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# Does going cashless make you tax-rich? Evidence from India's demonetization experiment

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**Abstract**

This paper investigates the effect of electronic payments technology on firms' tax compliance in a large developing economy. We consider India's demonetization policy which, by limiting the availability of cash, led to a large increase in the use of electronic forms of payments. Using administrative data on firms' tax returns and variation in the strength of the demonetization shock across local areas, we find that greater use of electronic payments leads to firms reporting more sales to the tax authorities. This effect is strong enough to explain roughly half of the large (11%) increase in reported sales observed during demonetization.

JEL: H26, O23, H25.

Keywords:tax compliance, electronic payments, demonetization.

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# 1 Introduction

Consumers and firms are increasingly going cashless. Even before the COVID-19 crisis, the share of adults using electronic payments in the developing world was increasing fast, by over a third over the 2014-2017 period (Demirguc-Kunt et al., 2018). This rapid technological change could make it easier for governments to tax transactions: electronic transactions, unlike cash, are processed by financial institutions acting as third parties, creating a paper trail which can be used by tax authorities to assess liabilities. There are, however, reasons to question whether this will lead to improvements in tax enforcement and compliance in contexts with limited tax capacity. Governments may not use the available information effectively (Almunia et al., 2019) and firms may react strategically to keep their liabilities low (Carrillo et al., 2017). While the policy world is optimistic on the revenue-raising potential of the shift to electronic payments (Rogoff, 2016; OECD, 2017; Awasthi and Engelschalk, 2018), empirical evidence is scarce.

This paper provides new evidence on the effect of electronic payments on tax compliance in the context of India's demonetization episode. On November 8th, 2016, the government of India declared 86% of the existing currency in circulation illegal tender. Printing constraints prevented the immediate replacement of old currency with new notes, leading to a sharp decrease in cash in circulation and a large increase in electronic payments (Chodorow-Reich et al., 2019; Crouzet et al., 2019; Aggarwal et al., 2020). Using firm-level data on reported tax liabilities and tax payments in the state of West Bengal, we find that this change in payment technology increased the amount of sales firms reported to the tax authorities, despite the overall negative effect of demonetization on economic activity (Chodorow-Reich et al., 2019). A 10% increase in electronic payments in an area increased average reported sales by 0.3%. We find effects of similar size, albeit less precise, on firm-level tax payments, suggesting the rise of electronic payments may have increased overall tax compliance.

We combine novel data to investigate the effect of electronic payments on firms' tax behavior. First, we use administrative data on firms' quarterly tax returns for the universe of firms paying Value-Added Tax in West Bengal. The availability of tax data at a high frequency enables us to study the effect of the demonetization policy, despite the existence of another potentially large shock to tax compliance shortly afterwards (a wide-ranging tax reform in July 2017), which makes the analysis of medium-run effects infeasible. We combine this with data from the National Payments Corporation of India (NPCI) on the use of electronic payments at the local level. These datasets enable us to plot the evolution of key variables of interest in West Bengal in Figure 1. We see large increases in electronic transaction amounts and the number of electronic payment machines in use from the month of the policy announcement, as well as a sharp increase in reported sales and tax payments during the two quarters immediately following demonetization.[1]

To go beyond correlations in the time-series evidence, we apply the identification strategy used by Chodorow-Reich et al. (2019), Crouzet et al. (2019) and Aggarwal et al. (2020) and use the strength of the demonetization shock at the local level to instrument for the increase in electronic payments. Newly printed currency notes were distributed through local bank branches known as currency chests. Variation across local areas in the presence of currency chests determined how easily agents were able to access the new notes, and how much they shifted to electronic forms of payment. The local deposit share of currency chests is a strong predictor of the growth in electronic payments: we use this as an instrument to estimate the causal effect of the growth in electronic payments on tax return variables. We control flexibly for the effect of demonetization on real economic activity, so any effect of electronic payments on sales reported to the tax authorities likely reflects a local compliance effect and not an increase in firms' true sales.

We find that the growth in electronic payments due to demonetization can explain a substantial share (roughly half) of the large increase in sales reported by firms to the tax authorities over the period seen in Figure 1, in line with the idea that the availability of third-party information increases compliance. The evidence regarding the effect on tax payments is mixed: we find effects of similar magnitudes for total tax liabilities as for sales, though less precisely estimated.

This paper's main contribution speaks to the role of technology in improving state capacity in the developing world. Recent work shows that the use of new monitoring and computerization technologies can substantially improve the delivery of public services (Muralidharan et al., 2016, 2020), the performance of frontline workers (Duflo et al., 2012) or tax revenues (Fan et al., 2020; Okunogbe and Santoro, 2021).[2] Research on taxation in particular has emphasized the role of third-party

[1] The data on reported sales and tax payments is de-seasonalized using data on the pre-demonetization period, as explained below.

[2] See also Demirguc-Kunt et al. (2017) on the impact of electronic payment technologies in devel-

reported transactions in improving compliance, starting with the seminal contribution by Kleven et al. (2011). Pomeranz (2015) and Naritomi (2018) show that third-party reporting increases tax compliance in middle-income countries.[3] Kleven et al. (2016) and Jensen (2019) argue more generally that structural change over the course of development increases the use of third-party-reporting which in turn increases governments' capacity to tax. The evidence presented in this paper suggests the rapid spread of new payment technologies could boost tax capacity in the developing world. As such, this paper also speaks to the wider literature on the determinants of tax compliance in developing countries.[4]

The literature on the role of electronic payments specifically in improving tax compliance is scarce and mostly restricted to cross-country correlations (Awasthi and Engelschalk, 2018) or rich countries (Slemrod et al., 2017). In the developing world two recent papers investigate the effect of policy changes disincentivizing the use of cash. Brockmeyer and Saenz Somarriba (2021) study Uruguay's financial inclusion reform and find no effect on tax payments, whilst Bachas et al. (2020) find that Mexico's tax on cash was unsuccessful in increasing the use of electronic transaction technologies. The policy experiment we study was in contrast very successful at moving transactions into the digital sphere, at least in the short run.

This paper's second contribution lies in its study of the effect of India's radical demonetization policy on one of its outcomes of interest. The idea that demonetization would increase tax compliance was often put forward by government figures (Lahiri, 2020). Our paper is, to the best of our knowledge, the first to test this claim using data on firm-level tax payments and a credible source of policy variation. Of course, the results should be interpreted in the context of demonetization's well-documented negative impact on the economy as a whole (Banerjee and Kala, 2017; Chodorow-Reich et al., 2019; Karmakar and Narayanan, 2020), as well as its failure to achieve its most commonly discussed aim — the eradication of proceeds from the black economy. While our results suggest demonetization may have had positive effects on firms' reporting behavior, the economic cost of the policy should be kept in mind while assessing its overall effect.

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oping countries.

[3] See also Mittal and Mahajan (2017).

[4] See for example Gordon and Li (2009); Best et al. (2015); Brockmeyer and Hernandez (2016); Carrillo et al. (2017); Gadenne (2017); Jensen (2019); Bachas et al. (2021); Best et al. (2021); Okunogbe and Pouliquen (2021).

# 2 Context and data

## 2.1 Background on demonetization

Our context of study is West Bengal, a large state in the East of India with 90 million inhabitants and a GDP per capita of USD 6,000 (PPP) in 2018, close to the all-India average. The main source of tax revenue at the state level is the Value Added Tax (VAT), which during our period of study was administered by the state governments. All firms with a turnover of more than INR 500,000 (USD 7,000) are required to pay taxes to the state.[5]

On November 8, 2016, the Prime Minister of India announced that the two largest denomination notes would cease to be legal tender and would be replaced by new notes. Agents had until December 31 2016 to return the notes; households and firms were thus forced to deposit their cash into banks. However, to maintain secrecy prior to the announcement the government had not printed a large quantity of new notes. Printing press constraints meant the new notes were made available slowly: the large denomination notes represented 86% of pre-demonetization currency and by the end of our period of study (April 2017) only half of these notes had been replaced. There were restrictions on the amount of cash that could be withdrawn from accounts until the spring of 2017 but payment by electronic means remained available.

