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

Satadru Das, Lucie Gadenne, Tushar Nandi, Ross Warwick*

September 2022

Abstract

This paper investigates the effect of electronic payment technology on tax compliance in a large developing economy. We consider India's demonetization policy which, by limiting cash availability, 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. Our estimates imply that the shift to electronic payments increased reported sales by 5% despite demonetization's negative effect on economic activity.

JEL: H26, O23, H25.

Keywords: tax compliance, electronic payments, demonetization.

---

*Das: Reserve Bank of India. Gadenne: Queen Mary, University of London, Institute for Fiscal Studies and CEPR. Nandi: Indian Institute of Science Education and Research (IISER), Kolkata. Warwick: Institute for Fiscal Studies. We would like to thank Miguel Almunia, Michael Best, Anne Brockmeyer, Michael Devereux, Maitreesh Ghatak, Malay Ghosh, Hazel Granger, Jyotsna Jalan, Sudip Kumar Sinha, Sugata Marjit, Harshil Parekh, David Phillips, Victor Pouliquen, and numerous seminar participants for helpful comments. We are particularly thankful to the Reserve Bank of India and the Directorate of Commercial Taxes of West Bengal, India for the permission to use their data, to Robert Beyer and his team at the World Bank for sharing their nightlights data and to Crouzet et al. (2022) for sharing their data on exposure to demonetization. We gratefully acknowledge financial support from CAGE, the ESRC (grant reference ES/M010147/1), and FCDO through TaxDev at IFS. All errors are our own. This paper reflects the views of the authors and does not necessarily represent the views of Reserve Bank of India or any other institutions they are affiliated with.

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 third party 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., 2022; 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). Doubling the value of electronic payments in an area increased average reported sales by 2.8%. We find positive effects on the probability that firms report positive tax liabilities and on tax payments, but the latter are imprecise.

We leverage novel data to investigate the effect of electronic payments on firms' tax behavior. We use administrative data on firms' quarterly tax returns for the universe of firms paying Value Added Tax in West Bengal combined with data on electronic payments at the local level. The evolution of key variables over the period can be seen in Figure 1. There is a large increase in the sales firms report to the tax authority and their tax payments in the two quarters following the policy announcement (left panels).[1] Did this shift in payment technology change firms' tax compliance behavior? This is puzzling as the negative effect of demonetization on economic activity suggests *real* sales likely fell for the average firm. The right panels of Figure 1 offer a potential explanation for this puzzle: we see a large contemporaneous increase in electronic transaction amounts and the number of electronic payment machines in use from the month of the policy announcement.

To go beyond correlations in the time-series evidence, we apply an identification strategy similar to that of Chodorow-Reich et al. (2019) and Crouzet et al. (2022) 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 for demonetization's effect on real economic activity throughout using proxies obtained from different sources.

We find that the growth in electronic payments due to demonetization can explain roughly half of the large increase in reported sales and tax payments over the period seen in Figure 1, in line with the claim that the availability of third-party information increases compliance. Overall, our results are consistent with the idea that electronic payments increase the share of economic activity on which taxes are paid. This could reflect both higher compliance at the firm level and tax compliant firms that use electronic payments gaining market shares because of consumers' reluctance to use cash.

This paper's main contribution relates to the role of technology in improving state capacity in the developing world. Recent work shows that the use of new electronic technologies can substantially improve the delivery of public services (Muralidharan et al., 2016, 2020; Dodge et al., 2021), the quality of procurement (Lewis-Faupel et al., 2016) or tax revenues (Bellon et al., 2019; Fan et al., 2020; Ali et al., 2021; Okunogbe and Santoro, 2022). Research on taxation has emphasized the importance of third-party reporting in particular (Kleven et al., 2011); Pomeranz (2015) and Naritomi (2018) show that third-party reported transactions can increase tax compliance.

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

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.[2]

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; Adhikari et al., 2021). In the developing world, Brockmeyer and Saenz Somarriba (2021) find no effects on tax payments of a Uruguayan reform incentivizing the use of bank cards, whilst Bachas et al. (2020) find that Mexico's tax on cash failed to increase the use of electronic payments. The policy experiment we study in contrast led to a large shift towards digital payments.[3] Overall, our results provide tentative good news for the many developing countries that incentivize the use of electronic payments in the hope that these policies will 'pay for themselves' via compliance effects (see Brockmeyer and Saenz Somarriba, 2021, for a review of these policies) .

This paper's second contribution lies in its study of the effect of India's radical demonetization policy on tax compliance. The idea that demonetization would increase 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 main aim — the eradication of proceeds from the black economy. While our results suggest demonetization may have had positive effects on tax collection, the economic cost of the policy should be kept in mind while assessing its overall effect.

[2]See for example Gordon and Li (2009); Best et al. (2015); Brockmeyer and Hernandez (2016); Carrillo et al. (2017); Mittal and Mahajan (2017); Gadenne (2017); Jensen (2019); Pomeranz and Vila-Belda (2019); Bachas et al. (2021); Best et al. (2021); Okunogbe and Pouliquen (2021).

[3]Higgins (2020) finds that a rollout of debit cards in Mexico led small firms to formalize, though he cannot disentangle whether this is due to them using electronic payments or making higher profits.

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.[4]

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. Households and firms had until December 31 2016 to return the notes and were thus forced to deposit their cash into banks. 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 around 60% of them had been replaced (see Figure 1 in Chodorow-Reich et al. (2019)). 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. (2022), this led households to switch to electronic forms of payments like debit cards, credit cards and e-wallets, which can be used to pay via Electronic Point Of Sale (EPOS) machines.

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 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 motives for the policy).

[4]Amongst those, firms with a turnover of less than INR 5 million can choose to pay taxes under a simplified regime (Gadenne et al., 2021). The 10% of firms choosing this option are excluded from our analysis because they do not file quarterly returns.

