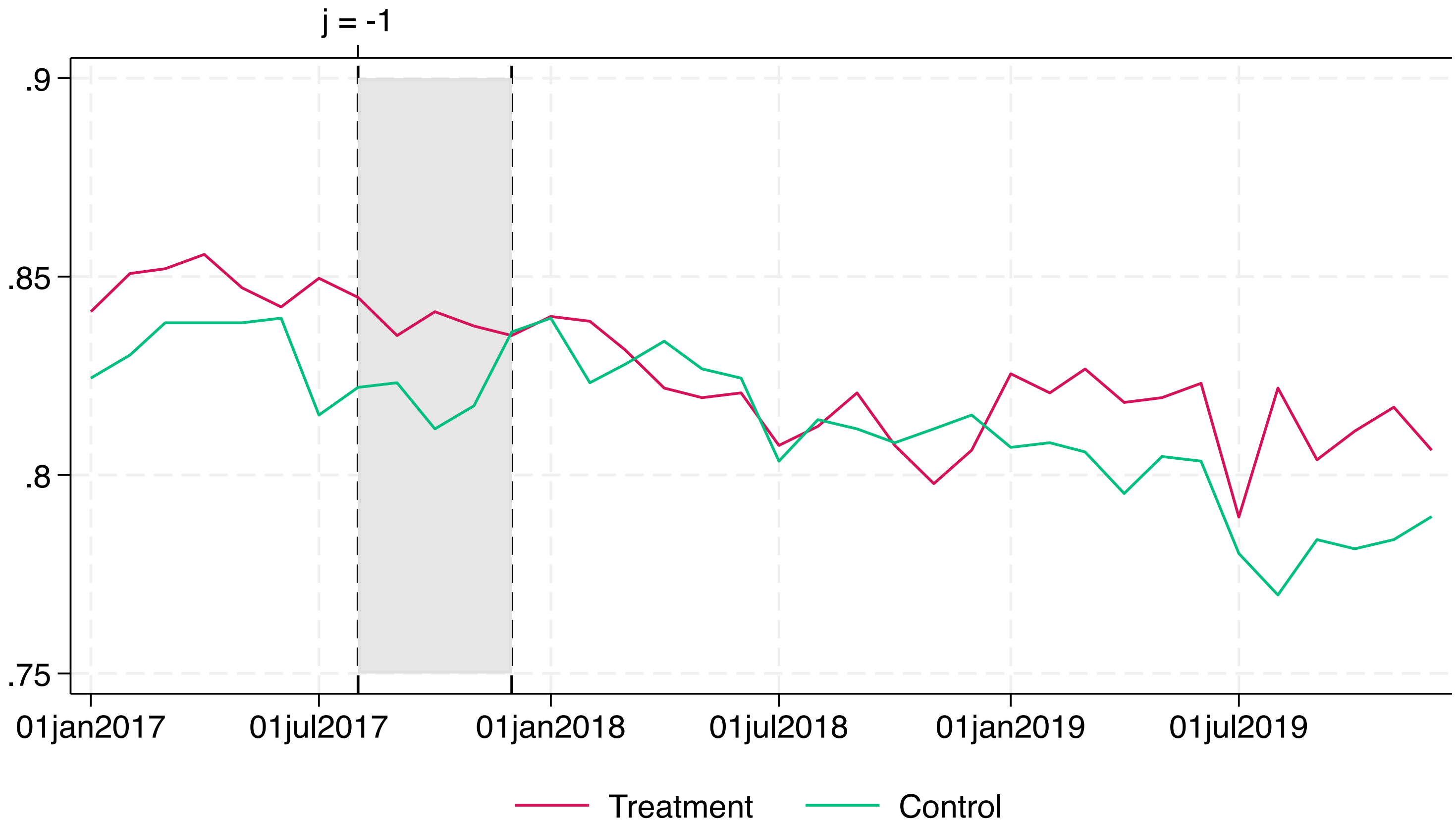
(c) Sensitivity to extremely large taxpayers



Notes. The Figure displays variations in treatment impact (post-reform) and placebo test (pre-reform) as we trim the treated group (taxpayers monitored be the LTC) by incrementally removing the 5 largest taxpayers. LATE coefficients (green line) are obtained by estimating model 2 for the post-reform period, using two stages least square method and instrumenting $(Treat_{it} \cdot Post_{it})$ by an indicator $Etax_{it}$ equal to one if and only if taxpayer i has started using Etax by t. Coefficients of placebo tests (blue line) are obtained by estimating model 2 for the pre-reform period replacing $(Treat_{it} \cdot Post_{it})$ by $(Treat_{it} \cdot Pre_{it})$ and using Ordinary Least Square method. The reference period is always August 2017. All samples are balanced, and all estimations use time and taxpayer fixed-effect. The axis indicates the number of taxpayers in the treatment group after trimming. The initial sample size of the treatment group is 831. The size of the control group is 860 and remains constant through all specifications.

Figure 11 – Tax paid is positive

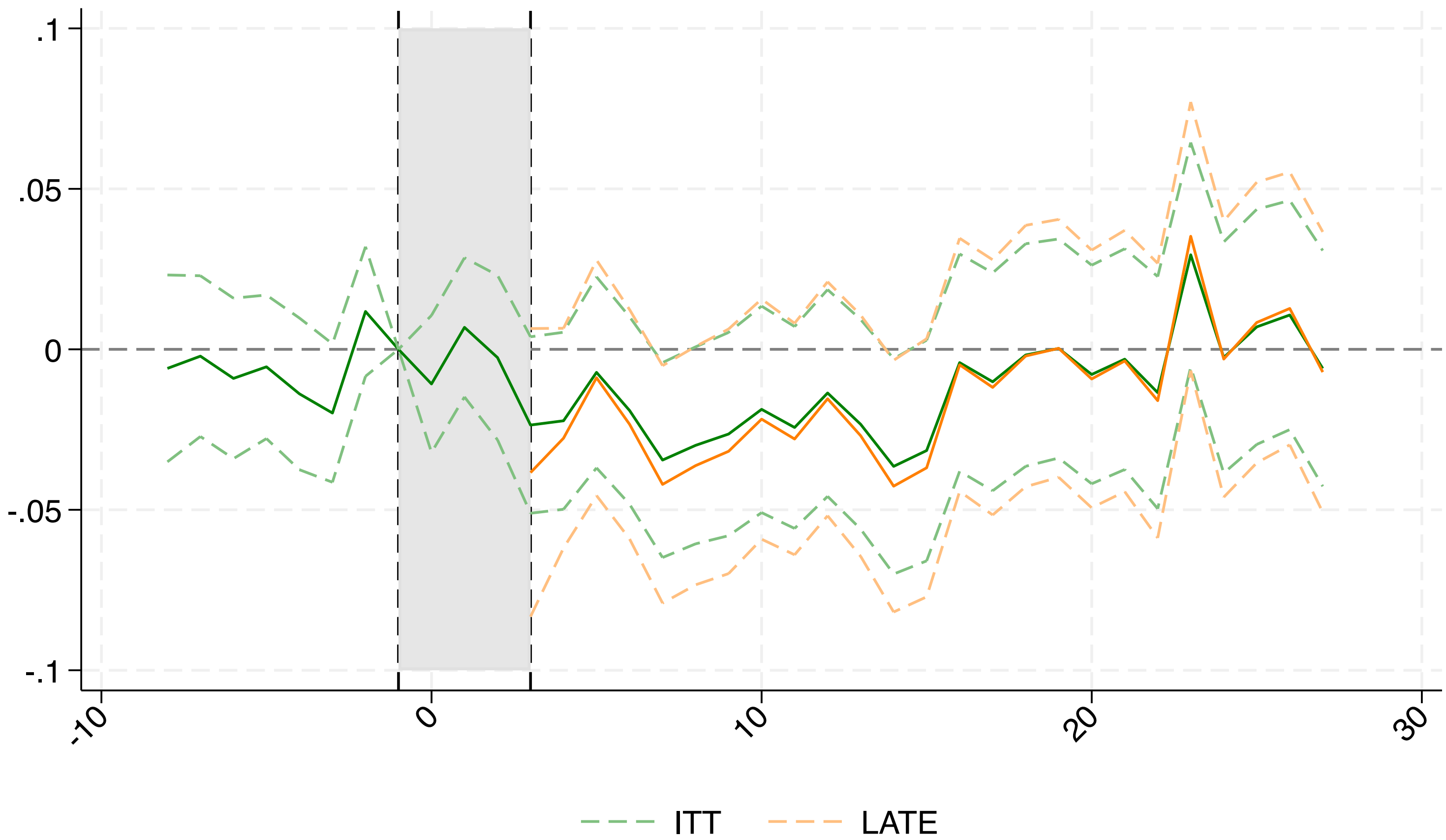
(a) Time trends



The figure displays average values of the outcome by group and across time. Treated taxpayers taxpayers monitored by the Large Taxpayers Center (LTC), where the adoption of Etax was made mandatory by a ministerial decree issued in July 2017. Controlled taxpayers are the largest 860 taxpayers monitored by other centers of Dakar. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876.

Figure 11 – Tax paid is positive

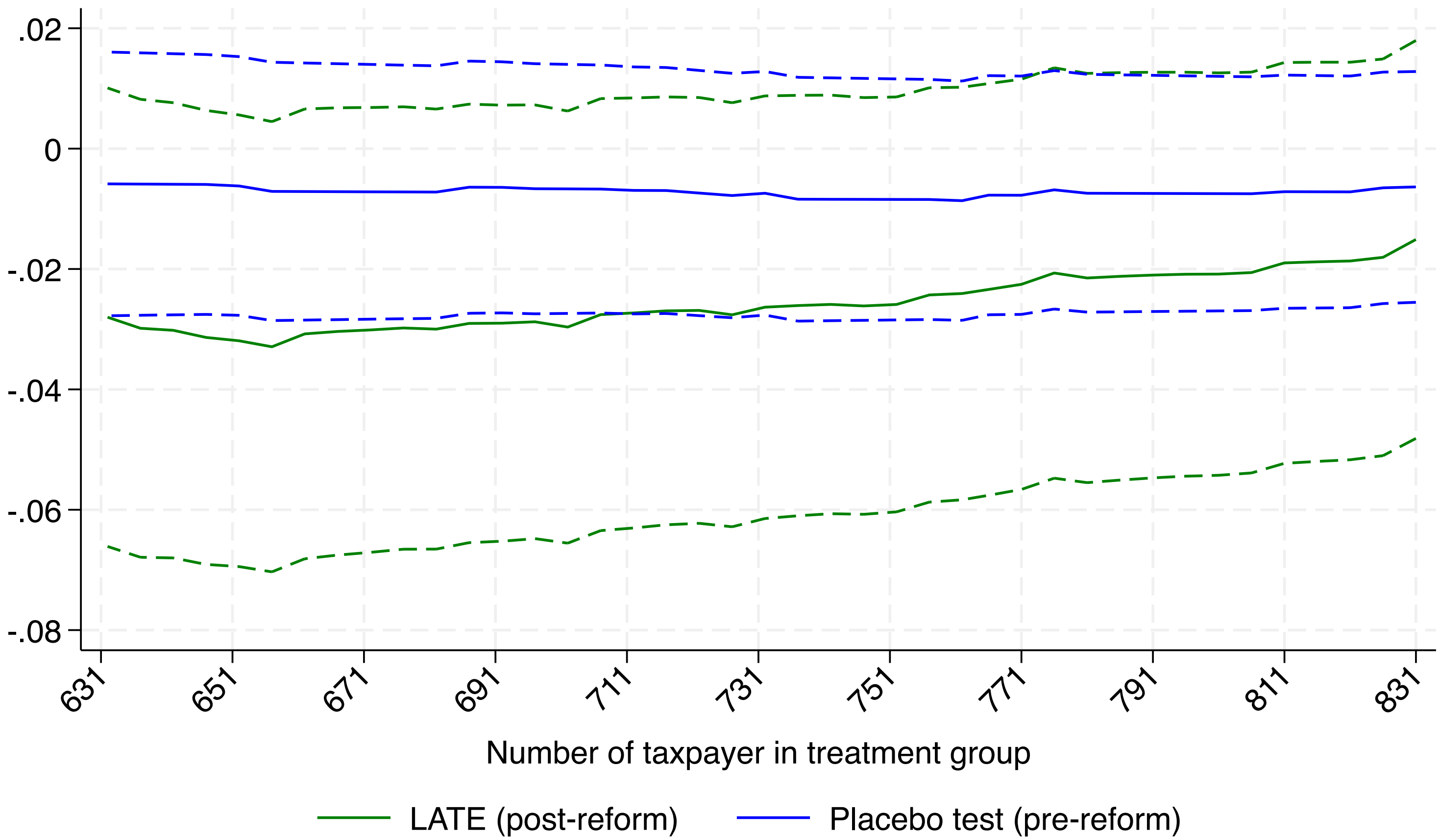
(b) Time-varying impact



Notes. The figure displays coefficients of lags, and leads (ITT or LATE) indicators of model 1 using the mandatory adoption of Etax for the Largest Taxpayers Center, set by ministerial decree in July 2017, as a source of exogenous variation. The reference period ($t = -1$) is August 2017. A transition period is highlighted in grey, during which adoption progressively took place. In the LATE specification, we instrument each leads of model 1 by an indicator $Etax_{it}^{k}$ equal to one if and only if $t = k$ and taxpayer i has effectively adopted etax by t. All estimations use time and taxpayer fixed-effects. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Figure 11 – Tax paid is positive

