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Wait no more: how the administration of VAT refunds impacts firm behaviour

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Abstract: Refunds are an essential feature of well-functioning VAT systems and take up a sizeable portion of government spending. In South Africa, refunds amount to 50 per cent of gross VAT collection, representing a substantial transfer from the government to taxpayers that has to occur at relatively high frequency, often monthly. We show that delays in these refund payments reduce domestic investment, especially by small firms. We use administrative data to provide extensive evidence that firms respond to incentives created by delays and denials of refunds. We exploit a change in the attitude towards refunds of the South African Revenue Authority to quantify these effects via an event study. We find that approximately halving the audit rates of refund-claiming returns and speeding up their processing increased investment by 31 per cent and output by 24 per cent.

Key words: VAT collection, VAT refunds, event study

JEL classification: H21, H25, H83

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1 Introduction

Refunds are an essential feature of any well-functioning value-added tax (VAT) system. Without refunds, VAT would not be able to function as a destination-based tax, and many of its efficiency-improving features—such as the deductibility of capital purchases—would be partially lost. The disbursement of VAT refunds occupies a major portion of government budgets worldwide, amounting to an average of 2.5 percent of GDP each year.[1] This is comparable to other major government payments—procurement, the principal way through which governments purchase goods and services, for example, amounts to about 8 percent of GDP worldwide (Schapper et al. 2006). The importance of VAT refunds is underscored by the wide adoption of VAT worldwide, including all major economies except the United States, and its fundamental contribution to government revenues, especially in developing countries (Prichard 2016).

Refund administration has long been described as the 'Achilles' heel' of VAT because it needs to balance the loss of production efficiency from delays and denials with the loss of revenue efficiency through evasion (Harrison and Krelove 2005). Indeed, refund requests often raise flags in the audit process because of the potential for evasion through over-claimed credits, which delays the process of paying out refunds even when they are legitimate (Brockmeyer et al. 2024). Despite their prevalence in government budgets and their importance to a well-functioning VAT system, we still lack key empirical evidence to inform this trade-off. Indeed, there is hardly any research using firm-level micro-data that could speak to the empirical relevance of these competing considerations. In this paper we exploit a change in the strictness of refund administration and use the universe of VAT returns in South Africa to show the impact that refund administration can have on firm behaviour, including that of non-exporters.

Refunds in a VAT system arise whenever a taxpayer has negative taxable value added, which occurs in three main ways. First, through the sale of exported goods and services: as VAT systems are destination-based, export sales are zero-rated, meaning they face no liability on their output and are due tax credits on their inputs. Second, through sales of domestically zero-rated goods. In the case of South Africa this applies to many fuel goods, which are subject to their own excise tax system. Third, through large capital or inventory expansions relative to firm size, which result in negative taxable value added in the short term. Though much attention has been paid to the impact of refund administration on exporters, non-exporters represent a large share of refund-claimers. The impact on the third category of refund claims is particularly insidious as it characterizes firms with high growth potential. We focus our analysis on this group of firms and provide the first evidence on the impact of VAT refund delays on non-exporters.

There are several reasons to believe that refund delays can adversely impact firm activity. An aggressive administration of refunds, resulting in delays and denials of potentially legitimate refunds, ties up a firm's cash flow and working capital, which reduces investment, regardless of access to external finance (Bilicka 2020). Evidence from government procurement shows that even payment delays of a few days can reduce firm growth (Barrot and Nanda 2020). At the extreme, refund denials amount to denial of input tax credits above some amount, which undermines the production efficiency of VAT and hurts investment (Chen et al. 2023). Yet, many countries require VAT taxpayers to carry forward excess credits for a few months or even indefinitely, delaying and denying refunds as a matter of policy (Pessoa et al. 2021). What are the consequences of these policy- or practice-driven delays of VAT refunds for firm growth, and how do firms adapt?

[1] Authors' calculations based on data from the World Development Indicators and IMF ISORA. This applies to 131 countries in 2020 for which data on VAT refunds and GDP is available.

On the other hand, the input tax credit mechanism and VAT refunds have been exploited for tax evasion. There are many pieces of anecdotal evidence for this, in countries across the development path,[2] as well as more systematic empirical research (Keen 2007; Waseem 2023). Does expediting refund payments by reducing oversight result in more evasion of this kind?

We address both of these questions in the South African context. South Africa is a particularly interesting case study for VAT refunds for three reasons. First, there is no mandatory carryforward of tax credits, meaning refunds need to be claimed and paid out immediately. Second, the combination of the lack of carryforward, high exports, and domestic zero-rating results in a large number of refund claims, amounting to 46 percent of gross VAT revenue (National Treasury, Republic of South Africa 2018). Third, and most importantly, there was a dramatic change in refund administration following a change in the leadership of the South African Revenue Service (SARS) in October 2018, which we can exploit for identification. SARS committed to paying out more expediently the existing stock of refund credits and refund claims going forward. As we detail in Section 2, we confirm in the data that SARS followed through on this commitment. Refunds were processed about 60 percent faster, with average waiting times falling from 30 days to 10 days even for very small refund claims.

This reduced delay seems to have been achieved by a decrease in the likelihood of being selected for additional assessment, and conditional on having been selected, a reduction in the likelihood of modification. Refund-claiming returns were still more likely to be audited than liability-declaring returns, but less so than before. This change was particularly acute for taxpayers that only seldom claimed refunds. These taxpayers are more likely to trigger flags in the return process than taxpayers that habitually claim refunds. Under the new policy regime, audit rates dropped by only a few percentage points for habitual claimers, but by some 10–15 percentage points for occasional refund claimers. In general, audit rates for refund-claiming audits were approximately halved across the board.

We examine how these changes in refund policy affected firm behaviour, particularly that of non-exporters. In Section 3 we develop a simple static model that shows that firms should be particularly affected when they make investments in excess of the value added from non-capital deductible inputs. Simply put, delays and additional scrutiny on refunds make refund-triggering capital purchases discontinuously more expensive at the margin.

Consistently with our theoretical predictions, we show that firms declared their capital purchases to bunch at zero tax liability, and that this is particularly true for firms that would not be expected to claim refunds, such as exporters. After October 2018, we find this bunching to be lessened, indicating a behavioural response to the change in policy.

Through an event-study approach which we discuss in Section 4, we show in Section 5 that this resulted in big changes to firm behaviour, particularly for new firms. Firms investing for the first time, and 'learning' about the policy regime towards refunds, became more likely to claim refunds and purchase more capital goods, while at the same time increasing their output and VAT payments. We believe these effects to be real rather than driven by pure reporting, as they relate to a six-month window following the first investment. These effects were also relatively large, as capital purchases and output increased by more than 20 percent. The short-term effect on net VAT collections was more ambiguous, as one would expect. On one hand, increases in output would drive up VAT payments, while on the other, making it easier to claim refunds drives VAT collections down. We find that refund claims increased by 7.9 percentage points, over a baseline of about 40 percent. This notwithstanding, net VAT payments increased on average by about 25 percent.

[2] See, for instance, some cases reported specifically in South Africa (Roelf and Toyana 2019) and even suspected by SARS itself (SARS 2023), as well as in the EU (Euronews 2022) and Australia (Chenoweth 2023).

Exports are one of the most salient refund-generating activities. Exports are zero-rated in any destination-based VAT, and so a firm with mainly export sales will always be due a net refund. Evidence from Chinese firms shows that VAT refund denials severely hurt exports (Chandra and Long 2013; Lu and Ma 2023). Sharma (2020) shows that the introduction of VAT in a country adversely affects their exports, and that this is likely due to their refund administration. But many other activities also generate refunds. Another such crucial activity is large investments that exceed all other taxable value added. In other words, firms that experience positive productivity shocks that are large enough to warrant a large capital investment relative to their value added. An implicit tax on refunds therefore hurts exactly the type of investment that a planner would want to encourage. This is the first paper to examine how refund denials and delays affect investment by non-exporters.

When capital purchases are not allowed to generate input tax credits in a VAT system, firm investment is reduced substantially—Chen et al. (2023) find that reversing such a policy in China increased investment by over 30 percent. We show in this paper that refund delays and denials have a similar impact on investment to outright denial of VAT input tax credits. Many countries treat refund requests with suspicion. The Uganda Revenue Authority, for example, explicitly states that first-time refund requesters will be automatically subjected to audit (Uganda Revenue Authority n.d.).

Our work contributes to recent literature on how diversions from an 'ideal' VAT system (Slemrod and Velayudhan 2022) influence the efficiency and equity of VATs around the world. This literature has explored the effects of these diversions on international trade (Benzarti and Tazhitdinova 2021; Keen 2008; Schneider et al. 2022), how information systems affect reporting and compliance (Carrillo et al. 2017; Morrow et al. 2022; Pomeranz 2015), and how registration thresholds influence VAT progressivity and incidence by interacting with firm behaviour and consumption patterns (Bachas et al. 2020; Brusco and Velayudhan 2024; Harju et al. 2019; Jenkins et al. 2006), among other aspects. Work on the effects of the administration of VAT refunds, however, is still very much in its infancy. Besides Waseem's (2023) work documenting that a significant portion of VAT evasion is happening through over-claimed refunds, indeed, we hardly have any research on how different degrees of scrutiny over refunds influence firm behaviour. This paper aims to fill this gap.

The rest of the paper proceeds as follows. Section 2 describes the data and the policy change we exploit for identification. Section 3 discusses our theoretical framework. Section 4 discusses our methodology to identify behavioural responses to the change in the VAT administration of refunds and to measure how this heterogeneously impacted different firms, such as credit-constrained vs. unconstrained firms. Section 5 contains our main empirical results. We conclude in Section 6.

2 Background and data

2.1 Data

We observe the universe of filed VAT[3] and corporate income tax (CIT) returns[4] in South Africa, between 2014 and 2021.[5] These data contain everything that firms report in their filed returns, as well as information firms declare the first time they register for the South African VAT or CIT system. We analyse VAT taxpayers and link information from the CIT returns for corporations. About 70 percent of VAT taxpayers are also corporations, giving us a rich set of balance sheet information. Nevertheless, the data

[3] VAT periodic returns data Version e5_v1 (National Treasury and UNU-WIDER 2023).

[4] Corporate tax return data Version CIT_IRP_v5 (National Treasury and UNU-WIDER 2021).

[5] See Appendix B for more information on data.

span a wide distribution of firm ages and sizes. The average firm has monthly sales of about 1.7 million rand (approx. US$90,000) and is about two years old. Seventeen percent of returns make a refund claim and 4 percent of all returns have been audited and modified from the original submission. About 20 percent of capital purchases are large enough to result in a negative taxable value added.

We also have data on the universe of VAT audits that were concluded after 2020. These data can be linked to the original audited return, and contain information on the original liability declared in a return, the adjustment that SARS believed necessary, as well as processing times for the audit.