How could an increase in electronic transactions increase tax compliance? We estimate that total electronic sales represent only a small share of sales reported to the tax authorities (see Appendix A.1.5). This indicates that firms typically pay taxes on more than just their electronic sales and is consistent with evidence in Gadenne et al. (2021) that firms pay taxes on sales that are much higher than their total third-party reported sales. We therefore do not expect firms to only increase their reported sales by the amount of extra electronic transactions they make during demonetization. Greater use of electronic transactions may increase reported sales more than one-for-one if firms suspect that information on electronic sales gives tax authorities a better proxy of their true sales, or provides them with more 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 described in more detail in Appendix A. 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. The main variables we use are total declared sales and total tax liability. 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'. 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 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, which means the data for fiscal year 2017-2018 is unavailable. 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 and thus cannot construct an instrument value for them (see below), and restrict our analysis to firms that registered by April 2016.

We therefore cannot study the effect of demonetization on new firms' decision to register. This restriction means that our results are obtained on a consistent sample of all firms that were already registered to pay taxes prior to the start of demonetization.

Second, we use data from the Reserve Bank of India (RBI) on the location of currency chests (see below) and bank deposits in March 2016. The latter contains information on all customer deposits in all Indian banks by bank type. Third, we use data from the National Payments Corporation of India 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.[5]

Finally, we follow Chodorow-Reich et al. (2019) in our choice of proxies for local real economic activity. Official data on economic activity at a subnational level and high frequency does not exist for India. Our first proxy comes from satellite data on human-generated nightlight activity, available at the district and quarter level. We use the data constructed by Beyer et al. (2018) to extend the analysis in Henderson et al. (2012) to South Asia.[6] Our second proxy is a measure of average household income at the district and quarter level obtained from the nationally representative monthly household survey conducted by the Centre for Monitoring the Indian Economy (CMIE).

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 of our specifications. Our main analysis sample contains 113,349 firms over 5 quarters. As shown in Figure A1, our key outcome variables display consistent patterns of seasonality. To account for this, we use the longer time period for which the administrative tax data is available. We run a regression of each tax return variable on quarter-of-year$\times$district fixed effects on the pre-demonetization period (April 2014 to September 2016) and use the residuals from these regressions in our analysis.[7] We similarly use the entire pre-demonetization period for which

[5]This covers electronic transactions that transit through India's RuPay system, which represent 30% of all electronic transactions in India. Evidence in Chodorow-Reich et al. (2019) indicates that the effect of demonetization on electronic transactions in the RuPay data is similar to that on other forms of electronic transactions. See Appendix A for more details.

[6]Beyer et al. (2018) show that this data provides a good proxy for economic activity and captures demonetization's negative effect on the economy. Our baseline analysis uses the data cleaned of outliers and stray light; we present robustness checks using the original variable.

[7]We similarly residualize the nightlights data by taking quarter-of-year$\times$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). There is no evidence

tax data is available (10 quarters) to construct a proxy for firm size, using average reported sales.

2.3 Time series evidence

We plot the evolution over time of key variables in Figure 1, where we indicate the first demonetization period by a solid line and the first and last periods of our estimation sample by dashed 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 10% increase when we control for local economic activity (see Table A4). There is a similarly large (9%) increase in tax payments. It is highly unlikely that this rise in average reported firm sales reflects a true increase in real economic activity, given the negative effect of demonetization on the economy. A potential alternative explanation is suggested in the graphs on the right, which plot the evolution of postcode-level EPOS variables, weighted by the number of firms in each postcode. The top graph shows a large increase in the amount of transactions going through electronic point of sales machines, equating to more than a 500% increase between 2016Q1-2016Q3 and 2016Q4-2017Q1 after controlling for economic activity.[8] There is both an increase in the number of firms using electronic payments and more intensive use of these forms of payments by firms which already had acquired the technology: the bottom-right graph shows an increase in the number of EPOS machines in use from the start of demonetization, albeit of a smaller size (130%) than the growth in EPOS transaction amounts.

Beyond the rise of electronic payments, another mechanism could have led to a short-term increase in both reported sales and tax payments. Indian law states that 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

of seasonality for the EPOS variables; residualizing these variables leaves our results unaffected (see Appendix Tables A1, A2 and A3). The household income data available to us only covers 2016 and 2017 and thus it is not possible to conduct the residualization using pre- or post-demonetization data only.

[8]Table A4 presents estimates from a regression of these variables on an indicator for the demonetization period and our GDP proxies. The estimate for the effect of demonetization on log electronic transactions is 1.91, which corresponds to a 575% increase.

demonetization to 'justify' abnormally large cash deposits. This behavior could in turn have led to higher tax payments as firms applied their existing VAT rates on this increase in reported sales. Note that there is no reason to expect this behavior to be correlated with the speed at which new notes became available locally, which we use for identification (see next section).

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, EP_{pt} are log electronic payments in period t and postcode p, γ_i and γ_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. (2022) 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 chests in a postcode using data 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). Our instrument is similar to that used by Crouzet et al. (2022): the only difference is that we use data at a more granular geographical level (postcode, not district). This enables us to better capture local variations in exposure to the shock at the cost of introducing some measurement error because some of the data is only available at the district level – see Appendix A.2 for a discussion and more details on the construction of the instrument. Using the Crouzet et al. (2022) instrument defined at the district level and clustering standard errors at the district level instead mostly affects the precision of our estimates (see Appendix Tables A1, A2 and A3).[9]

As shown in Chodorow-Reich et al. (2019), exposure to currency chests affected local economic growth as well as the speed at which agents adopted electronic payments. This likely affected firms' real tax liabilities and this could, in turn, lower the sales they report to the tax authorities. To capture the effect of electronic payment growth on outcomes conditional on local economic growth we therefore control throughout for the best available proxies for local economic activity (nightlights and local household income) described above. Our instrument's exclusion restriction requires that, once local economic effects are controlled for, exposure to currency chests during demonetization only affects firms' tax behavior via the change in payment technology; we consider the robustness of our results to alternative ways of controlling for these real economic effects. Controlling for local economic activity implies that any effect of

[9]Several recent papers use exposure to currency chests at the local area or firm level during demonetization to instrument for the use of electronic payments, see for example Aggarwal et al. (2020) and Ghosh et al. (2021). Chodorow-Reich et al. (2019) in addition use confidential RBI data on the speed of arrival of new notes in currency chests but that dataset is no longer available to outside researchers.

electronic payments on, for example, firm-level reported sales, should be interpreted as a compliance effect and not an effect on firm's real sales.