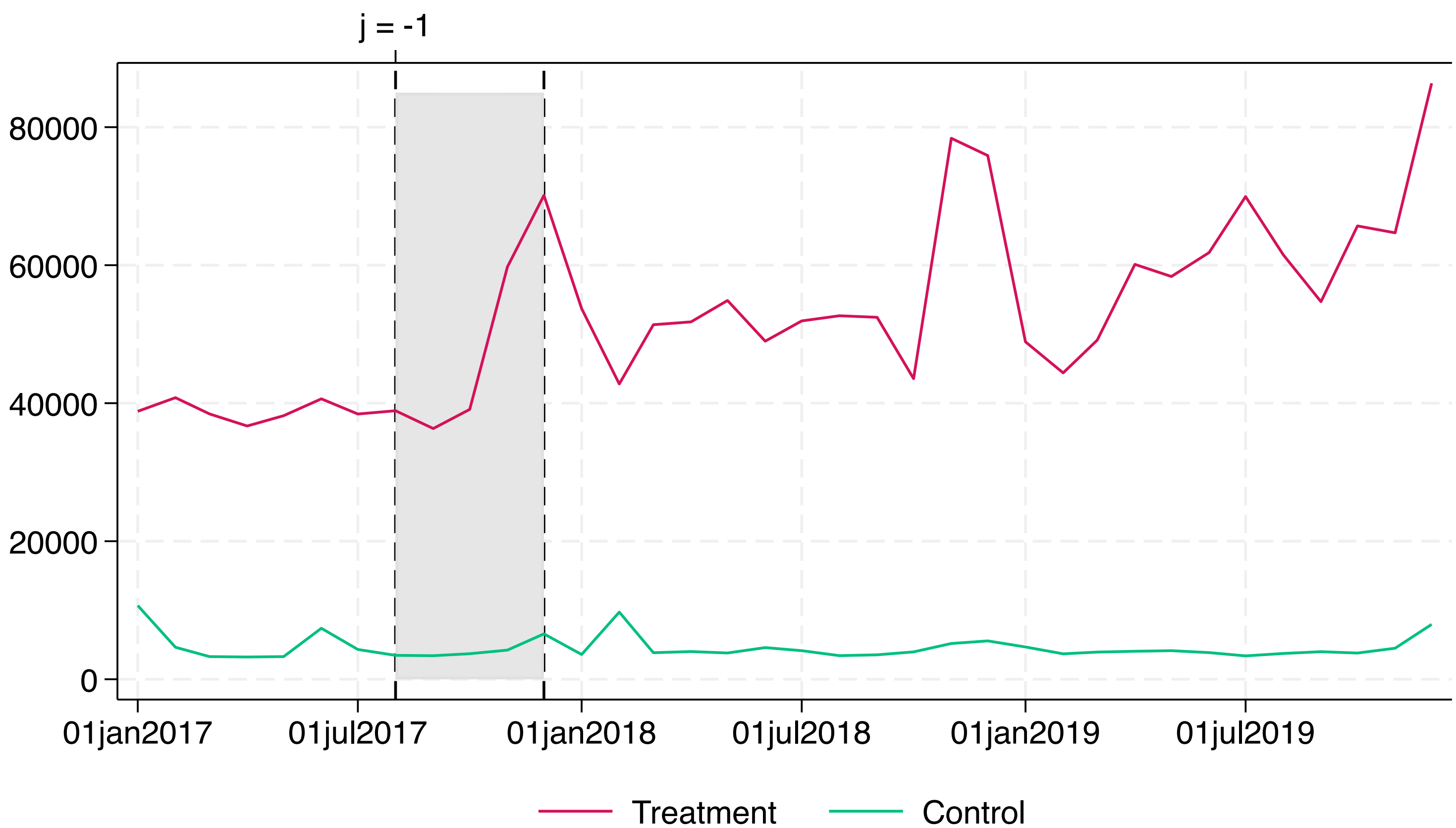
(c) Sensitivity to extremely large taxpayers



Notes. The Figure displays variations in treatment impact (post-reform) and placebo test (pre-reform) as we trim the treated group (taxpayers monitored be the LTC) by incrementally removing the 5 largest taxpayers. LATE coefficients (green line) are obtained by estimating model 2 for the post-reform period, using two stages least square method and instrumenting $(Treat_{it} \cdot Post_{it})$ by an indicator $Etax_{it}$ equal to one if and only if taxpayer i has started using Etax by t. Coefficients of placebo tests (blue line) are obtained by estimating model 2 for the pre-reform period replacing $(Treat_{it} \cdot Post_{it})$ by $(Treat_{it} \cdot Pre_{it})$ and using Ordinary Least Square method. The reference period is always August 2017. All samples are balanced, and all estimations use time and taxpayer fixed-effect. The axis indicates the number of taxpayers in the treatment group after trimming. The initial sample size of the treatment group is 831. The size of the control group is 860 and remains constant through all specifications.

Figure 12 – Amount paid (USD)

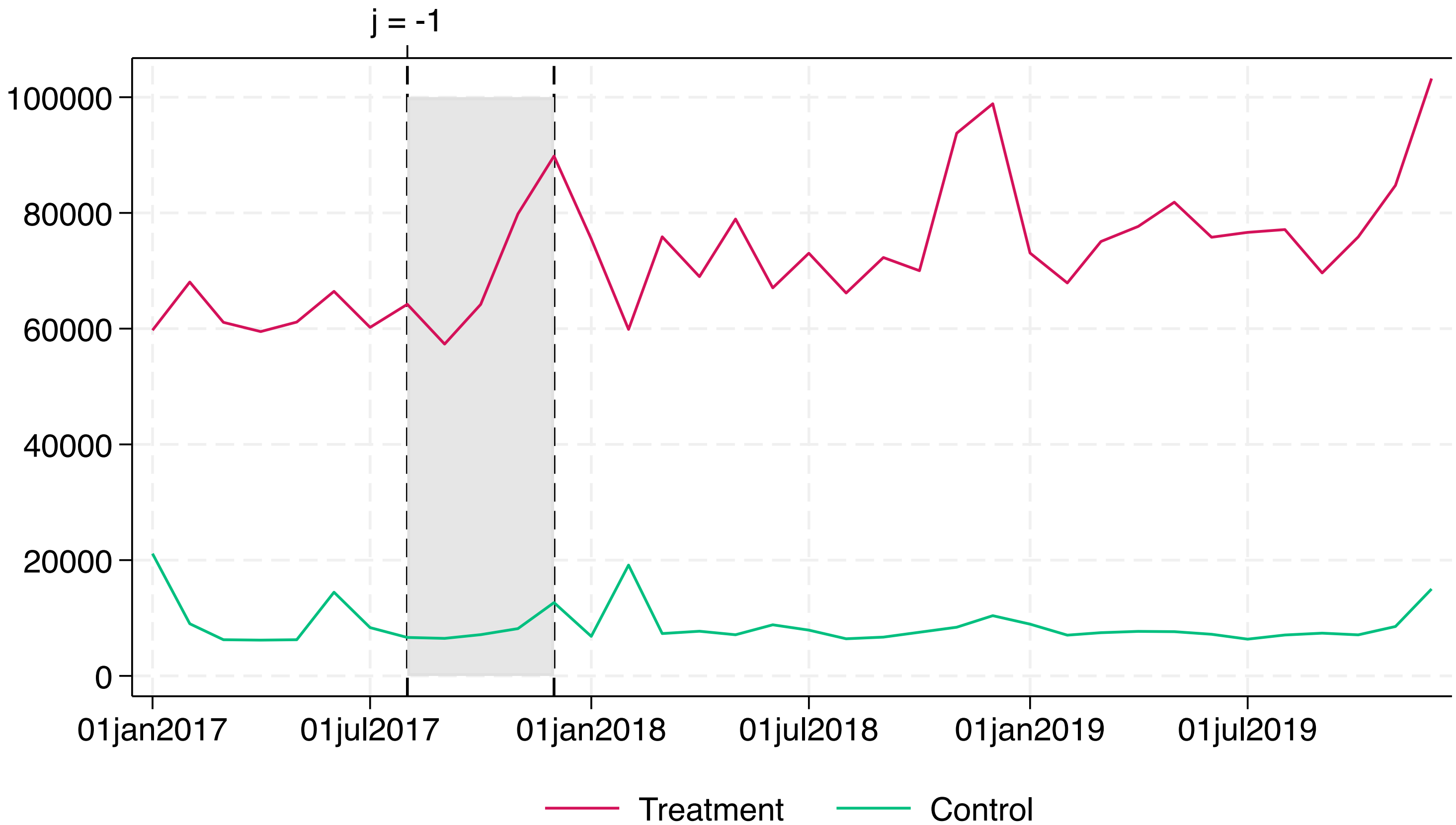
(a) Time trends



Notes. The figure displays average values of the outcome by group and across time. Treated taxpayers taxpayers monitored by the Large Taxpayers Center (LTC), where the adoption of Etax was made mandatory by a ministerial decree issued in July 2017. Controlled taxpayers are the largest 860 taxpayers monitored by other centers of Dakar. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876.

Figure 12 – Amount paid (USD)

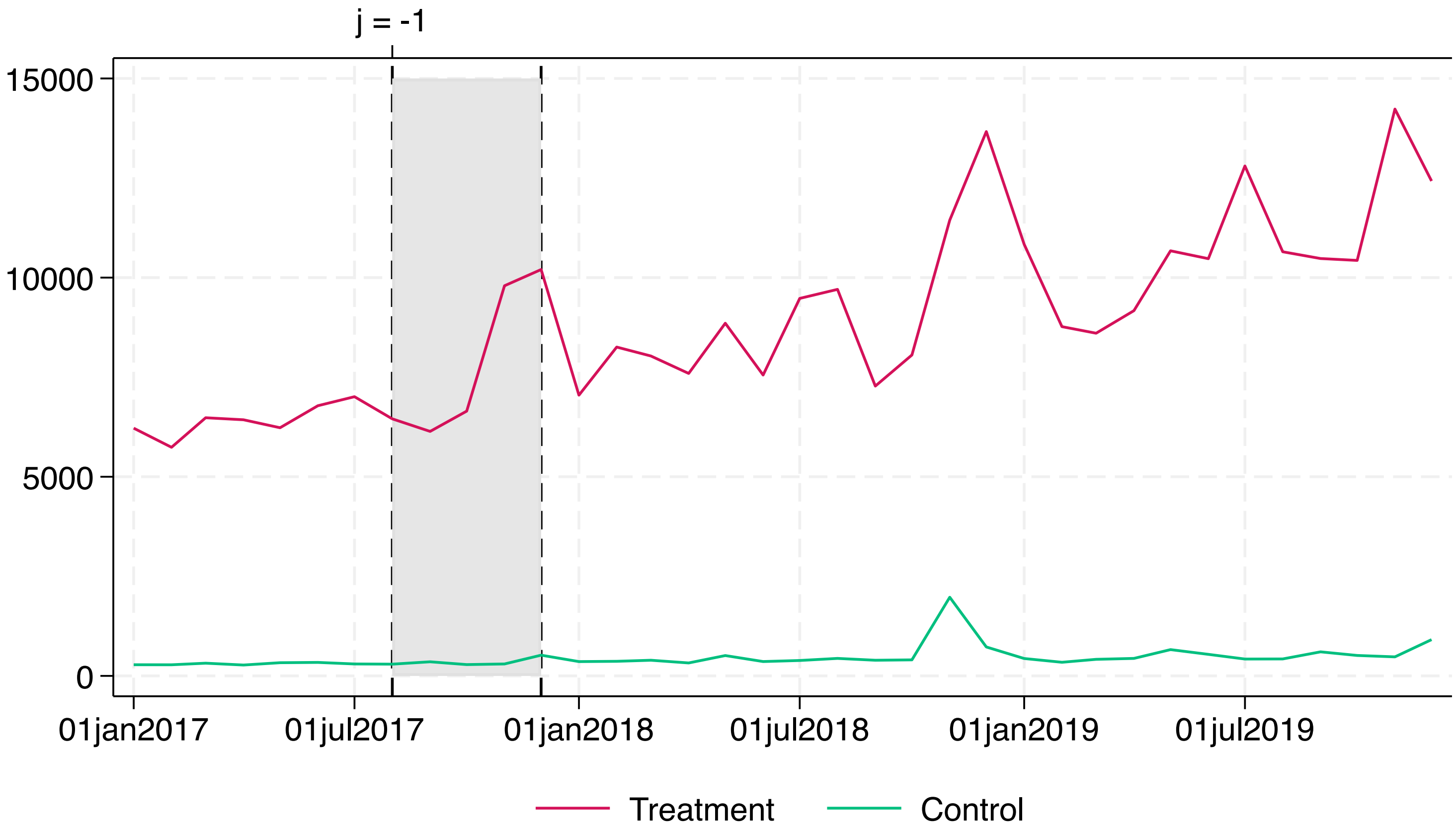
(b) Time trends - high compliance group



Notes. The figure displays average values of the outcome by group and across time. Treated taxpayers taxpayers monitored by the Large Taxpayers Center (LTC), where the adoption of Etax was made mandatory by a ministerial decree issued in July 2017. Controlled taxpayers are the largest 860 taxpayers monitored by other centers of Dakar. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876.

Figure 12 – Amount paid (USD)

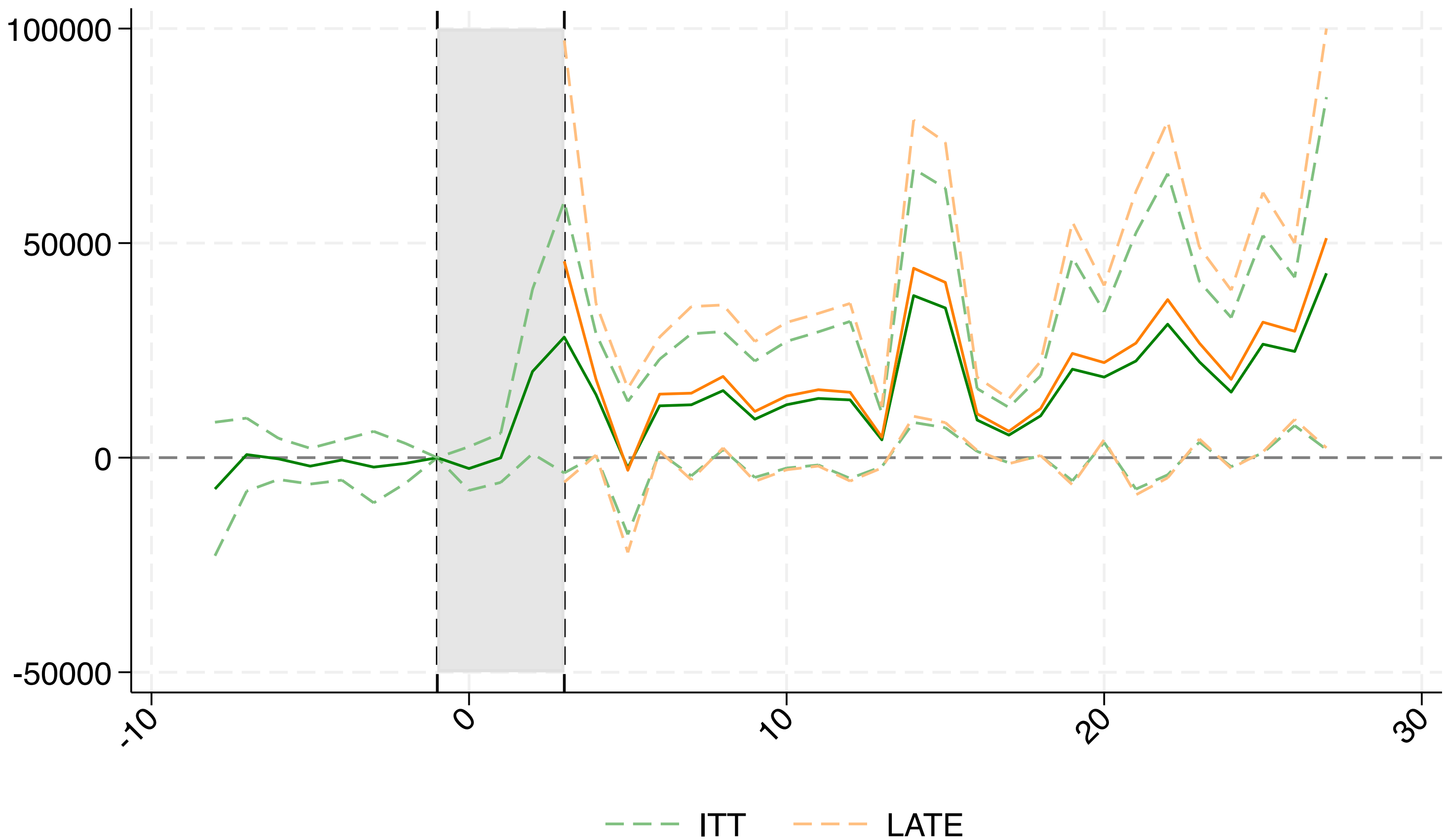
(c) Time trends - low compliance group



Notes. The figure displays average values of the outcome by group and across time. Treated taxpayers taxpayers monitored by the Large Taxpayers Center (LTC), where the adoption of Etax was made mandatory by a ministerial decree issued in July 2017. Controlled taxpayers are the largest 860 taxpayers monitored by other centers of Dakar. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876.