Demonetization therefore drastically limited the availability of cash, whilst increasing the amounts agents had in bank deposits and could access through electronic means. As well documented in Chodorow-Reich et al. (2019) and Crouzet et al. (2019), this led households to switch to electronic forms of payments like debit cards, credit cards and e-wallets. Using data from the National Payments Corporation of India, described below, Figure 1 shows that electronic payments in West Bengal increased more than four-fold between October and December 2016, and remained more than twice as high as their pre-demonetization level thereafter.

When it was announced, the policy's stated objectives were to target 'black money' (wealth illegally accumulated in cash) and eliminate counterfeit currency. In subsequent months, however, a fiscal motive was added to the narrative: by creating

[5] Amongst those, firms with a turnover of less than INR 5 million can choose to pay taxes under a simplified regime, which replaces the VAT with a tax on turnover (see Gadenne et al., 2021). 10% of firms choose this option. These firms are excluded from our analysis because our empirical strategy relies on using quarterly tax returns information, which is not available for these firms.

electronic footprints for transactions, the shift to electronic forms of payments would make it easier for tax authorities to assess firms' tax liabilities, thereby increasing tax compliance (see Lahiri, 2020, for an overview of the stated motives for the policy).

How could an increase in electronic transactions increase tax compliance? We estimate that, in aggregate, firms report total sales to the tax authorities that are substantially higher than the total electronic sales in their areas (see Appendix A.1.6). This is consistent with evidence in Gadenne et al. (2021) that firms report sales that are much higher than their total third-party reported sales and suggests firms pay taxes on more than just their electronic sales. We therefore do not expect firms to simply increase their reported sales by the amount of extra electronic transactions they make during demonetization. The increase in electronic transactions may instead increase reported sales if firms suspect that information on third-party verified sales gives tax authorities a better proxy of their true sales, or provides them with more readily available information on the basis of which to start an audit investigation. If, in addition, firms cannot adjust other aspects of their tax returns (the amount of input tax they claim back for example), this increase in reported sales will translate into higher tax payments.

## 2.2 Data

This paper combines three main datasets. The first is administrative data on quarterly tax returns and tax registration information for the universe of firms paying VAT in West Bengal for the fiscal years 2014-2015 to 2016-2017.[6] The main variables we use are total declared sales and total taxes paid. The latter is the difference between VAT paid on a firm's sales and the VAT paid on its inputs, which gives rise to an 'input tax credit' deducted from 'output VAT'. We use a firm's registration postcode to identify their location.

The availability of quarterly tax returns data enables us to study firms' response to the growth in electronic payments. The data suffers from two main shortcomings, however. First, the last period available is the first quarter of 2017. This is due to the implementation in summer 2017 of another large policy change, the replacement of the state-level VAT with a nationwide Goods and Services Tax (GST), which means the data for fiscal year 2017-2018 is unavailable. Even it were, the close

[6] This data is similar, and comes from the same source, as that used in Gadenne et al. (2021). The main difference is that the data used in this paper comes from quarterly, not annual, tax returns.

succession of these two policy shocks which are both likely to affect tax revenues means it would be hard to disentangle any medium-run effect of demonetization from an effect of the introduction of the GST. We therefore only consider the short-run effect of demonetization on tax revenues, though we discuss potential medium-run effects below. Second, registration data for firms registered in fiscal year 2016-2017 was unfortunately not saved by the system. This means we cannot assign a location to firms that enter our data during this year or construct an instrument value for them (see below). We therefore restrict most of our analysis to firms that registered by the end of fiscal year 2015-2016. This implies that we cannot study the effect of demonetization on new firms' decision to register. We report heterogeneous effects by firm size below, and discuss their potential implications for this decision.

Second, we use data from the Reserve Bank of India (RBI) on bank deposits and the location of currency chests (see below). Bank deposit information comes from before demonetization (specifically, in March 2016). This contains information on all customer deposits in all Indian banks by bank type. Third, we use data from the National Payments Corporation of India (NPCI) on monthly payments through electronic point of sales (EPOS) and the number of EPOS machines in use in each postcode, available monthly over the period January 2016 until July 2019.[7]

Finally, we follow Chodorow-Reich et al. (2019) and use satellite data on human-generated nightlight activity to proxy for demonetization's effects on real economic activity at the district and quarter level. Such data are often used when official data are unavailable (Henderson et al., 2012). Official data on economic activity at a subnational level and high frequency does not exist for India; nightlights data also allows us to capture all economic activity, including informal activity, which is typically not included in official sources. We use the data constructed by Beyer et al. (2018) to extend the analysis in Henderson et al. (2012) to South Asia. This data is available from the second quarter of 2012.[8] We use data on quarterly average

[7]This covers electronic transactions that transit through India's RuPay system, which represent 20% of all electronic transactions in India. See Appendix A for more details. We assume throughout that the growth in electronic transactions that we observe in our data is similar to that of all electronic transactions. Evidence in Chodorow-Reich et al. (2019) suggests this assumption is plausible: they find similar effects of the demonetization shock on electronic transactions, using the same data as ours, and on another form of electronic payments, those occurring through a private e-wallet firm.

[8]Beyer et al. (2018) presents a wide battery of checks suggesting this data provides a good proxy for economic activity. Of particular interest to this paper is the fact that their result that demonetization's disruptive effect on economic activity is highly visible in the data (see Beyer et al., 2018, for more details). Different measures are available, however, depending on the method used to

household income at the district level from CMIE, a private firm, as an alternative proxy for economic activity in a robustness check (see Appendix A for more details).

The administrative tax data and the payments and deposits data are simultaneously available for the period January 2016 to March 2017, so this is the time period of the estimation sample in all our specifications. Our main analysis sample contains 594,986 observations at the firm-quarter level for 134,451 firms over 5 quarters. We use the fact that the administrative tax data is available for a much longer time period, however, to take into account seasonality in variables observed on tax returns. As shown in Figure A1, our key outcome variables of interest display consistent patterns of seasonality. To account for this, we run a regression of each outcome from the administrative data on quarter-of-year×district fixed effects using the entire sample for which this data is available prior to demonetization (April 2014 to September 2016) and use the residuals from these regressions in our analysis.[9] We similarly use the entire pre-demonetization period for which tax data is available (10 quarters) to construct a proxy for firm size, using average reported sales.

## 2.3 Time series evidence

We start by plotting the evolution over time of key variables in Figure 1, where we indicate the first demonetization period by a solid red line and the first and last periods of our estimation sample by dashed grey lines. Figures on the left plot the evolution of firm-level tax return variables. We see a large increase in reported sales during the two demonetization quarters in the top graph, corresponding to an 11% (standard error 0.005) increase when we control for local economic activity (see Table A1).[10] There is a similarly large (10%) increase in tax payments. It is highly unlikely that this rise in reported firm sales reflects a similar increase in firms' real sales, given the negative effect of demonetization on the Indian economy as a whole. A potential alternative explanation is presented in the graphs on the right, which plot the evolution of district level EPOS variables. The top graph shows a large increase in

clean and aggregate the raw satellite data. They are highly correlated: our baseline analysis uses the version cleaned of outliers and stray light, we present robustness checks using the original variable.

[9] We similarly residualize the nightlights data by taking quarter-of-year×district fixed effects using the entire period for which the nightlights data is available, 2012 to 2019. This is a standard transformation for this type of data, and follows Chodorow-Reich et al. (2019).

[10] Table A1 presents estimates from a regression of these variables on an indicator for the demonetization period and our proxy for GDP for our main period of study, going from the first quarter of 2016 to the first quarter of 2017.

the amount of transactions going through electronic point of sales machines, equating to more than a 500% increase for the average firm in our data during the first two quarters of demonetization compared to the prior three quarters, after controlling for economic activity (Table A1).[11] This increase seems due to both new firms using electronic forms of payments and a more intensive use of these forms of payments by firms which already had acquired the technology: the bottom graph shows an increase in the number of EPOS machines in use from the start of demonetization, albeit of a smaller size (134%) than the growth in EPOS transaction amounts.

Beyond the rise of electronic payments, other mechanisms could have led to a short-term increase in both reported sales and tax payments. Under 'Know Your Customer' rules banks cannot accept large cash deposits from their customers unless they provide evidence regarding their source of income. Anecdotal evidence from the field during demonetization suggests that firms that had hoarded large amounts of cash prior to demonetization may have chosen to report higher sales to the tax authorities during demonetization to 'justify' sudden large cash deposits.[12] This behavior could in turn have led to higher tax payments as firms applied their existing VAT rates on this increase in reported sales.

