Short-Lived Effects of Electronic Cash Registers on Reported Revenue*

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Abstract

We assess the impact of a Swedish regulatory change, which required businesses with any business-to-consumer transactions, whether by cash or card, to use a certified electronic cash register (ECR), on reported revenue. To do this, we use administrative data on the monthly reported revenue of all affected firms and a staggered difference-in-differences approach. Our findings indicate that there was an immediate increase of about 2–4% in reported revenue following the implementation of a certified ECR. However, the effect was temporary and diminished to zero after just a few months. The decline in revenue can be attributed either to firms finding innovative methods to underreport their revenue or to demand decreasing in response to a price increase brought about by the ECR.

JEL classification: H25, H26, M21, O17

Keywords: tax compliance, tax enforcement, electronic cash register, regulatory change, small business

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1 Introduction

Tax administrations around the world embrace digital transformation to enhance tax compliance. A notable example is the introduction of mandatory electronic cash registers (ECR). Motivated by its potential to process and monitor transactions in cash-intensive businesses, 16 OECD countries have already made certified ECR:s mandatory by 2020.[1] Mandatory ECRs have also been raised by the European Commission as a way to curb under-reporting of value-added tax (VAT).[2] This has led to a recent initiative on "VAT in the digital age", including recommendations on mandatory ECRs (European Commission, 2022b).

Despite the widespread adoption of mandatory ECRs, there is surprisingly limited availability of reliable firm-level data regarding their impact. Recent evidence from various developing countries suggests that the impact of ECRs can vary significantly depending on factors such as the implementation process and local context (Ali et al., 2015; Awasthi and Engelschalk, 2018; Fjeldstadt et al., 2020). An exception to the lack of evidence on the impact of ECRs in developed countries is a policy report by Lovics et al. (2019), which focuses on Hungary. The report shows that the introduction of ECRs led to a significant increase in reported revenue of 23-35% in the retail, accommodation, and food service sectors.

We contribute to this literature by studying the effects on tax filing behavior of a reform in Sweden that enforced businesses that handle cash payments to use a certified ECR. Under the legislation, all transactions made at point of sale, whether by cash, debit, or credit card, were required to be recorded on a certified ECR. Before the reform, businesses were not required to use a specific type of ECR to record their revenue, nor were they obligated to issue a receipt unless specifically requested by the customer (Skatteverket, 2013).

The Swedish reform presents an interesting case for analysis due to a number of reasons. Firstly, Sweden is a country that shows high tax morale, trust in the government and generally low tax evasion. On one hand, high tax morale and trust in the government imply that businesses in Sweden may be more willing to comply with tax regulations, including the use of certified ECRs. On the other hand, low initial tax evasion may also contribute to a modest effect on reported revenue. Secondly, the reform not only facilitated the electronic storage of pertinent tax information, enhancing auditors' ability to identify instances of tax evasion, but it was also accompanied by an increase in on-site tax inspections. Indeed, many new and sometimes innovative approaches developed to support the formalization of cash transactions will have little impact on the shadow economy

[1] These countries are Austria, Belgium, Czech Republic, France, Greece, Hungary, Israel, Italy, Korea, Latvia, Lithuania, Norway, Poland, Slovak Republic, Slovenia, and Sweden (OECD, 2020).

[2] The EU wide VAT compliance gap as percent of the VAT total tax liability was estimated to 9.1% in 2020 (European Commission, 2022a).

if applied without accompanied enforcement (Casey and Castro, 2015).

Our study benefits from access to unique administrative data containing information on high-frequency (monthly) output VAT for all Swedish firms that reported the use of ECRs to the Swedish Tax Agency. Our identification strategy exploits the fact that we can track firms both before and after acquiring ECRs at different points in time, providing robust insights into the causal effect of ECRs on tax compliance.

We find that the implementation of an ECR by firms results in an immediate increase in reported revenue (inferred from VAT reports) of 2-4%. Nevertheless, the impact appears to be temporary. Within a few months, reported revenue exhibits no substantial deviation from the levels witnessed before the implementation of the ECR. This finding is consistent across several industries, including cash-intensive industries, such as restaurants, hairdressers, and small-scale retail stores. The results are robust to two distinct difference-in-differences (DiD) estimators that are typically applied in settings with staggered treatment timing: a standard two-way fixed effects (TWFE) estimator and a multi-period DiD estimator following Callaway and Sant'Anna (2021).

To establish a causal interpretation of our results, we demonstrate that the immediate increase in reported revenue subsequent to the acquisition of the ECR is preceded by a period of similar trends in reported revenue, covering a duration of up to one year. Moreover, we rule out the possibility that changes in the underlying economic conditions might have influenced the observed shift in reported revenue. To be specific, we demonstrate that there is no discernible alteration in the total wage cost of the firms, suggesting that they did not encounter significant changes in their economic environment around the time of the ECR implementation.

We cannot fully disentangle the mechanisms behind the temporary nature of the effect. We may, however, start by noting that the pattern is consistent with permanently reduced tax evasion among firms with some degree of market power. Since profit maximizing firms with market power always operate on the elastic part of the demand function, an increase in price will result in reduced revenue. The pattern we estimate – an initial increase in revenue followed be a gradual decline – is thus consistent with a reduction in hidden revenue followed by an increase in price. For a firm that evades taxes, an increase in the cost of tax evasion will have the same effect as an increase in the effective tax rate, and the profit maximizing response is to increase the price. The slow decline in revenue may thus be explained by either gradual price increases or a gradual response to an initial price increase, due to demand being more elastic in the long run. This explanation is partly corroborated by suggestive evidence that input VAT declines the months after the firm adopted an ECR.

An alternative interpretation of the temporary nature of the effect is that firms have discovered novel methods of under-reporting their tax base. In a survey conducted by the Swedish Tax Agency, which targeted business owners affected by the ECR legislation,

the majority of respondents viewed the reform as a favorable development that made it harder to evade taxes. However, some respondents pointed out that firms quickly adapted to the new regulations and found alternative ways to evade taxes (Skatteverket, 2013). This perspective is shared by employees at the tax authorities who work with auditing firms' compliance with the ECR legislation. For instance, instead of canceling recorded revenues on their previous ECRs, firms now refrained completely from registering certain revenues on the new ECR.

Our main finding of a small and significant, but short-lived, impact on reported revenue underscore the mixed evidence presented in previous studies concerning the effectiveness of improved information exchange in mitigating tax evasion, which have mainly focused on third-party reporting.[3] For instance, Slemrod et al. (2017) and Adhikari et al. (2020, 2021) demonstrate that the effect of increased third-party reporting of credit card transactions on small business tax compliance is relatively modest on average, but more significant in business-to-consumer industries and among firms with initially weak tax compliance. Slemrod et al. (2017) recommend that information reporting be targeted specifically at groups suspected of significant tax evasion and those with large shares of income subject to such reporting. Adhikari et al. (2022) discover that the increase in reported revenue following third-party reporting of credit card transactions in the United States was partially offset by increases in reported expenses. Carrillo et al. (2017) observe that most firms in Ecuador failed to respond to a threat of third-party reporting and conclude that governments face constraints in enforcement policies, even with access to information.

The remainder of the paper is structured as follows: Section 2 presents the institutional background regarding the Swedish cash-register reform. Section 3 describes the data utilized in the analysis, while Section 4 outlines the empirical approach employed. The empirical results are presented in Section 5 and Section 6 concludes.