Because our instrument is defined at the postcode level, our estimates likely capture two related compliance effects affecting how much of local economic activity is effectively taxed. 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 shares at the expense of non-VAT-registered firms.[10]

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 areas with high and low shocks. There is a clear difference in the scale of growth in EPOS transaction values in areas with a low shock value compared to high shock areas. In high shock areas the total EPOS value peaks at around 180 log points higher than pre-demonetization; in low shock areas the average increase in EPOS sales is closer to 120 log points. On top of the exclusion restriction, our identifying assumption states 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 prior to November 2016 is reassuring in this respect.

Table 1 presents descriptive statistics for the pre-demonetization period. 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.[11] We conduct heterogeneity analysis by firm size below and present effects obtained by winsorizing the top 1% and 5% of firms as a robustness check. Nearly 30% of firms report a null or negative tax liability in any given period, so 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 (value of 1).

[10] A small share of these firms pay taxes under the simplified tax regime but the vast majority are informal and pay no taxes at all (Gadenne et al., 2021).

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

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 year×quarter fixed effects. Our preferred specification in column 1 uses both our proxies for the effect of demonetization on local economic activity (nightlights and household income); we present results obtained using each proxy in turn in columns 2 and 3. First stage results, presented in Table A5, 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%.

We find a positive effect of electronic payments on firm-level sales reported to the tax authorities in column (1) of Panel A: doubling the value of EPOS transactions increases reported sales by around 2.8%. We also see a small positive impact of electronic payments on the probability that firms report a positive tax liability in column (2), equating to 0.9 percentage points for a doubling in local EPOS. Among firms that report positive tax liabilities, we find a 1.5% increase in taxes paid in column (3), imprecisely estimated. This estimate is consistent with both a zero effect on tax liabilities and a positive effect of the same magnitude of the one on reported sales. Controlling for only one of our two proxies for economic activity in columns 2 and 3 hardly affects the results; this is reassuring as these variables are obtained from very different sources. Tables A1, A2 and A3 show in addition that results are robust to alternative ways of controlling for local economic activity.

The magnitude of the estimate of the effect on reported sales suggest 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 0.33% of reported sales prior to demonetization (see Appendix A.1.5); if firms only increased their reported sales in line with their electronic sales, we would only observe an 0.33% increase in sales when EPOS transactions double. Our estimate is more than eight 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.[12]

Appendix Table A1 presents additional robustness checks for our main estimate of the effect on reported sales. Estimates are unaffected by: winsorizing the top 1% or 5% observations; using EPOS variables that are not interpolated for missing values or that are residualized by taking district×quarter-of-year fixed effects; changing the method used to construct our instrument; using instrument values at the district level and clustering at that level following Crouzet et al. (2022); or allowing for district×quarter-of-year fixed effects (see Appendix A for more details). Estimates of the effects on both tax liability variables are often larger and/or more precisely estimated across these robustness checks than with our baseline specification (see Appendix Tables A2 and A3). Across most specifications, we see an increase in the share of firms paying positive taxes of 0.8-1.2 percentage point, and a rise in tax payments in the 1.5-4% range.[13]

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 files taxes (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).[14] We do, however, see that firms faced with higher electronic payment growth 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 is similar to results 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 our context firms have limited discretion regarding how much input tax they can claim back because the tax considered is a VAT: their claims are systematically cross-checked against the tax paid by their suppliers (Gadenne et al.,

[12]There is evidence of strategic shopping during the short period (4 hours) between the policy announcement and the time at which the old notes stopped being legal tender (Kim et al., 2022). This short-run effect was likely too small to explain our results during the first quarter immediately after the policy announcement, and cannot explain any of the observed increased during the second quarter after demonetization.

[13]The only exception are the estimates for positive tax liability obtained using an instrument defined at the district level, which are very imprecise. This is expected as there are only 23 districts in West Bengal.

[14]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.

2021). These results are consistent however with firms choosing to buy a higher share of their inputs from VAT-paying suppliers and/or these firms' suppliers becoming more likely to pay VAT, and may explain why the increased in reported sales we observed does not lead to a clear increase in tax liabilities.[15]

Finally, we consider whether there is evidence of heterogeneous effects by 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. 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.

How much of the increase in reported sales and taxes paid during the demonetization period in Figure 1 could be explained by our results? EPOS transactions increased by more than 500% during the period; using our baseline estimate for the effect on reported sales of 0.028, the increase in EPOS transactions leads to a 5% increase in reported sales, roughly half of the total increase observed during demonetization.[16] 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 in Figure 1.

[15] We cannot test this directly as the transaction data used in Gadenne et al. (2021) isn't available at the quarterly level but the evidence in this paper that firms' tax decisions are linked within supply chains is in line with this interpretation.

[16] 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 $\beta = 0.028$ 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 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 a positive effect on the probability that firms report positive tax liabilities, and positive but imprecise effects 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 speculate on the likelihood that this aim was reached by considering the evolution of electronic payments for 2.5 years after demonetization in Figure 1: we see that the use of electronic payments remained at a higher level in West Bengal even after the end of our period of study, consistent with the argument in Crouzet et al. (2022) that the temporary shock had a persistent effect on payment technology. Assuming the effect of electronic payments we observe in the short run persists, demonetization may have had a positive effect on tax compliance in the medium run. The medium-run effects of changes in payment technology on firm compliance, as both firms and tax authorities find out more about the monitoring potential of the new technology, remains an fruitful open question for future research.

References

Adhikari, B., J. Alm, and T. F. Harris (2021): "Small business tax compliance under third-party reporting," *Journal of Public Economics*, 203, 104514.

Aggarwal, B., N. Kulkarni, and S. Ritadhi (2020): "Cash is King: The Role of Financial Infrastructure in Digital Adoption," Tech. rep.

Ali, M., A. B. Shifa, A. Shimeles, and F. Woldeyes (2021): "Building Fiscal

Capacity in Developing Countries: Evidence on the Role of Information Technology," *National Tax Journal*, 74, 591–620.