We exclude some taxpayers from our analysis who are registered but seem to have had periods with no economic activity. We analyse taxpayers with and without zero-rated sales separately. Taxpayers without any zero-rated sales are much less likely to be requesting refunds without making a capital purchase. Unlike those with mainly exports and other zero-rated sales, they can choose to avoid refunds altogether by managing their sales and input purchases to target a net zero or greater tax liability. We examine non-zero-rated sellers' behaviour along this margin of investment. For taxpayers with mainly zero-rated sales, we expect their investments to increase, but we do not expect to see a change in the likelihood that they trigger a refund.

We use these data to create key outcome and explanatory variables:

Wait times We would ideally like to observe the time between when firms file their refund claim (i.e. their tax return) and when they receive their refund. We are unable to observe the date of refund payments directly, but we do observe when a return is considered to be 'processed' by SARS. SARS incurs a penalty for any payment made later than 21 days after processing. Reports show that SARS rarely pays interest and penalties, so we use the time between date processed and date of submission as a proxy for the delay.

Audits and assessments Refunds are delayed because they undergo additional review. We have two measures of whether a return was reviewed. The first measure comes from audit data. We observe all audits that were closed after January 2020, and we link these to the returns data, which tells us whether a particular return was audited *and* closed after January 2020. While this measure has the drawback that we cannot tell whether a return was audited and closed before January 2020, it allows us to observe any discontinuous changes in audit trends around the policy change period of October 2018.

The second measure comes from the return data itself. For any return, we observe any modified returns filed following assessment by SARS. This information allows us to measure whether a return was audited *and* modified. We use this measure to examine differential trends in assessments and modification around October 2018.

Taxable liability excluding capital purchase As Section 3 explains, investment becomes more costly when it results in a refund claim, which occurs when it results in negative taxable value added. For each firm, we calculate their taxable liability excluding capital tax credits from return periods when they made no capital purchase. This represents their taxable value added arising from the difference between sales and intermediate inputs during their normal course of business. For firms that make zero-rated sales, we include their zero-rated output in the taxable value added before capital purchase.

Capital purchases Our main measure of investment is tax credits from capital purchases of both domestically sourced and imported capital reported in VAT returns. SARS requires firms to report tax credits from non-capital intermediate inputs separately from capital purchases. Because South Africa has only a single VAT rate, it is straightforward to calculate the original value of the purchased capital. Nevertheless, our main outcomes use the value of the tax credits themselves to avoid imputation. Our main outcome is whether capital tax credits are sufficient to result in a net refund for the taxpayer.

Table 1 presents a summary of our data, while Table 2 shows our event-study outcomes.

Table 1: Summary statistics

	Mean	Std. dev.	Min.	Max.	Obs.
Panel A: Return level					
Purchased capital (refund return)	0.19	0.39	0.00	1.00	10,500,716
Labour costs over revenue	1.69	1,002.65	0.00	1,022,618.00	6,361,150
Had export sales	0.03	0.16	0.00	1.00	10,500,716
Had zero-rated sales	0.13	0.33	0.00	1.00	10,500,716
Refund requested	0.17	0.38	0.00	1.00	10,500,716
Return process time (days)	0.70	13.10	–29.00	13,783.00	10,500,716
Refund return process time (days)	0.57	17.18	–1.00	9,968.00	1,806,607
Payment return process time (days)	0.73	12.08	–29.00	13,783.00	8,694,109
Return audited and changed	0.04	0.20	0.00	1.00	10,500,716
Amount of refund requested (rand)	8,479.01	63,828.19	0.00	120,586,480.00	10,500,716
Capital purchase (rand)	10,366.67	86,549.26	–0.06	40,381,384.00	10,500,716
Total output (rand)	1,670,044.91	19976566.78	–1.00	3.26e+10	10,500,716
Months firm appears in data	26.48	16.83	0.00	58.00	10,500,716
First capital purchase	0.02	0.14	0.00	1.00	10,500,716
Panel B: Taxpayer level					
Had export sales	0.03	0.18	0.00	1.00	394,267
Had zero-rated sales	0.17	0.38	0.00	1.00	394,267
CIT registered	0.68	0.47	0.00	1.00	394,267
Panel C: Return level, no zero-rated sellers					
Purchased capital (refund return)	0.14	0.35	0.00	1.00	8,091,893
Refund requested	0.12	0.33	0.00	1.00	8,091,893
Return process time (days)	0.63	13.99	–29.00	13,783.00	8,091,893
Refund return process time (days)	0.73	20.65	–1.00	9,968.00	999,717
Payment return process time (days)	0.62	12.77	–29.00	13,783.00	7,092,176
Return audited and changed	0.04	0.19	0.00	1.00	8,091,893
Amount of refund requested (rand)	4,648.25	62,818.47	0.00	120,586,480.00	8,091,893
Capital purchase (rand)	9,279.42	78,334.13	–0.06	40,381,384.00	8,091,893
Total output (rand)	1,132,089.33	10,643,823.91	–1.00	1.48e+10	8,091,893
Months firm appears in data	26.24	16.83	0.00	58.00	8,091,893
Share of returns with capital purchase	0.13	0.25	0.00	1.00	8,091,893
First capital purchase	0.02	0.14	0.00	1.00	8,091,893

Note: the characteristics of our data.

Source: authors' compilation.

Table 2: Summary statistics: event-study outcomes

	Mean	Std. dev.	Min.	Max.	Obs.
Log(capital purchases)	10.23	2.06	–9.77	18.00	43,987
Log(VAT liability)	11.08	1.71	–3.91	15.57	30,192
Log(output)	14.12	1.60	0.00	20.27	42,759
Investment to sales ratio	1.39	129.54	–0.00	24,640.36	42,759
Investment triggers refund within 6 mo. of first investment	0.49	0.50	0.00	1.00	43,990
Any refund	0.40	0.49	0.00	1.00	43,990
Any large capital purchase	0.70	0.46	0.00	1.00	42,707
Age (months)	10.10	12.95	0.00	69.00	43,990

Note: the sample is restricted to those included in 'actual' and 'placebo' event studies. Includes only taxpayer-months in which a firm is observed to purchase capital for the first time, among newly registered VAT firms.

Source: authors' compilation.

2.2 Refund policy and change in October 2018

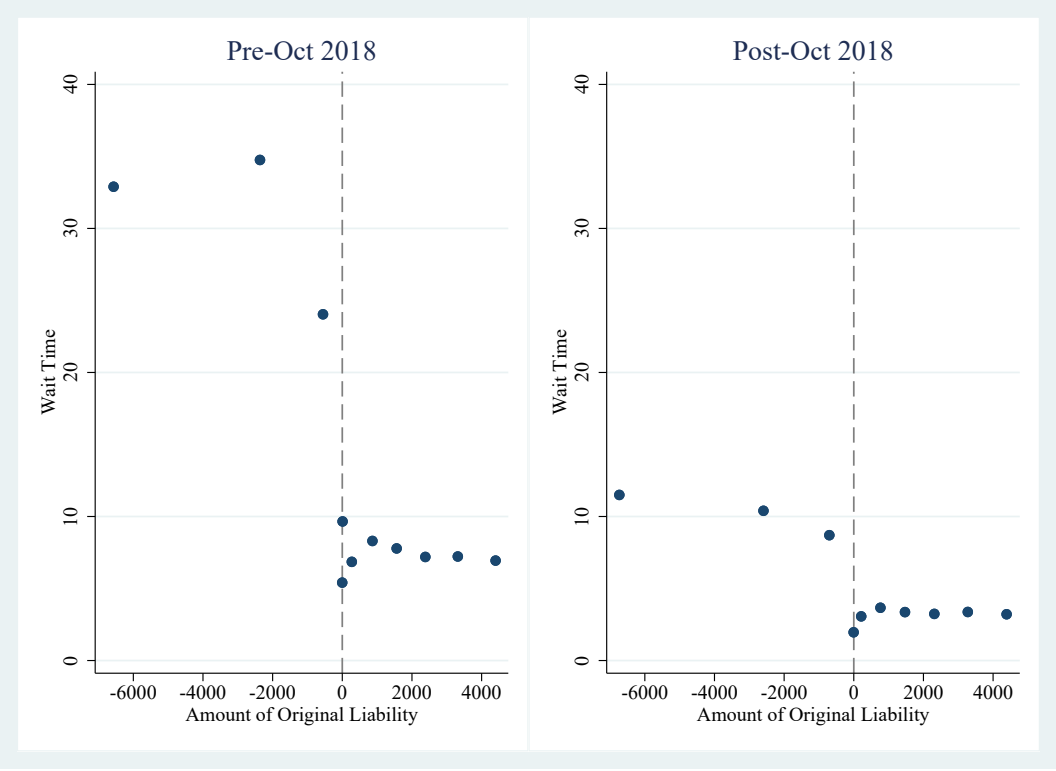
Refund payments in South Africa have amounted to an average of 46 percent of gross VAT revenue since 2011 (SARS n.d.). About 17 percent of returns claim a net refund. Simultaneously, refund delays are one of the biggest complaints of taxpayers in South Africa. The 2018–19 report of the Tax Ombud

listed refunds as the second-highest source of complaints, representing 24.43 percent of all complaints received by the office (Tax Ombud 2019). The administration of VAT refunds is therefore a salient issue for many firms.

A sudden change in the leadership at the tax authority also resulted in a major change in the administration of these refunds (Gebrekidan and Onishi 2018). Starting in October 2018, SARS instituted a new policy to pay out pending refund credits at a much faster pace—both by paying out a backlog of approved refunds and expediting the processing of new refund claims (National Treasury, Republic of South Africa 2018, 2019). While the impetus for change in VAT refunds signalled a change in tax administration more generally, our empirical strategy ensures that we are identifying changes in firm behaviour caused by the refund policy specifically.

Both before and after the policy change, we see that requesting a refund as opposed to paying a small amount results in a delay in return processing (Figure 1). In both panels of Figure 1 we see a discontinuous change in the average processing time of a return around zero reported liability. Before October 2018, the average waiting time for a refund request was at least 20 days, even for small refund requests. This waiting time fell to about 10 days after October 2018.