2 The Cash Register Reform

The cash register reform that we study in this paper was implemented in 2010. Prior to the reform, businesses could use any type of cash register to keep track of their revenues and were not obliged to provide a receipt unless requested to do so by the customer. This made it relatively easy for businesses to manipulate registered revenue or not report revenue at all (Prop. 2006/07:105). As in many countries, tax evasion among Swedish businesses in industries that handle large volumes of cash, such as retail, hospitality, and hair care, was widespread and considered a threat to the legitimacy of the tax system (SOU 2005:35).

[3]By mandating specific storage conditions and requiring the issuance of receipts and reports, ECRs enhance the efficient transformation of transaction data to tax authorities.

The new legislation required businesses handling either cash or credit/debit card payments to use a certified ECR. A certified ECR should have a manufacturer declaration and a special control unit, a black box, accessible only by the staff at the Swedish Tax Agency. As part of the legislation, businesses were mandated to report their acquisition of a certified ECR to the tax authorities. However, small businesses that handle low volumes of cash were exempt from this requirement. At the time of the reform, the threshold for low cash balances was set at SEK 170,000 per year (1 USD $\approx$ 8 SEK). Certain industries were also exempt, including the taxicab industry. The vast majority of firms purchased an ECR (as opposed to leasing one) with an average investment cost of approximately SEK 23,000 (Skatteverket, 2013).

Within two years of the reform, there were around 75,000 active firms with a certified ECR.[4] Overall, the firms that acquired an ECR due to the reform correspond quite well to the specified target industries, i.e. small firms that rely heavily on cash transactions and whose customers are individual consumers with low interest in getting a receipt (Skatteverket, 2013). In the industries with the highest ECR density, between 60 and 70% of all firms have a certified ECR, including restaurants, hairdressers, and small-scale retail stores.

There were two additional elements of the cash register reform with potential implications for tax compliance. First, firms always have to print and offer the customer a receipt, independent of whether the customer asked for a receipt or not. Second, the reform gave the tax authorities enhanced audit rights, including unannounced inspections.

Within three years of the implementation of the new law, the tax authorities conducted more than 100,000 on-site inspections. The purpose of the majority of these inspections was to ensure that businesses complied with the new ECR requirements. Businesses not complying with the law were fined by SEK 10,000, and if the company once again failed to comply with the law within a year, a fine of SEK 20,000 was charged.[5] These inspections were proportionally distributed across sectors depending on the sector's share of the total number of ECRs. A recent study by Swedish Tax Agency does not show any signs that these inspections lead to improved compliance with regulations (Skatteverket, 2022). We therefore do not anticipate that the potential effects of ECRs will be confounded by the impact of these audits.

Due to the high demand for ECRs at the time of the reform, delivery times were often long and unpredictable for firms. For this reason, inspected firms without an ECR were not fined if they could verify that they had ordered an ECR. From July 2010, the authorities fined all non-complying firms. In 2010, 500 fines were implemented compared

[4]The legislators expected that around 110,000 – 130,000 firms would be affected by the ECR requirement. Around 90,000 firms had reported an ECR by 2012 and 5,000 firms had been granted an exemption (Skatteverket, 2013). The remaining firms had either failed to unsubscribe their ECR upon closure or transfer of their business, or failed to report an ECR at all.

[5]The penalty currently amounts to SEK 12,500 and 25,000, respectively.

to 2,900 fines in 2012. Initially, the Tax Agency mainly fined firms for not having a certified ECR. Later on, fines tilted more towards reporting errors, such as not registering revenues or providing receipts (Skatteverket, 2013).

3 Data

3.1 Data sources and sample restrictions

We use administrative data from the Swedish Tax Agency, which encompasses all firms operating in Sweden that reported an ECR between September 2009 and March 2013, amounting to a total of 94,470 firms. By leveraging unique firm identifiers, we merge the ECR data with periodic VAT data, which is available between January 2008 and March 2013.

Out of the total population of 94,470 firms with a certified ECR, we successfully match VAT records for 75,530 firms. The remaining firms either closed down or sold their business without de-registering their ECR, or failed to become active to the extent of reporting VAT (Skatteverket, 2013). Moreover, we supplement the analysis with firm-level information on total monthly wage payments, which we obtain solely for those firms with at least one employee. Such firms account for about half of the total number of firms that have an ECR and report VAT.

In order to arrive at our analysis sample, we apply four sample restrictions. First, we narrow our focus to firms that acquired an ECR between October 2009 and December 2010. This period covers the vast majority of ECRs in the data; after that, the number of registered ECRs at the monthly level become very small. This first restriction reduces the sample size to 53,326 firms. Second, we limit the sample to firms that report VAT on a monthly basis during the event window to ensure high data frequency for our event study-based empirical approach, resulting in a further reduction to 35,169 firms. Third, we restrict the sample to firms that register one ECR only, leaving us with 31,277 firms. Finally, we further narrow down our sample by selecting firms that consistently report positive VAT 12 months before and after the acquisition of the ECR. This criterion helps us avoiding the risk of attributing changes in VAT solely to the timing of business re-starts or expansions, rather than the actual impact of the cash register reform.

After applying these additional sample restrictions, we end up with a final analysis sample of 19,427 firms and 485,023 firm-month observations. We also perform several robustness tests to verify the sensitivity of our findings to variations in the sample specification. Section 5.3 presents the results of these tests, which generally support the qualitative and quantitative implications of our main results. Table A.1 in the Appendix reports the sample size as well as summary statistics for reported revenue after imposing each sample restriction.

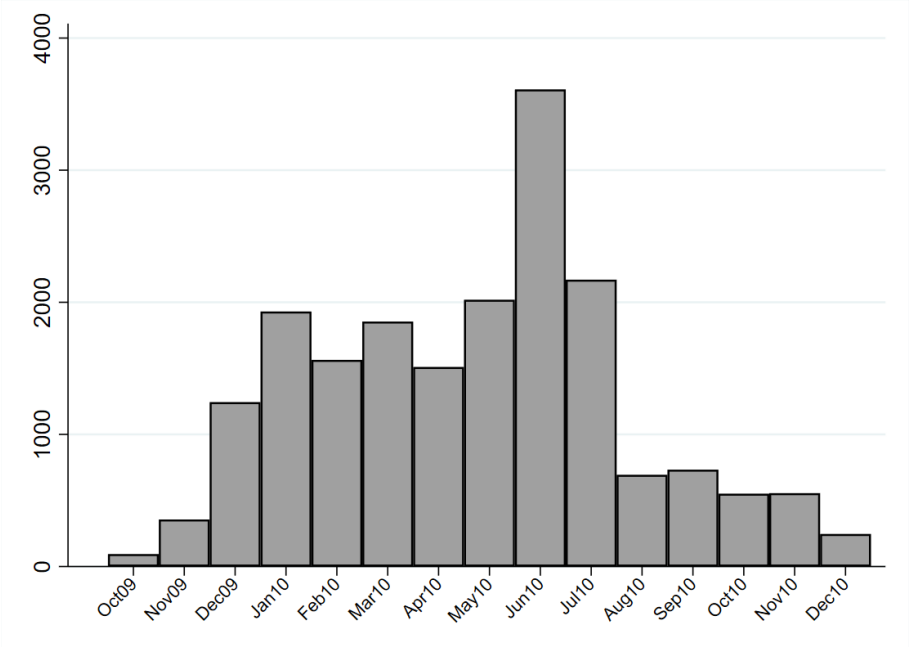
3.2 Key variables and descriptive statistics

The primary dependent variable under examination is the revenue reported by firms, which refers to the total value of products (goods and services) sold within a specified time frame. Monthly VAT records are used to calculate reported revenue. Sweden implements three different VAT tax rates: 25%, 12%, and 6%. To compute revenue (excluding VAT) for a firm i in period t, we employ the formula $y_{i,t} = \frac{\text{VAT25}_{i,t}}{0.25} + \frac{\text{VAT12}_{i,t}}{0.12} + \frac{\text{VAT6}_{i,t}}{0.06}$.[6] Additionally, in Subsections 5.4 and 5.5, respectively, we use the total sum of wages and reported input VAT of a company as dependent variables.