The next section investigates how much of the large increase in reported sales and tax payments observed in Figure 1 can be explained by the shift to electronic payment technologies by using the location and size of currency chest banks to instrument for growth in electronic payments during demonetization at the local level.

# 3 Empirical strategy

Our object of interest is the causal effect of the increased use of electronic payment technologies on tax collection. Our baseline specification takes the form:

$$Y_{ipt} = \beta EP_{pt} + \delta X_{ipt} + \gamma_i + \gamma_t + \epsilon_{it} \tag{1}$$

for firm $i$ in period $t$ and postcode $p$, where $Y$ is one of several tax return variables,

[11] The specification in Table A1 regresses log electronic transactions on an indicator for the demonetization period. The indicator's estimated coefficient is 1.8, which corresponds to a 505% increase.

[12] Even in the absence of checks by banks, deposits of cash in bank accounts are traceable, which means the government can link deposits to taxpayers to attempt to recover tax payments ex-post. This potential use of deposit information was discussed during the period (Lahiri, 2020).

$EP_{pt}$ are log electronic payments in period $t$ and postcode $p$, $\gamma_i$ and $\gamma_t$ are firm and period fixed effects. We allow for serial correlation in the error term and correlation across firms in local areas by clustering standard errors at the postcode level.

Our baseline specification thus considers how the growth in electronic payments in a firm's local area affects its reported sales and tax liabilities over time. Taking within-firm and within-period variations still leaves several potential sources of bias in our estimate $\hat{\beta}$. Reverse causality in particular is a cause for concern: firms that experience tax enforcement shocks (such as visits by tax inspectors) may start paying more taxes and therefore find it less costly to offer electronic forms of payments to their consumers. As explained above, demonetization sharply increased customers' demand for electronic forms of payment. We therefore use differences across postcodes in the strength of the demonetization shock to instrument for the magnitude of the growth in electronic payments. To do so, we leverage heterogeneity across postcodes in the availability and customer market share of currency chests. Currency chests are branches of commercial banks that act as local agents of the RBI in cash management: they receive new currency from the central bank and distribute it locally. As well documented by Chodorow-Reich et al. (2019), Crouzet et al. (2019) and Aggarwal et al. (2020), local areas with currency chest branches, or currency chests branches with more customers, gained access to the new notes faster than others. Those areas therefore experienced smaller decreases in the availability of cash during the demonetization period. This, in turn, led to smaller growth in electronic payments.

We construct the deposit market share of currency chest banks in a postcode using data from the RBI on the location and deposits of all banks, including currency chests. Our instrument for electronic payments in our baseline specification (1) is one minus this market share (so that a higher value of the instrument corresponds to a stronger demonetization shock) interacted with an indicator for the demonetization period (the last quarter of 2016 and the first quarter of 2017).[13] Appendix A.2 provides more details on the construction of the instrument.

Figure 2 splits our sample in half according to the value of the demonetization shock, and plots the evolution of residualized EPOS transaction values over time in

[13]Our definition of the instrument is very similar to that of Crouzet et al. (2019). Chodorow-Reich et al. (2019) also use data on the speed of arrival of new notes in currency chests but sadly the RBI has decided to no longer make this data available for research following the publication of results in Chodorow-Reich et al. (2019).

areas with high (red) vs low (blue) values.[14] There is a clear difference in the scale of growth in EPOS transaction values in areas with a low currency chest market share compared to high share areas. In the former case, the total EPOS value peaks at nearly 200 log points higher than pre-demonetization; in areas where new currency could be circulated more quickly, the average increase in EPOS sales is closer to 130 log points. Our identifying assumption is that areas with different currency chest market shares would have experienced the same evolution of outcomes over time in the absence of demonetization. The absence of different trends in electronic payments growth prior to November 2016 is reassuring in this respect.

As shown in Chodorow-Reich et al. (2019), the strength of the demonetization shock affected local economic growth, which in turn likely determined firms' real tax liabilities. To avoid capturing this effect we control throughout for local economic activity at the district level using the log of the nightlights variable.[15] Controlling for local economic activity implies that any effect of electronic payments on, for example, firm-level reported sales, should be interpreted as a compliance effect (firms choosing to report more sales to the tax authorities) and not an effect on firm's real sales. However, because our instrument is defined at the postcode level and not the firm level, we cannot distinguish between two related compliance interpretations. The first is a compliance effect at the firm level, whereby firms use electronic payments more and report a higher share of their sales to the tax authorities. The second is a compliance effect at the postcode level, whereby firms that sell more electronically and are more compliant gain market share at the expense of less compliant firms.

Table 1 presents descriptive statistics for the average of the periods we observe prior to demonetization. As expected, the distribution of both reported sales and tax liability are skewed to the right: our sample contains many small firms and a few very large firms.[16] We conduct heterogeneity analysis by firm size below to investigate whether these firms respond differently to local growth in electronic payments, and present effects obtained by winsorizing the top 1% and 5% of firms as a robustness check. 30% of firms report a null or negative tax liability in any given period, so

[14] The residuals are obtained by regressing log total EPOS sales in a postcode on postcode fixed effects and a linear time trend.

[15] As with the tax return variables, we first de-seasonalize this variable. We average across months to create a quarterly series, and then regress log nightlights on quarter-of-year$\times$district fixed effects using data from 2012Q2 to 2016Q3. We use the residuals from this equation in our regressions.

[16] The mean total sales, 14.34 million INR, is equal to 190,000 USD.

we systematically consider both the extensive margin (paying positive taxes) and the intensive margin (amount of taxes paid) when looking at tax compliance below. Figure A2 plots the distribution of the demonetization shock variable. It takes values in the 0.2-1 range, indicating that some areas in West Bengal (representing less than 3% of observations) had no direct access to currency chests (instrument value of 1).

# 4 Results

Table 2 presents our main results: the outcome variable is log reported sales in panel A, an indicator for positive tax liability in panel B, and log tax liability (when positive) in panel C. Following specification (1) we use the demonetization shock variable to instrument for electronic payments and all specifications include firm and period fixed effects. First stage results, presented in Table A2, show that our instrument has a large effect on the growth of electronic payments at the local level: a one standard deviation increase in the currency chest market share decreases the growth in electronic payment amounts during the demonetization period by 25-35%. Our preferred specification in column 2 controls linearly for the log of nightlights to proxy for the effect of demonetization on local economic activity but we present results obtained without this control (column 1), with a more flexible specification (column 3) and with an alternative proxy for robustness (see Table A3 and Table A5).

We find a positive effect of electronic payments on firm-level sales reported to the tax authorities: doubling the value of EPOS transactions increases reported sales by around 3%. Our proxy for economic activity has a positive effect on reported sales, as expected. Flexibly controlling for local economy effects mostly affects the precision of our estimates, suggesting our instrument is successful at isolating the effect of demonetization on outcomes going through the change in payment technology. The magnitude of this effect suggests the response cannot be driven by firms simply increasing their reported sales by the amount of extra electronic transactions: given the small share of EPOS transactions in reported sales such a response would lead to a much smaller increase in reported sales. We estimate that EPOS transactions represented roughly 0.7% of reported sales prior to demonetization (see Appendix A.1.6); if firms only increased their reported sales in line with their electronic sales, we would only observe an 0.7% increase in sales when EPOS transactions double. Our estimate is five times bigger. Our results are on the other hand consistent with the idea that firms report a higher share of their sales to the tax authorities when their use of electronic payment technology increases.

There is some evidence of a small positive impact – around 1 percentage point – of electronic payments on the probability that firms report a positive tax liability to the authorities. Among firms that report positive tax liabilities, results suggest a 1-3% increase in taxes paid. Estimates are imprecise and less robust to changes in how we control for economic activity, but are of roughly the same magnitude as the increase in reported sales.

Appendix Table A3 presents robustness checks for our main estimate of the effect on reported sales. Estimates are unaffected by: winsorizing the top 1% or 5% observations; using an alternative nightlights variable provided by Beyer et al. (2018); using average household income from the CMIE data as an alternative proxy for economic activity; or constructing the electronic payments variable or the instrument using slightly different methods (see Appendix A.2 for more details). Robustness checks for the probability of reporting a positive liability also show consistent results (Appendix Table A4), and our estimates of the effect of electronic payments on taxes paid are similarly in the same range as in Table 2 across these robustness checks (though somewhat larger and more precisely estimated, see Appendix Table A5).