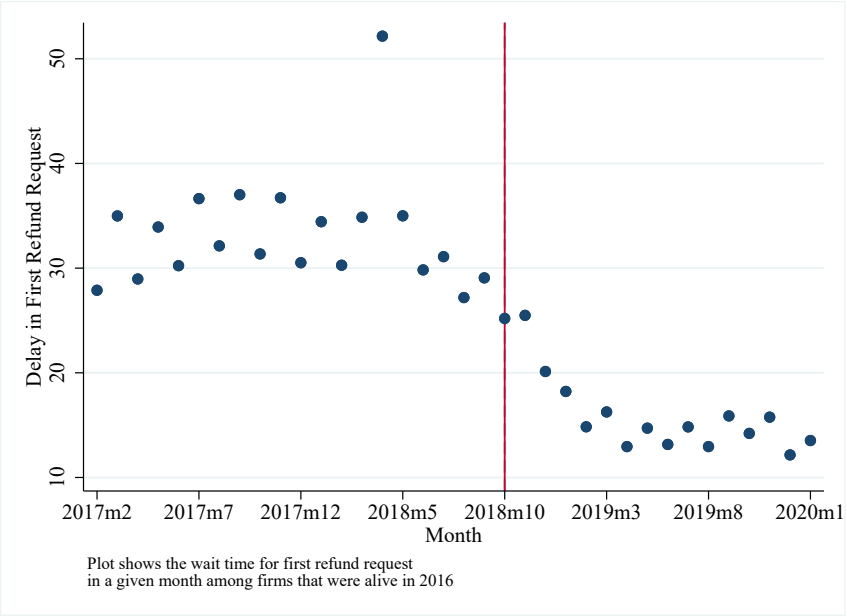
Figure 1: Waiting times conditional on reported liability



Source: authors' compilation.

We can see in Figure 2 that the change in processing time happened just around October 2018, though the faster payment of backlogs is also reflected in the shorter processing duration of refund claims starting in June 2018. Again, we can observe that processing times fell by more than 50 percent after October 2018. It should be noted that these measured waiting times reflect only a portion of the time required to process a VAT return, meaning overall waiting times from filing to payment were substantially longer, although it is difficult to say by how much. Nevertheless, it is clear that firms making refund claims after October 2018 tended to receive their refunds sooner.

Figure 2: Waiting time for first refund request (days)

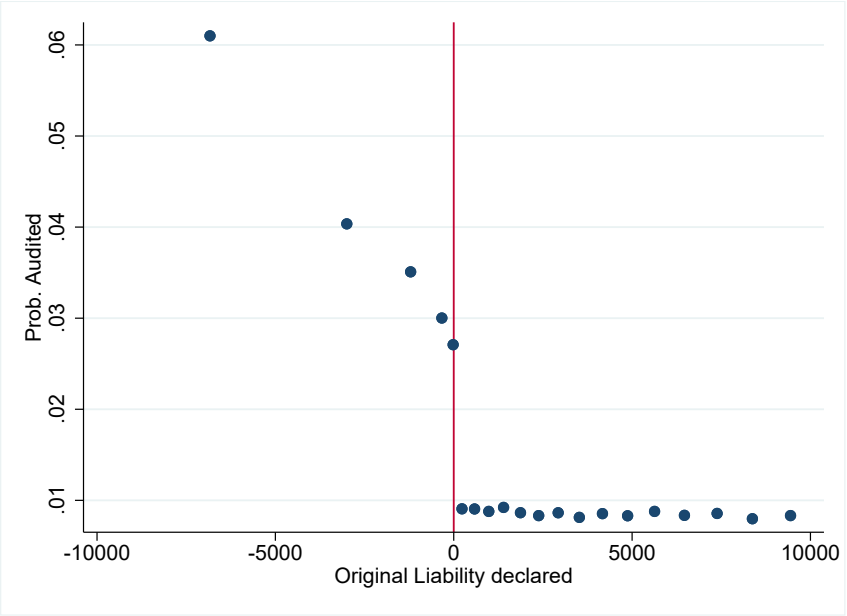


Note: waiting time for the first refund request in a given month among firms that were observed in 2016, in days.

Source: authors' compilation.

There are good reasons to believe that delays in paying out refunds are due to the increased likelihood of triggering an audit if a refund is requested. Figure 3 shows how the likelihood of an audit varies with the original declared liability.[6]

Figure 3: Probability of audit conditional on original reported liability



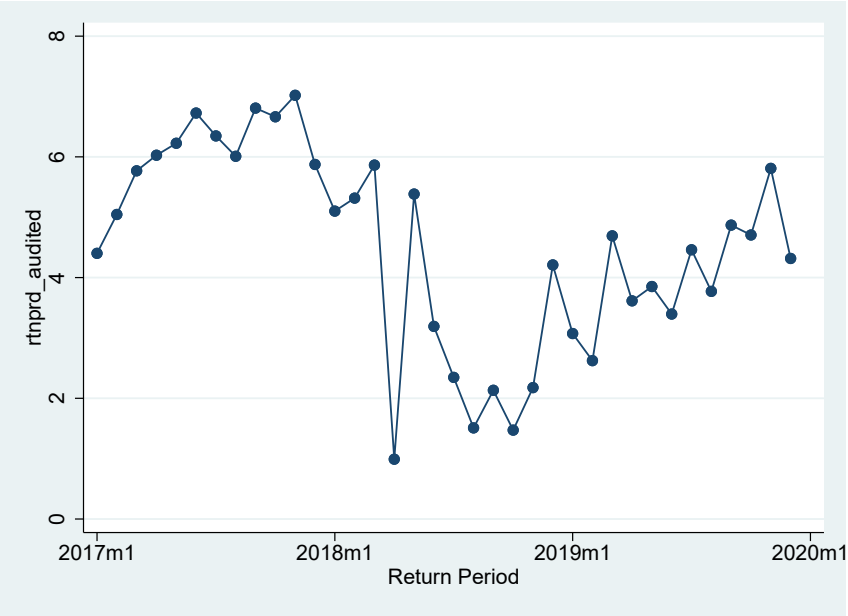
Note: we have the universe of audits that closed after January 2020. The probability of an audit is therefore the likelihood that a return was selected for audit *and* was only closed after January 2020. In the Appendix we show the discontinuity in audit selection for return periods after January 2020.

Source: authors' compilation based on data.

[6] We have the universe of audits that closed after January 2020. The probability of an audit is therefore the likelihood that a return was selected for audit *and* was only closed after January 2020. In the Appendix we show the discontinuity in audit selection for return periods after January 2020.

We see that this discontinuity in the likelihood of being selected for audit based on declared liability falls starting in financial year 2018–19. Figure 4 shows the difference in average probability of appearing in the audit data between small refunds and small payments, divided by the likelihood of appearing in the audit data for small payments. There is a sharp fall in this ratio starting in April 2018, and then a steady rise.

Figure 4: Difference in audit probability for small refunds and small payments



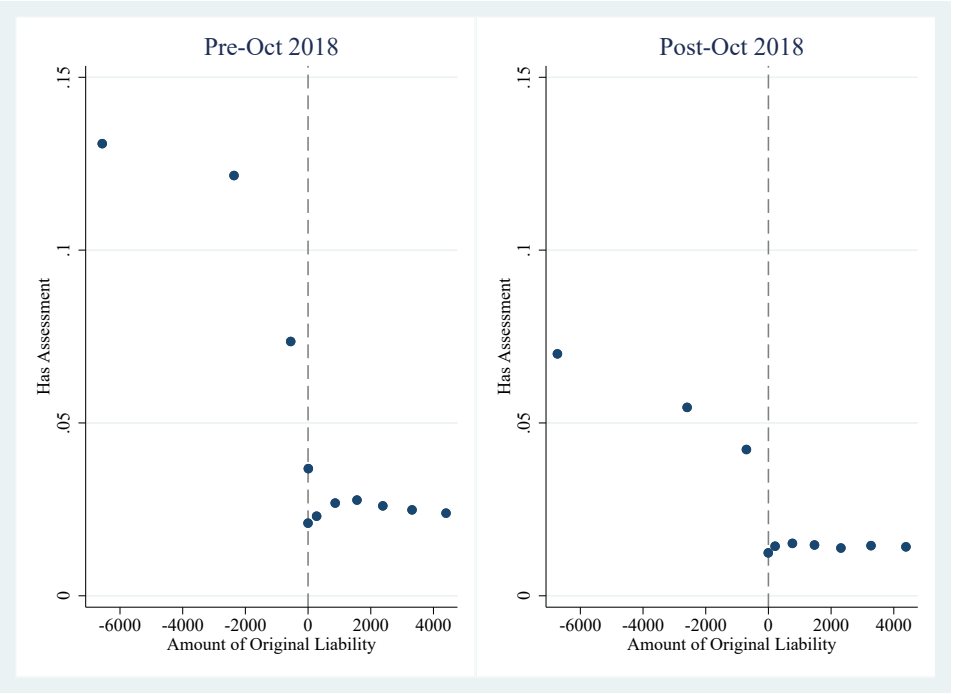
Note: the figure shows the difference in probability of a return appearing in our audit sample for small refunds (0–10,000 rand) and small payments (0–10,000 rand), in each month, divided by the probability of appearing in the audit data for small payments. We scale by the probability for small payments because the likelihood of audit and of appearing in the audit data grows over time.

Source: authors' compilation.

In general, we see additional assessments if a refund is requested. As Figure 5 shows, there is a discontinuous change in the likelihood of assessment if a taxpayer requests even a small refund as opposed to reporting a small, positive liability. The discontinuity decreases substantially after October 2018. Before this, the probability of having an assessment that results in an adjustment is about 8 percent for small refunds, rising to 12 percent for larger refunds. Afterwards, the probability drops to about 5 percent and the drop is larger for larger refunds.

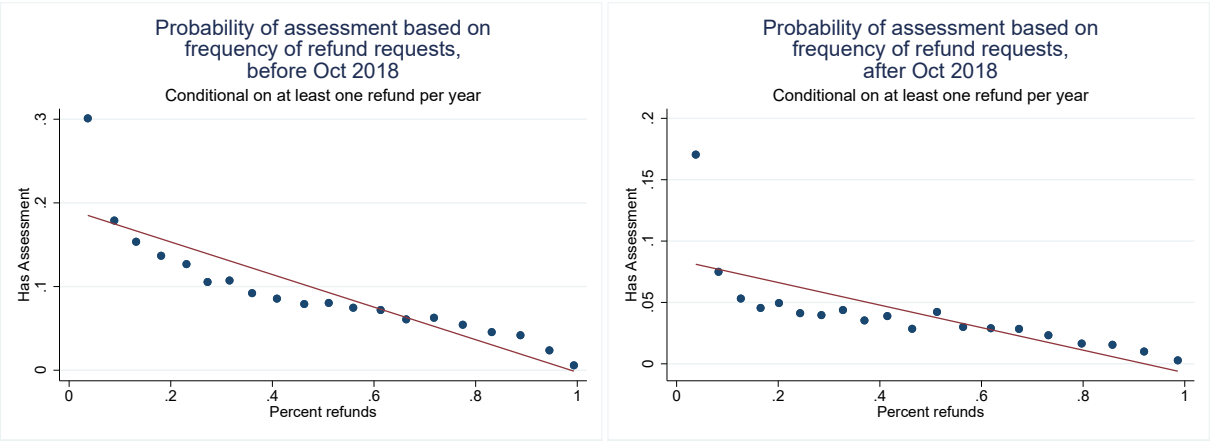
The likelihood of assessment does seem to depend on the frequency with which taxpayers request refunds. Those who request refunds more frequently, which can include exporters and sellers of zero-rated domestic goods, are less likely to have their refund requests assessed (Figures 6a and 6b). The change in likelihood of assessment and delay is therefore greatest for those who infrequently request refunds.