The independent variable of interest is the event when a business registers and begins using a certified ECR. Our identification approach, which we discuss in more detail in Section 4, leverages the fact that companies adopted ECRs at various points in time, and we have access to reported revenue data both before and after this event.

Figure 1 depicts the distribution of months during which the Swedish Tax Agency received ECR reports in 2010.[7] We have included firms that reported an ECR in late 2009, a few months before the new legislation came into effect. Anticipating the new legislation that had already been announced, these firms likely procured an ECR and registered it accordingly. The peak in the number of registrations occurred in June 2010, which coincided with the tax authorities' announcement of adopting a stricter stance during their control visits.

Figure 1: Distribution of ECR acquisition over time



[6]The VAT tax rates remained unaltered during the study's time period.

[7]The ECR distribution of our analysis sample is similar to that of the unregulated sample comprising 75,530 firms.

Table 1: Descriptive statistics

Dependent variables	**Mean**	**SD**
Reported revenue	1,189,702	19,359,913
Output VAT	249,865	4,097,668
Input VAT	221,268	3,695,475
Total wage costs	628,800	1,3872,703
Number of firms	19,427	

Notes: The dependent variables are measured on a monthly basis and reported in Swedish Krona (SEK) at the 2010 price level (1 USD $\approx$ SEK 8).

Table 1 presents the descriptive statistics of our analysis sample. On average, the monthly output VAT and corresponding revenue are approximately SEK 250,000 and SEK 1.2 million, respectively. The input VAT averages around SEK 221,000. Revenues vary widely across firms, and to address the significant left-skew of the revenue distribution (see Figure A.1 in Appendix), we use a log transformation of the revenue variable. For the subset of firms with at least one employee, the average monthly wage bill is nearly SEK 628,000.

The diversity in firm size is reflective of the various sectors we examine in this study. In our analysis of heterogeneity (see Subsection 5.2), we examine four sub-groups of industries. These industries are distributed as follows: restaurants comprise 21% of all firms, hairdressers 19%, food 8%, and retail 33%. The remaining firms fall into the "Others" category.[8]

It is worth noting that hairdressers and restaurants make up 40% of the sample. These types of businesses are often small in scale, with average monthly revenue of less than SEK 100,000 and SEK 400,000, respectively.

4 Empirical Framework

We use a staggered difference-in-differences (DiD) approach to estimate the causal impact of ECR adoption on reported revenue among Swedish firms. The staggered treatment timing comes from the time variation in the adoption of ECRs across firms, following the new legislation in 2010 that enforced firms that handle cash payments to use a certified ECR. Our models are estimated using monthly VAT data at the firm level, with a symmetric window of 25 months around the treatment month.

Two-way Fixed Effects: We first evaluate the impact of ECR adoption using a standard TWFE model. We estimate both a static model and an event-study model that

[8]The sub-industries are based on the following NACE codes: Restaurants: 56, 55101; Hairdressers: 96012, 9602, 9604, 9609, 96021; Food: 463, 471, 472; Retail: 45 except 451, 46 except 463 and 468, 47 except 471, 472.

allows for dynamic treatment effects. In the static TWFE model, we estimate a single indicator measuring the average effect of implementing an ECR. The model specification takes the following form:

$$y_{i,t} = \alpha + \nu_i + \delta_t + \lambda D_{i,t,l} + \epsilon_{i,t} \tag{1}$$

The dependent variable, $y_{i,t}$, is the log of reported revenue of firm i at month t. Firm-fixed effects are captured by ν_i and month-fixed-effects by δ_t. The indicator $D_{i,t,l} \in \{0, 1\}$ captures the treatment status of firm i at month t being l month away from the first treatment. If a firm i has an ECR in month t it is treated and $D_{i,t,l} = 1$; otherwise it is $D_{i,t,l} = 0$. The first month of treatment is captured by $l = 0$. All firms are treated at some point between September 2009 and December 2010 and treatment is absorbing, i.e., $D_{i,t,l} = 1$ for all $l \geq 0$. Our main interest lies in the parameter λ which captures the change in reported revenue after the introduction of the ECR. The parameter λ captures, in general, a weighted average of treatment effects.

The event-study version of the TWFE estimator allows for dynamic treatment effects. The purpose of this model is to examine whether the potential changes in reported revenue that follow from the adoption of an ECR are permanent or temporary. We estimate the following equation:

$$y_{i,t} = \nu_i + \delta_t + \sum_{\substack{l=-12 \\ l \neq -1}}^{12} \lambda_l D_{i,t,l} + \epsilon_{i,t} \tag{2}$$

Again, $y_{i,t}$ denotes log reported revenue of firm i at month t, the indicator $D_{i,t,l} \in \{0, 1\}$ captures the treatment status of firm i at month t being l month away from the first treatment, and ν_i and δ_t denote firm- and month-fixed effects, respectively. The coefficients of interest are the λ_l that capture monthly reported revenue between 12 months before and 12 months after the adoption of an ECR. The month prior to the ECR, $l = -1$, is excluded so that the estimate for each month is relative to the revenue level in that month.

Multi-period difference-in-differences: Recent econometric work raises concerns about the causal interpretation of two-way fixed effects parameters when there is treatment effect heterogeneity. Such concerns apply both to static TWFE estimators and dynamic event study models (Sun and Abraham, 2021). We therefore estimate the average treatment effect on the treated using the alternative estimator proposed by Callaway and Sant'Anna (2021). We measure group-time ATTs comparing the expected change in the log reported monthly revenue of firms that receive the ECR in a specific month with the respective change for firms that are not-yet treated, i.e., have not acquired an ECR until then.

The group-time ATTs are then aggregated into a single parameter measuring the average treatment effect on the treated. More precisely, we implement the doubly robust

DiD estimator based on inverse probability weighting and OLS (Callaway and Sant'Anna, 2021). This parameter is comparable with λ in the TWFE estimation in Eq. (1) but accounts for heterogeneity of treatment effects across different groups (e.g., Goodman-Bacon, 2021; Baker et al., 2022). Similar to the TWFE estimation, we are interested in understanding the dynamic group-time ATT, which examines the heterogeneity of treatment effects in relation to the duration of exposure to treatment.

Given our sample restrictions, we cannot estimate treatment effects for the 11th and 12th months after the implementation of the ECR when we use the multi-period DiD model because there are too few observations in the control group of firms that have not implemented an ECR yet. Therefore, we will estimate the effects for the 10 months following the introduction of the ECR. In addition to estimating the effects for the 10 months following the introduction of the ECR, we also estimate the pre-treatment effects for the 10 months preceding the treatment.