To investigate firms' response to the increase in electronic payments further, Table 3 presents evidence on other tax return variables of interest. We find no evidence that electronic payments growth affects the likelihood that a firm already filing tax returns prior to demonetization still files (first column), and no evidence that the effective tax rates (ETRs) paid by firms on their sales or on their inputs are affected (last two columns).[17] We do, however, see that firms are slightly more likely to report input costs on which they claim taxes back (second column), and may report higher amounts when they do (third column) — though this effect is not statistically significant. This could be consistent with evidence in Carrillo et al. (2017) who find that, when firms report more sales in response to an enforcement shock, they also report more inputs to keep their tax liabilities low. In this context however firms have limited discretion regarding how much input tax they can claim back because the tax considered is a VAT: their claims can be systematically cross-checked against the tax paid by their suppliers (Gadenne et al., 2021). Nonetheless, this effect is consistent with firms

[17]The variable 'Filing' takes a value one if the firm files a tax returns with positive sales, and zero otherwise. All other regressions are obtained on a sample consisting of firms filing positive sales.

choosing to buy a higher share of their inputs from VAT-paying suppliers and/or these firms' suppliers becoming more likely to pay VAT.[18]

Finally, we investigate whether there is evidence of heterogeneous effects by pre-demonetization firm size (Appendix Table A6). A large literature argues that smaller firms are more likely to evade taxes (see for example Kleven et al., 2016; Bachas et al., 2021); this could make them more responsive to a change in payment technology limiting their capacity to under-report sales. We find some evidence in line with this idea, as the effect of electronic payments on reported sales seems mostly driven by smaller firms (Panel A). This, together with the evidence presented in Figure 1 that the number of EPOS machines in use increases during demonetization, suggests the shift to electronic payments may have led to more firms starting to pay taxes, as firms are typically small when they enter the tax data. This hypothesis is not one we can test formally because of the data limitations described above.[19] Somewhat surprisingly, the effects on tax payments in Panels B and C seem mostly driven by responses amongst larger firms, though the differences across groups are rarely statistically significant.

Overall, we find evidence of a small increase in the sales reported to the tax authorities as demonetization caused rapid uptake of electronic payment technologies. How much of the increase in reported sales and taxes paid during the demonetization period in Figure 1 could be explained by our results? As discussed above the value of EPOS transactions increased by 500% at the firm level during the period after controlling for our GDP proxy; using our baseline estimate for the effect on reported sales of 0.03, the increase in EPOS transactions leads to a 5.5% increase in reported sales, roughly half the total increase observed during demonetization.[20] The estimate of the effect on taxes is substantially noisier, but the range of estimates implies an increase in tax liability of between 2.7% and 6.6% due to the increase in EPOS transactions over the period — between one-third and one-half of the total increase over the period. This back of the envelope calculation suggests that the spread of electronic payments was an important part of the aggregate patterns shown above.

[18] We cannot test this directly as the transaction data used in Gadenne et al. (2021) isn't available at the quarterly level.

[19] This would be in line with evidence of increased formalization of small retailers after increased financial technology adoption in Mexico in Higgins (2020).

[20] To see this, note that the % increase in reported sales for a 500% increase in EPOS transactions is given by $100 * (6^{\beta} - 1)$ where $\beta = 0.03$ is our estimate of the causal effect of EPOS transactions on reported sales.

# 5 Conclusion

This paper investigates the effect of a shift to electronic payment technology during India's demonetization period on firms' tax behavior. Using variation in the strength of the demonetization shock across local areas, which led to different rates of adoption of electronic technologies, we find that a higher use of electronic payments leads to firms reporting higher sales to the tax authorities. This effect is strong enough to explain roughly half of the large increase in reported sales observed during demonetization. The evidence regarding the effect on tax payments is more mixed: we find effects of similar sizes but less precisely estimated on total taxes paid. Overall, our results suggest cautious optimism regarding the potential of new payment technologies to increase tax capacity in developing countries.

The start of another large-scale policy reform shortly after demonetization implies that we can only estimate short term effects of the policy but one of the stated aims of demonetization was to increase tax compliance in the long run. We can speak to the likelihood that this aim was reached by considering the evolution of electronic payments for 2.5 years after demonetization in Figure 1 where we see that the use of electronic payments continued to increase in West Bengal even after the end of our period of study, April 2019. Assuming the effect of electronic payments we observe in the short run persists, this suggests demonetization may have had an effect on firms' reported sales and tax compliance in the medium run.

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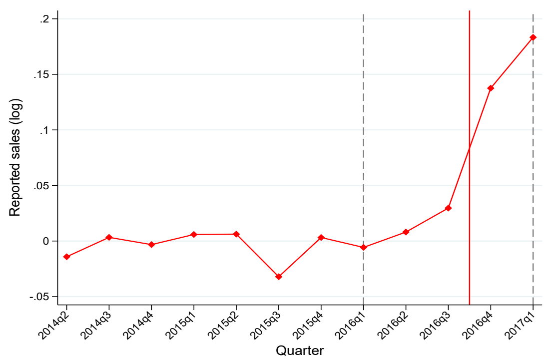
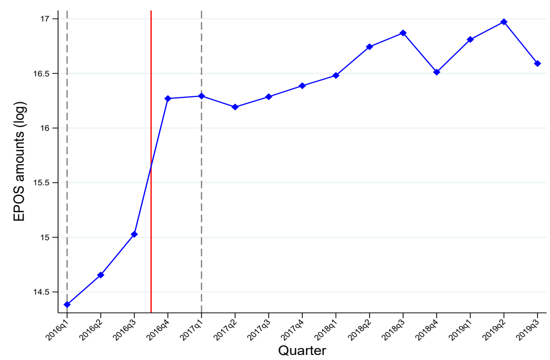
Figure 1: Evolution of key tax returns and electronic transaction variables over time