Almunia, M., J. Hjor, J. Knebelmann, and L. Tian (2019): "Strategic or Confused Firms? Evidence from "Missing" Transactions in Uganda," Tech. rep., Mimeo, Columbia University.

Awasthi, R. and M. Engelschalk (2018): "Taxation and the shadow economy : how the tax system can stimulate and enforce the formalization of business activities," Policy Research Working Paper Series 8391, The World Bank.

Bachas, P., L. Gadenne, and A. Jensen (2021): "Informality, Consumption Taxes, and Redistribution," Working Paper 27429, National Bureau of Economic Research.

Bachas, P., S. Higgins, and A. Jensen (2020): "Towards a Cashless Economy? Evidence from the Elasticity of Cash Deposits of Mexican Firms," Tech. rep., Mimeo, World Bank.

Banerjee, A. and N. Kala (2017): "The Economic and Political Consequences of India's Demonetization," Tech. rep.

Bellon, M. M., F. Lima, S. Khalid, J. Chang, E. Rojas, P. Villena, and M. E. Dabla-Norris (2019): "Digitalization to Improve Tax Compliance: Evidence from VAT e-Invoicing in Peru," IMF Working Papers 2019/231, International Monetary Fund.

Best, M., A. Brockmeyer, H. J. Kleven, J. Spinnewijn, and M. Waseem (2015): "Production vs Revenue Efficiency With Limited Tax Capacity: Theory and Evidence From Pakistan," *Journal of Political Economy*, 123.

Best, M., J. Shah, and M. Waseem (2021): "Detection Without Deterrence: Long-Run Effects of Tax Audit on Firm Behavior," Tech. rep., Mimeo, University of Manchester.

Beyer, R. C. M., E. Chhabra, V. Galdo, and M. G. Rama (2018): "Measuring districts'monthly economic activity from outer space," Policy Research Working Paper Series 8523, The World Bank.

Brockmeyer, A. and M. Hernandez (2016): “Taxation, information, and withholding : evidence from Costa Rica,” Policy Research Working Paper Series 7600, The World Bank.

Brockmeyer, A. and M. Saenz Somarriba (2021): “Electronic Payment Technology and Tax Compliance: Evidence from a Financial Inclusion Reform,” Tech. rep.

Carrillo, P., D. Pomeranz, and M. Singhal (2017): “Dodging the Taxman: Firm Misreporting and Limits to Tax Enforcement,” *American Economic Journal: Applied Economics*, 9, 144–164.

Chodorow-Reich, G., G. Gopinath, P. Mishra, and A. Narayanan (2019): “Cash and the Economy: Evidence from India’s Demonetization*,” *The Quarterly Journal of Economics*, 135, 57–103.

Crouzet, N., A. Gupta, and F. Mezzanotti (2022): “Shocks and Technology Adoption: Evidence from Electronic Payment Systems,” Tech. rep., Mimeo, Kellog School of Management.

Demirguc-Kunt, A., L. Klapper, D. Singer, A. Saniya, and H. Jake (2018): “The Global Findex Database 2017: Measuring Financial Inclusion and the Fintech Revolution,” Working Papers id:12735, eSocialSciences.

Dodge, E., Y. Neggers, R. Pande, and C. T. Moore (2021): “Updating the State: Information Acquisition Costs and Public Benefit Delivery,” Tech. rep., EDI Working Paper Series.

Fan, H., Y. Liu, N. Qian, and J. Wen (2020): “Computerizing VAT Invoices in China,” Working Paper 24414, National Bureau of Economic Research.

Gadenne, L. (2017): “Tax Me, but Spend Wisely? Sources of Public Finance and Government Accountability,” *American Economic Journal: Applied Economics*, 9, 274–314.

Gadenne, L., T. Nandi, and R. Rathelot (2021): “Taxation and Supplier Networks: Evidence from India,” Tech. rep., Mimeo, University of Warwick.

Garg, S. and S. Gupta (2020): "Financial Access of Unbanked Villages in India from 1951 to 2019: A Spatial Approach," Tech. rep.

Ghosh, P., B. Vallee, and Y. Zeng (2021): "FinTech Lending and Cashless Payments," *SSRN Electronic Journal*.

Gordon, R. and W. Li (2009): "Tax Structures in Developing Countries: Many Puzzles and a Possible Explanation," *Journal of Public Economics*, 93, 855–866.

Henderson, J. V., A. Storeygard, and D. N. Weil (2012): "Measuring Economic Growth from Outer Space," *American Economic Review*, 102, 994–1028.

Higgins, S. (2020): "Financial Technology Adoption," Tech. rep., Mimeo, Northwestern University.

Jensen, A. (2019): "Employment Structure and the Rise of the Modern Tax System," NBER Working Papers 25502, National Bureau of Economic Research, Inc.

Karmakar, S. and A. Narayanan (2020): "Do households care about cash? Exploring the heterogeneous effects of India's demonetization," *Journal of Asian Economics*, 69.

Kim, Y., P. K. Chintagunta, and B. Pareek (2022): "Government Policy, Strategic Consumer Behavior, and Spillovers to Retailers: The Case of Demonetization in India," *Marketing Science*.

Kleven, H. J., M. B. Knudsen, C. T. Kreiner, S. Pedersen, and E. Saez (2011): "Unwilling or Unable to Cheat? Evidence From a Tax Audit Experiment in Denmark," *Econometrica*, 79, 651–692.

Kleven, H. J., C. T. Kreiner, and E. Saez (2016): "Why Can Modern Governments Tax So Much? An Agency Model of Firms as Fiscal Intermediaries," *Economica*, 83, 219–246.

Lahiri, A. (2020): "The Great Indian Demonetization," *Journal of Economic Perspectives*, 34, 55–74.

Lewis-Faupel, S., Y. Neggers, B. A. Olken, and R. Pande (2016): "Can Electronic Procurement Improve Infrastructure Provision? Evidence from Public

Works in India and Indonesia," *American Economic Journal: Economic Policy*, 8, 258–83.

Mittal, S. and A. Mahajan (2017): "VAT in Emerging Economies: Does Third Party Verification Matter?" Tech. rep.

Muralidharan, K., P. Niehaus, and S. Sukhtankar (2016): "Building State Capacity: Evidence from Biometric Smartcards in India," *American Economic Review*, 106, 2895–2929.