5 Empirical Results

5.1 Effect on Reported Revenue

Table 2 shows our main findings. Columns (1) and (2) present the outcomes of our analysis using the static and dynamic versions of the TWFE model, respectively. Columns (3) and (4) illustrate static and dynamic group-time effects using the multi-period DiD model. In the dynamic models, we have reported ten coefficients on each side of the treatment month.[9]

The static TWFE estimates indicate a significant increase of approximately 3.6% in reported revenue after the introduction of a certified ECR. In Column (3), the group-time ATT from the multi-period DiD model shows a slightly smaller increase of 2.1%.

When considering the dynamic effects, both models demonstrate a positive effect on reported revenue in the four months after the ECR acquisition. However, after five months, the effect becomes insignificant and approaches zero. Towards the end of the observation period, the models produce slightly different outcomes. While the TWFE estimates remain consistently close to zero and are estimated with good precision, the point estimates of the multi-period DiD show an increase towards the end of the post-treatment period. The estimates for the multi-period DiD are, however, imprecise in later periods as there are increasingly fewer not-yet-treated firms that can act as a control.

To examine pre-trends, Figure 2 displays the event studies for the TWFE and the multi-period DiD 10 months before and after the first ECR acquisition. The outcomes from both models indicate parallel pre-trends, where most pre-treatment estimates are insignificant and close to zero.

[9]For comparison reasons, we also only report coefficients for t=-10 to t=10 for the TWFE estimation.

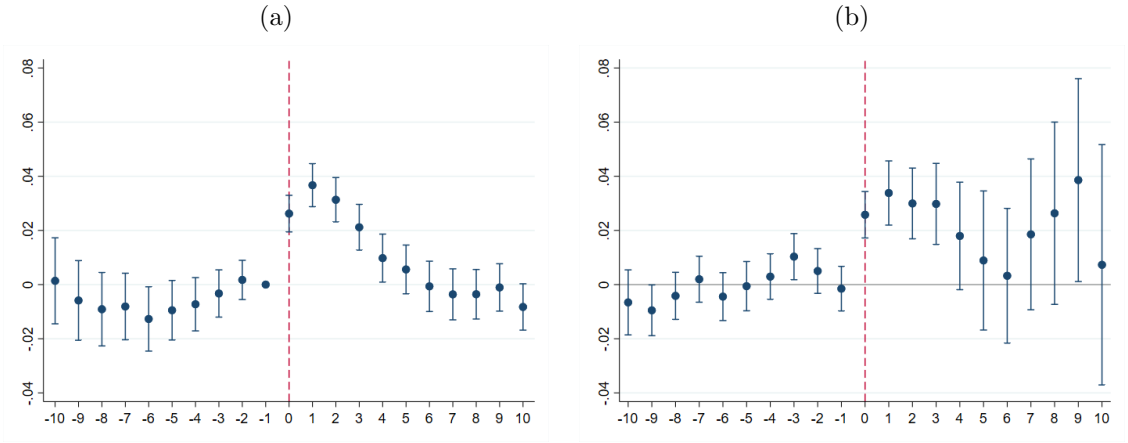
Table 2: Effect of ECR acquisition on reported revenue

	(1) TWFE	(2) TWFE	(3) DiD	(4) DiD
Static TWFE	0.0362***			
	(9.63)			
Group-time ATT			0.0218**	
			(2.37)	
t=-10		0.0013		-0.0065
		(0.17)		(-1.07)
t=-9		-0.0058		-0.0094**
		(-0.78)		(-1.99)
t=-8		-0.0091*		-0.0041
		(-1.32)		(-0.94)
t=-7		-0.0080*		0.0019
		(-1.29)		(0.46)
t=-6		-0.0126**		-0.0044
		(-2.09)		(-0.98)
t=-5		-0.0094*		-0.0005
		(-1.69)		(-0.13)
t=-4		-0.0072		0.0029
		(-1.45)		(0.69)
t=-3		-0.0032		0.0103**
		(-0.74)		(2.37)
t=-2		0.0017		0.0050
		(0.47)		(1.19)
t=-1				-0.0015
				(-0.36)
t=0		0.0262***		0.0257***
		(7.60)		(5.90)
t=1		0.0367***		0.0338***
		(9.06)		(5.59)
t=2		0.0313***		0.0299***
		(7.50)		(4.50)
t=3		0.0211***		0.0297***
		(4.91)		(3.90)
t=4		0.0097**		0.0179*
		(2.16)		(1.77)
t=5		0.0055		0.0088
		(1.22)		(0.68)
t=6		-0.0006		0.0032
		(-0.14)		(0.26)
t=7		-0.0036		0.0185
		(-0.75)		(1.31)
t=8		-0.0035		0.0263
		(-0.77)		(1.53)
t=9		-0.0010		0.0385**
		(-0.24)		(2.02)
t=10		-0.0082*		0.0073
		(-1.89)		(0.32)
N	485 023	485 023	371 647	371 647

Notes: Dependent variable: log reported revenue; t/z-statistics in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Is the increase in reported revenue due to a modification in the pricing strategy, in reported revenue quantity, or a combination of both? Although our data does not allow us to distinguish between sales and price adjustments, if a firm has some degree of control over the price, it may be in the firm's interest to raise prices in response to the introduction of

Figure 2: Event studies: Effects on reported revenue



Notes: The figures depict the event study of log revenue 10 month before and after treatment. (a) shows the event study for the TWFE estimation; (b) shows the event study for the multi-period DiD.

ECRs. For tax evading firms, the ECR implies an increase in variable costs and therefore reduces the profit margin per unit sold. Consequently, a firm that engages in tax evasion may choose to increase prices to offset the decrease in profit margin. An initial increase in revenue followed be a gradual decline is thus consistent with a reduction in hidden revenue followed by an increase in price. That we observe a slow decline in revenue may be explained by either gradual price increases or a gradual response to an initial price increase, due to customers responding more elastic to price changes in the long run.

5.2 Industry Heterogeneity

To examine whether the effects that we find in the previous section masks heterogeneous effects across firms in different sectors, we repeat the analysis for firms in various industries. We focus on the multi-period DiD estimates and relegate TWFE estimates to the Appendix.

Table 3 reports the aggregated group-time ATT separately for restaurants, hairdressers, wholesale of food, retail sale, and all other firms. The estimates for the three first categories are insignificant and close to zero. Retailers and firms in the category "Others" show a positive and significant effect of around 4%. For the complete dynamic group-time ATTs and the event studies we refer to Table A.2 and Figure A.2 in the Appendix. We do not find any evidence of a particular trend in reported revenue prior to the adoption of ECRs in any of the industries. Additionally, we observe a discontinuous increase in reported revenue after the treatment. The temporary positive effect on reported revenue that we previously observed is primarily driven by restaurants and hairdressers. While the positive effects on reported revenue for retailers seem to persist, the initial increase observed for the other industries diminishes after a few months.

The dynamic TWFE estimates also reported in Table A.2 in the Appendix show

Table 3: Sector-specific effects of ECR acquisition on reported revenue

	Restaurants	Hairdressers	Food	Retailers	Others
Group-time ATT	0.0138	-0.0032	-0.0042	0.0417***	0.0468*
	(0.58)	(-0.15)	(-0.25)	(3.00)	(1.85)
N	74 461	72 453	27 656	123 169	73 908

Notes: Dependent variable: log reported revenue; t/z-statistics in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

somewhat different results. Firstly, the average effect is positive and significant for all industries, not just for retailers. In fact, the point estimates for restaurants and hairdressers are twice as high as that for retailers. Secondly, the effects are temporary across all sectors.

5.3 Robustness Checks

We present three sets of results in which we assess the sensitivity of our main findings to the sample selection. We report the corresponding group-time ATT estimates in Table 4 while the dynamic estimates are shown in Table A.3 in the Appendix.