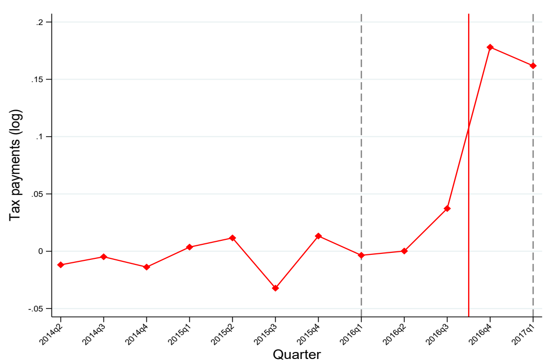
(a) Reported sales



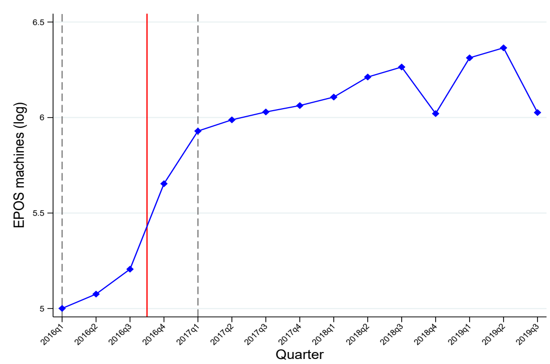

(b) EPOS transaction amounts




(c) Tax payments

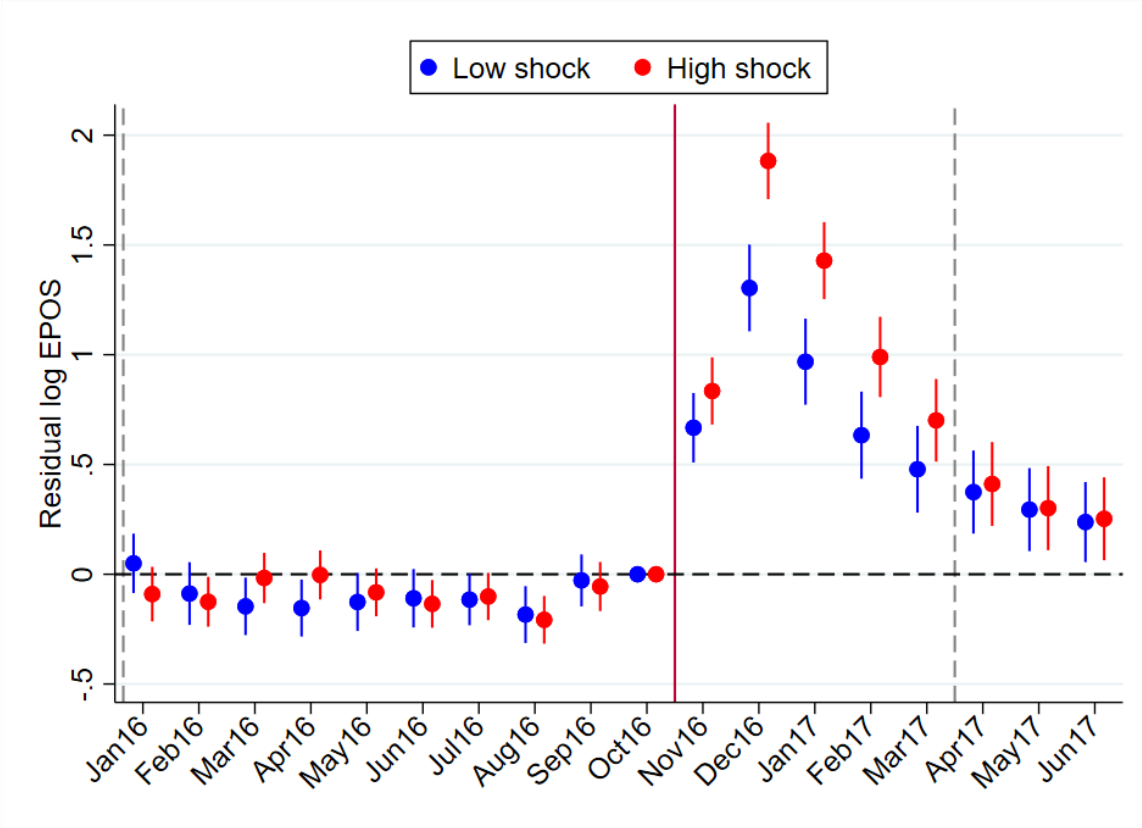



(d) EPOS machines in use




*Notes:* These graphs plot the evolution of key variables over time for the period during which they are available in our data. The grey dashed lines indicate the start and end of the period defining our estimation sample; the red vertical line indicates the start of demonetization. Graphs on the left (in red) plot the evolution of tax returns variable: log reported sales (top graph) and log tax payments (bottom graph). These variables are residualized by taking quarter-of-year×district fixed effects (see text). Graphs on the right (in blue) plot the evolution of electronic payments: log transaction amounts (top graph) and log number of electronic payments machines in use (bottom graph). Each point is an average across firms for each period (quarter). See the text for a description of the variables and data sources.

Figure 2: Electronic payments in low and high demonetization shock areas




*Notes*: The figure shows the mean residualized value of log total electronic payments sales in areas (postcodes) with a value of the demonetization shock variable above and below the median value, where the median is determined by weighting each postcode by the number of firms present in it in the administrative tax data. The demonetization shock variable corresponds to one minus the currency chest market share in a postcode; see the text for more details. The grey dashed lines indicate the start and end of the period defining our estimation sample and the red line indicates the start of demonetization. The residuals are obtained using pre-demonetization data only: we regress log total EPOS sales in a postcode on postcode fixed effects and a linear time trend that is allowed to vary below and above the median shock value, and use this to generate residuals throughout the period shown. The figure shows coefficient estimates from a linear regression of these residuals on monthly fixed effects, estimated separately for areas below and above the median shock value, with 95% confidence intervals based on robust standard errors.

Table 1: Descriptive statistics pre-demonetization

| | Mean | Std.Dev. | Median |
|---|---|---|---|
| Reported sales (millions) | 14.343 | 221.462 | 1.218 |
| Positive tax liability | 0.707 | 0.455 | 1.000 |
| Tax liability (millions) | 0.443 | 9.043 | 0.015 |
| Local EPOS sales (millions) | 8.233 | 10.177 | 4.362 |
| % EPOS in locally reported sales | 0.138 | 0.694 | 0.039 |
| Demonetization shock | 0.495 | 0.274 | 0.519 |
| Nightlights per sq. km (log) | 3.244 | 2.080 | 2.670 |

*Notes:* The table shows descriptive statistics for the first three quarters of 2016, the pre-demonetization quarters in our main period of study. The first three variables are measured at the firm level and tax liability is described only for firms with positive tax liability; local EPOS sales and the demonetization shock are both measured at the postcode level; and nightlights are measured at the district level. The demonetization shock variable corresponds to 1 minus the market share of banks with a currency chests in a postcode. Nightlights are the quarterly mean of nanowatts per kilometre measured at the monthly level, after filtering and removing outliers.

Table 2: Firm-level evidence: tax return variables

| *A. Outcome: Log(Reported Sales)* | (1) | (2) | (3) |
|---|---|---|---|
| Log(EPOS) | 0.037*** | 0.030** | 0.032** |
| | (0.012) | (0.014) | (0.015) |
| Log(Nightlights) | | 0.033 | 0.025 |
| | | (0.026) | (0.032) |
| Log(Nightlights)$^2$ | | | 0.014 |
| | | | (0.034) |
| Observations | 594986 | 594986 | 594986 |
| First stage F-stat | 71.35 | 46.58 | 31.48 |
| Pre-demonetization mean | 14.03 | 14.03 | 14.03 |
| *B. Outcome: Tax Liability > 0* | (1) | (2) | (3) |
| Log(EPOS) | 1.038*** | 0.862* | 0.636 |
| | (0.382) | (0.470) | (0.559) |
| Log(Nightlights) | | 0.885 | 1.858 |
| | | (1.122) | (1.623) |
| Log(Nightlights)$^2$ | | | -1.611 |
| | | | (1.651) |
| Observations | 594986 | 594986 | 594986 |
| First stage F-stat | 71.35 | 46.58 | 31.48 |
| Pre-demonetization mean | 70.74 | 70.74 | 70.74 |
| *C. Outcome: Log(Tax Liability)* | (1) | (2) | (3) |
| Log(EPOS) | 0.036*** | 0.021 | 0.015 |
| | (0.011) | (0.015) | (0.018) |
| Log(Nightlights) | | 0.071* | 0.100* |
| | | (0.038) | (0.052) |
| Log(Nightlights)$^2$ | | | -0.047 |
| | | | (0.054) |
| Observations | 424974 | 424974 | 424974 |
| First stage F-stat | 72.05 | 46.56 | 31.30 |
| Pre-demonetization mean | 9.72 | 9.72 | 9.72 |

*Notes:* The table shows results from running specification (1) using different tax return variables as outcomes. Observations are at the firm×quarter level, and all outcome variables are residualized by taking district×quarter-of-year fixed effects based on all pre-demonetization quarters (see the text for more details). All specifications include year×quarter and firm fixed effects. Log EPOS measures sales through electronic payments machines in the firms' postcode, and is instrumented for using the demonetization shock variable described in the text and in Appendix A.2. The sample includes all firms with non-zero returns who were in the tax return data in fiscal year 2015-2016 in the top two panels, and only firms with positive reported tax liability in the third panel. In the second panel the outcome has been scaled by 100 so that coefficients can be read in percentage point terms. Standard errors clustered at the postcode level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Firm-level evidence: other tax return outcomes

| | Filing | Reported Inputs>0 | Log(Reported Inputs) | Output ETR | Input ETR |
|---|---|---|---|---|---|
| Log(EPOS) | 0.327 | 0.906** | 0.026 | 0.001 | -0.021 |
| | (0.335) | (0.360) | (0.017) | (0.014) | (0.019) |
| Log(Nightlights) | -1.837 | -1.949** | 0.018 | -0.075** | -0.058 |
| | (1.329) | (0.915) | (0.028) | (0.032) | (0.046) |
| Observations | 673582 | 594986 | 507508 | 594986 | 507508 |
| First stage F-stat | 51.14 | 46.58 | 45.63 | 46.58 | 45.63 |
| Pre-demonetization mean | 88.35 | 84.74 | 13.72 | 6.12 | 6.52 |

*Notes:* The table shows results obtained from running specification (1) using different tax return variables as outcomes. Observations are at the firm×quarter level, and all outcome variables are residualized by taking district×quarter-of-year fixed effects based on all pre-demonetization quarters (see the text for more details). All specifications include year×quarter and firm fixed effects. The variable 'Filing' is equal to one if the firm submits a tax return with positive sales; all other columns include only observations where this is equal to 1. The outcome in the second column is equal to 1 if the firm reports non-zero input purchases; the third column shows the effect on total reported inputs among those with positive inputs. The fourth and fifth columns show effects on the output and input effective tax rates (ETR) reported by firms, calculated as output tax divided by total sales and input tax credit divided by total inputs, respectively. Log EPOS measures total sales through electronic payments machines in the firms' postcode, and is instrumented for using the demonetization shock variable described in the text and in Appendix A.2. Standard errors clustered at the postcode level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

# Online Appendix

# A Additional information on data and method

## A.1 Description of variables

### A.1.1 Quarterly tax return variables

Our main outcome variables are recorded in quarterly VAT returns obtained from the Directorate of Commercial Taxes in West Bengal. These cover the period 2014Q2 to 2017Q1. For all variables listed below, we undertake residualization using all pre-demonetization data in order to account for seasonality, as shown in A1. To do this, we estimate residuals from a regression of each outcome (or the log of the outcome in the case of continuous variables) on district×quarter-of-year fixed effects based on data from 2014Q2 to 2016Q3. The variables used from this data are defined as follows.