Figure 5: Probability of assessment with adjustment conditional on reported liability



Source: authors' compilation.

Figure 6: Frequency of refund requests and probability of assessment, before and after October 2018



(a) Before October 2018

(b) After October 2018

Source: authors' compilation.

3 Model

Suppose a firm has to pick how much capital, K, to employ to produce a good that obtains revenue net of intermediate and labour costs $F(K)$, with $F''(\cdot) < 0 < F'(\cdot)$. The price of capital is normalized to unity. Further, the firm exports a portion η of its output, on which a foreign VAT rate, τ^F, is applied. For the portion of the output that is not exported, domestic VAT applies at rate τ. When a firm generates excess credits such that it is due a tax refund (i.e. its taxable value added including the capital purchase is negative), there is a delay in refund payment. To model this delay, we simply assume that only a fraction $1-\varphi$, for $\varphi \in [0,1]$, of the (present discounted value of the) refund is actually paid out to the firm. A value of $\varphi = 1$ would indicate that the refund is never paid out, while a value of $\varphi = 0$ would indicate that the refund is paid out immediately and in full.

We denote by $V(K)$ the domestic taxable value added, so that

$$V(K) \equiv (1-\eta)F(K) - K \tag{1}$$

Let $\bar{K}$ denote the point at which this taxable value added is equal to zero, $V(\bar{K}) = 0$, implying that $V(K) > 0$ for any $K < \bar{K}$ and $V(K) < 0$ for any $K > \bar{K}$. Using this notation, the firm's static profit maximization problem can be stated as:

$$\max_K F(K) - K - \eta\tau^F F(K) - \tau\Big(V(K) + \varphi \max\{0, K - \bar{K}\}\Big) \tag{2}$$

Taking the first-order condition with respect to K gives us two cases, depending on whether optimal capital purchases are above or below $\bar{K}$. If the optimal $K^* < \bar{K}$, then $V(K^*) > 0$ and no refund is due. If, instead, the optimal $K^* > \bar{K}$, then $V(K^*) < 0$ and the firm is due a refund.

Case 1: $K^* < \bar{K}$ In this case, the first-order condition is

$$F'(K^*) = \frac{1-\tau}{1-\eta\tau^F - (1-\eta)\tau} \tag{3}$$

As we can see, in this case the firm's choice is unaffected by the policy towards refunds, governed by parameter φ. Further, in the case of a firm oriented entirely towards the domestic market, we see that the full and immediate deductibility of capital leaves the choice of the capital purchase unaffected. In other words, when $\eta = 0$, $F'(K^*) = 1$ and the firm picks the level of capital it would have chosen in the absence of taxation. This feature is a consequence of the fact that we are abstracting away from non-VAT-deductible input choices, in particular labour, that might distort decisions on other margins even under full and immediate deductibility.

Case 2: $K^* > \bar{K}$ In this case, the first-order condition is

$$F'(K^*) = \frac{1-\tau+\varphi\tau}{1-\eta\tau^F - (1-\eta)\tau} \tag{4}$$

From Equation 4 we can see that now an increase in φ entails a higher marginal revenue product of capital at the optimum. Since we are assuming that $F''(\cdot) < 0$, this means the optimal choice of capital will be lower, the higher the value of φ. If $\varphi = 0$, meaning the refund is paid out immediately and in full, then we can easily see that marginal incentives are exactly the same as they were in Equation 3. Moreover, the difference between first-order conditions 3 and 4 shows that firms face a discrete change in their marginal incentives to invest when they cross the threshold $\bar{K}$. This implies they will face an incentive to 'bunch' at $\bar{K}$.

This simple model tells us that capital purchases should increase after the policy change among those firms whose taxable value added excluding capital purchases is close to zero. This increase would come

from firms choosing to no longer bunch at exactly value-added pre-capital purchases (i.e. zero refund) and also firms making larger desired investments above this amount.

Credit constraints should also play a pivotal role in this discussion. A firm's credit constraints will determine the kink in investment cost at $\bar{K}$. A completely unconstrained firm will only face a small increase in the marginal cost of investment when the amount increases over $\bar{K}$. On the other hand, a firm that cannot borrow at all faces a large increase in the marginal cost of investment when it crosses $\bar{K}$.

The model also allows us to study how the effect of refund delays differs for exporters relative to non-exporters. As exports are zero-rated in the domestic VAT system, a larger η entails that the firm has less room to make capital purchases before having to request a refund. In the extreme case where the firm exports all of its output, we can see from Equation 1 that $\bar{K} = 0$, meaning any positive capital purchase will trigger a refund. Such a firm will always operate under first-order condition 4 and will therefore always be impacted by refund policy.

This model gives us some intuition as to how a 'naive' firm, which does not have any experience with refund policy, might operate. Denote by $\hat{\varphi}$ a firm's perceived effect of refund delays. In the most extreme case, an inexperienced firm might assume that refunds are paid out immediately and in full, $\hat{\varphi} = 0$. In this case, the firm will make investment decisions as if under first-order condition 3, meaning it will make higher capital purchases than otherwise and face no incentive to bunch at $\bar{K}$. In general, as long as $\hat{\varphi} < \varphi$, and firms naively expect that the refunds policy is more lenient than in reality, first-order condition 4 tells us they will make larger capital purchases than they would under $\hat{\varphi} = \varphi$.

4 Methods

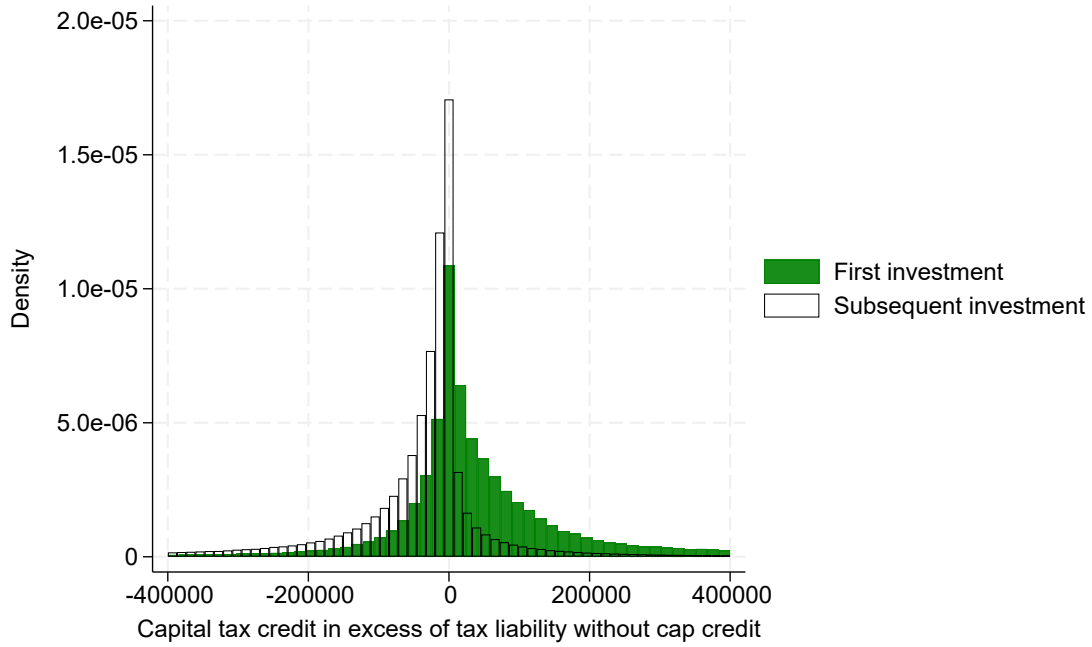
The change in VAT refund policy occurred at a time of broader change in tax administration. To isolate the causal impact of refund administration through a reduced-form analysis, we take two different approaches.

First, we zoom in on firms making their first capital purchase. The idea is that younger firms are more likely to be inexperienced regarding the administration of refunds, and therefore more likely to be influenced in their subsequent behaviour by what happens when they make their first investment. Even though our sample period does not allow us to estimate long-run effects, there are good reasons to believe that firms learn about the consequences of claiming a refund from their interactions with tax authorities. For example, we can see in Figure 7 that first-time capital purchasers are much less likely to strategically make capital purchases that do not cross the threshold into negative value added.

To establish the causal impact on firms making their first investments, we compare the behaviour of firms subsequent to their first capital purchase before and after the policy. We focus on firms making their first purchase because its timing is less likely to be influenced by the refund policy, due to their inexperience with claiming input tax credits from capital purchases. This assumption is supported by Figure 8, which shows that the probability of observing a firm making a capital purchase for the first time is unchanged before and after October 2018, even though, as we saw in Figure 2, the processing time falls. We also verify that firms that make their first capital purchase after October 2018 are not that different from earlier cohorts on other time-invariant unobservables.

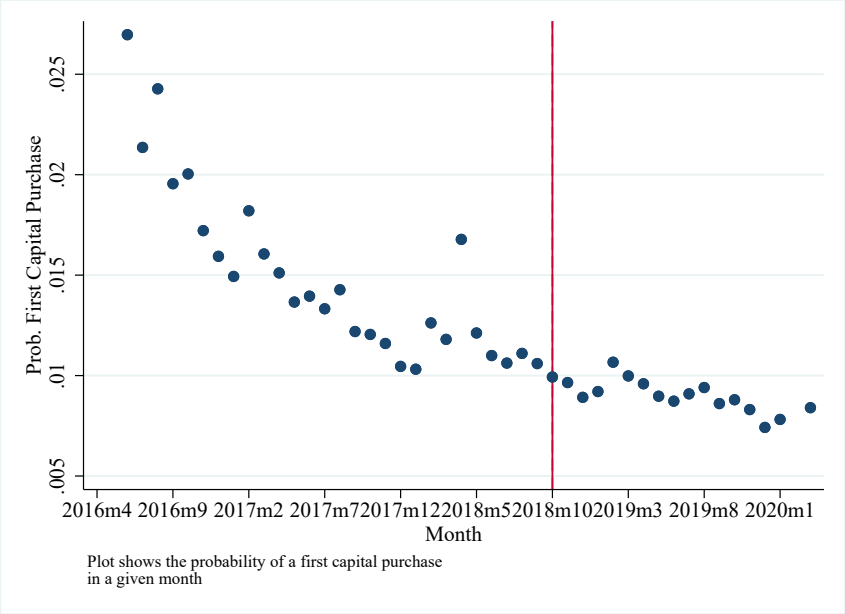
Analysing firms at the time of their first capital purchase also serves to inform us of the impact of the refund policy on a particular group of interest: small and growing firms making only domestic sales. While there is evidence that refund administration can adversely affect exports, there is little written about its impact on non-exporters who also claim refunds.