First, we include firms that have introduced more than one ECR. Second, we exclude firms for which we either observe missing, zero or negative VAT reports at some point during their reporting history (outside the 25-month window). The purpose of this restriction is to focus solely on companies that have demonstrated a consistent revenue performance both prior to and following the acquisition of the ECRr. Third, despite utilizing the logarithm of reported revenue as the output variable in our primary estimation, the possibility remains that outliers may be influencing our results. Therefore, we conduct a robustness check by removing the top and bottom percentiles of firms, based on their average reported revenue over the observed 25-month period.

The results of these robustness tests produce point estimates ranging from 2.7 to 3.2%, which are slightly higher than the main estimate presented in Table 2. Furthermore, the dynamic estimates suggest a short-term increase in reported revenue immediately following the acquisition of the ECR, followed by a decline in subsequent months. However, while the coefficients for later months were only marginally significant in the main estimation, they exhibit slightly higher values and statistically significant differences from zero in these robustness checks.

As previously pointed out, if firms expand their business in conjunction with acquiring an ECR, we may attribute pure timing effects to causal effects of ECR acquisition on reported revenue. As firms have the autonomy to decide when to purchase the ECR, such decisions may coincide with other crucial events for the firm, such as business expansion, annual bookkeeping efforts, or high revenue peaks. Such patterns could also explain the temporary nature of the effect that we observe. Although our model incorporates general time effects and seasonal patterns, it is possible that we cannot fully account for such

Table 4: Robustness checks: Effects of ECR acquisition on reported revenue

	(1) One or more ECRs	(2) Positive VAT	(3) Excluding outliers	(4) ECR during intensive period
Group-time ATT	0.0273***	0.0321***	0.0276***	
	(3.21)	(3.77)	(3.19)	
Static TWFE				0.0220***
				(3.21)
N	419 307	381 646	412 591	234 700

Notes: (1) firms that have acquired one or more ECRs, (2) firms that have reported positive VAT during all the months that we have observed them, (3) sample where outliers have been dropped, and (4) firms that have acquired an ECR between May and July 2010; t/z-statistics in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

firm-specific timing effects.

To strengthen the causal interpretation and rule out this mechanism, we utilize the fact that ECR adoption should be more exogenous to the firm's operations or expansions around the time when the tax authorities initiated a "strict approach" on inspections, and the delivery times for ECRs were long and uncertain. Between May and July 2010, the adoption of ECRs is more likely driven by suppliers catching up with excess demand and the tax authorities conducting stricter audits, as opposed to a demand-driven expansion on behalf of the firms.

The fourth column of Table 4 displays the average treatment effect derived from the TWFE model when the sample is limited to firms that acquired an ECR between May and July 2010. The dynamic estimates can be found in the fourth column of Table A.3 in the Appendix. Due to the constrained treatment period, a multi-period model is not feasible. The average treatment effect is found to be significant, amounting to 2.2%, although slightly lower than the main estimate of 3.6%. Similarly, the dynamic estimates also indicate a comparable pattern, with a brief increase in reported revenue, followed by a gradual decrease towards a null effect.

5.4 Effect on Total Wage Costs

We have found that the implementation of an ECR by firms results in a temporary increase in reported revenue. A natural follow-up question arises: Can this increase be attributed to companies increasing their tax compliance? Or is it possible that the implementation of ECRs coincide with larger shifts in the overall economic climate that impact firms' revenue? Firstly, since firms adopt ECRs at different points in time, it is improbable that the effect is caused by a broad-based change in the overall economic environment. Secondly, the econometric model takes into account general time effects, indicating that any observed effect is not simply due to changes in the time period studied.

To further address this concern, we investigate the impact of ECRs on total wage costs of firms. In contrast to changes in the economic environment, increases in detection

Table 5: Effect of ECR acquisition on reported input VAT and total wage costs

	Total sum of wages	Input VAT
	(1)	(2)
Group-time ATT	-0.0037	-0.0143
	(-0.40)	(-1.35)
N	205 746	370 565

Notes: Dependent variable in (1) log total wage costs; in (2) log input VAT; z-statistics in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

probabilities or fines should not affect firms' optimal production decisions. If the implementation of an ECR is combined with an additional change in the economic environment, we should expect changes in firms' output choices and input decisions. In contrast, if the reform increases tax compliance among firms, we should not expect any real changes in business activities. We therefore investigate the impact of ECRs on firms' total wage costs.

We re-estimate the static and dynamic group-time ATTs with the log of the total wage costs as the dependent variable. Since not all firms in our analysis sample have employees or report wages on a monthly basis, this restricts our sample size to approximately half of the firms.[10]

The results reveal that the adoption of ECRs has no significant effect on firms' total wage cost, with the estimated coefficient being close to zero and insignificant (see the group-time ATT in Table 5).

Additionally, our dynamic event study estimates (as shown in panel (a) of Figure 3) indicate insignificant effects in all post-treatment periods. Table A.4 in the Appendix provides complete dynamic effects for the TWFE and the multi-period DiD. Given that changes in the economic environment are unlikely to be the primary driver behind the temporary increase in reported revenue, the adjustment needs to occur at the firm level.

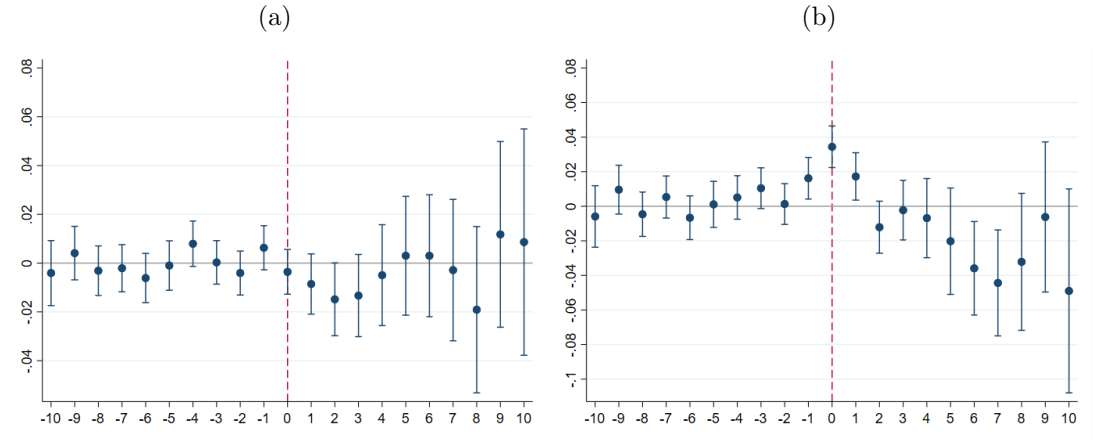
5.5 Effect on Input VAT

We proceed to estimate the impact of acquiring an ECR on input VAT. Unlike reported revenue (output VAT), the projections for input VAT are not as straightforward. This is because ECRs are not directly employed by firms in the procurement of input factors.

An alteration in VAT during the ECR acquisition could be interpreted in several ways. Firstly, it could suggest the existence of timing effects, i.e., that firms may acquire an ECR in conjunction with a general expansion of their business. Secondly, it could signify that firms reporting a larger proportion of their actual revenue after obtaining an ECR may compensate by reporting additional input VAT. Small businesses can engage in tax evasion on either side of the revenue function. To reduce their tax liability, firms may

[10]It is worth noting that our primary results for reported revenue remain unchanged even when we restrict the analysis to the sample of firms that report wages. Results are available on request.