- **Filing**: Equal to one if VAT return with positive sales submitted.
- **Reported Sales**: Total sales reported in VAT return.
- **Reported Inputs**: Total input purchases reported in VAT return.
- **Taxes**: Total estimated VAT liability generated in that quarter, defined as VAT charged on reported sales minus VAT paid on reported inputs.
- **Output ETR**: VAT charged on reported sales divided by total sales.
- **Input ETR**: VAT paid on reported inputs divided by total input purchases.

### A.1.2 Pre-demonetization tax data

Various firm characteristics are sourced from information provided before demonetization to the Directorate of Commercial Taxes in West Bengal, taken from annualised tax return and registration data for VAT firms in West Bengal from FY 2009/10 to FY 2015/16. Some information is provided at the point of registration and is thus assumed time-invariant. This includes:

- **Postcode**: Postcode where business is registered in West Bengal.

- **Business type**: Declared type of businesses from 14 categories. We group these into three main categories: wholesale, retailer and manufacturer.

In addition, some firm characteristics are taken from end-year annual VAT returns or from data pertaining to trading with other businesses in West Bengal which are provided alongside annual tax returns. These are:

- **Product sold**: The main commodity sold in the last observed period prior to demonetization. We group into these into 12 large categories.
- **VAT rate**: VAT bracket that applies to main commodity sold. There are 5 categories but we exclude the very highest rate due to few observations.
- **Third party reported sales**: Purchases from the firm that have been reported to the tax authority by other registered firms in West Bengal.

#### A.1.3 Nightlights data

We use nightlights to proxy for local economic activity, using district-level data from January 2012 to April 2018 supplied by Beyer et al. (2018). We account for seasonality in these measures by estimating residuals from a regression of log nightlights on district$\times$quarter fixed effects using the full pre-demonetization data. We use two measures of nightlights in this way:

- **Nightlights**: Sum of lights per square kilometre in nanowatts, after treating for outliers and with clustered noise removed.
- **Alt nightlights**: Sum of lights per square kilometre in nanowatts, without treating for outliers or removing clustered noise.

#### A.1.4 Income data

Data from Centre for Monitoring the Indian Economy (CMIE) Consumer Pyramids provides an alternative measure of local economic activity. Consumer Pyramids is a monthly household survey containing household income of roughly 100,000 households. Each household is visited every three or four months, we aggregate data at the district$\times$quarter level to obtain our proxy, for each quarter from from 2016Q1 to 2017Q4.

- **Income**: Mean household income from all sources.

### A.1.5 RBI data: bank deposits and currency chest location

From the Reserve Bank of India, we use data on bank deposits and the location of currency chests to construct our instrument: see A.2 for details.

### A.1.6 NPCI data: electronic-point-of-sales transactions and machines

Our data on electronic point-of-sales transactions (EPOS) is the same as that used in Chodorow-Reich et al. (2019) and is produced by the National Payments Corporation of India (NPCI), an organization set up by the Reserve Bank of India (RBI) to operate retail payment systems. We use data regarding EPOS machines and transactions recorded at the month and postcode level from January 2016 to August 2019, and aggregate it to the quarter level. This data contains information on most transactions that transit through India's payment card (RuPay) system. Internal estimates by the RBI shared with the research team indicate that 20% of all electronic transactions (including e-Wallet payments) are captured in the data.

The main variables of interest are:

- **EPOS**: Total value of EPOS transaction amounts, with missing monthly observations (26% of total) linearly interpolated.
- **Alt. EPOS**: Total value of EPOS sales, with missing monthly observations (26% of total) assumed to be zero.
- **EPOS Machines**: Total number of EPOS machines, with missing monthly observations (26% of total) linearly interpolated.
- **EPOS Transactions**: Total number of EPOS transactions, with missing monthly observations (26% of total) linearly interpolated.

We use our data to assess the share of electronic transactions in total reported sales. Comparing total sales reported by firms to the tax authorities to total EPOS transaction values in our data in the pre-demonetization period yields a share of NPCI-recorded EPOS in reported sales of 0.14%. This corresponds to a share of total EPOS transactions in reported sales of roughly 0.7% (0.14/0.20).

## A.2 Instrument construction

Our instrument is one minus the deposit market share of banks with currency chests in a postcode, interacted with an indicator for the demonetization period (the last quarter of 2016 and the first two quarters of 2017). To construct it we use data from the RBI available at the postcode or district level. Information on the location of bank branches, whether they act as a currency chest, and their bank name and group (public, private, foreign or cooperative) is available at the postcode level. Information on total deposits per bank group is available at the district level. We use data on bank deposits from March 2016, and the location of currency chests, which are relatively stable over time, comes from 2019.

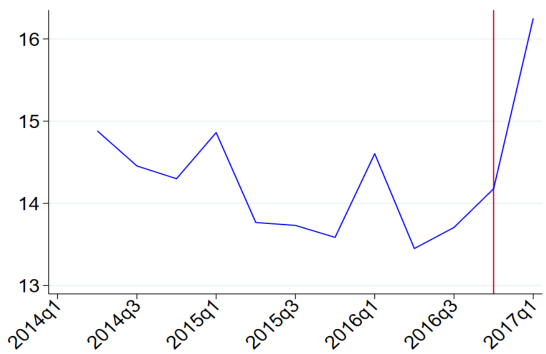
We construct this instrument by using an intermediate geographical unit, the *taluk*, which is smaller than a district but bigger than a postcode. There are 314 taluks in West Bengal, 23 districts and 984 postcodes. Our understanding from the field suggests that the taluk is the most appropriate unit to capture the area within which households use banking services. Evidence in Garg and Gupta (2020) that the mean household distance to the nearest bank is 4.5km is also consistent with households going further than their postcode to bank, but not to the other side of the district. To obtain a proxy for total deposits per bank group $g$ at the taluk $\tau$ level we construct $D_{\tau g} = D_{dg} N_{\tau g}/N_{dg}$ where $D_{dg}$ is total deposits for bank group $g$ in this taluk's district $d$, $N_{\tau g}$ and $N_{dg}$ are total number of bank branches in a district $d$ or taluk $\tau$. We then obtain a proxy for total deposits in currency chests at the taluk level by defining $D_{\tau c} = D^C_{\tau g} N^C_{\tau g}/N_{\tau g}$: where the superscript $^C$ indicates values summed over groups (for deposits) and branches (for number of branches) in which we know currency chest are located. We take total deposits in the taluk and bank group in which we know the currency chest is located (typically this is the public group), and scale this by the share of bank branches in which there is a currency chest in that taluk in total bank branches in that group and taluk.

Whilst each taluk is located in only one district, computations are slightly complicated by the fact that postcodes can spread over more than one taluk (slightly over half the postcodes in West Bengal are in more than one taluk). We have data on firm and chest locations at the postcode level, so need to allocate each of these postcodes across several taluks. Because currency chests are in charge of distributing cash around them, we assume that when a currency chest is in postcode that itself is in several taluks, all of these taluks contain a currency chest. To assign an instrument value to each firm based on its postcode, we assign a market share to each postcode based on the distribution of post offices (a readily available proxy for population at this level) across postcodes. We consider an alternative assignment method that assumes equal population per postcode and present results in the last column of Tables A3 and A5, showing that estimates are unaffected.