Figure 12 – Amount paid (USD)

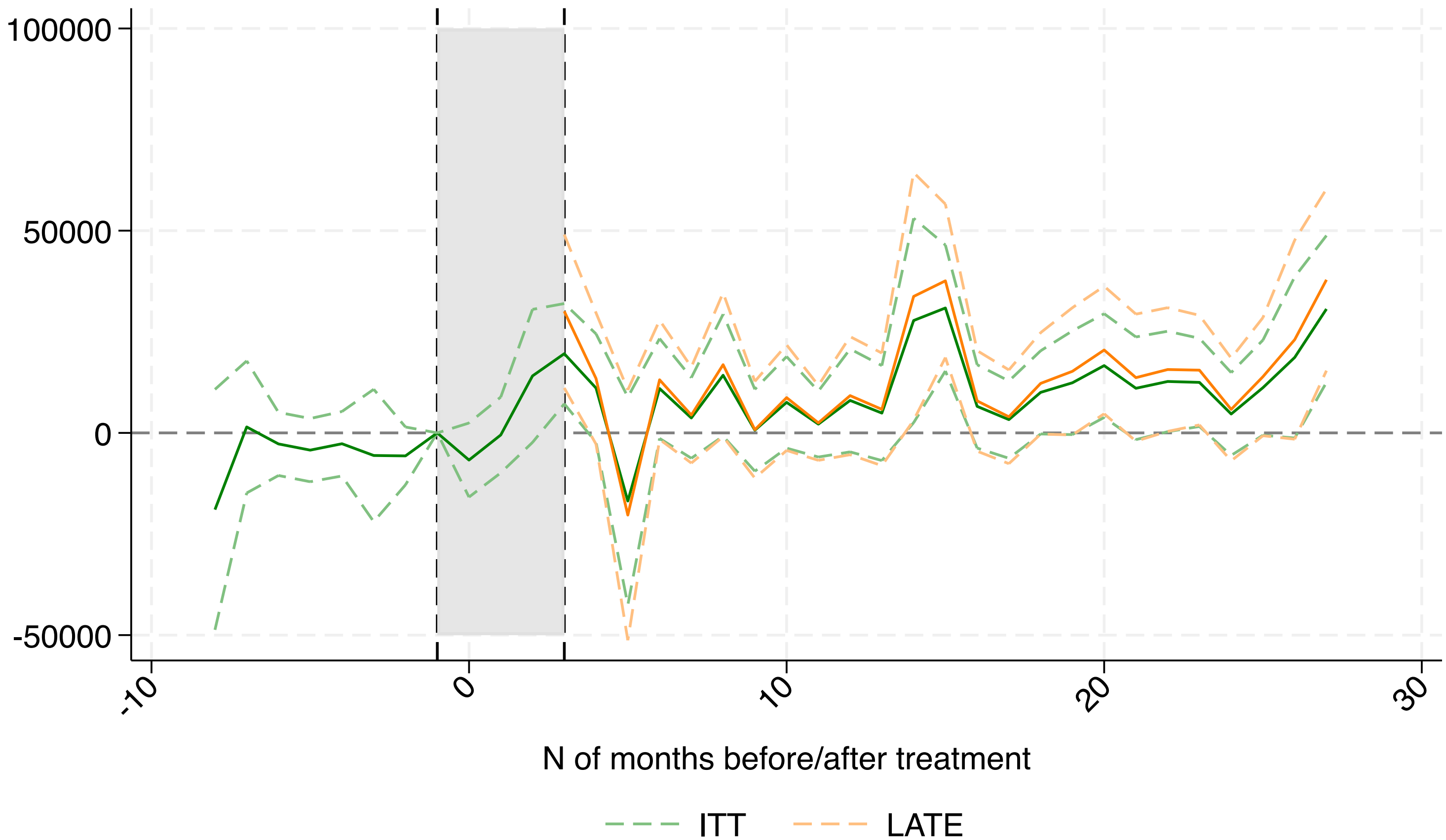
(d) Time-varying impact



Notes. The figure displays coefficients of lags, and leads (ITT or LATE) indicators of model 1 using the mandatory adoption of Etax for the Largest Taxpayers Center, set by ministerial decree in July 2017, as a source of exogenous variation. The reference period ($t = -1$) is August 2017. A transition period is highlighted in grey, during which adoption progressively took place. In the LATE specification, we instrument each leads of model 1 by an indicator $Etax_{it}^{k}$ equal to one if and only if $t = k$ and taxpayer i has effectively adopted etax by t. All estimations use time and taxpayer fixed-effects. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Figure 12 – Amount paid (USD)

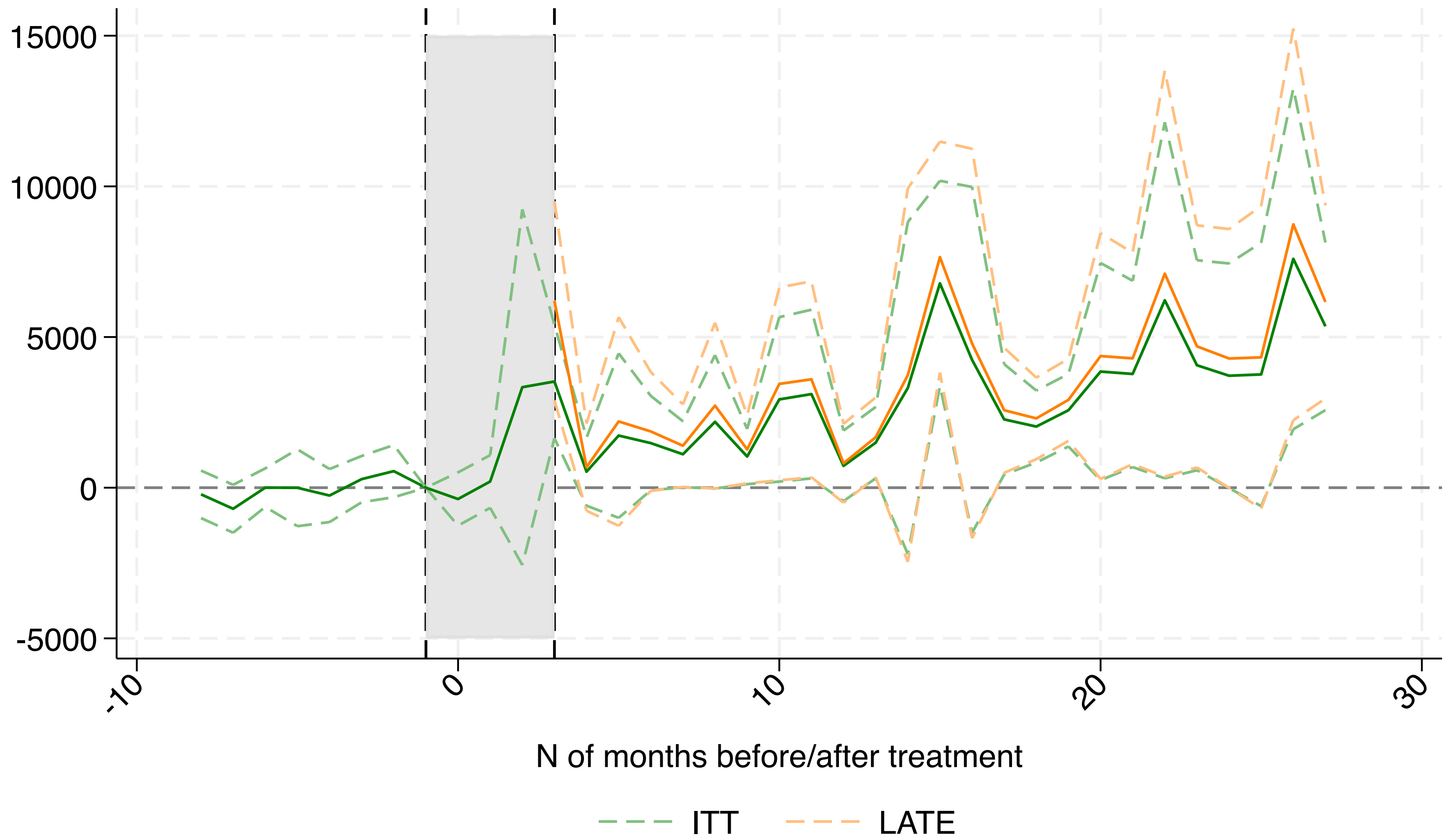
(e) Time-varying impact - high compliance group



Notes. The figure displays coefficients of lags, and leads (ITT or LATE) indicators of model 1 using the mandatory adoption of Etax for the Largest Taxpayers Center, set by ministerial decree in July 2017, as a source of exogenous variation. The reference period ($t = -1$) is August 2017. A transition period is highlighted in grey, during which adoption progressively took place. In the LATE specification, we instrument each leads of model 1 by an indicator $Etax_{it}^{k}$ equal to one if and only if $t = k$ and taxpayer i has effectively adopted etax by t. All estimations use time and taxpayer fixed-effects. Data is a balanced sample of 862 taxpayers observed for exactly 36 months. Total sample size is 31,032. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Figure 12 – Amount paid (USD)

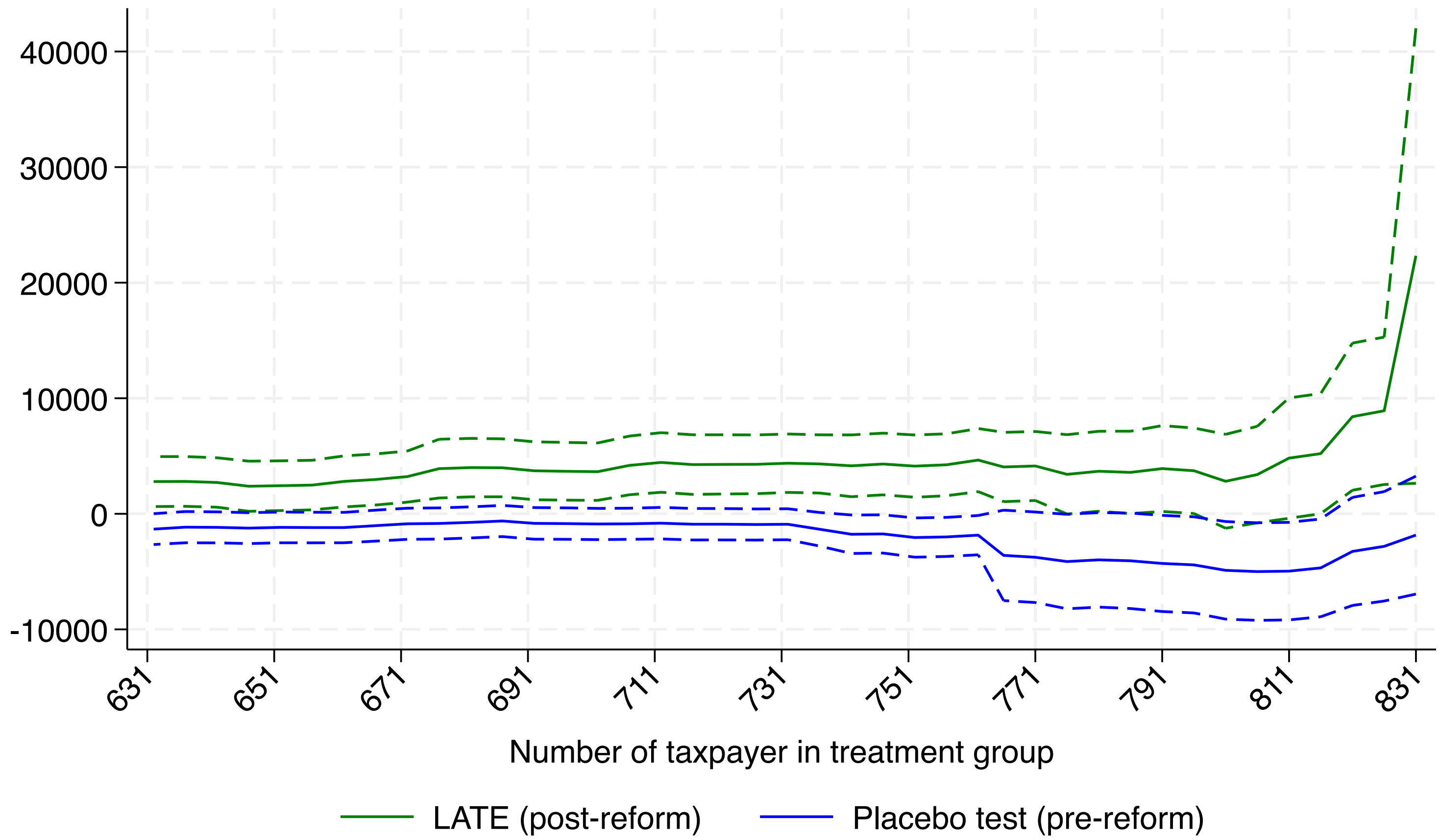
(f) Time-varying impact - low compliance group



Notes. The figure displays coefficients of lags, and leads (ITT or LATE) indicators of model 1 using the mandatory adoption of Etax for the Largest Taxpayers Center, set by ministerial decree in July 2017, as a source of exogenous variation. The reference period ($t = -1$) is August 2017. A transition period is highlighted in grey, during which adoption progressively took place. In the LATE specification, we instrument each leads of model 1 by an indicator $Etax_{it}^{k}$ equal to one if and only if $t = k$ and taxpayer i has effectively adopted etax by t. All estimations use time and taxpayer fixed-effects. Data is a balanced sample of 862 taxpayers observed for exactly 36 months. Total sample size is 31,032. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Figure 12 – Amount paid (USD)