Figure 3: Event studies: Effects on log total wage costs and log input VAT



Notes: The figures depict the event study of (a) log total wage costs and (b) log input VAT 10 month before and after treatment. Results according to multi-period DiD estimations.

either under-report output VAT, i.e. revenue, or over-report input VAT, as observed in prior research (see, e.g., Waseem, 2023). Additionally, firms may have a vested interest in maintaining a consistent ratio between input and output VAT, as any discrepancies could trigger suspicion from the tax authorities (see, e.g., Matthews and Lloyd-Wiliams, 2001). Thirdly, it could encompass the direct cost of buying the ECR.

We report estimates from the static and dynamic multi-period model after replacing the dependent variable with the log of input VAT.[11] The group-time ATT is shown in Table 5 while the dynamic estimates are shown in panel (b) in Figure 3. The group-time ATT shows an average negative impact on input VAT of 1.4%. The dynamic estimates reveal an initial increase of approximately 2% in the treatment month, followed by an immediate decline in the effect, which is statistically insignificant in most post-treatment months (see Table A.4 in the Appendix).

Given the average input VAT of SEK 221,000 (as reported in Table 1), the estimated 3.4% effect translates to an increase of around SEK 7,500 in input VAT. Assuming a VAT rate of 25%, this suggests an additional expense of SEK 30,000, which is only slightly higher than the typical cost of an ECR during the period of our study. Although we cannot determine which of the three aforementioned interpretations holds the most weight, we lean towards the view that the observed pattern is primarily driven by the additional VAT expense incurred due to the ECR acquisition.

[11] We have access to the monthly input VAT records for each firm. However, unlike output VAT, we do not possess this data by VAT rate, and thus cannot convert input VAT into real expenditures for input goods.

6 Discussion and Conclusions

We have shown that the introduction of mandatory ECRs in Sweden resulted in a short-term increase in reported revenue. We ascribe this response to companies anticipating greater costs associated with tax evasion. In our case, the expected costs can depend on multiple factors, such as the likelihood of being caught for tax evasion, the severity of the penalties imposed, or the efficacy of the monitoring mechanisms in place. According to survey data, most business owners believe that the tax authorities were able to monitor firms more efficiently due to the new legislation, which is a view that the tax authorities themselves endorse. Additional evidence supporting the claim that the reform enhanced tax compliance can be observed in an evaluation of the extent to which firms had to amend their revenues following a review by an auditor. The evaluation indicated that the audits conducted subsequent to the reform resulted in relatively minor increments in the reported amounts in comparison to those recorded prior to the reform (Skatteverket, 2013).

Our findings offer valuable insights for policymakers in other developed countries. The reform examined in this paper took place in 2010, during period when cash transactions were still relatively prevalent in Sweden, accounting for 39% of all point-of-sale transactions (Riksbanken, 2022). This means that our study captures a time period in Sweden when cash was still widely used and before the substantial decline in cash usage that has been observed in more recent years. Cash usage in European countries has been declining similar to the trend observed in Sweden, with an increasing number of transactions being conducted electronically. However, cash remained the most frequently used method for payments at the point of sale in the Euro Area (ECB, 2022). Furthermore, it is important to note that the cash register legislation in Sweden extends beyond cash payments and also covers other forms of payment such as credit card transactions.

In international comparison, Sweden belongs to a group of countries known for their higher tax morale, trust in the government, and relatively low incidents of tax evasion. Indeed, countries that mandate ECRs typically grapple with more substantial VAT tax discrepancies compared to Sweden (see Table A.5). Among the countries that have implemented mandatory ECRs, Hungary presents a stark contrast to Sweden, having one of the largest tax gaps. Variations in tax gaps and underlying institutional features could account for the pronounced differences between our findings and those documented in the policy report by Lovics et al. (2019) on Hungary's adoption of ECRs. It is, however, noteworthy that the reduction in the VAT tax gap over the last decade has not been markedly more pronounced in countries that have adopted mandatory ECRs compared to those that have not, as indicated in Table A.5.

To ensure effective tax compliance, policymakers should adopt a comprehensive approach that goes beyond the introduction of ECRs. Many new and sometimes innovative

approaches developed to support the formalization of point-of-sale transactions will have little impact on the shadow economy if applied without accompanied enforcement (Casey and Castro, 2015). Such complementary measures could include enduring tax inspections, third-party reporting, and improved information sharing between government agencies. In addition, requiring businesses to use certified ECRs, tax authorities can better monitor all transactions, regardless of the payment method used, and detect instances of non-compliance. Finally, to gain a more comprehensive insight into the influence of ECRs and other point-of-sale systems on tax compliance in developed nations, evidence from a broader range of countries is needed.

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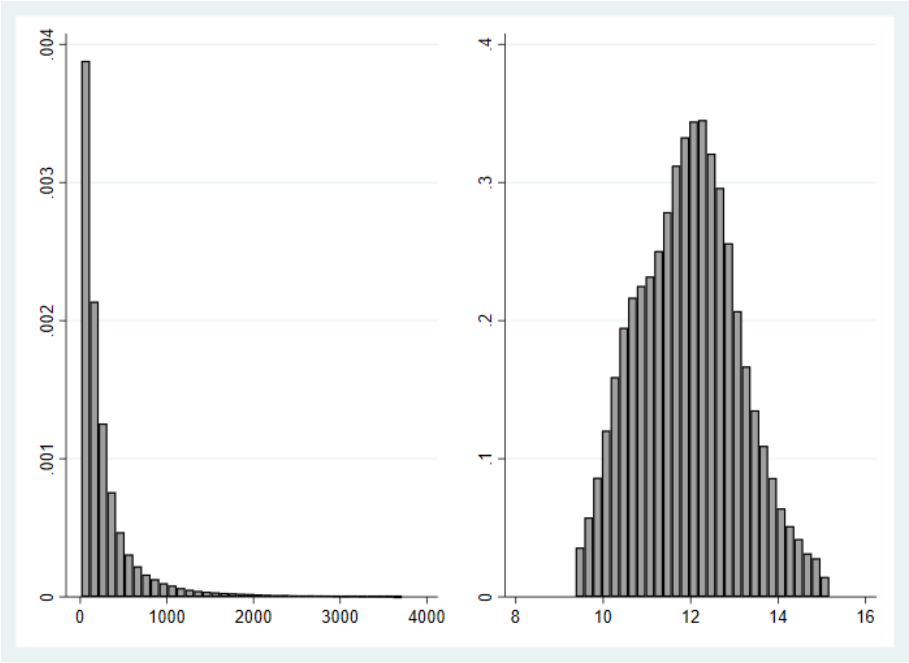
Appendix

Table A.1: Sample size and reported revenue by sample restriction

	Number of firms	Observations	Reported revenue (in million SEK)			
			Mean	SD	Min	Max
All firms with ECR and VAT records	75,530	1,155,403	1.24	21.02	-275	2989
Acquired a ECR in 2009 or 2010	53,326	911,967	1.11	16.3	-275	2112
Monthly VAT reporting	35,169	770,118	1.13	17.74	-275	2112
At most one ECR	31,277	681,542	0.95	17.01	-275	2112
Consecutive and positive revenue in event window	19,427	485,033	1.18	19.67	0.00	2112
Report wages	10,420	278,457	1.74	24.5	0.00	2112

Figure A.1: Distribution of monthly reported revenue (in SEK).