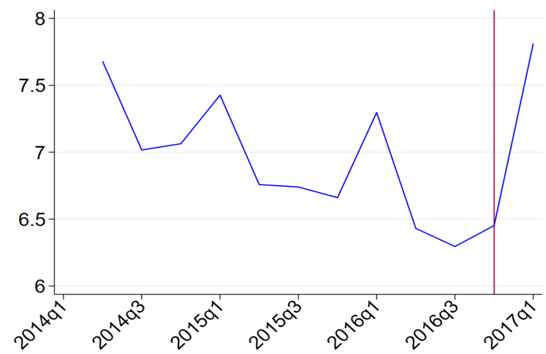
# B Additional Tables and Figures

Figure A1: Seasonality in outcome variables

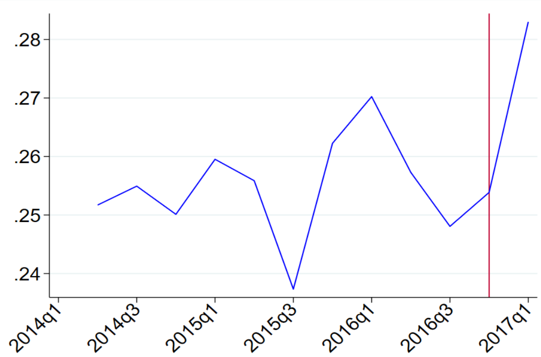
(a) Reported sales (millions)




(b) Reported purchases (millions)

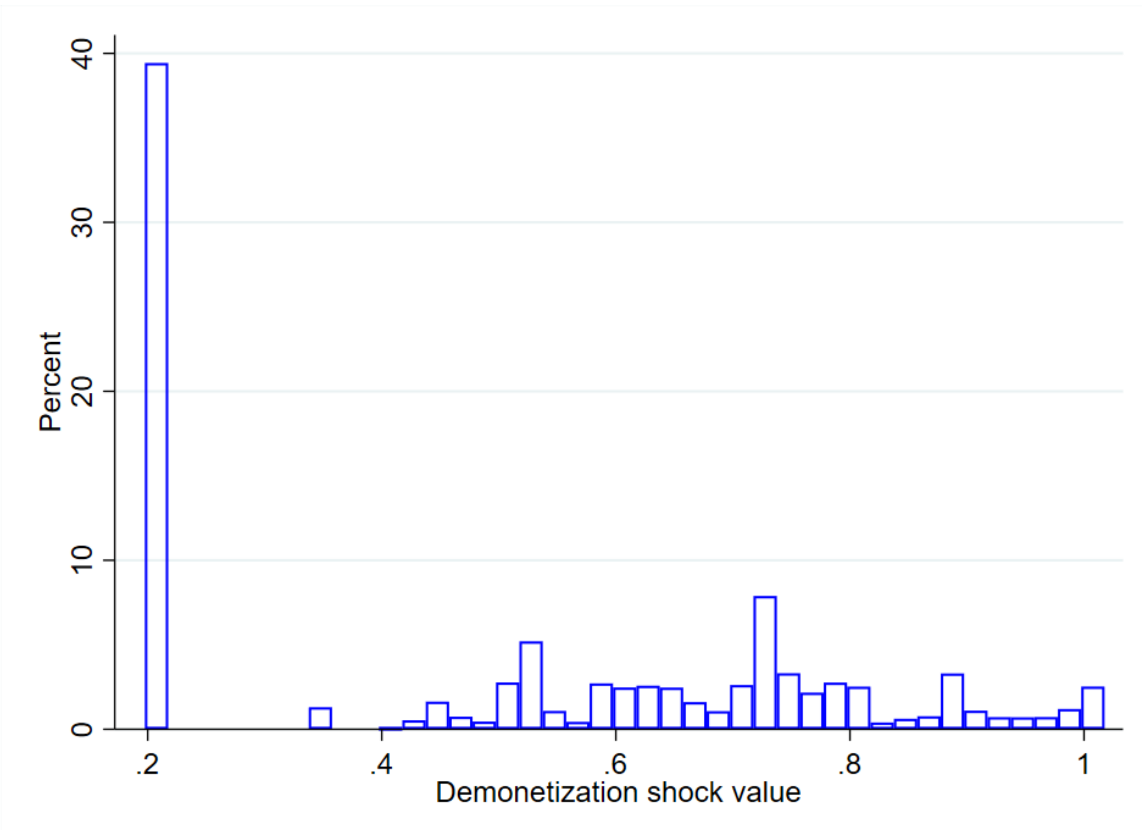



(c) Tax liability (millions)




*Notes:* These graphs plot the evolution of mean values in our key outcome variables over time among firms included in our main estimation sample, prior to any residualization. The red line indicates the start of demonetization. All three outcomes are measured at the firm level; see the text for a description of the variables and data sources.

Figure A2: Distribution of demonetization shock




*Notes:* The graph shows the distribution of the demonetization shock variable with each firm ever appearing in our data appearing once. The variable is defined as one minus the market share of local banks (at the postcode level) with a currency chest and is constructed as described in Appendix A.2.

Table A1: Time series evidence

| *A. Tax return variables* | | | |
|---|---|---|---|
| | Log(Reported Sales) | Taxes>0 | Log(Taxes) |
| Demonetization | 0.112*** | -0.075 | 0.096*** |
| | (0.005) | (0.161) | (0.005) |
| Log(Nightlights) | -0.015 | 0.141 | -0.075*** |
| | (0.009) | (0.366) | (0.012) |
| Observations | 594986 | 594986 | 424974 |
| Pre-demonetization mean | 14.03 | 70.74 | 9.72 |
| *B. EPOS variables* | | | |
| | Log(EPOS) | Log(Machines) | Log(Transactions) |
| Demonetization | 1.804*** | 0.848*** | 1.697*** |
| | (0.048) | (0.019) | (0.048) |
| Log(Nightlights) | 1.393*** | 0.787*** | 1.346*** |
| | (0.079) | (0.036) | (0.083) |
| Observations | 594986 | 594976 | 594986 |
| Pre-demonetization mean | 14.76 | 5.15 | 7.08 |

*Notes:* The table presents estimates of a regression of the outcome variable in the column header on log nightlights and an indicator for the demonetization period (the last quarter of 2016 and the first two quarters of 2017). Observations are at the firm×quarter level. Tax return variables in the top panel are residualized by taking district×quarter-of-year fixed effects (see the text for more details). The sample includes firms who appear in the tax return data prior to 2016Q4 and who report positive sales, except in the third column of the panel A where it includes only firms reporting positive tax liabilities. In the second column of panel A, the outcome has been scaled by 100 such that coefficients can be interpreted in percentage point terms. Standard errors in parentheses are clustered at the postcode level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A2: First stage results

| *Outcome: Log(EPOS)* | (1) | (2) | (3) |
|---|---|---|---|
| Demonetization shock | 1.340*** | 1.157*** | 1.071*** |
| | (0.159) | (0.169) | (0.191) |
| 2016Q4 | 1.316*** | 1.436*** | 1.504*** |
| | (0.058) | (0.072) | (0.094) |
| 2017Q1 | 1.376*** | 1.313*** | 1.322*** |
| | (0.060) | (0.063) | (0.064) |
| Log(Nightlights) | | 0.685*** | 0.953*** |
| | | (0.248) | (0.313) |
| Log(Nightlights)$^2$ | | | -0.527 |
| | | | (0.327) |
| Observations | 594986 | 594986 | 594986 |
| F-stat | 71.35 | 46.58 | 31.48 |