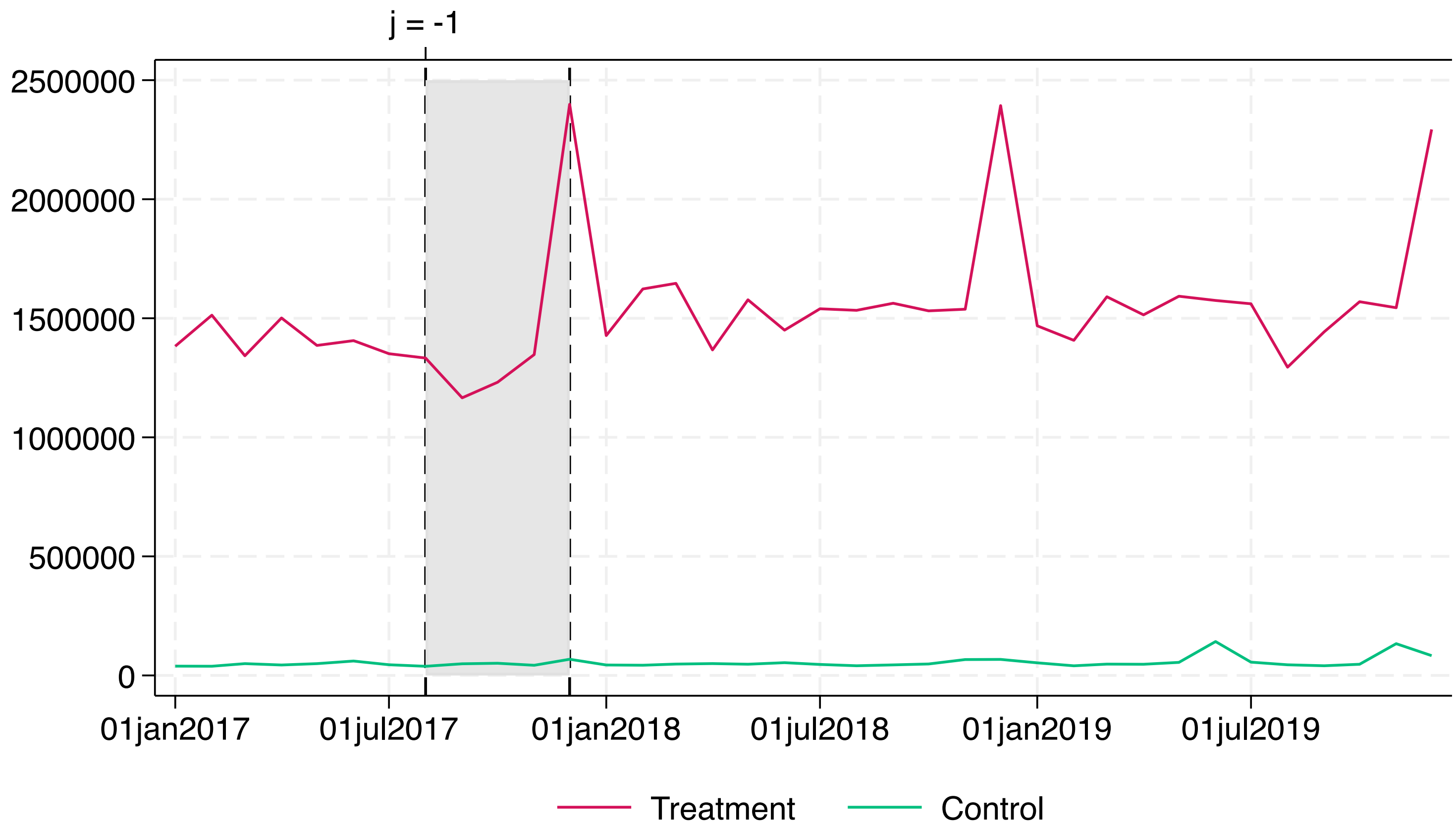
(g) Sensitivity to extremely large taxpayers



Notes. The Figure displays variations in treatment impact (post-reform) and placebo test (pre-reform) as we trim the treated group (taxpayers monitored be the LTC) by incrementally removing the 5 largest taxpayers. LATE coefficients (green line) are obtained by estimating model 2 for the post-reform period, using two stages least square method and instrumenting $(Treat_{it} \cdot Post_{it})$ by an indicator $Etax_{it}$ equal to one if and only if taxpayer i has started using Etax by t. Coefficients of placebo tests (blue line) are obtained by estimating model 2 for the pre-reform period replacing $(Treat_{it} \cdot Post_{it})$ by $(Treat_{it} \cdot Pre_{it})$ and using Ordinary Least Square method. The reference period is always August 2017. All samples are balanced, and all estimations use time and taxpayer fixed-effect. The axis indicates the number of taxpayers in the treatment group after trimming. The initial sample size of the treatment group is 831. The size of the control group is 860 and remains constant through all specifications.

Figure 13 – Turnover (USD)

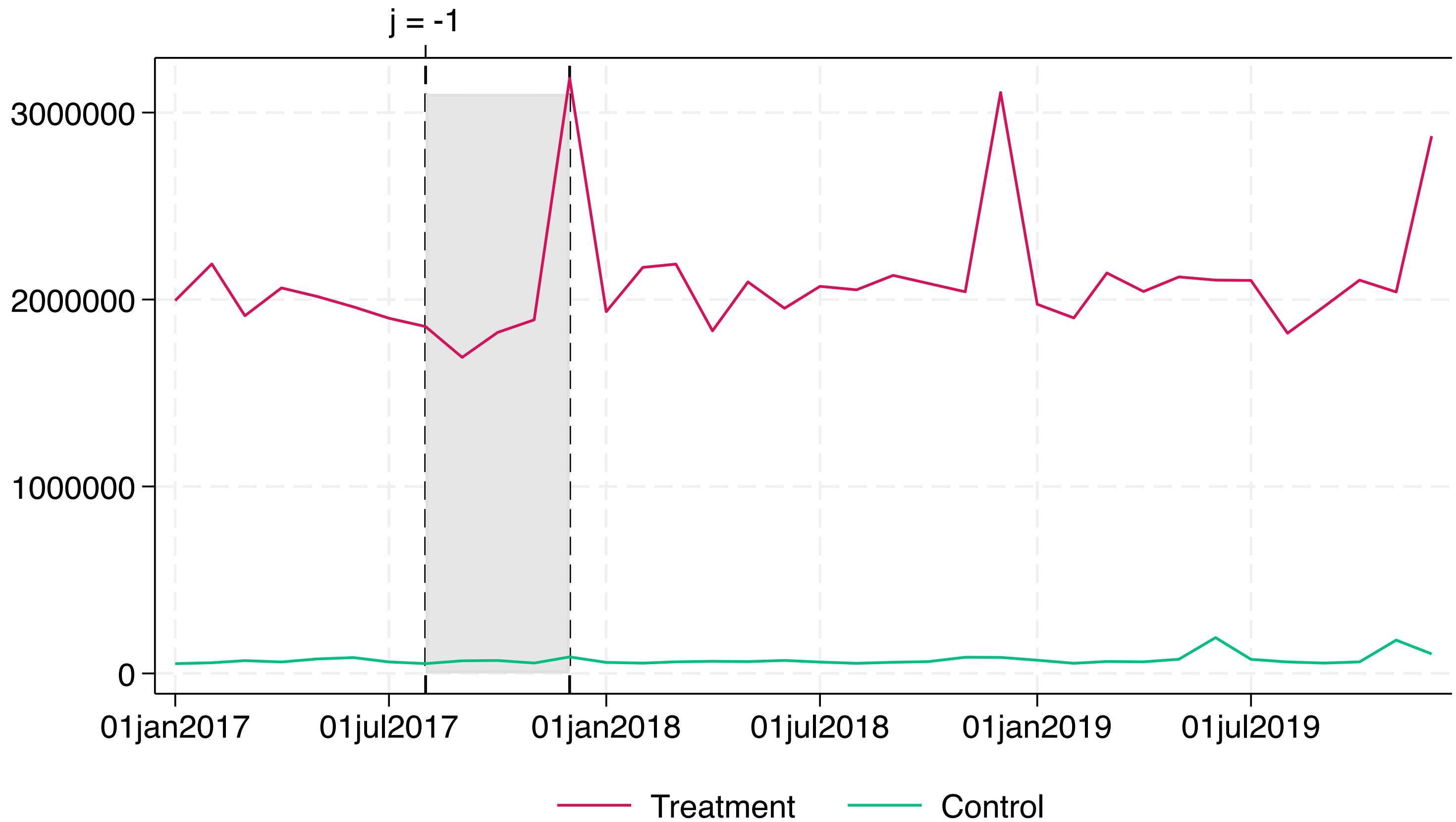
(a) Time trends



Notes. The figure displays average values of the outcome by group and across time. Treated taxpayers taxpayers monitored by the Large Taxpayers Center (LTC), where the adoption of Etax was made mandatory by a ministerial decree issued in July 2017. Controlled taxpayers are the largest 860 taxpayers monitored by other centers of Dakar. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876.

Figure 13 – Turnover (USD) - conditional on being positive

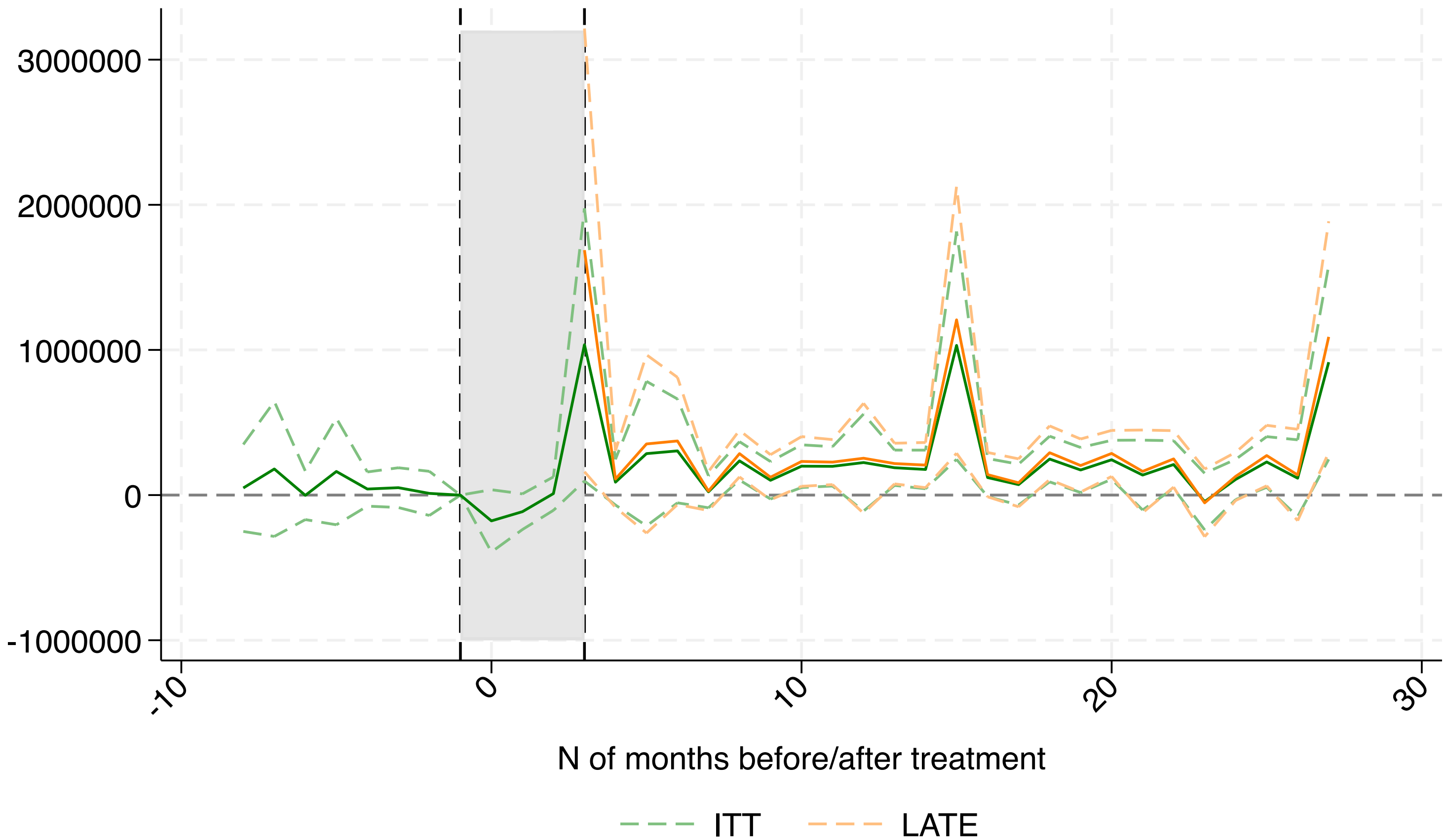
(b) Time trends



Notes. The figure displays average values of the outcome by group and across time. Treated taxpayers taxpayers monitored by the Large Taxpayers Center (LTC), where the adoption of Etax was made mandatory by a ministerial decree issued in July 2017. Controlled taxpayers are the largest 860 taxpayers monitored by other centers of Dakar. Data is an *unbalanced* sample of 1,549 taxpayers.