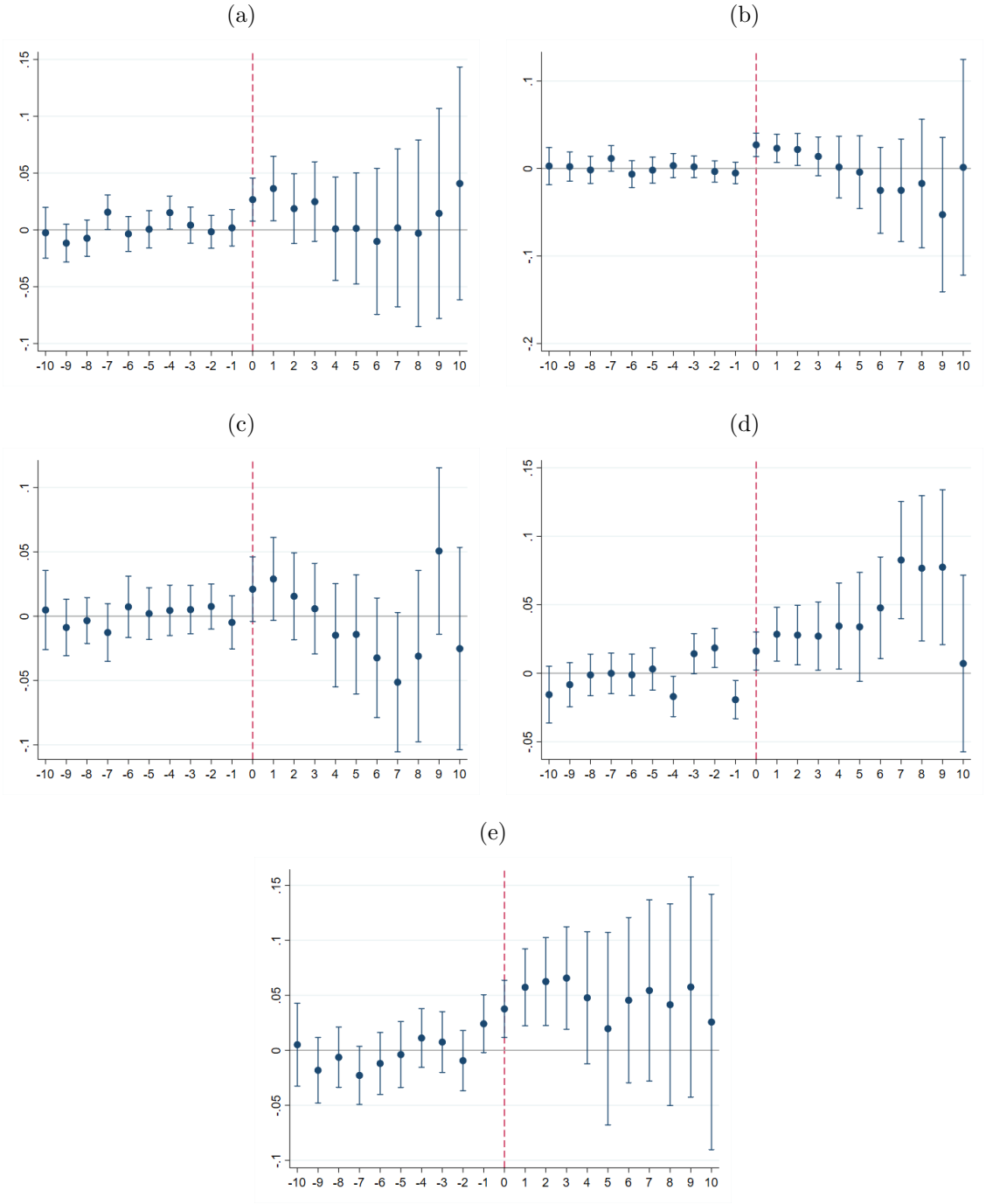
Note: Monthly reported revenue in SEK, excluding bottom and top 5%for illustration purposes. Reported revenue in 1000s of SEK to the left. Log revenue to the right.

Table A.2: Sector-specific dynamic TWFE and group-time ATTs of ECR acquisition on reported revenue

	Restaurants		Hairdressers		Food		Retailers		Others	
	TWFE	DiD	TWFE	DiD	TWFE	DiD	TWFE	DiD	TWFE	DiD
Static	0.0370***		0.0326***		0.0209**		0.0174***		0.0589***	
TWFE	(4.66)		(6.66)		(2.06)		(2.67)		(5.07)	
Group-time		0.0138		-0.0032		-0.0042		0.0417***		0.0468*
ATT		(0.58)		(-0.15)		(-0.25)		(3.00)		(1.85)
t=0	0.0280***	0.0226***	0.0337***	0.0269***	0.0194**	0.0209	0.0140**	0.0161**	0.0319***	0.0376***
	(4.02)	(2.76)	(6.22)	(3.97)	(2.00)	(1.64)	(2.51)	(2.28)	(2.94)	(2.84)
t=1	0.0379***	0.0364**	0.0319***	0.0230***	0.0252**	0.0289*	0.0301***	0.0284***	0.0373***	0.0572***
	(4.47)	(2.51)	(5.54)	(2.80)	(2.30)	(1.76)	(4.34)	(2.84)	(3.03)	(3.21)
t=2	0.0232***	0.0186	0.0382***	0.0217**	0.0136	0.0154	0.0238***	0.0278**	0.0562***	0.0625***
	(2.68)	(1.19)	(6.40)	(2.33)	(1.26)	(0.90)	(3.28)	(2.51)	(4.41)	(3.06)
t=3	0.0185**	0.0247	0.0290***	0.0137	0.0044	0.0058	0.0143*	0.0270**	0.0550***	0.0656***
	(2.17)	(1.39)	(4.52)	(1.22)	(0.41)	(0.32)	(1.84)	(2.13)	(4.27)	(2.76)
t=4	0.0052	0.0009	0.0244***	0.0015	0.0020	-0.0148	0.0092	0.0344**	0.0308**	0.0477
	(0.59)	(0.04)	(3.50)	(0.09)	(0.18)	(-0.72)	(1.12)	(2.15)	(2.31)	(1.56)
t=5	0.0097	0.0012	0.0161**	-0.0042	-0.0082	-0.0141	0.0119	0.0338*	0.0138	0.0196
	(1.11)	(0.05)	(2.31)	(-0.20)	(-0.71)	(-0.60)	(1.38)	(1.67)	(1.03)	(0.44)
t=6	-0.0072	-0.0101	0.0303***	-0.0250	-0.0194*	-0.0324	0.0091	0.0476**	0.0072	0.0454
	(-0.78)	(-0.31)	(4.35)	(-1.00)	(-1.72)	(-1.37)	(1.06)	(2.52)	(0.51)	(1.19)
t=7	0.0003	0.0017	0.0153**	-0.0249	-0.0241*	-0.0513*	0.0089	0.0826***	-0.0022	0.0543
	(0.04)	(0.05)	(2.21)	(-0.84)	(-1.96)	(-1.86)	(1.04)	(3.79)	(-0.16)	(1.29)
t=8	-0.0004	-0.0029	0.0163**	-0.0171	-0.0021	-0.0310	-0.0023	0.0766***	0.0006	0.0414
	(-0.05)	(-0.07)	(2.41)	(-0.46)	(-0.17)	(-0.91)	(-0.28)	(2.83)	(0.05)	(0.89)
t=9	0.0022	0.0144*	0.0058	-0.0527	0.0067	0.0506	0.0083	0.0774***	-0.0068	0.0574
	(0.24)	(0.31)	(0.89)	(-1.17)	(0.54)	(1.54)	(1.05)	(2.68)	(-0.52)	(1.13)
t=10	-0.0097	0.0408	0.0018	0.0012	-0.0013	-0.0252	0.0090	0.0070	-0.0335**	0.0256
	(-1.05)	(0.78)	(0.30)	(0.02)	(-0.12)	(-0.63)	(1.21)	(0.22)	(-2.51)	(0.43)
N	101 020	74 461	91 001	72 453	36 932	27 656	160 277	123 169	95 793	73 908

Notes: Dependent variable: log reported revenue; t-statistics/z-statistics in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; post-treatment effects for 10 month after acquisition of the first ECR

Figure A.2: Event studies for the multi-period DiD by industry



Notes: The figures depict the event study of log reported revenue 10 month before and after treatment for (a) restaurants, (b) hairdressers, (c) the food sector, (d) retailers, and (e) others. Results according to multi-period DiD estimations.