Figure 7: Histogram of tax credits from capital purchases in excess of reported value added, for first-time capital (green) and for firms that already reported a capital purchase in the past (white)



Source: authors' compilation.

Figure 8: Probability of first capital purchase



Source: authors' compilation.

4.1 Event-study following first capital purchase

We run the following specification focusing on firms in months when they made their first capital investment. We compare the subsequent behaviour of cohorts making their first investment after the refund policy change to those who made their first investment before the change:

$$Y_{itmy} = \alpha_m + \beta_m \times Post_t + \delta_y + \varepsilon_{it} \tag{5}$$

where Y_{itmfc} is the outcome for firm i in period t, which falls in month-of-the-year m, and calendar year y. The dummy variable $Post_t$ is equal to 0 in months preceding the policy change (October 2018) and 1 thereafter. The idea is to compare all firms making their first investment in, for example, the month of February, of different years, allowing for average outcomes to differ between Februarys before and after the policy change. The coefficients β_m, $m = 1, \ldots 12$ identify this average difference for each of the 12 months of the year. We do this both around the documented policy change in October 2018 as well as around a placebo policy change arbitrarily placed in October 2016, which allows us to control for time trends assuming these did not change over the years.

A similar specification can also be run averaging out the effect of the policy change across all months of the year:

$$Y_{itmy} = \alpha_m + \beta \times Post_t + \delta_y + \varepsilon_{it} \tag{6}$$

Coefficient β will then capture the average change in outcome y after the policy change, conditional on month-of-the-year fixed effects and calendar year fixed effects. Again, doing this both for the documented policy change in October 2018 and a placebo policy change in October 2016 allows us to measure more accurately the causal effects of the policy change, as well as getting a confidence interval for this causal effect.

4.2 Intensity of treatment and heterogeneous treatment effects

We estimate the impact of the refund policy reform on avoiding refunds and limiting capital purchases, using a difference-in-differences strategy, splitting firms according to characteristics that determine whether they are more or less constrained by refund delays.

Whether a capital purchase triggers a refund depends on the tax liability of a firm when they do not make any capital purchases—let's call this the 'tax cushion'. This tax cushion is the taxable value added of the firm before any capital purchase, which to some extent is determined by their production function and market conditions.

Of course, firms make their capital and other intermediate goods purchase decisions jointly. We try to capture only the production-function-determined tax cushion by taking the average over all tax returns when no capital purchase was made. Firms with smaller tax cushions are more constrained—the same amount of capital purchase is more likely to trigger a refund relative to a firm with a larger cushion. We therefore expect that when refund delays decrease, firms with smaller cushions are more likely to make a capital purchase that triggers a refund.

We estimate the following specification:

$$Y_{it} = \sum_h \beta_h Treat_i \times Post_{ht} + \gamma_i + \delta_t + \varepsilon_{it} \tag{7}$$

where β_t captures the differential impact of the refund policy change in each period t after October 2018 between more or less constrained firms as identified by the $Treat_i$ indicator. Our main definition of $Treat_i$ is whether a firm has below-median taxable value added before capital purchases, as defined in Section 2. Taxpayer fixed effects (γ_i) control for time-invariant characteristics of taxpayers, such as frequency of filing, type of ownership, industry, etc. We aggregate the data to quarters to reduce noise and seasonality. Quarter fixed effects (δ_t) control for aggregate, time-specific shocks. Our main outcome variable (Y_{it}) is whether firms make capital purchases that are large enough to result in a net refund.

A second margin of treatment intensity we examine is whether a firm has a high level of export or other zero-rated sales. Firms that are mainly making zero-rated sales are likely to be due a net refund regardless of the size of their capital purchase. That also means that any capital purchase they make

incurs the implicit tax due to refund delays, meaning they will make smaller investments than they otherwise would have. We expect the ease in refund delays to have a larger impact on these zero-rated sellers.

Evidence for the comparability of the treatment and control groups defined in this manner is given by observing parallel pre-trends.

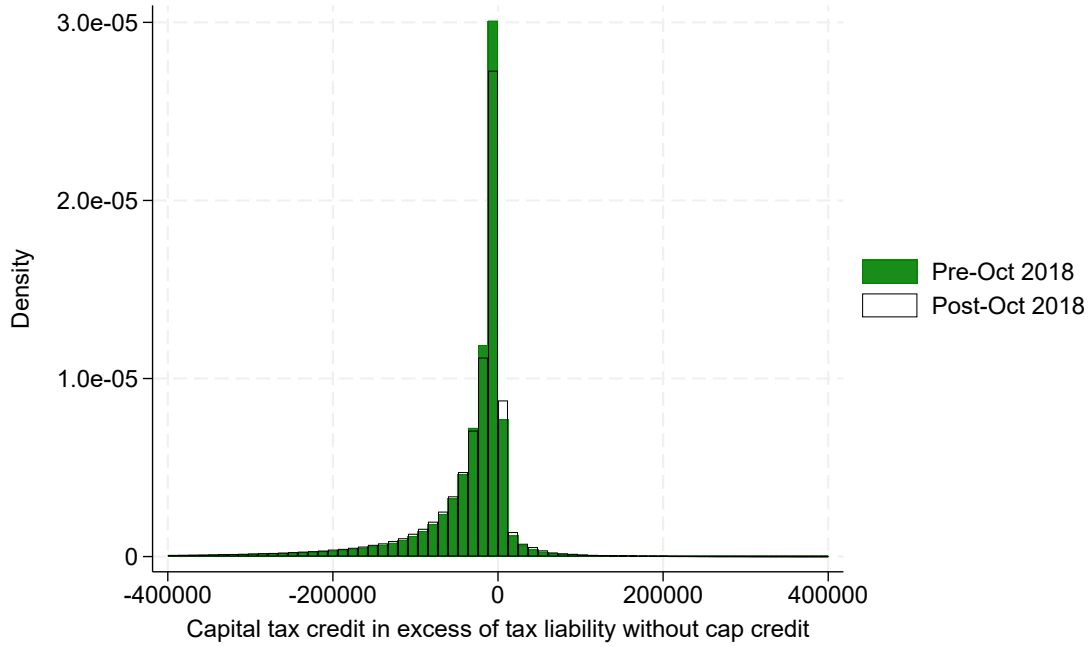
5 Results

5.1 Investment response: firms limit capital purchases to avoid refunds

There is a strong incentive for firms to avoid small refunds. We see evidence that firms act on this incentive in the excess density of returns reporting zero, or very small, liability. One way that firms target zero or small, positive liability seems to be by limiting their reported capital purchases.

We calculate firms' reported taxable value added excluding tax credits arising from capital purchases, and calculate the difference in their reported capital purchase and this value-added amount. A value of zero indicates that the firm's capital purchase amounts to exactly this taxable value added, meaning it will not trigger a refund. Figure 9 shows the distribution of difference around a 400,000 rand band from zero, before and after October 2018. First, note that there is severe bunching of capital tax credits just below the amount that would trigger a refund. Second, this bunching falls and the distribution shifts to the right after October 2018, suggesting that firms are less likely to limit their capital purchases to avoid triggering refunds (clear, white bars).

Figure 9: Reduction in bunching of capital purchase to avoid triggering a refund



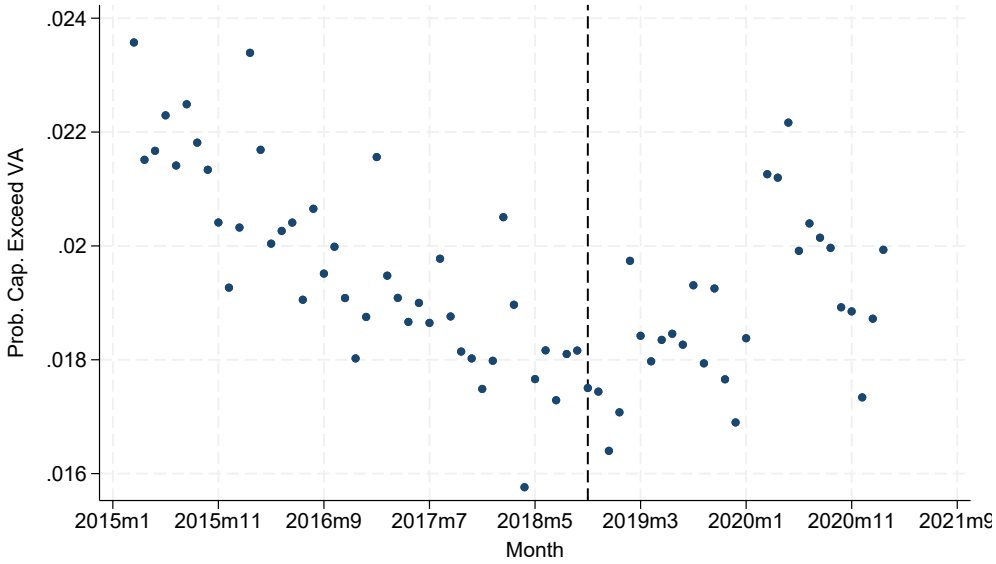
Note: the sample is restricted to firms that have never had any zero-rated sales in the data. We restrict the range of capital purchases in excess of taxable value added to between –400,000 rand and 400,000 rand for visibility.

Source: authors' compilation.

That this bunching is driven by the refund environment is apparent from Figure 10, where we plot the likelihood that a capital purchase exceeds taxable value added in each month. We see that this likelihood was falling until October 2018, at which point it reverses trend and starts to rise. The timing of the

reversal strongly suggests that capital purchases are likely a discretionary margin through which firms limit refund requests.