——— (2020): "Identity Verification Standards in Welfare Programs: Experimental Evidence from India," NBER Working Papers 26744, National Bureau of Economic Research, Inc.

Naritomi, J. (2018): "Consumers as Tax Auditors," Mimeo, London School of Economics.

OECD (2017): "Shining Light on the Shadow Economy: Opportunities and Threats," Tech. rep., Organization for Economic Development and Cooperation.

Okunogbe, O. and V. Pouliquen (2021): "Technology, taxation, and corruption: evidence from the introduction of electronic tax filing," *American Economic Journal: Economic Policy*.

Okunogbe, O. and F. Santoro (2022): "The Promise and Limitations of Information Technology for Tax Mobilization," Policy Research Working Paper Series 9848, The World Bank.

Pomeranz, D. (2015): "No Taxation without Information: Deterrence and Self-Enforcement in the Value Added Tax," *American Economic Review*, 105, 2539–2569.

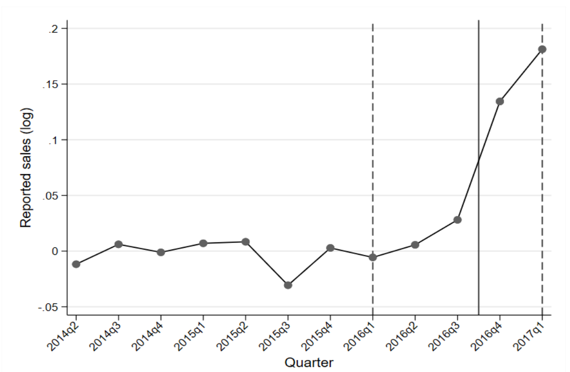
Pomeranz, D. and J. Vila-Belda (2019): "Taking State-Capacity Research to the Field: Insights from Collaborations with Tax Authorities," *Annual Review of Economics*, 11, 755–781.

Rogoff, K. (2016): *The Curse of Cash*, Princeton University Press.

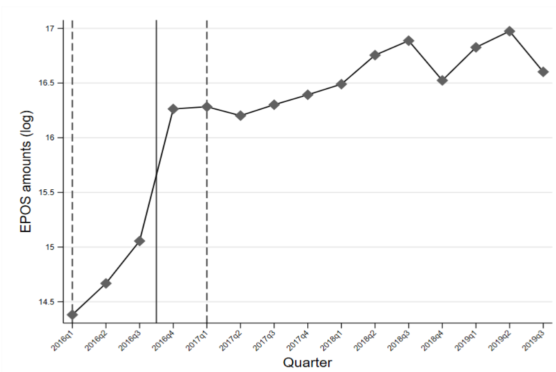
Slemrod, J., B. Collins, J. L. Hoopes, D. Reck, and M. Sebastiani (2017): "Does credit-card information reporting improve small-business tax compliance?" *Journal of Public Economics*, 149, 1 – 19.