*Notes:* The table shows results from the first stage underlying panels A and B of Table 2. The outcome variable is log EPOS, where EPOS is measured at the postcode level, and the demonetization shock variable is one minus the market share of currency chest banks in the postcode interacted with an indicator for the demonetization period, as described in the text and in Appendix A.2. All specifications include year×quarter and firm fixed effects, and standard errors are clustered at the postcode level. 2016Q4 and 2017Q4 are period fixed effects showing the overall increase in EPOS during demonetization. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A3: Effect on reported sales: robustness checks

| *Log(Reported Sales)* | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Log(EPOS) | 0.031** | 0.033** | 0.034*** | 0.036*** | | 0.032** |
| | (0.014) | (0.013) | (0.013) | (0.012) | | (0.014) |
| Alt. Log(EPOS) | | | | | 0.030** | |
| | | | | | (0.013) | |
| Log(Nightlights) | 0.030 | 0.024 | | | 0.032 | 0.030 |
| | (0.026) | (0.026) | | | (0.025) | (0.026) |
| Alt. Log(Nightlights) | | | 0.010 | | | |
| | | | (0.014) | | | |
| Log(HH Income) | | | | 0.022 | | |
| | | | | (0.024) | | |
| Winsorization | 1% | 5% | None | None | None | None |
| Observations | 594986 | 594986 | 594986 | 566745 | 593554 | 594986 |
| First stage F-stat | 51.14 | 51.14 | 47.59 | 75.10 | 51.91 | 52.77 |
| Pre-demonetization mean | 14.02 | 13.98 | 14.03 | 14.02 | 14.03 | 14.03 |

*Notes:* The table shows robustness checks for specification (1). The outcome variable is log of sales reported in tax returns, residualized of district×quarter-of-year fixed effects using data from the pre-demonetization period, and all specifications include firm and quarter fixed effects. Log EPOS measures total sales through EPOS machines in the firms' postcode, and is instrumented for using the demonetization exposure measure described in Appendix A.2. Columns (1) and (2) show results for reported sales at the firm level, where the outcome variable is winsorised at the 99th and 95th percentiles, respectively. Column (3) uses an alternative measure of nightlights which has not had outliers removed or been filtered of stray light. Column (4) controls for an alternative measure of economic activity: average household income at the district level according to Consumer Pyramids CMIE data. This data is missing for one district. Column (5) uses an alternative measure of EPOS, where missing values are assumed to be zeroes, rather than interpolated. This assumption means that some observations are dropped in the log transformation. Column (6) uses an alternative construction of our instrument described in Appendix A.2. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4: Effect on positive tax liability: robustness checks

| *Taxes>0* | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Log(EPOS) | 1.076*** | 0.841** | | 0.864* |
| | (0.392) | (0.348) | | (0.448) |
| Alt. Log(EPOS) | | | 1.001** | |
| | | | (0.439) | |
| Log(Nightlights) | | | -0.772 | -0.412 |
| | | | (1.552) | (1.567) |
| Alt. Log(Nightlights) | -0.591 | | | |
| | (0.747) | | | |
| Log(Income) | | 0.441 | | |
| | | (1.120) | | |
| Observations | 673582 | 642354 | 671813 | 673582 |
| First stage F-stat | 47.59 | 75.10 | 51.91 | 52.77 |
| Pre-demonetization mean | 63.04 | 63.09 | 63.08 | 63.04 |

*Notes:* The table shows robustness checks for specification (1). The outcome variable is equal to 100 if the firm reports a positive net tax liability and 0 otherwise, residualized of district×quarter-of-year fixed effects using data from the pre-demonetization period, and all specifications include firm and quarter fixed effects. Log EPOS measures total sales through EPOS machines in the firms' post-code, and is instrumented for using the demonetization exposure measure described in Appendix A.2. Column (1) uses an alternative measure of nightlights which has not had outliers removed or been filtered of stray light. Column (2) controls for an alternative measure of economic activity: average household income at the district level according to Consumer Pyramids CMIE data. This data is missing for one district. Column (3) uses an alternative measure of EPOS, where missing values are assumed to be zeroes, rather than interpolated. This assumption means that some observations are dropped in the log transformation. Column (4) uses an alternative construction of our instrument described in Appendix A.2. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5: Effect on tax payments: robustness checks

| *Log(Taxes)* | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Log(EPOS) | 0.020 | 0.018 | 0.040*** | 0.030*** | | 0.020 |
| | (0.015) | (0.014) | (0.014) | (0.011) | | (0.015) |
| Alt. Log(EPOS) | | | | | 0.019 | |
| | | | | | (0.014) | |
| Log(Nightlights) | 0.071* | 0.062* | | | 0.076** | 0.072* |
| | (0.037) | (0.036) | | | (0.037) | (0.037) |
| Alt. Log(Nightlights) | | | -0.022 | | | |
| | | | (0.022) | | | |
| Log(HH Income) | | | | 0.042 | | |
| | | | | (0.041) | | |
| Winsorization | 1% | 5% | None | None | None | None |
| Observations | 428785 | 428785 | 428785 | 408985 | 427829 | 428785 |
| First stage F-stat | 46.94 | 46.94 | 43.94 | 68.58 | 47.79 | 47.88 |
| Pre-demonetization mean | 9.70 | 9.65 | 9.71 | 9.70 | 9.71 | 9.71 |

*Notes:* The table shows robustness checks for specification eq:baseline. The outcome variable is log of tax payments, residualized of district×quarter-of-year fixed effects using data from the pre-demonetization period, and all specifications include firm and quarter fixed effects. Log EPOS measures total sales through EPOS machines in the firms' postcode, and is instrumented for using the demonetization exposure measure described in Appendix A.2. Columns (1) and (2) show results for reported sales at the firm level, where the outcome variable is winsorised at the 99th and 95th percentiles, respectively. Column (3) uses an alternative measure of nightlights which has not had outliers removed or been filtered of stray light. Column (4) controls for an alternative measure of economic activity: average household income at the district level according to Consumer Pyramids CMIE data. This data is missing for one district. Column (5) uses an alternative measure of EPOS, where missing values are assumed to be zeroes, rather than interpolated. This assumption means that some observations are dropped in the log transformation. Column (6) uses an alternative construction of our instrument described in Appendix A.2. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6: Firm-level evidence: tax return variables by sales quartile

| Sales quartile | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *A. Outcome: Log(Reported Sales)* | | | | |
| Log(EPOS) | 0.063** | 0.026 | -0.000 | -0.029 |
| | (0.031) | (0.020) | (0.014) | (0.022) |
| Log(Nightlights) | 0.069 | 0.038 | 0.036 | 0.062 |
| | (0.078) | (0.047) | (0.032) | (0.039) |
| Observations | 93936 | 153319 | 171352 | 176330 |
| First stage F-stat | 50.26 | 43.89 | 42.94 | 41.94 |
| Pre-demonetization mean | 11.28 | 12.94 | 14.25 | 16.24 |
| *B. Outcome: Taxes>0* | | | | |
| Log(EPOS) | 0.010 | 0.001 | 0.017* | 0.014* |
| | (0.008) | (0.007) | (0.009) | (0.007) |
| Log(Nightlights) | -0.026 | 0.005 | 0.011 | 0.013 |
| | (0.021) | (0.019) | (0.020) | (0.019) |
| Observations | 93936 | 153319 | 171352 | 176330 |
| First stage F-stat | 50.26 | 43.89 | 42.94 | 41.94 |
| Pre-demonetization mean | 0.77 | 0.73 | 0.68 | 0.68 |
| *C. Outcome: Log(Taxes)* | | | | |
| Log(EPOS) | -0.025 | -0.016 | 0.007 | 0.073*** |
| | (0.030) | (0.026) | (0.024) | (0.028) |
| Log(Nightlights) | 0.183** | 0.070 | 0.070 | -0.004 |
| | (0.086) | (0.063) | (0.058) | (0.070) |
| Observations | 72127 | 112503 | 119127 | 121191 |
| First stage F-stat | 49.65 | 44.49 | 42.56 | 40.07 |
| Pre-demonetization mean | 7.70 | 8.91 | 9.86 | 11.55 |

*Notes:* The table shows results from running specification (1) using different tax return variables as outcomes, separately for four groups of firms based on their average reported sales pre-demonetization. Column (1) shows the smallest 25% of firms while column (4) show the largest 25%. Observations are at the firm*quarter level, and all outcome variables are residualized by taking district*quarter-of-year fixed effects based on all pre-demonetization quarters. All specifications include year*quarter and firm fixed effect. Log EPOS measures total sales through EPOS machines in the firms' postcode, and is instrumented for using the demonetization exposure measure described in Appendix A.2. The sample includes all firms in the tax return data in the fiscal year 2015-2016 in the top two panel, and only firms with positive reported tax liability in the third panel. Sample sizes are not equal across columns because we only include firms filing tax returns with non-zero sales, and larger firms are more likely to do so. Standard errors clustered at the postcode level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.