Figure 13 – Turnover (USD)

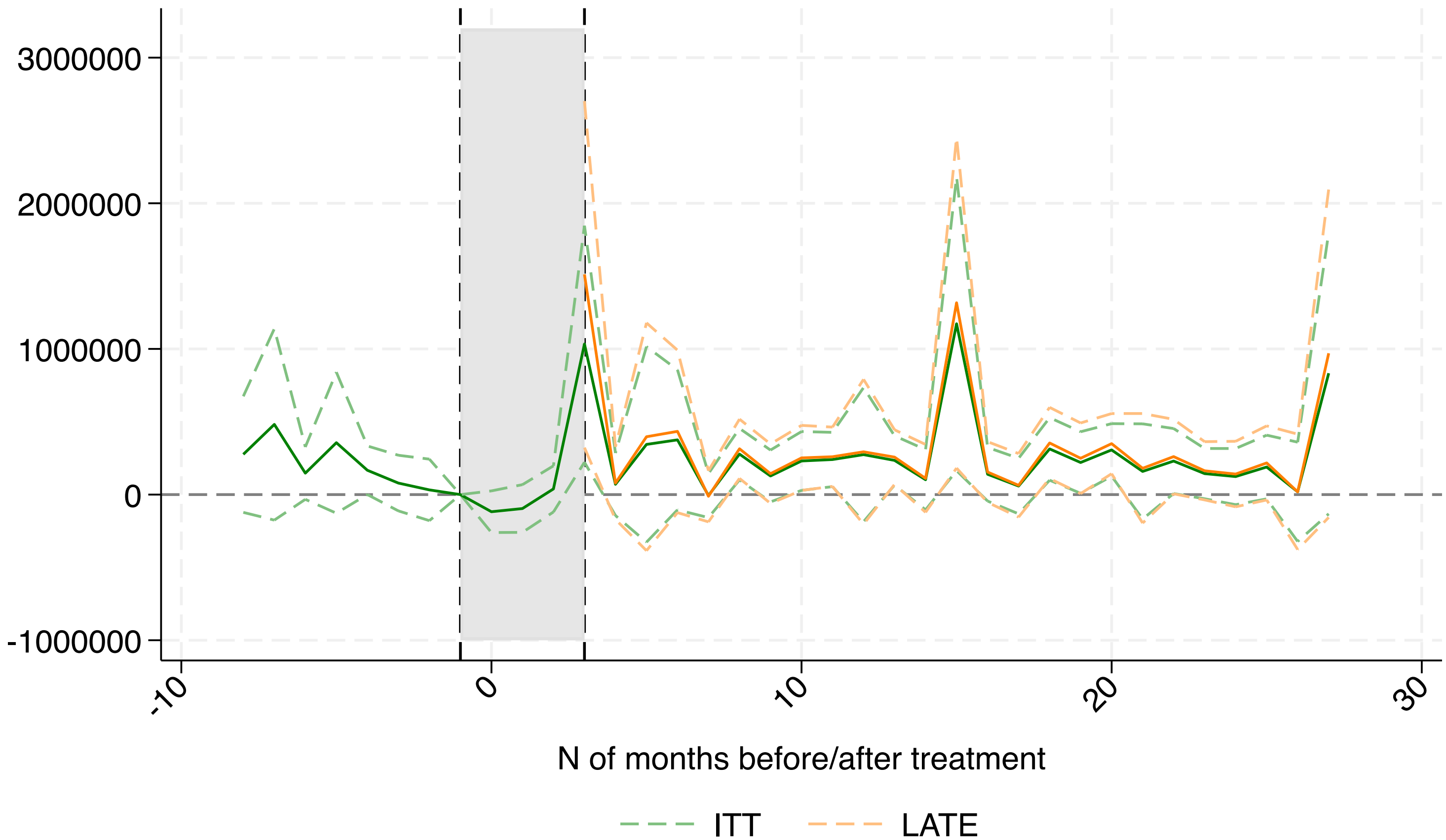
(c) Time-varying impact



Notes. The figure displays coefficients of lags, and leads (ITT or LATE) indicators of model 1 using the mandatory adoption of Etax for the Largest Taxpayers Center, set by ministerial decree in July 2017, as a source of exogenous variation. The reference period ($t = -1$) is August 2017. A transition period is highlighted in grey, during which adoption progressively took place. In the LATE specification, we instrument each leads of model 1 by an indicator $Etax_{it}^{k}$ equal to one if and only if $t = k$ and taxpayer i has effectively adopted etax by t. All estimations use time and taxpayer fixed-effects. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Figure 13 – Turnover (USD) - conditional on being positive

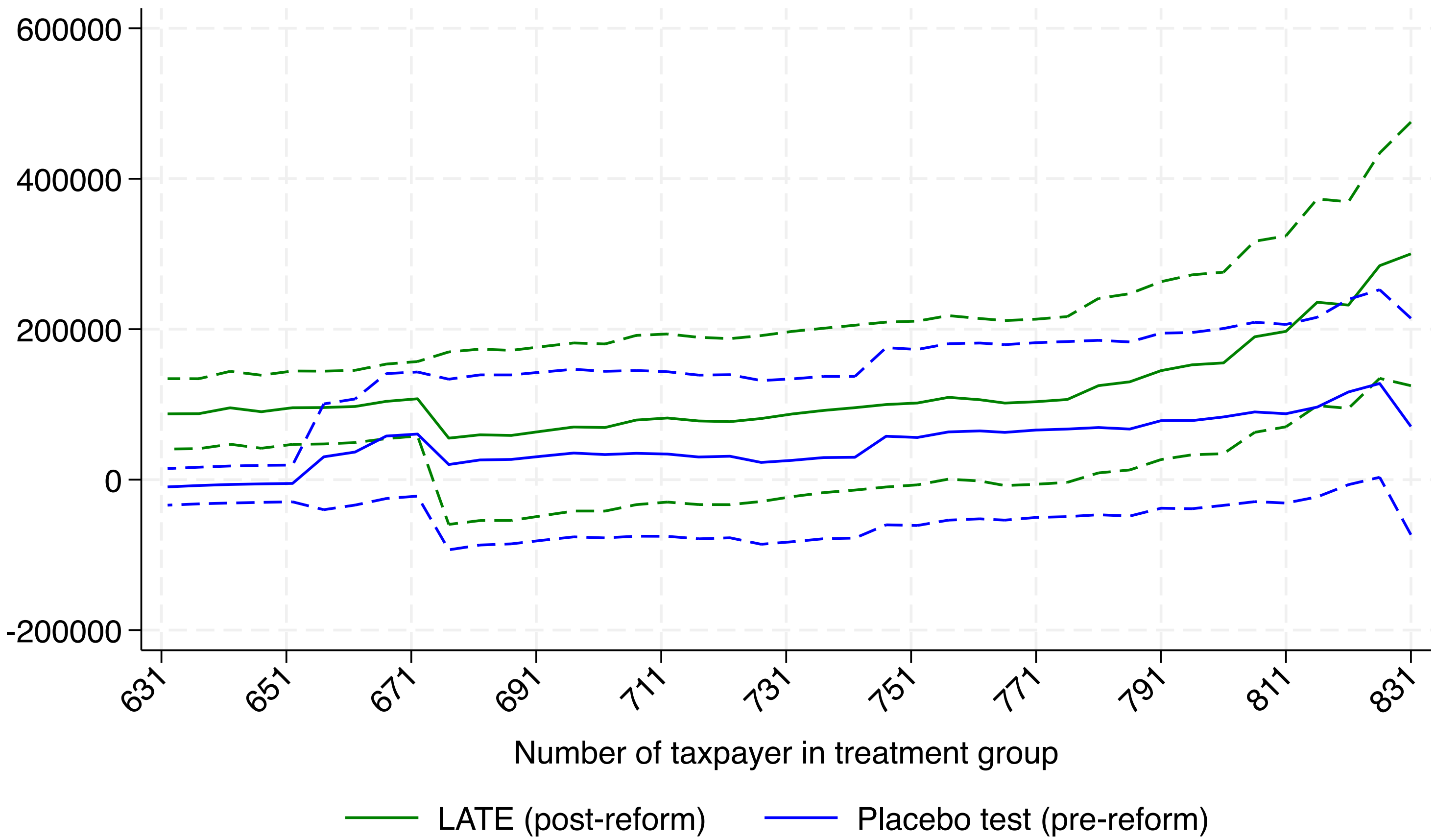
(d) Time-varying impact



Notes. The figure displays coefficients of lags, and leads (ITT or LATE) indicators of model 1 using the mandatory adoption of Etax for the Largest Taxpayers Center, set by ministerial decree in July 2017, as a source of exogenous variation. The reference period ($t = -1$) is August 2017. A transition period is highlighted in grey, during which adoption progressively took place. In the LATE specification, we instrument each leads of model 1 by an indicator $Etax_{it}^{k}$ equal to one if and only if $t = k$ and taxpayer i has effectively adopted etax by t. All estimations use time and taxpayer fixed-effects. Data is an unbalanced sample of 1,549 taxpayers. Total sample size is 44,958. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Figure 13 – Turnover (USD)

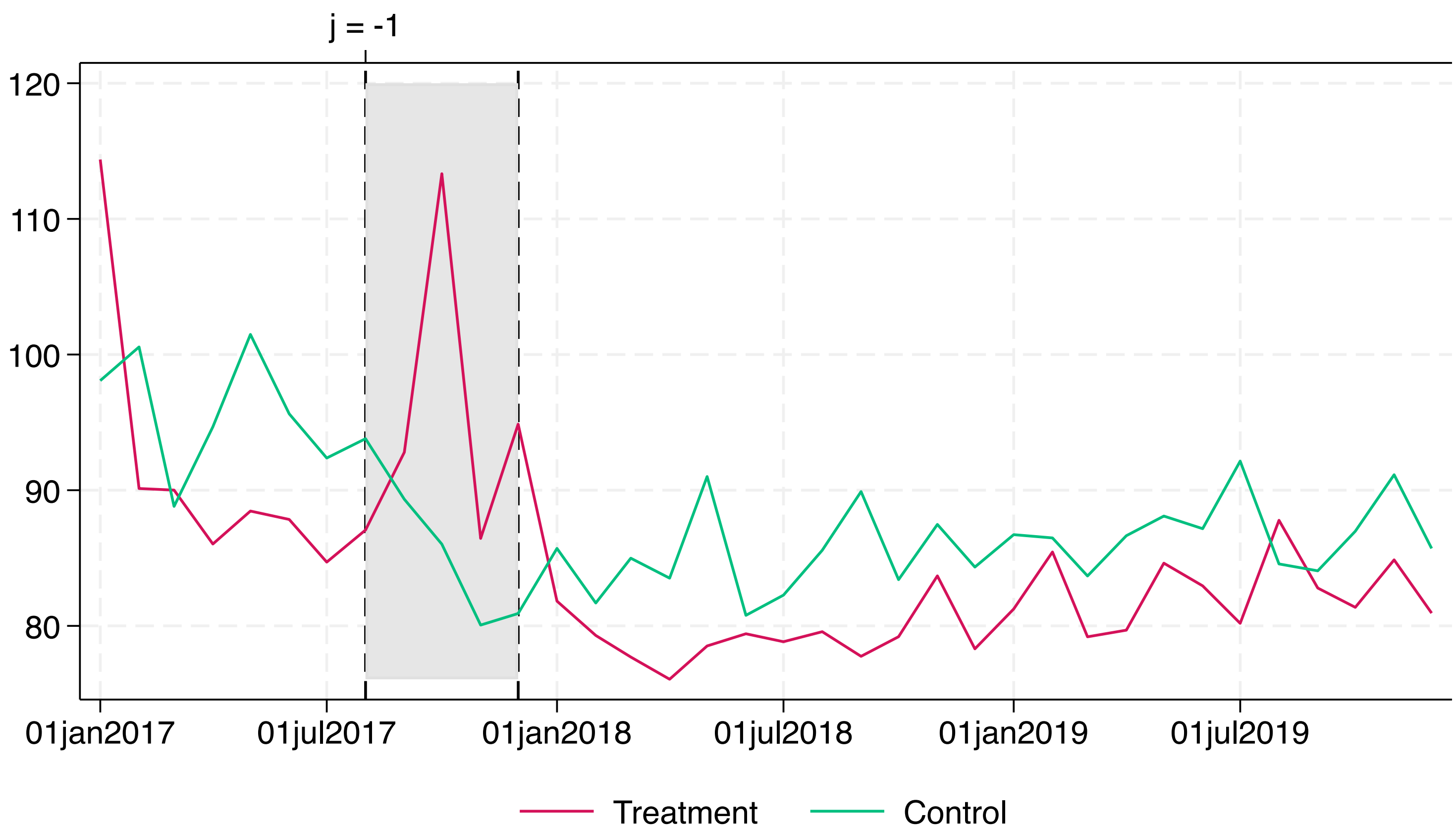
(e) Sensitivity to extremely large taxpayers



Notes. The Figure displays variations in treatment impact (post-reform) and placebo test (pre-reform) as we trim the treated group (taxpayers monitored be the LTC) by incrementally removing the 5 largest taxpayers. LATE coefficients (green line) are obtained by estimating model 2 for the post-reform period, using two stages least square method and instrumenting $(Treat_{it} \cdot Post_{it})$ by an indicator $Etax_{it}$ equal to one if and only if taxpayer i has started using Etax by t. Coefficients of placebo tests (blue line) are obtained by estimating model 2 for the pre-reform period replacing $(Treat_{it} \cdot Post_{it})$ by $(Treat_{it} \cdot Pre_{it})$ and using Ordinary Least Square method. The reference period is always August 2017. All samples are balanced, and all estimations use time and taxpayer fixed-effect. The axis indicates the number of taxpayers in the treatment group after trimming. The initial sample size of the treatment group is 831. The size of the control group is 860 and remains constant through all specifications.

Figure 14 – Payment as a percentage of total liability paid

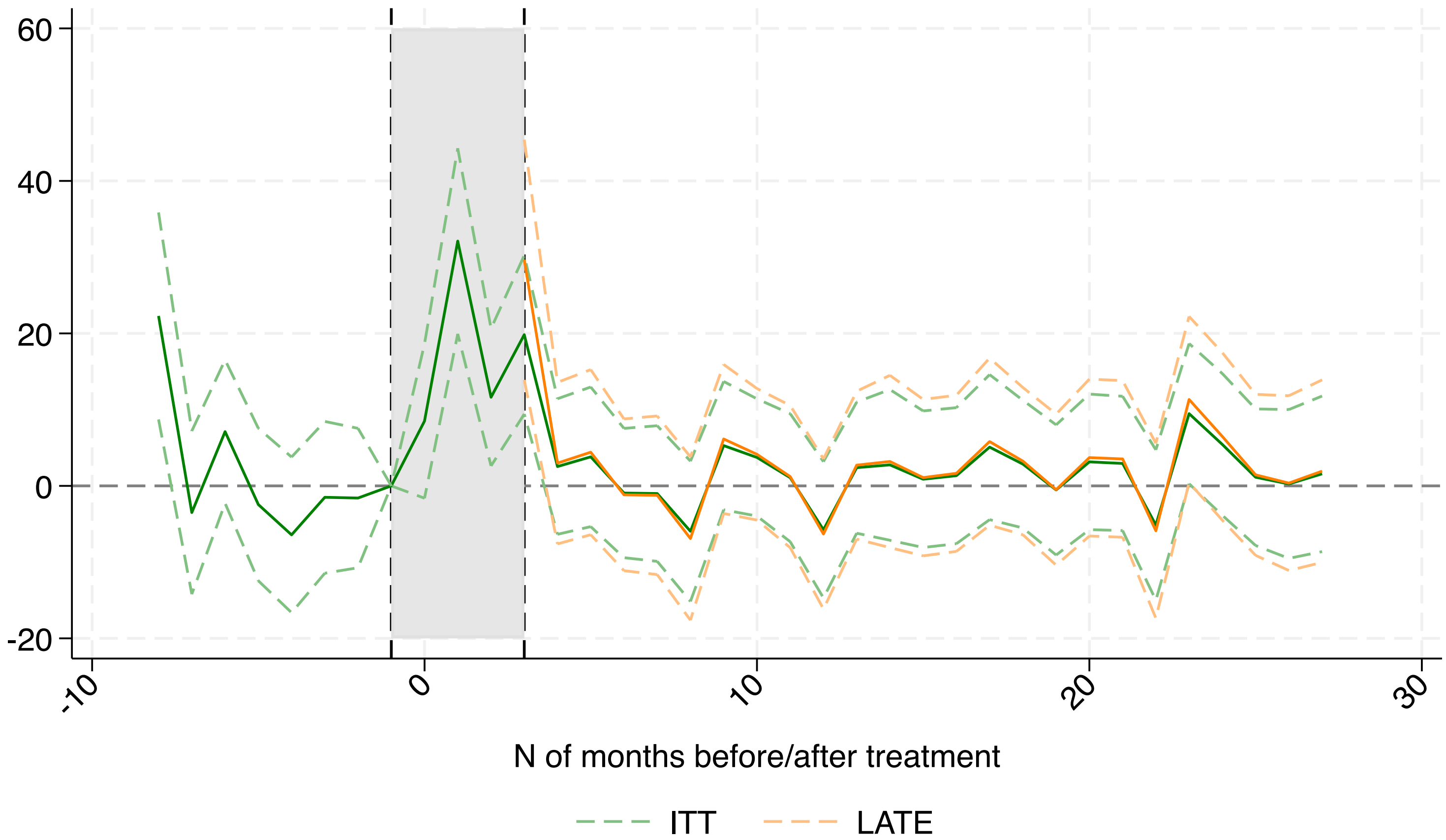
(a) Time trends



Notes. The figure displays average values of the outcome by group and across time. Treated taxpayers taxpayers monitored by the Large Taxpayers Center (LTC), where the adoption of Etax was made mandatory by a ministerial decree issued in July 2017. Controlled taxpayers are the largest 860 taxpayers monitored by other centers of Dakar. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876.