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

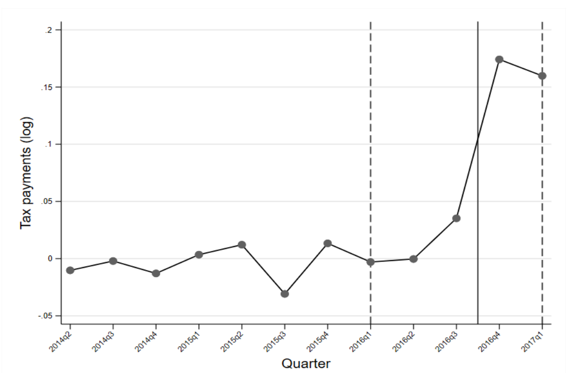
(a) Reported sales



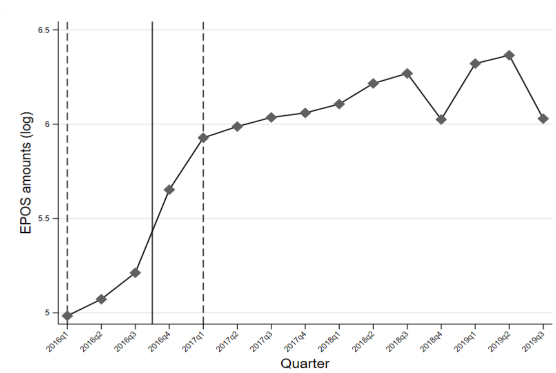
(b) EPOS transaction amounts



(c) Tax payments

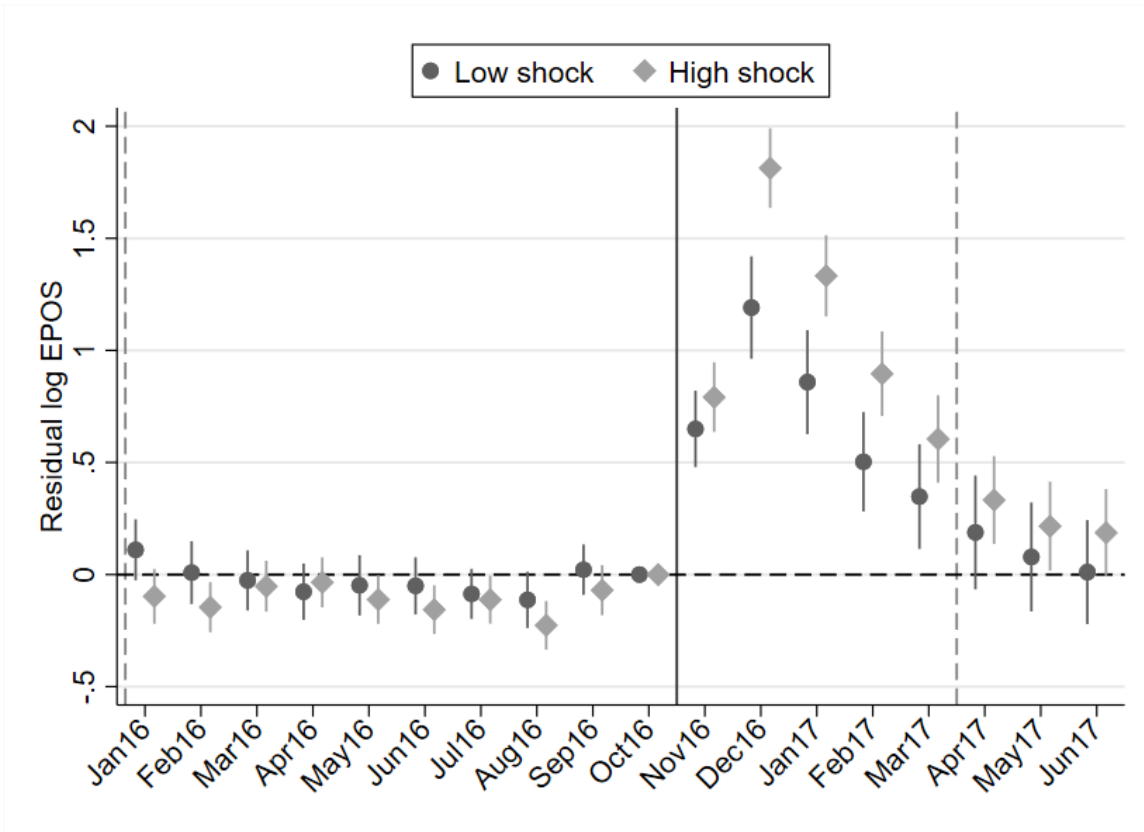


(d) EPOS machines in use



Notes: These graphs plot the evolution of key variables over time for the period during which they are available in our data, with each point an average across firms for each period (quarter). The vertical dashed lines indicate the start and end of the period defining our estimation sample; the vertical solid line indicates the start of demonetization. Graphs on the left (with circles) plot the evolution of tax return variables which vary at the firm level: 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 (with diamonds) plot the evolution of electronic payments which vary at the postcode level: log aggregate transaction amounts (top graph) and log total electronic payments machines in use (bottom graph). 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 vertical dashed lines indicate the start and end of the period defining our estimation sample and the vertical solid 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, 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. Figure A3 shows the evolution of these residuals obtained using different estimation approaches (weighting each postcode by the number of firms in the postcode or excluding the time trend), the difference between low and high shock areas is clear regardless of the approach used.

Table 1: Descriptive statistics pre-demonetization

	Mean	Std.Dev.	Median
Reported sales (millions)	14.666	226.709	1.202
Positive tax liability	0.708	0.455	1.000
Tax liability (millions)	0.452	9.238	0.015
Local EPOS sales (millions)	7.781	9.863	4.092
Demonetization shock	0.483	0.275	0.515
Nightlights per sq. km (log)	3.381	2.034	2.672
Household income (log)	9.534	0.284	9.550

Notes: The table shows descriptive statistics for the first three quarters of 2016, the pre-demonetization quarters in our main period of study. 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)	0.028**	0.030**	0.036***
	(0.014)	(0.014)	(0.012)
Observations	566745	566745	566745
First stage F-stat	48.71	43.12	69.33
GDP Proxy	Both	Nightlights	Income
B. Outcome: Tax Liability > 0	(1)	(2)	(3)
Log(EPOS)	0.861*	0.816*	0.987**
	(0.486)	(0.478)	(0.385)
Observations	566745	566745	566745
First stage F-stat	48.71	43.12	69.33
GDP Proxy	Both	Nightlights	Income
C. Outcome: Log(Tax Liability)	(1)	(2)	(3)
Log(EPOS)	0.015	0.018	0.031***
	(0.015)	(0.015)	(0.012)
Observations	405345	405345	405345
First stage F-stat	47.37	43.41	68.15
GDP Proxy	Both	Nightlights	Income

Notes: The table shows results from specification (1) using different tax return variables as outcomes; robustness checks can be found in Tables A1, A2 and A3. Observations are at the firm×quarter level, and all outcomes are residualized by taking district×quarter-of-year fixed effects based on all pre-demonetization quarters (see the text for more details). The first column includes our two proxies for local GDP as controls (nighlights and household income), only the nightlights (income) variable is used in the second (third) column. All specifications include year×quarter and firm fixed effects. Log EPOS is the log of electronic sales in the firm's postcode and is instrumented for using the demonetization shock variable, described in the text and in Appendix A.2, interacted with an indicator for the demonetization period. The sample includes all firms filing positive sales 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

	(1) Filing	(2) Inputs>0	(3) Log(Inputs)	(4) Output ETR	(5) Input ETR
Log(EPOS)	0.219	0.897**	0.023	0.011	-0.015
	(0.334)	(0.368)	(0.017)	(0.015)	(0.020)
Observations	642354	566745	482940	566745	482940
First stage F-stat	50.85	48.71	48.91	48.71	48.91
Pre-demonetization mean	88.25	84.65	13.71	6.10	6.50

Notes: The table shows results obtained from specification (1) using different tax return variables as outcomes. Observations are at the firm×quarter level, and all outcomes 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 and control for our two proxies for economic activity (nightlights and income). The variable 'Filing' is equal to 100 if the firm submits a tax return with positive sales; all other columns include only observations where this is equal to 100. The outcome in the second column is equal to 100 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, in percentages) reported by firms, calculated as output tax divided by total sales and input tax credit divided by total inputs, respectively. Log EPOS is the log of electronic sales in the firm's postcode and is instrumented for using the demonetization shock variable, described in the text and in Appendix A.2, interacted with an indicator for the demonetization period. 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$\times$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 100 if VAT return with positive sales submitted, and 0 otherwise.
- **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.

In addition, we obtain the postcode in which the firm is registered from the tax registration data.

A.1.2 Nightlights data

We use data on 'nightlights' (sum of lights per square kilometre in nanowatts) constructed and graciously shared by Beyer et al. (2018) to proxy for local economic activity. This data is available at the district×month level from January 2012 to April 2018; we obtain quarterly variables by taking averages. As is standard in the literature we account for seasonality by using residuals from a regression of log night-lights on district×quarter fixed effects using the full pre-demonetization period. We use two variables provided by Beyer et al. (2018): the first is treated by removing outliers and clustered noise, the second is a 'raw' version. We use the first variable as baseline and the second variable as a robustness check (see Appendix Tables A1, A2 andA3).