Figure 10: Likelihood that a capital purchase triggers a refund

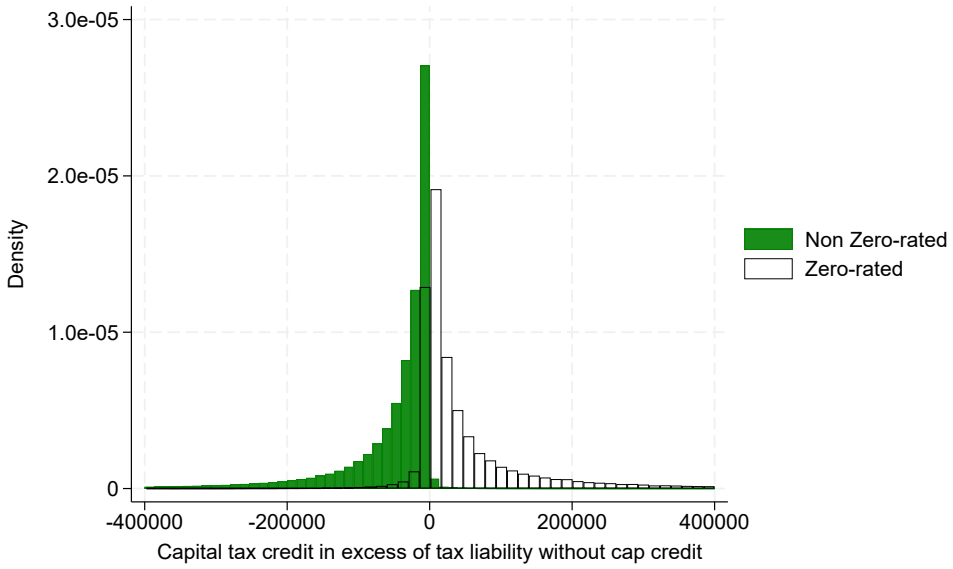


Note: the figure shows the likelihood that a capital purchase provides input tax credits that exceed tax liability excluding capital input tax credits, therefore triggering a refund. The sample is restricted to returns where the taxable value added is positive when excluding the capital tax credits.

Source: authors' compilation.

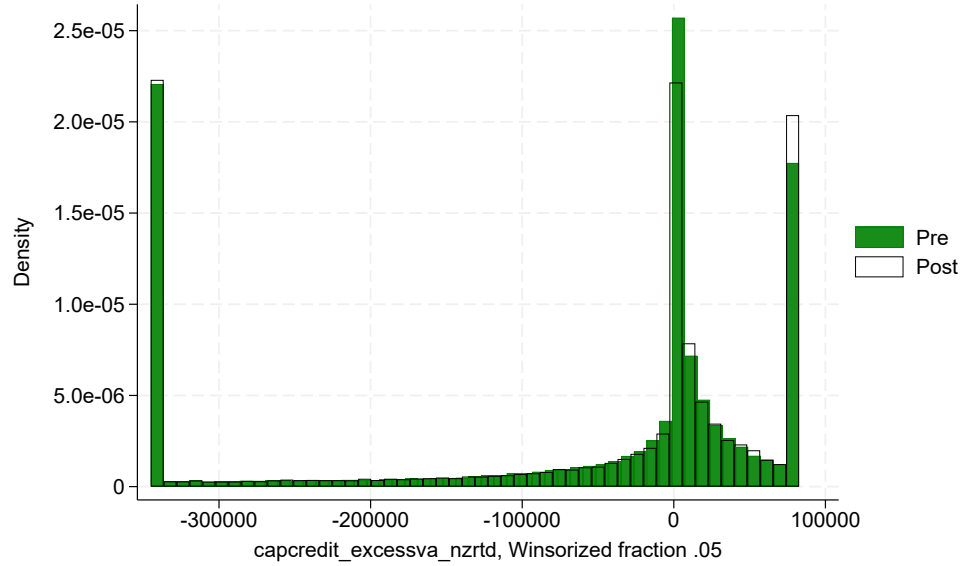
We also see no bunching among firms that are due a refund regardless of whether they make capital purchases. Figure 11 shows capital in excess of taxable value added for firms with mainly zero-rated output and for firms with no zero-rated sales. We see that only firms with non-zero-rated sales bunch below zero, though it is common for firms to make capital purchases that are about the same size as their taxable value added even among zero-rated sellers. In Figure 12 we see that zero-rated sellers increase the size of their capital purchases after the policy change.

Figure 11: Capital purchase in excess of taxable value added among zero-rated and non-zero-rated firms



Source: authors' compilation.

Figure 12: Capital purchase in excess of taxable value added among zero-rated firms



Note: the sample is restricted to firms that request a refund because their output is zero-rated. They would have positive taxable value added if their output was taxed at the standard VAT rate. Capital purchases in excess of taxable value added are calculated as if their output would have been taxed at the standard rate.

Source: authors' compilation.

We see that firms' capital purchases are below this amount even when we calculate the value-added cushion based on return periods when there was no capital purchase. We do this because we recognize that firms choose their reported capital purchase simultaneously with their other inputs. But the value added calculated based on months with no capital purchase should approximate the undistorted amount of taxable intermediate inputs that go into a firm's production without the need to avoid triggering a refund.

5.2 Event-study results

Figure 13 displays the outcome of our event-study methodology for several outcomes among firms that started filing VAT returns in 2014 or after, and which make their first recorded capital purchase. Here we can see that for most outcomes we do not record any effect around the placebo treatment. Each calendar month in the figures averages the difference in outcomes in that month between pre- and post-policy years. For example, the coefficient displayed for month 1 for the 'Actual' policy change series is the difference in the average outcome in January 2019, 2020, and 2021 (i.e. post-policy) and the average outcome in January 2017 and 2018 (pre-policy). For each of the outcomes, the coefficients of the placebo series for all months are close to zero and statistically insignificant, indicating that the differences we observe in the actual policy change are not simply picking up time trends or seasonal effects. In contrast, we see statistically significant differences after the actual policy change. The size of the effects vary by month and are usually smallest from October to December, which includes periods shortly after the change in policy.

We estimate the pooled average effect across all months for 'new entrants'—that is, firms that first appear in our data starting 2014—in Table 3. Appendix Tables A.2 and A.3 show the results of the same exercise respectively for all firms and for firms with a history of zero-rated sales. In each table, Panel A shows the event-study estimates using the real policy change date of October 2018, while Panel B displays the results of the placebo event study. The differences between the actual and placebo event studies are displayed below, along with the standard error of this difference. As we can see, after differencing out the effect of a placebo, we still find substantial changes in firm behaviour.

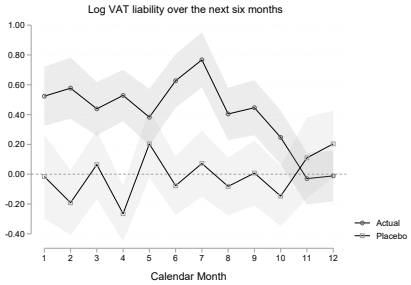
Across the board, we see large responses in refund-claiming behaviour. In the largest sample, after accounting for the trend estimated with the placebo, we measure an increase of 8 percentage points in the probability of claiming a refund within six months after the first investment. Over a baseline of 40 percent of firms requesting refunds in a six-month window after their first investment, this represents a 20 percent increase in the probability of claiming a refund. This is basic evidence showing that the aggressive treatment of refunds before October 2018 was dissuading firms from claiming them.

We see the strongest behavioural responses among new entrants. We find that cumulative capital purchases within six months of their first capital purchase are 26 percent higher for firms making their first purchase after October 2018. Later entrants in the placebo event study make somewhat smaller capital purchases, though this is not a statistically significant difference. Relative to the trend estimated in the placebo, the impact of the actual policy change on capital purchases is even higher—an increase of 31 percent, which is statistically significant at the 99 percent confidence level.

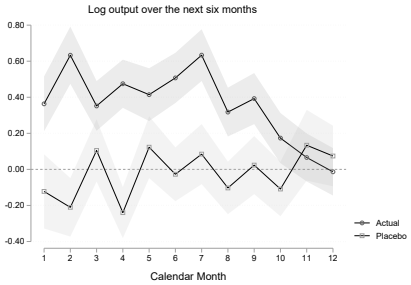
This represents evidence that not only were firms affected in their refund-claiming behaviour, but also in their investment behaviour. The fact that results are so strong particularly for new entrants, on whom the 'naivety' argument of our identification strategy is based, is particularly credible. This gives us more confidence in the causal validity of our results.

The fact that we measure our outcomes over a six-month window gives more credence to the idea that what we observe are real responses in investment behaviour, rather than mere effects on the timing of reporting. If firms were merely less likely to report their capital purchase 'piecemeal' across several months to avoid triggering a refund after the policy change, we would not observe the strong responses we see over a six-month period. Indeed, firms were also 5 percentage points more likely to report 'large' capital purchases, defined as investments that took up at least 20 percent of reported sales.

Figure 13: The outcome of our event-study methodology: results of specification 5

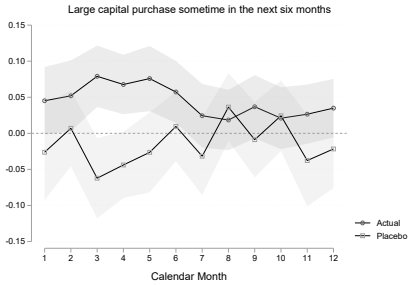


(a) Log(VAT liability)

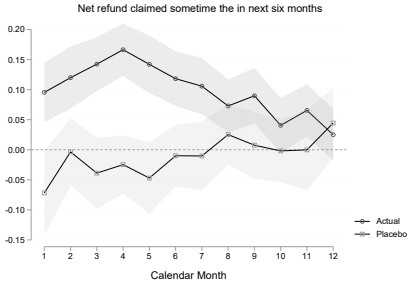


(b) Log(output)

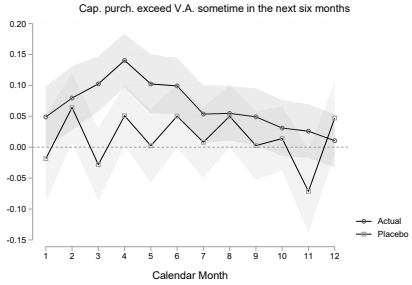
(c) Log(capital purchases)



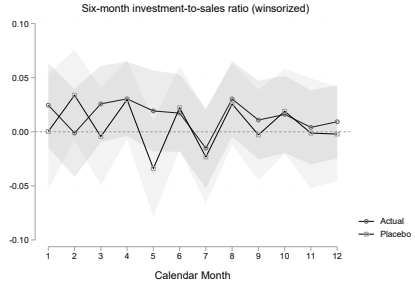
(d) Capital purchase $>$ 20% of sales



(e) Firm makes any refund request



(f) Capital purchase triggers a refund



(g) Investment to sales ratio (winsorized)

Note: the results of specification 5 for several cumulative outcomes, six months after the first investment, for a placebo sample in 2014–19 with a placebo treatment in October 2016, and for the real treatment in October 2018, on a sample going from 2016 to 2021. The sample is firms that registered in the VAT system in 2014 or afterwards.

Source: authors' compilation.