Figure 14 – Payment as a percentage of total liability paid

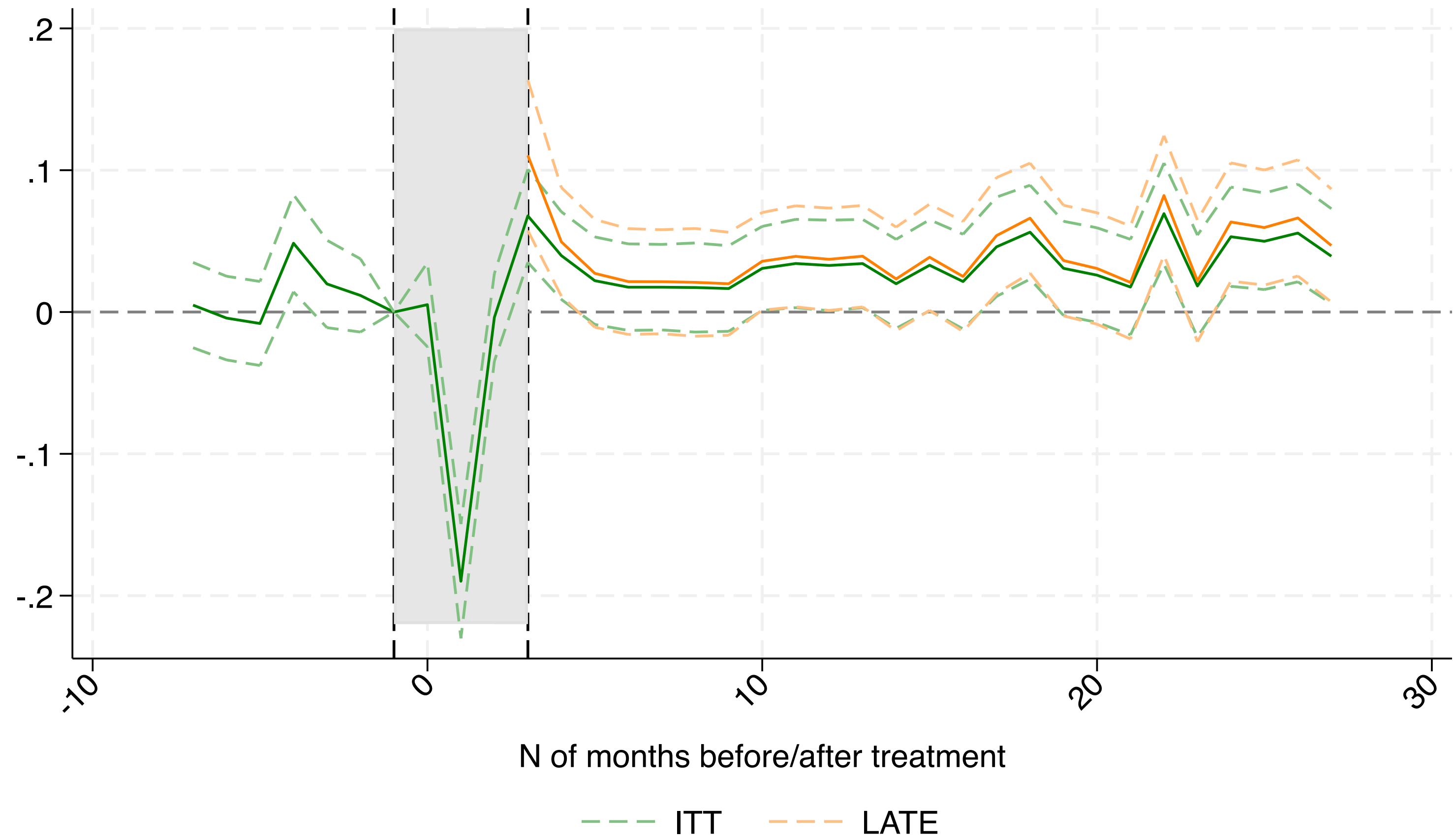
(b) Time-varying impact



Notes. The figure displays coefficients of lags, and leads (ITT or LATE) indicators of model 1 using the mandatory adoption of Etax for the Largest Taxpayers Center, set by ministerial decree in July 2017, as a source of exogenous variation. The reference period ($t = -1$) is August 2017. A transition period is highlighted in grey, during which adoption progressively took place. In the LATE specification, we instrument each leads of model 1 by an indicator $Etax_{it}^{k}$ equal to one if and only if $t = k$ and taxpayer i has effectively adopted etax by t. All estimations use time and taxpayer fixed-effects. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Figure 15 – On measurement and missing information

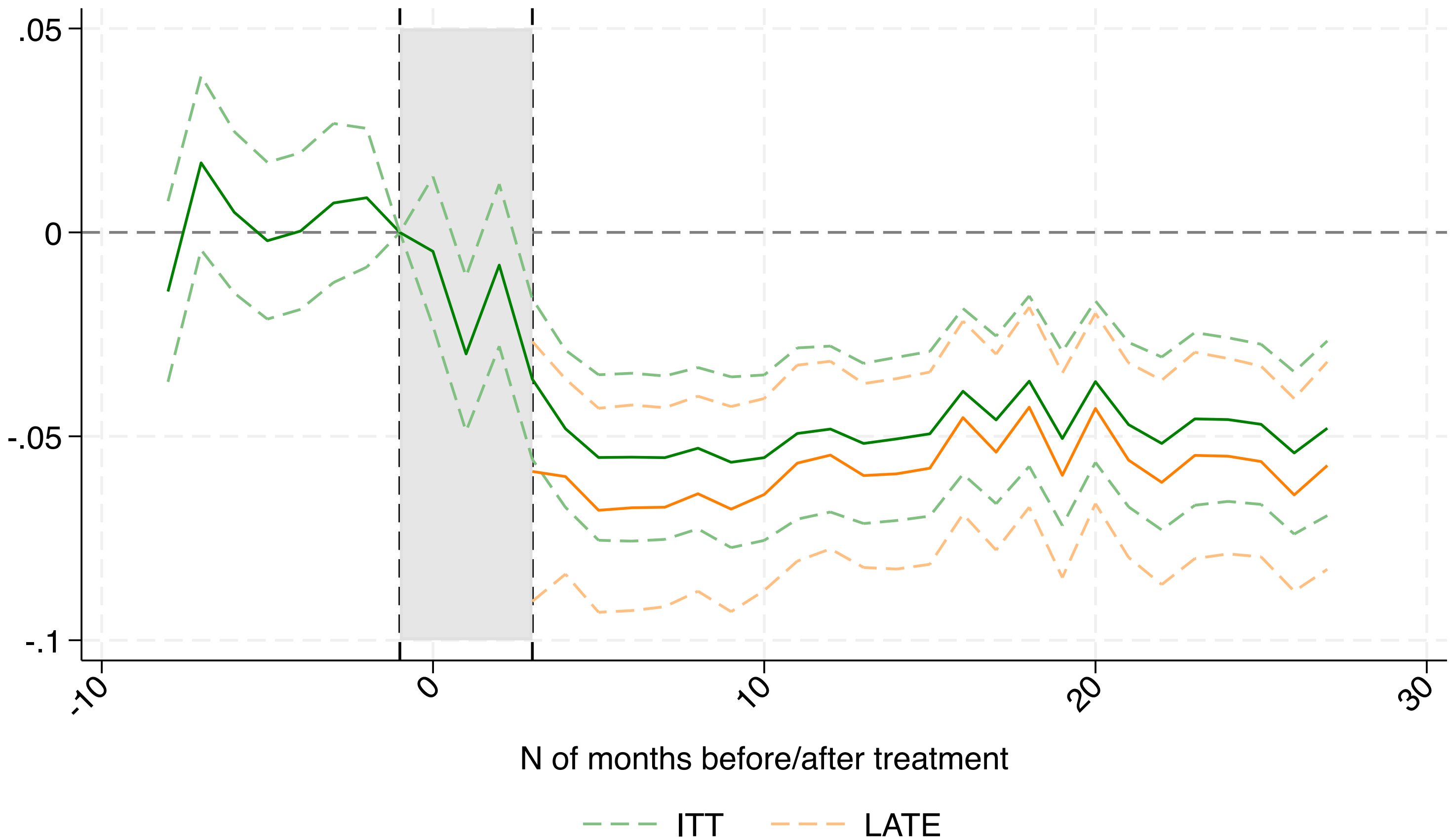
(a) Outcome : Declares withholding on employees



Notes. The figure displays coefficients of lags, and leads (ITT or LATE) indicators of model 1 using the mandatory adoption of Etax for the Largest Taxpayers Center, set by ministerial decree in July 2017, as a source of exogenous variation. The reference period ($t = -1$) is August 2017. A transition period is highlighted in grey, during which adoption progressively took place. In the LATE specification, we instrument each leads of model 1 by an indicator $Etax_{it}^{k}$ equal to one if and only if $t = k$ and taxpayer i has effectively adopted etax by t. All estimations use time and taxpayer fixed-effects. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Figure 15 – On measurement and missing information

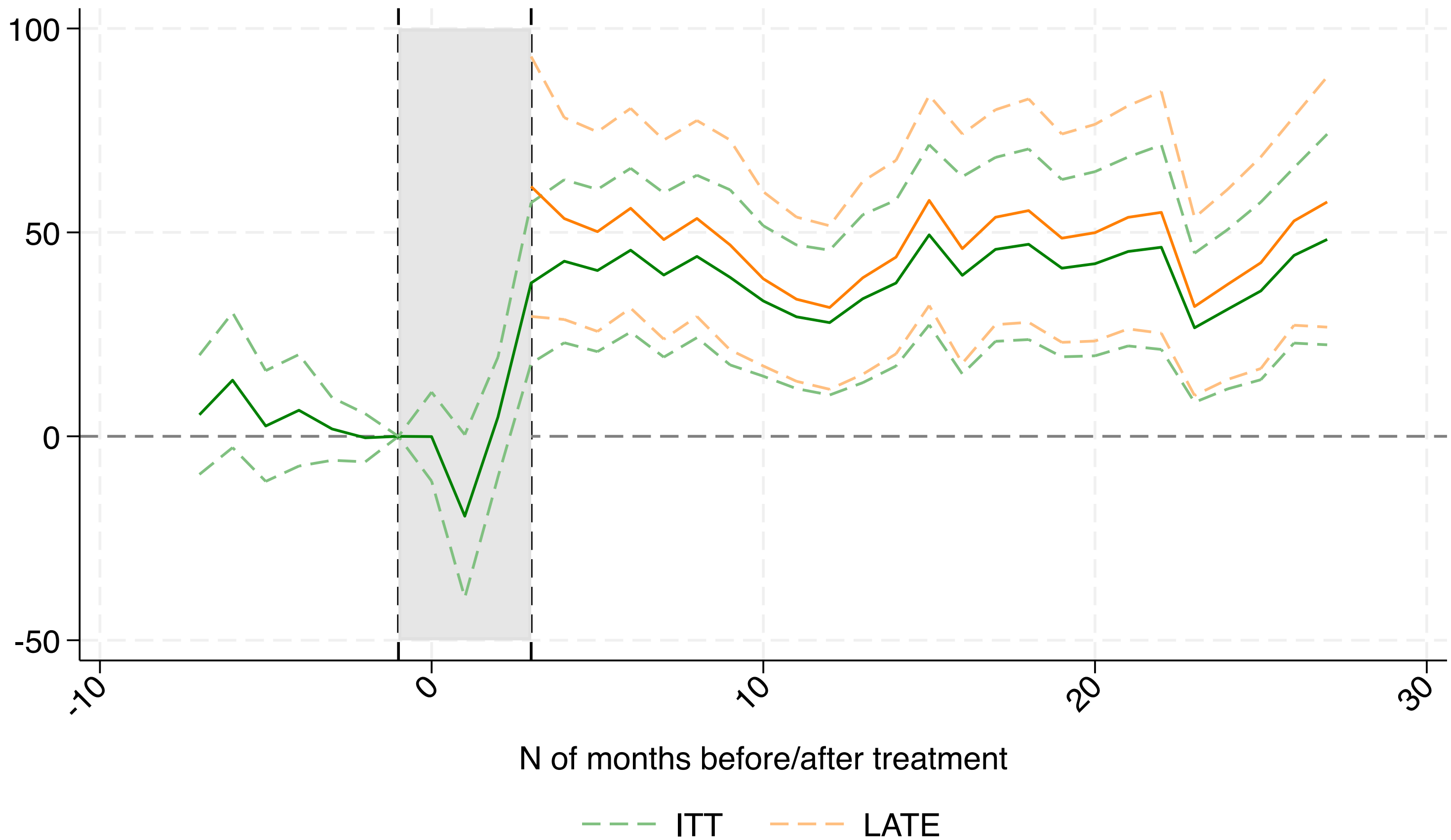
(b) Outcome : The information on the number of employees is missing