A.1.3 Income data

Data from the 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 for roughly 100,000 households. Each household is visited every three or four months: we aggregate data at the district×quarter level to obtain our proxy, for each quarter from from 2016Q1 to 2017Q4. The variable we use is mean household income from all sources.

A.1.4 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.1.5 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 the total value of EPOS transactions recorded at the month and postcode level from January 2016 to August 2019, and aggregate it to the quarterly level. This data contains information on most transactions that transit through India's domestic payment system, RuPay. RuPay has a lower processing fee and typically faster processing times than its international competitors (Visa and Mastercard) but is more recent (it was launched in 2012) and has a smaller, but growing, market share. Internal estimates by the RBI shared with the research team indicate that 30% of all electronic transactions in India (including e-Wallet payments) are captured in the RuPay data. Data on use of RuPay by transaction or merchant type isn't available, but its characteristics (and internal RBI analysis) suggest it is likely to be more popular amongst customers banking with state-owned banks that are poorer and less urban than the average. We note that Chodorow-Reich et al. (2019) find effects of the demonetization shock on electronic transactions that are similar when they use this data and when they use e-wallet payments data provided by a private firms; this suggests that our data is reasonably representative of the type of electronic transactions likely to have been affected by the demonetization shock.

26% of monthly observations are missing for both our variables of interest. Internal RBI information suggests these are missing for technical reasons and not because there are no electronic transactions in that month and postcode. We therefore linearly interpolate these observations for our baseline results, and present results assuming they are zero instead as a robustness check (see Appendix Tables A1, A2 andA3).

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 0.1%. This corresponds to a share of total EPOS transactions in reported sales of roughly 0.33% (0.1/0.30).

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 (stable over time during our period of study) 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 23 districts in West Bengal, and our data includes 343 taluks and 1,013 postcodes in our main sample. Our understanding from the field and talking to RBI staff 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 but not outside their taluk to retrieve cash. To obtain a proxy for total deposits per bank group g at the taluk τ 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 τ. 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. We have data on firm and chest locations at the postcode level, so need to allocate each of these postcodes to 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 as a robustness check (see Appendix Tables A1, A2 and A3).

Our instrument is therefore constructed using the same data and method as Crouzet et al. (2022), except that they aggregate all postcode-level information to the district level, and their instrument variable only varies across districts. Our method instead enables us to construct an instrument variable that varies across postcodes. This captures variations in households' exposure to the new banknotes which is both economically meaningful and useful in a context with only 23 districts (Crouzet et al., 2022, consider the whole of India), at the cost of introducing some measurement error. We consider results obtained using the district-level Crouzet et al. (2022) variable instead (and clustering standard errors at the district rather than postcode level) as a robustness check (see Appendix Tables A1, A2 andA3). The district-level variable was graciously shared with us by Crouzet et al. (2022) but their data did not include Kolkata so we constructed it ourselves using the RBI data; the correlation between our variable and theirs is 0.94.

B Additional Tables and Figures

Table A1: Effect on reported sales: robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Log(EPOS)	0.028**	0.029**	0.032**	0.028**	0.028**	0.030**	0.045***	0.030**	0.030*	0.017
	(0.014)	(0.014)	(0.013)	(0.013)	(0.014)	(0.014)	(0.014)	(0.013)	(0.018)	(0.018)
Obs	566745	566745	566745	565332	566745	566745	566745	566745	566745	566745
F-stat	48.71	48.71	48.71	49.55	48.71	51.33	78.37	44.50	23.24	23.66
Winsorized	No	1%	5%	No	No	No	No	No	No	No
EPOS	Baseline	Baseline	Baseline	No ipo	Resid	Baseline	Baseline	Baseline	Baseline	Baseline
Instrument	Baseline	Baseline	Baseline	Baseline	Baseline	Alt	District	Baseline	Baseline	Baseline
GDP Proxy	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Raw	Poly	Baseline
Q*Dist FE	No	No	No	No	No	No	No	No	No	Yes

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 year×quarter fixed effects and control for our two proxies for economic activity (nightlights and income). Column (1) shows our baseline results from column (1) of Table 2. Columns (2) and (3) show the same results but with the outcome variable winsorized at the 99th and 95th percentiles, respectively. Columns (4) and (5) alter our EPOS measure: the former sets any missing values in the raw data to zero rather than interpolating them, and the latter residualizes the EPOS data of district×quarter-of-year fixed effects using data from July 2017 onwards. Column (6) uses an alternative construction of our instrumental variable, as described in Appendix A.2 and column (7) constructs the same variable at the district level, as in Crouzet et al. (2022). Column (8) uses an alternative nighlights variable, based on the 'raw' data before treatment for outliers and stray light. Column (9) uses a second-order polynomial specification to control for both our proxies for local economic activity. Column (10) adds district×quarter-of-year fixed effects, in addition to using these in residualization. Standard errors, shown in parentheses, are clustered at the level of the instrument: at the postcode level in all cases except column (7), where clustering is at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A2: Effect on positive tax liability: robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log(EPOS)	0.861*	0.778*	0.861*	0.776*	0.534	1.146**	0.918	1.170
	(0.486)	(0.466)	(0.486)	(0.465)	(0.493)	(0.498)	(0.734)	(0.805)
Obs	566745	565332	566745	566745	566745	566745	566745	566745
F-stat	48.71	49.55	48.71	51.33	78.37	44.50	23.24	23.66
Winsorized	No	No	No	No	No	No	No	No
EPOS	Baseline	No ipo	Resid	Baseline	Baseline	Baseline	Baseline	Baseline
Instrument	Baseline	Baseline	Baseline	Alt	District	Baseline	Baseline	Baseline
GDP Proxy	Baseline	Baseline	Baseline	Baseline	Baseline	Raw	Poly	Baseline
Q*Dist FE	No	No	No	No	No	No	No	Yes