Table 3: Impact on first-time capital purchasers, new entrants

Panel A: Actual policy change

	Cap. purch.	VAT liab.	Output	Inv. to sales	Cap. exceed VA	Claimed ref.	Large inv.	I to S (w)
Post October 2018	0.260***	0.254***	0.236***	–4.871	0.050***	0.079***	0.040***	0.012
	(0.064)	(0.063)	(0.049)	(3.939)	(0.016)	(0.016)	(0.015)	(0.012)
Observations	49,320	31,865	48,127	48,127	49,323	49,323	47,329	48,127

Panel B: Placebo policy change

	Cap. purch.	VAT liab.	Output	Inv. to sales	Cap. exceed VA	Claimed ref.	Large inv.	I to S (w)
Post October 2016	–0.053	0.002	–0.015	–0.340	0.017	–0.003	–0.014	0.006
	(0.076)	(0.073)	(0.055)	(1.591)	(0.019)	(0.019)	(0.018)	(0.014)
Observations	35,525	23,362	34,850	34,850	35,528	35,528	34,320	34,850
Difference	0.313***	0.253**	0.251***	–4.531	0.033	0.083***	0.053**	0.006
SE	0.118	0.110	0.082	4.906	0.029	0.029	0.027	0.019

Note: standard errors reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: authors' compilation.

One would reasonably expect that larger capital purchases result in larger output. The effect on VAT liability, however, is more ambiguous. On one hand, higher reported sales would result in higher VAT liability; on the other, larger capital purchases would result in higher input tax credits. Despite this, we find that both sales output and net VAT liability increase. Reported sales are about 24 percent higher, while declared VAT liability is about 25 percent higher after October 2018. These effects are also similar in size and highly statistically significant after accounting for the trend estimated in the placebo.

As an additional robustness check of these results, we perform a similar analysis of zero-rated sellers. These firms nearly always make refund claims, regardless of the size of their input purchases relative to their sales. Their awareness of the refund administration is not likely to be linked to the first time they make a capital purchase. We should not expect the timing of their first capital purchase to affect their response to refund administration. The results in Table A.3 support this presumption. Although there is a difference in capital purchases between later and earlier capital purchasers in the actual event study, we see a very similar difference in the placebo.

We interpret the changes induced by the policy as being a result of the quicker availability of their working capital, as opposed to a change in the type of firms purchasing capital for the first time. In addition to the cross-sectional evidence that first-time capital purchasers do not seem to avoid refunds, we show that pre-capital purchase characteristics of firms are unchanged by the refund policy. Table 4 shows the difference in time-invariant or time-varying but pre-policy characteristics of those who make capital purchases for the first time before and after the policy change. There is no statistically significant differences in the likelihood that a first-time buyer is a corporation, or from the primary, secondary, or tertiary sectors. The point estimates of the differences are also economically small.

Those making capital purchases for the first time after the policy change seem to come from industries with smaller median value added. There is very little difference in firms' net VAT liability six months before their first capital purchase, although it does seem to be slightly larger firms making their first capital purchase after the policy change.

Table 4: Baseline characteristics of first-time capital purchasers

Panel A: Actual policy change							
	ln CIT	Primary	Secondary	Tertiary	Ind. VA	l6.Log(Output)	l6.Log(VAT liab.)
Post October 2018	0.009	–0.020**	0.014	0.001	–1,522.997***	–0.156	0.254
	(0.013)	(0.009)	(0.014)	(0.017)	(251.633)	(0.407)	(0.451)
Observations	58,346	58,346	58,346	58,346	58,346	1,268	1,090
Panel B: Placebo policy change							
	ln CIT	Primary	Secondary	Tertiary	Ind. VA	l6.Log(Output)	l6.Log(VAT liab.)
Post Oct. 2016	–0.012	0.007	–0.007	0.006	834.995**	0.561	0.848*
	(0.014)	(0.009)	(0.015)	(0.017)	(338.452)	(0.424)	(0.500)
Observations	55,039	55,039	55,039	55,039	55,039	1,040	913
Difference	0	-0	0	–0	–2,358	–1	–0
SE	0.022	0.014	0.024	0.028	492.999	0.609	0.603

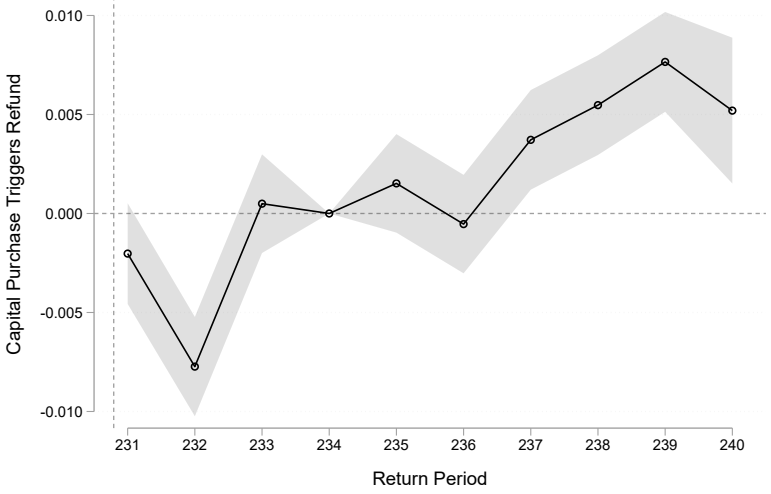
Note: standard errors reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: authors' compilation.

5.3 Intensity of treatment

We find that firms with below-median taxable value added, for whom a given level of capital investment is more likely to generate a refund due, become more likely to make a refund-triggering capital purchase after the policy change. Figure 14 shows the results of specification 7, where $Treat_i$ is defined as having below-median taxable value added. We see that these more-constrained firms are about 1 percentage point more likely to make a capital purchase and request a refund, starting three quarters after the refund policy change. This is only true among firms that have no export or other zero-rated sales.

Figure 14: Differential impact on probability of a refund-triggering capital purchase

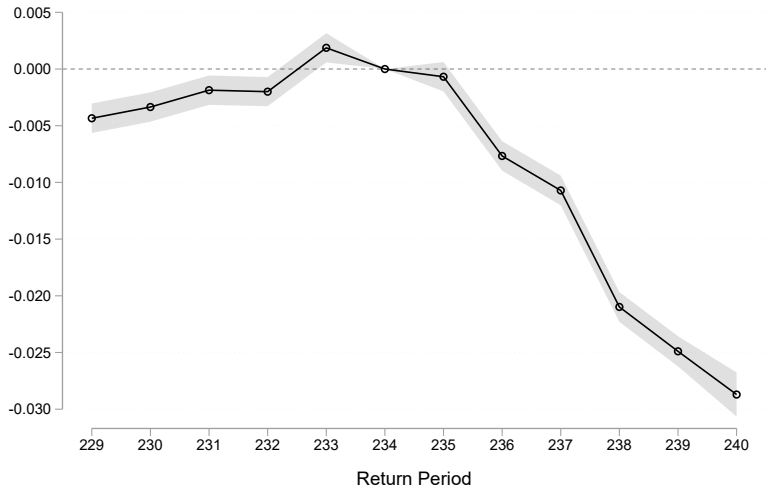


Note: the sample is restricted to firms that have never had any zero-rated sales. 'Treatment' is defined as firms with below-median taxable value added excluding capital purchases. Taxable value added is calculated as an average across all returns without a capital purchase within a financial year.

Source: authors' compilation.

We then compare the behaviour of firms who are mainly exporters or zero-rated sellers to firms with no zero-rated sales (Figure 15). Here, $Treat_i$ is any firm without any history of zero-rated sales, while the control group is any firm whose output is mainly composed of zero-rated sales over the entire sample period. We calculate the counterfactual taxable value added of sellers with zero-rated sales, if their output were to be taxed at the standard rate. Because their output is zero-rated, these firms routinely request refunds even without making any capital purchases. As we discuss in Section 3, the refund delays therefore make it more costly for them to make a capital purchase of any size. We find that these zero-rated sellers become much more likely to make larger capital purchases (exceeding their counterfactual taxable value added) after the policy change relative to those without any zero-rated sales.

Figure 15: Differential impact on probability of a refund-triggering capital purchase, non-zero-rated vs. high zero-rated

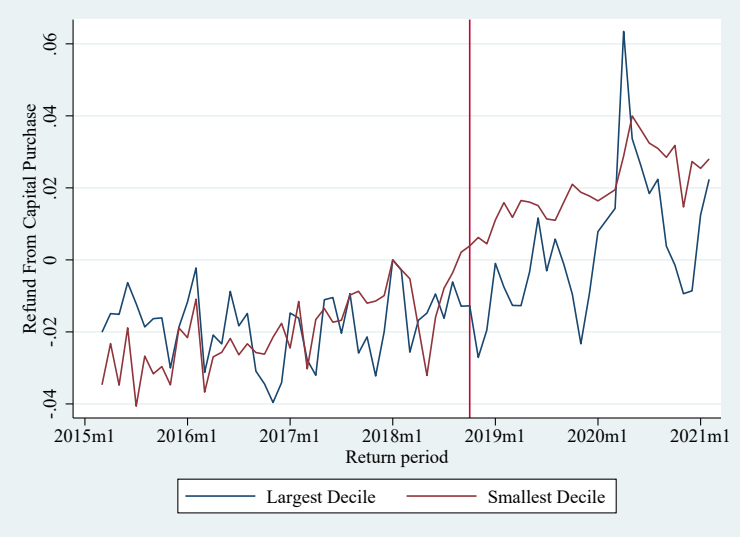


Note: 'treatment' is defined as firms with no history of zero-rated sales, while the control group is composed of firms with a percentage of zero-rated output over total output of at least 95 percent.

Source: authors' compilation.

We compare the change in small and large firms' investment after the refund policy change. Small firms are more likely to have internal and external financing constraints that make the refund threshold more salient. Figure 16 shows the trends in the likelihood that the largest firms and smallest firms (as categorized by being in the top 10 percent or bottom 10 percent of the total sales distribution) make a capital purchase large enough to trigger a refund. Both the largest and smallest firms were on similar trends relative to the likelihood in January 2018. Starting from October 2018 (as indicated by the vertical red line), the smallest firms become more likely to make a capital purchase large enough to trigger a refund claim. Again, this difference in behaviour of the most and least constrained firms is suggestive that capital investment was constrained by the strict refund administration.

Figure 16: Differential impact on probability of a refund-triggering capital purchase, small vs. large firms



Note: the largest and smallest deciles are firm–month observations falling into the top 10 percent of firm–months by total sales and the bottom 10 percent of total sales, respectively. The *y*-axis is the likelihood in any given month that a firm belonging to the decile category makes a capital purchase and a refund claim in that month. The rates are normalized to be relative to the rate in January 2018. The sample here is restricted to firms with no export or other zero-rated sales.