Notes. The figure displays coefficients of lags, and leads (ITT or LATE) indicators of model 1 using the mandatory adoption of Etax for the Largest Taxpayers Center, set by ministerial decree in July 2017, as a source of exogenous variation. The reference period ($t = -1$) is August 2017. A transition period is highlighted in grey, during which adoption progressively took place. In the LATE specification, we instrument each leads of model 1 by an indicator $Etax_{it}^{k}$ equal to one if and only if $t = k$ and taxpayer i has effectively adopted etax by t. All estimations use time and taxpayer fixed-effects. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Figure 15 – On measurement and missing information

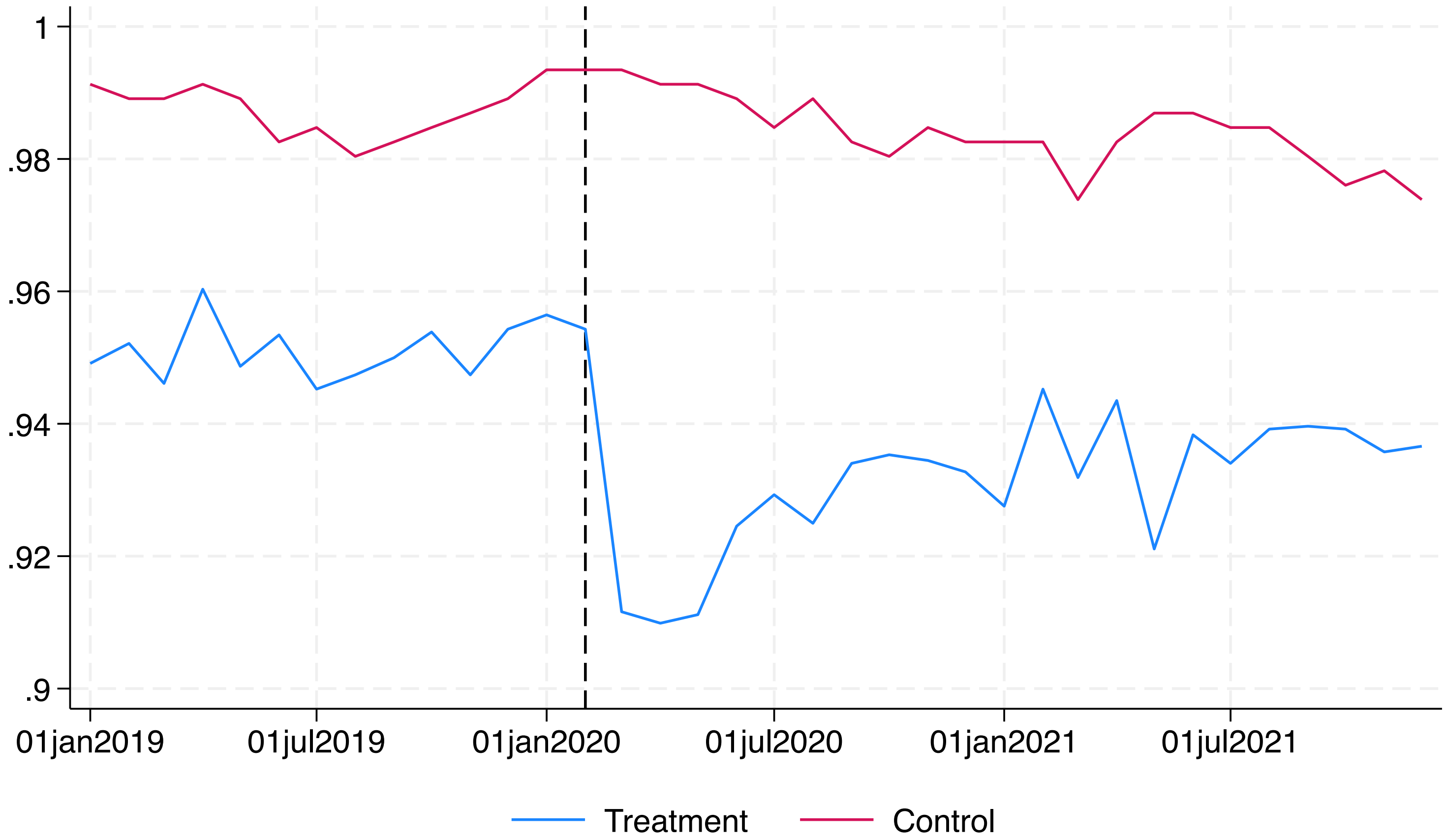
(c) Outcome : Number of employees



Notes. The figure displays coefficients of lags, and leads (ITT or LATE) indicators of model 1 using the mandatory adoption of Etax for the Largest Taxpayers Center, set by ministerial decree in July 2017, as a source of exogenous variation. The reference period ($t = -1$) is August 2017. A transition period is highlighted in grey, during which adoption progressively took place. In the LATE specification, we instrument each leads of model 1 by an indicator $Etax_{it}^{k}$ equal to one if and only if $t = k$ and taxpayer i has effectively adopted etax by t. All estimations use time and taxpayer fixed-effects. Data is a balanced sample of 1,691 taxpayers observed for exactly 36 months. Total sample size is 60,876. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Figure 16 – COVID shock on the Probability to submit at least one declaration

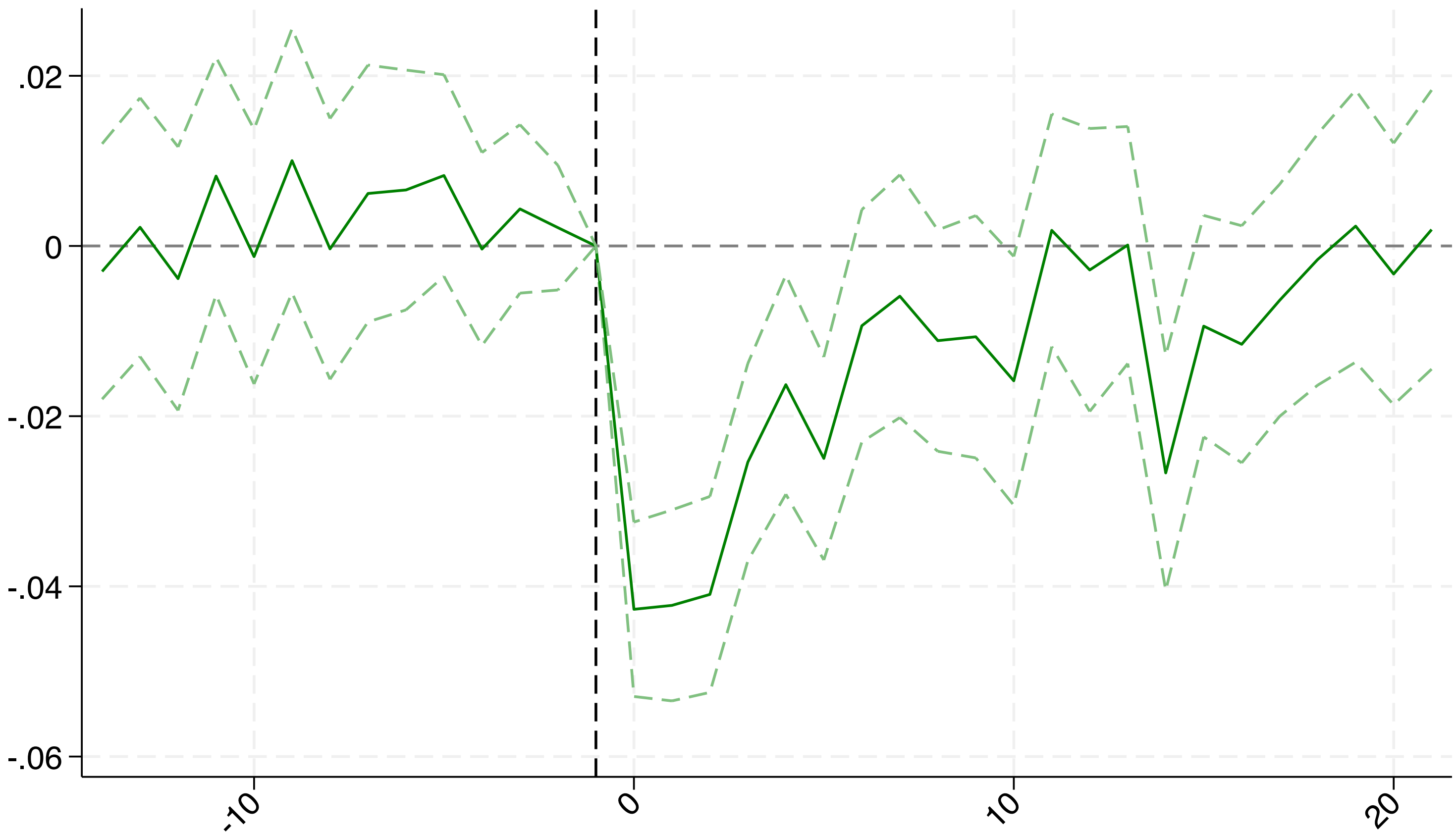
(a) Time trends



Notes. The figure displays average values of the outcome by group and across time. Treated taxpayers are those who had not adopted Etax by March 2020, the reference period. Data is a balanced sample of 2,778 taxpayers, of which 2,319 are in the treatment group, observed for exactly 36 months. Total sample size is 100,008.

Figure 16 – COVID shock on the probability to submit at least one declaration

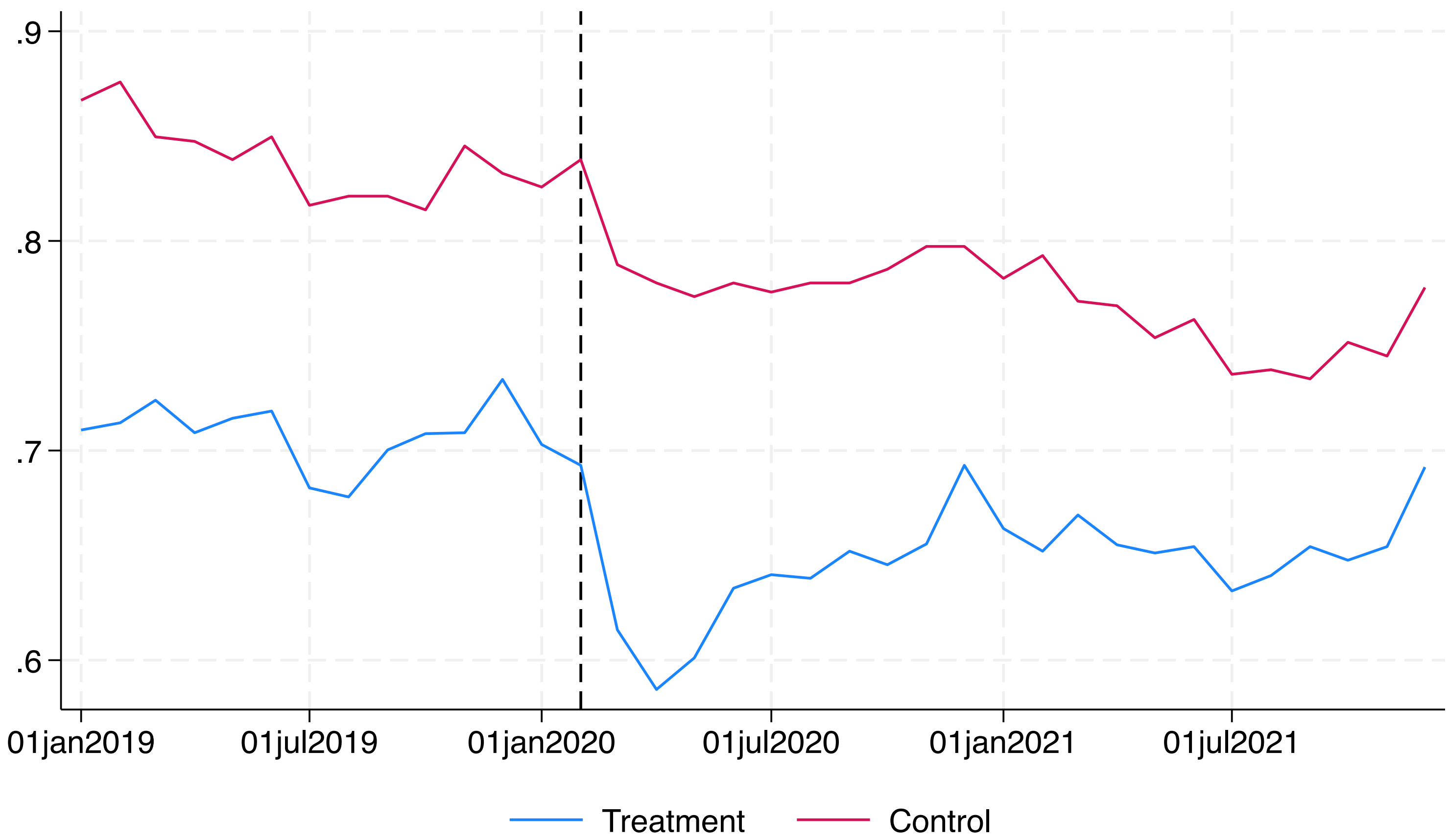
(b) Time-varying impact



Notes. The figure displays coefficients of lags and leads indicators of model 1 using the COVID shock as exogenous source of variation and defining treated taxpayers as those who had not adopted Etax by March 2020, the reference period ($t = -1$). Data is a balanced sample of 2,778 taxpayers, of which 2,319 are in the treatment group, observed for exactly 36 months. Total sample size is 100,008. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Figure 17 – COVID shock on the probability to pay a positive amount