Table A.3: Robustness checks: Effect of ECR acquisition on reported revenue

	(1) One or more ECRs DiD	(2) Positive VAT DiD	(3) Excluding outliers DiD	(4) ECR during intensive period TWFE
Group-time ATT	0.0273***	0.0321***	0.0276***	
	(3.21)	(3.77)	(3.19)	
Static TWFE				0.0220***
				(3.21)
t=0	0.0276***	0.0236***	0.0273***	0.0245***
	(6.67)	(5.75)	(6.55)	(4.33)
t=1	0.0381***	0.0343***	0.0380***	0.0320***
	(6.58)	(5.98)	(6.48)	(3.02)
t=2	0.0361***	0.0371***	0.0368***	0.0223
	(5.67)	(5.85)	(5.70)	(1.43)
t=3	0.0350***	0.0385***	0.0347***	0.0179
	(4.82)	(5.34)	(4.72)	(1.04)
t=4	0.0199**	0.0275***	0.0196**	0.0150
	(2.04)	(2.93)	(1.98)	(0.88)
t=5	0.0161	0.0314***	0.0161	0.0258
	(1.28)	(2.67)	(1.26)	(1.61)
t=6	0.0110	0.0217*	0.0105	0.0275*
	(0.89)	(1.76)	(0.84)	(1.79)
t=7	0.0275**	0.0344**	0.0281**	0.0165
	(2.03)	(2.54)	(2.04)	(1.15)
t=8	0.0389**	0.0471***	0.0382**	0.0060
	(2.44)	(2.89)	(2.36)	(0.48)
t=9	0.0476***	0.0515***	0.0485***	0.0102
	(2.73)	(2.92)	(2.74)	(0.90)
t=10	0.0030	0.0060	0.0053	0.0130
	(0.15)	(0.30)	(0.26)	(1.36)
N	419 307	381 646	412 591	234 700

Notes: (1) firms that have acquired one or more ECRs, (2)firms that have reported positive VAT during all the months that we have observed them, (3) sample where outliers have been dropped, and (4) firms that have acquired an ECR between May and July 2010; (1)-(3) shows multi-period DiD estimations, (4) shows dynamic TWFE estimation; t/z-statistics in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.4: Effect of ECR acquisition on total wage costs and reported input VAT

	Total sum of wages		Input VAT	
	TWFE	DiD	TWFE	DiD
	(1)	(2)	(3)	(4)
Static TWFE	0.0001		0.0432***	
	(0.03)		(11.22)	
Group-time ATT		-0.0037		-0.0143
		(-0.40)		(-1.35)
t=0	-0.0005	-0.0035	0.0461***	0.0344***
	(-0.14)	(-0.77)	(9.20)	(5.61)
t=1	-0.0056	-0.0085	0.0343***	0.0172**
	(-1.22)	(-1.36)	(6.61)	(2.47)
t=2	-0.0074	-0.0148*	0.0075	-0.0121
	(-1.53)	(-1.95)	(1.48)	(-1.58)
t=3	-0.0068	-0.0133	0.0087*	-0.0022
	(-1.33)	(-1.55)	(1.71)	(-0.26)
t=4	-0.0012	-0.0049	0.0025	-0.0068
	(-0.24)	(-0.47)	(0.51)	(-0.59)
t=5	0.0044	0.0030	-0.0036	-0.0202
	(0.81)	(0.24)	(-0.73)	(-1.44)
t=6	-0.0055	0.0030	-0.0023	-0.0359***
	(-1.03)	(0.24)	(-0.46)	(-2.60)
t=7	-0.0019	-0.0028	-0.0144***	-0.0443***
	(-0.36)	(-0.19)	(-2.63)	(-2.84)
t=8	0.0013	-0.0191	-0.0025	-0.0321
	(0.26)	(-1.10)	(-0.47)	(-1.59)
t=9	-0.0001	0.0117	0.0011	-0.0062
	(-0.02)	(0.61)	(0.22)	(-0.28)
t=10	0.0054	0.0086	0.0021	-0.0489
	(1.14)	(0.36)	(0.40)	(-1.63)
N	278 253	205 746	483 917	370 565

Notes: Dependent variable in (1) and (2) log total wage costs, in (3) and (4) log input VAT; (1) and (3) TWFE estimations t-statistics in parentheses; (2) and (4) multi-period DiD estimation z-statistics in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: VAT rate and VAT gap in 2011 and 2020 for countries in the EU-27

Country	Standard VAT rate 2011/2020	VAT gap 2011 (in %)	VAT gap 2020 (in %)	Change in VAT gap 2011 - 2020
Panel A: Countries with mandatory ECRs				
Austria	20	13	8.6	-4.4
Belgium	21	12	14	2.0
Bulgaria	20	24	6.3	-17.7
Czech Republic	21	17	11.9	-5.1
Denmark	25	8	5	-3.0
France	20	14	8	-6.0
Greece	24	38	19.7	-18.3
Hungary	27	24	5.1	-18.9
Italy	22	32	20.8	-11.2
Latvia	21	37	3.6	-33.4
Poland	23	19	11.3	-7.7
Slovakia	20	33	13.9	-19.1
Slovenia	22	9	5.5	-3.5
Sweden	25	4	2	-2.0
Average	**22.2**	**20.3**	**9.7**	**-10.6**
Panel B: Countries without mandatory ECRs				
Estonia	20	14	1.8	-12.2
Finland	24	5	1.3	-3.7
Germany	19	10	4.8	-5.2
Ireland	23	12	12.5	0.5
Lithuania	21	36	19.3	-16.7
Luxembourg	17	5	6	1.0
Portugal	23	11	8	-3.0
Spain	21	19	4.7	-14.3
The Netherlands	21	4	2.8	-1.2
Average	**21**	**12.9**	**6.8**	**-6.1**

Sources: Standard VAT rate: European Commission (2014, 2022a); VAT gap 2011 (European Commission, 2014); VAT gap 2020 (European Commission, 2022a); Change in VAT gap (own calculations); information whether a countries has implemented mandatory ECRs until 2020 (OECD, 2022).

Notes: The table lists information on the standard VAT tax rate and VAT gap for the EU-27 countries in 2011 and 2020. Cyprus and Croatia have been excluded as data for the VAT gap in 2011 has not been available. Malta and Romania have been excluded because they are not part of the OECD so that information on the ECRs is missing. VAT tax rates have not changed between 2011 and 2020 for the listed countries.