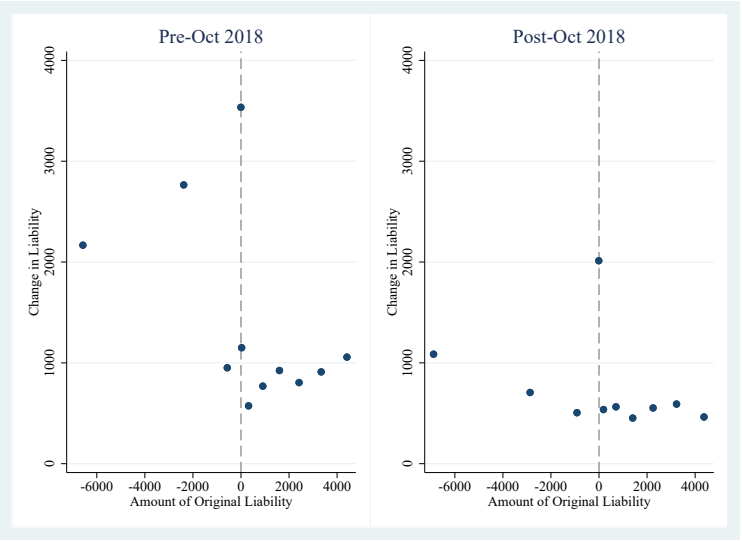
Source: authors' compilation.

5.4 Impact on audit recoveries

Since the improvement in refund administration seems to have occurred through a decrease in likelihood of verifications, what impact did it have on the average and overall recovery?

There is a small decrease in the average adjustment from assessed returns. Figure 17 shows the average change in liability after an assessment for return, with original liability as given by the *x*-axis. Before October 2018, returns requesting refunds of up to 6,000 rand had their liability adjusted upward by about 3,000 rand, cutting down the size of the requested refund by about half. After October 2018, the average adjustment for small refund requests in this range is about 1,000 rand. It appears that the average assessed and modified return now generates less revenue. The loss of revenue through these assessments has to be weighed against revenue gained from improvements in production efficiency.

Figure 17: Impact on assessment recoveries



Note: the *y*-axis is the difference in the final liability on a return and the original liability when the return was first filed by the taxpayer. The sample is restricted to returns where there was any change in the liability.

Source: authors' compilation.

6 Conclusion

VAT refund delays are widespread and consequential, perhaps even unavoidable given the legitimate concerns of evasion. In addition to their documented impact on exports, we find that they curtail investment by firms at a crucial point in their life cycle when they would optimally make large investments relative to size. These investment impacts are substantial. A decrease in delay of about 20 days results in a 26 percent increase in investment within six months of a new firm's first capital purchase.

That this increase in investment is due to the treatment of refunds is apparent from the fact that it is more likely to occur among firms that had smaller taxable value added where a capital purchase of a given size is more likely to trigger a refund. Smaller firms that are more likely to be financially constrained by the loss of working capital also become more likely to make refund-triggering capital purchases.

As in other contexts, exporters and those with domestic zero-rated sales who frequently request refunds increase their investment by even more when refunds are expedited. They become 20 percent more likely to make large investments after the policy change relative to those without any zero-rated sales.

Choices about how to deal with VAT refunds present tax administrations with a difficult policy trade-off. On one hand, too lax a treatment of refunds might result in rampant fraud and evasion. On the other, an excessively aggressive treatment of refunds can hamper investment and firm growth. This paper is the first to speak directly to this trade-off using comprehensive administrative data. The reduction in waiting times for refund claims occurred through a 50 percent reduction in the probability of audit for refund-claimers. Among these more selective audits, returns saw smaller adjustments on average, resulting in a decrease in revenue from adjustments through both fewer and smaller adjustments.

In future work, we will more precisely quantify the net change in revenue in the short and long term from the change in refund policy. The investment impact on high-growth firms is likely to affect their growth trajectory over a much longer horizon. A more careful reading of the investment incentives will also require a dynamic model with adjustment costs, to account for the lumpiness of investment,

and with collateral constraints, to better analyse the distinct effects on working capital as opposed to discontinuous jumps in marginal incentives at the zero-liability threshold.

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Appendix A: Tables

Table A.1: Baseline characteristics of first-time capital purchasers

Panel A: Actual policy change							
	ln CIT	Primary	Secondary	Tertiary	Ind. VA	l6.Log(Output)	l6.Log(VAT liab.)
Post October 2018	0.010	–0.017**	0.013	0.007	–1,301.349***	–0.105	0.328
	(0.011)	(0.007)	(0.009)	(0.012)	(175.385)	(0.194)	(0.227)
Observations	139,892	139,892	139,892	139,892	139,892	5,912	4,494
Panel B: Placebo policy change							
	ln CIT	Primary	Secondary	Tertiary	Ind. VA	l6.Log(Output)	l6.Log(VAT liab.)
Post October 2016	–0.016*	0.005	0.000	0.002	341.692*	0.208	0.306
	(0.009)	(0.006)	(0.008)	(0.010)	(199.508)	(0.157)	(0.195)
Observations	187,163	187,163	187,163	187,163	187,163	8,843	6,613
Difference	0	–0	0	0	–1,643	–0	0
SE	0.016	0.011	0.014	0.018	301.464	0.295	0.354

Note: standard errors reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: authors' compilation.

Table A.2: Impact on first-time capital purchasers

Panel A: Actual policy change

	Cap. purch.	VAT liab.	Output	Inv. to sales	Cap. exceed VA	Claimed ref.	Large inv.	I to S (w)
Post October 2018	0.225***	0.179***	0.211***	–0.742	0.056***	0.076***	0.029***	0.004
	(0.043)	(0.041)	(0.034)	(3.653)	(0.010)	(0.010)	(0.010)	(0.007)
Observations	129,013	90,766	126,531	126,531	129,016	129,016	124,755	126,531

Panel B: Placebo policy change

	Cap. purch.	VAT liab.	Output	Inv. to sales	Cap. exceed VA	Claimed ref.	Large inv.	I to S (w)
Post October 2016	0.134***	0.023	–0.029	1.772	–0.015	–0.004	0.018**	0.009*
	(0.036)	(0.035)	(0.029)	(4.906)	(0.009)	(0.009)	(0.008)	(0.005)
Observations	189,992	141,091	187,360	187,360	189,999	189,999	185,210	187,360
Difference	0.091	0.156**	0.240***	–2.514	0.071***	0.080***	0.011	–0.005
SE	0.065	0.061	0.051	2.441	0.016	0.016	0.016	0.009

Note: standard errors reported in parentheses. *$p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: authors' compilation.

Table A.3: Impact on first-time capital purchasers, zero-rated sellers

Panel A: Actual policy change

	Cap. purch.	VAT liab.	Output	Inv. to sales	Cap. exceed VA	Claimed ref.	Large inv.	I to S (w)
Post October 2018	0.305	0.366	0.368	–26.447	0.027	0.032	0.003	0.039
	(0.214)	(0.827)	(0.232)	(82.465)	(0.031)	(0.038)	(0.043)	(0.065)
Observations	7,353	1,057	6,383	6,383	7,355	7,355	5,226	6,383

Panel B: Placebo policy change

	Cap. purch.	VAT liab.	Output	Inv. to sales	Cap. exceed VA	Claimed ref.	Large inv.	I to S (w)
Post October 2016	0.360*	2.099**	–0.315	21.276***	–0.108***	–0.125***	0.103***	0.096**
	(0.191)	(1.005)	(0.196)	(8.026)	(0.036)	(0.038)	(0.033)	(0.040)
Observations	11,514	3,499	10,532	10,532	11,519	11,519	9,175	10,532
Difference	–0.054	–1.733	0.683*	–47.723	0.135***	0.158***	–0.100	–0.056
SE	0.344	1.534	0.356	49.734	0.035	0.047	0.077	0.080

Note: standard errors reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: authors' compilation.

Appendix B: Data appendix

Data access

The data used for this project was accessed from the NT-SDF. All authors signed a non-disclosure agreement, and all output was first checked by the NT-SDF data lab to ensure that anonymity of individuals and firms was preserved. Our results do not represent official NT or SARS statistics. The views expressed in the paper are the authors' own and not necessarily the views of NT or SARS.

Data used:

We use the CIT_IRP_v5 corporate tax return data and the VAT periodic returns data Version e5_v1. Date of first access: 29 November 2022 Last accessed: 14 May 2024

Software:

Our analysis was conducted using Stata 17.

Variables:

Key variables used from the VAT periodic returns data include:

vat_refno taxrefno taxyear taxperiod tax_year dateprocessed dateprocess datetransaction nummonth fiscal_year process_fiscal_year processmonth subactivity clientstatus clientareacode region industry micro_sector micro_sector_desc areacodedescription paymentfrequency liability_ind natureofperson transactionstype seq_no rectype amtliability debitorderind paymentmethod userid productid vat_srec vat_sroc vat_zree vat_zroe vat_exmptns vat_vatsroc vat_vatsrec vat_acce28tamt vat_acce28txv vat_accne28txv vat_acctval vat_acctvat vat_adjexpshg vat_adjvatexpshg vat_vatois vat_outputtax vat_cgssup vat_cgsimp vat_ogsnc vat_oimpgnc vat_adjchnguse vat_adjbddebts vat_adjoth vat_inputtax vat_vatpayref fld1 fld1a fld10 fld12 fld14 fld14a fld15 fld15a fld16 fld17 fld18 va_total_output va_total_input dieselind vat_loctpurch vat_locnepurch vat_locepurch vat_locelpurch80 vat_locdiesel vat_fgntpurch vat_fgnnepurch vat_fgnepurch vat_fgndiesel vat_rhtpurch vat_rhnepurch vat_rhepurch vat_rhservices cnt_import cnt_export cnt_standard flag revenue_derived input_costs_derived

Key variables used from the CIT data include:

taxrefno taxyear ITR14_c_divdec ITR14_k_totca ITR14_k_totassets k_cash k_sars l_trade x_labcost mic_sic7_5d g_sales g_cos

Cleaning and sample notes

Our sample begins with all VAT-registered firms in each year. We exclude any taxpayer-years when a taxpayer reported both zero output and zero input in any return period. We exclude taxpayer-years when the taxpayer reported extreme values of payments and refunds (i.e. above that 99th percentile). We also exclude a few observations where the reported output tax or input tax did exactly match the sum of its reported components.

The periodic returns data can contain multiple returns for each taxpayer and return period. There is a single original return filed by the taxpayer, which is coded as "100" in the "transactionstype" variable. Other returns are amendments or updates, generally created because the return has been assessed by SARS. These are coded as "105" in the "transactionstype" variable. We collapse the data to the return-period level, i.e. at most a single observation for each return period (month) and taxpayer, by keeping all of the original return type returns and their characteristics as well as summary information from the subsequent returns. For example, we construct a variable that records the last date processed from the last-filed return for a given return period. We also construct a variable containing the final amount payable/ refundable from the last-filed return, and so on. Details of the variables constructed are available in the associated Dofiles available at NT-SDF.

We then merge this taxpayer-month dataset from the VAT with the CIT data for VAT taxpayers who are also corporations. Our main analysis is conducted on this dataset.