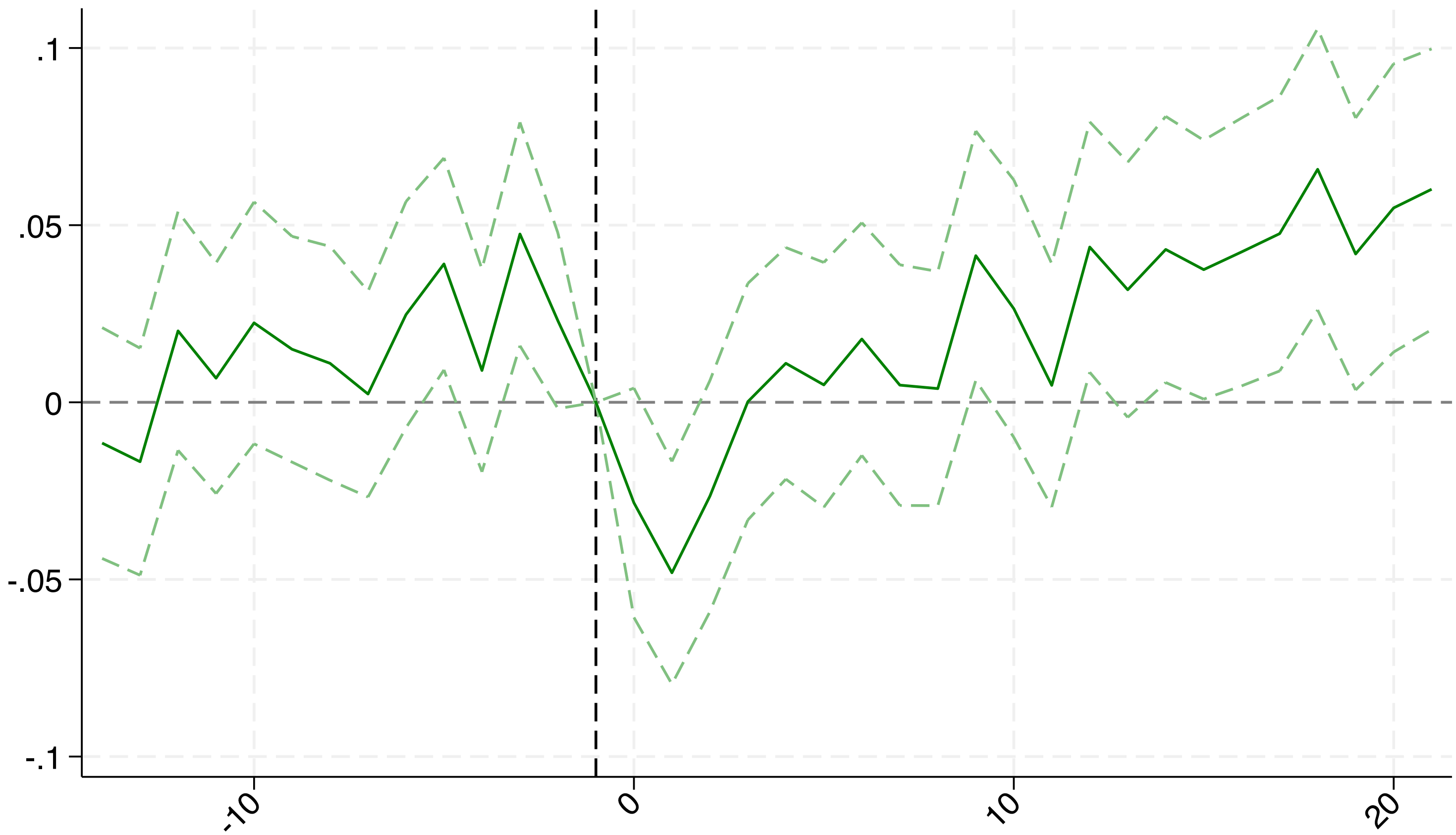
(a) Time trends



Notes. The figure displays average values of the outcome by group and across time. Treated taxpayers are those who had not adopted Etax by March 2020, the reference period. Data is a balanced sample of 2,778 taxpayers, of which 2,319 are in the treatment group, observed for exactly 36 months. Total sample size is 100,008.

Figure 17 – COVID shock on the probability to pay a positive amount

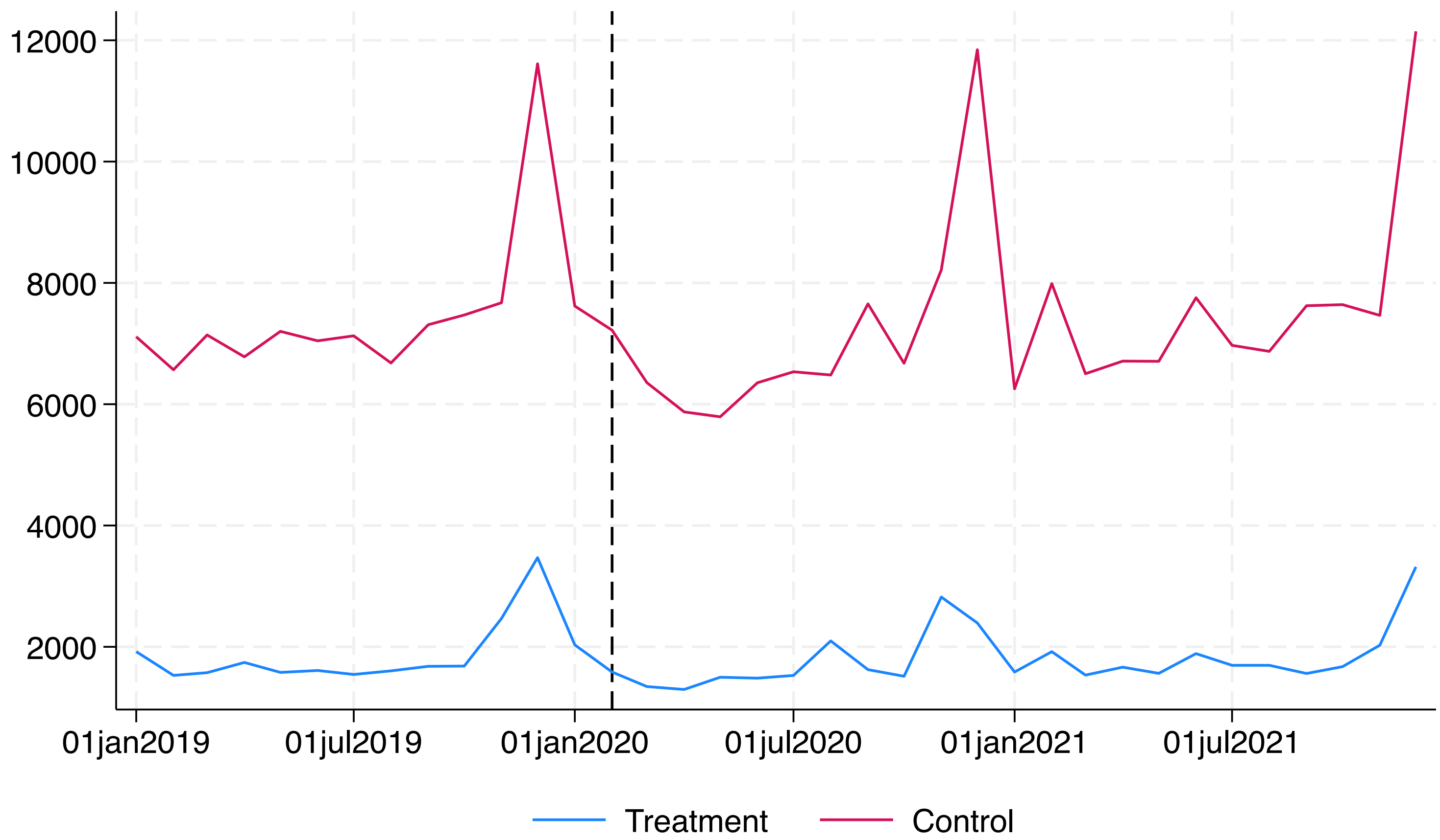
(b) Time-varying impact



Notes. The figure displays coefficients of lags and leads indicators of model 1 using the COVID shock as exogenous source of variation and defining treated taxpayers as those who had not adopted Etax by March 2020, the reference period ($t = -1$). Data is a balanced sample of 2,778 taxpayers, of which 2,319 are in the treatment group, observed for exactly 36 months. Total sample size is 100,008. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Figure 18 – COVID shock on the amount of tax paid

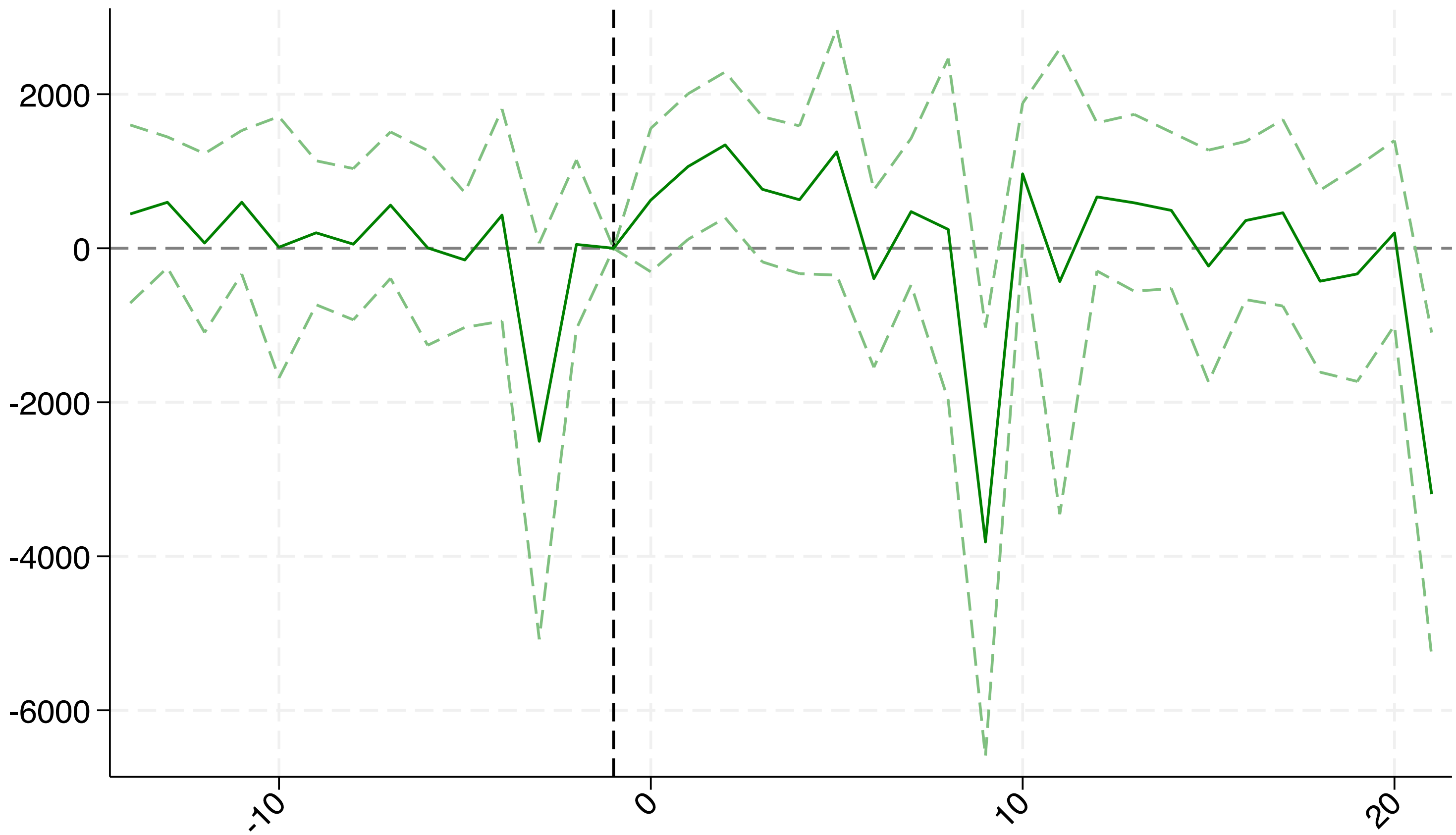
(a) Time trends



Notes. The figure displays average values of the outcome by group and across time. Treated taxpayers are those who had not adopted Etax by March 2020, the reference period. Data is a balanced sample of 2,778 taxpayers, of which 2,319 are in the treatment group, observed for exactly 36 months. Total sample size is 100,008.

Figure 18 – COVID shock on the amount of tax paid

(b) Time-varying impact



Notes. The figure displays coefficients of lags and leads indicators of model 1 using the COVID shock as exogenous source of variation and defining treated taxpayers as those who had not adopted Etax by March 2020, the reference period ($t = -1$). Data is a balanced sample of 2,778 taxpayers, of which 2,319 are in the treatment group, observed for exactly 36 months. Total sample size is 100,008. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Dashed lines indicate 95% confidence interval.

Table 1 – Main Results

Outcome	β	Std. err.	Specification
Submits at least one declaration	0.011	0.009	ITT
	0.015	0.012	LATE
Some declaration has missing information	-0.052***	0.010	ITT
	-0.069***	0.014	LATE
Some declaration has inconsistent information	-0.020	0.012	ITT
	-0.026	0.016	LATE
Some declaration has implausible information	0.006	0.005	ITT
	0.008	0.007	LATE
Liability declared is positive	0.021*	0.013	ITT
	0.028*	0.017	LATE
Liability declared (USD)	2,812	4,874	ITT
	3,726	6,459	LATE
Tax paid is positive	-0.011	0.012	ITT
	-0.015	0.016	LATE
Amount paid (USD)	16,851**	7,588	ITT
	22,327**	10,045	LATE

Notes. The table reports main coefficients obtained by estimating model 2 with Ordinary Least Squares (ITT) or two stages least square method (LATE) when instrumenting $Treat_{it} \cdot Post_{it}$ by a indicator $Etax_{it}$ equal to one if and only if taxpayer i has started using Etax by t (eiter through e-filling or e-payment). All estimations uses time and taxpayer fixed-effect. Data is a balanced sample of 1,691 taxpayers observed for exactly 29 months, the first of which is the reference period of the model: August 2017. Months of the transition period are included in the post-period. Total sample size is 49,039. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Significance level are indicated by * (10%), ** (5%) and *** (1%).

Table 2 – Main Results - excluding the transition period

Outcome	β	Std. err.	Specification
Submits at least one declaration	0.015	0.009	ITT
	0.017	0.011	LATE
Some declaration has missing information	-0.057***	0.011	ITT
	-0.069***	0.013	LATE
Some declaration has inconsistent information	-0.018	0.013	ITT
	-0.021	0.015	LATE
Some declaration has implausible information	0.005	0.006	ITT
	0.006	0.007	LATE
Liability declared is positive	0.028**	0.014	ITT
	0.032**	0.016	LATE
Liability declared (USD)	3,735	5,283	ITT
	4,430	6,267	LATE
Tax paid is positive	-0.012	0.013	ITT
	-0.014	0.016	LATE
Amount paid (USD)	17,760**	7,819	ITT
	21,064**	9,271	LATE

Notes. The table is the same as table 1, but for the fact that we drop the four months of the transition period (September, October, November and December 2017). The table reports main coefficients obtained by estimating model 2 with Ordinary Least Squares (ITT) or two stages least square method (LATE) when instrumenting $Treat_{it} \cdot Post_{it}$ by a indicator $Etax_{it}$ equal to one if and only if taxpayer i has started using Etax by t (eiter through e-filling or e-payment). All estimations uses time and taxpayer fixed-effect. Data is a balanced sample of 1,691 taxpayers observed for exactly 29 months, the first of which is the reference period of the model: August 2017. Months of the transition period are included in the post-period. Total sample size is 49,039. Standard errors are adjusted using Huber-White ("robust") Sandwich estimator. Significance level are indicated by * (10%), ** (5%) and *** (1%).