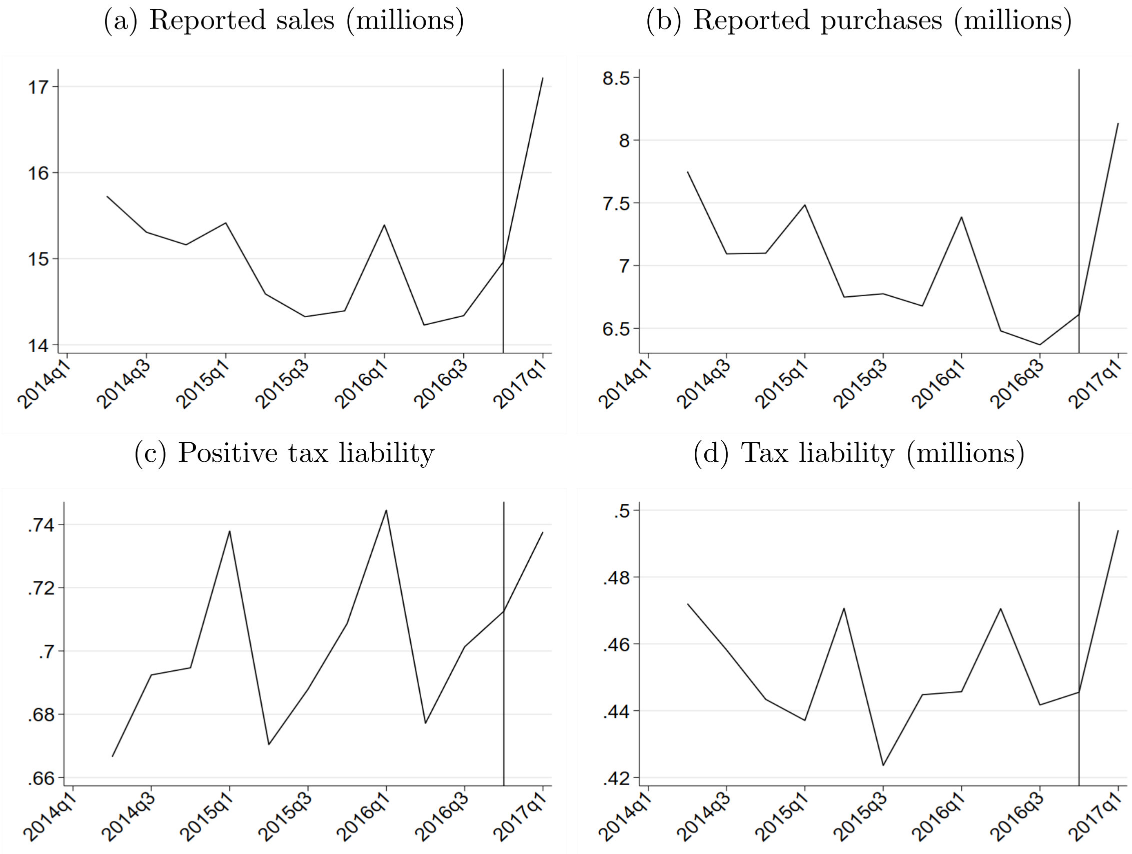
Notes: The table shows robustness checks for specification (1). The outcome variable is equal to 100 if the firm reports a positive net tax liability and 0 otherwise, residualized of district×quarter-of-year fixed effects using data from the pre-demonetization period, and all specifications include firm and year×quarter fixed effects and control for our two proxies for economic activity (nightlights and income). Column (1) shows our baseline results from column (1) of Table 2. Columns (2) and (3) alter our EPOS measure: the former sets any missing values in the raw data to zero rather than interpolating them, and the latter residualizes the EPOS data of district×quarter-of-year fixed effects using data from July 2017 onwards. Column (4) uses an alternative construction of our instrumental variable, as described in Appendix A.2 and column (5) constructs the same variable at the district level, as in Crouzet et al. (2022). Column (6) uses an alternative nighlights variable, based on the 'raw' data before treatment for outliers and stray light. Column (7) uses a second-order polynomial specification to control for both our proxies for local economic activity. Column (8) adds district×quarter-of-year fixed effects, in addition to using these in residualization. Standard errors, shown in parentheses, are clustered at the level of the instrument: at the postcode level in all cases except column (5), where clustering is at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table A3: Effect on tax payments: robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Log(EPOS)	0.015	0.016	0.015	0.015	0.015	0.016	0.012	0.037**	0.022	0.028
	(0.015)	(0.015)	(0.014)	(0.014)	(0.015)	(0.015)	(0.016)	(0.015)	(0.021)	(0.019)
Obs	405345	405345	405345	404416	405345	405345	405345	405345	405345	405345
F-stat	47.37	47.37	47.37	48.35	47.37	49.12	76.57	43.38	22.86	22.33
Winsorized	No	1%	5%	No	No	No	No	No	No	No
EPOS	Baseline	Baseline	Baseline	No ipo	Resid	Baseline	Baseline	Baseline	Baseline	Baseline
Instrument	Baseline	Baseline	Baseline	Baseline	Baseline	Alt	District	Baseline	Baseline	Baseline
GDP Proxy	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Baseline	Raw	Poly	Baseline
Q*Dist FE	No	No	No	No	No	No	No	No	No	Yes

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 year×quarter fixed effects and control for our two proxies for economic activity (nightlights and income). Column (1) shows our baseline results from column (1) of Table 2. Columns (2) and (3) show the same results but with the outcome variable winsorized at the 99th and 95th percentiles, respectively. Columns (4) and (5) alter our EPOS measure: the former sets any missing values in the raw data to zero rather than interpolating them, and the latter residualizes the EPOS data of district×quarter-of-year fixed effects using data from July 2017 onwards. Column (6) uses an alternative construction of our instrumental variable, as described in Appendix A.2 and column (7) constructs the same variable at the district level, as in Crouzet et al. (2022). Column (8) uses an alternative nighlights variable, based on the 'raw' data before treatment for outliers and stray light. Column (9) uses a second-order polynomial specification to control for both our proxies for local economic activity. Column (10) adds district×quarter-of-year fixed effects, in addition to using these in residualization. Standard errors, shown in parentheses, are clustered at the level of the instrument: at the postcode level in all cases except column (7), where clustering is at the district level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Figure A1: Seasonality in outcome variables



(a) Reported sales (millions)

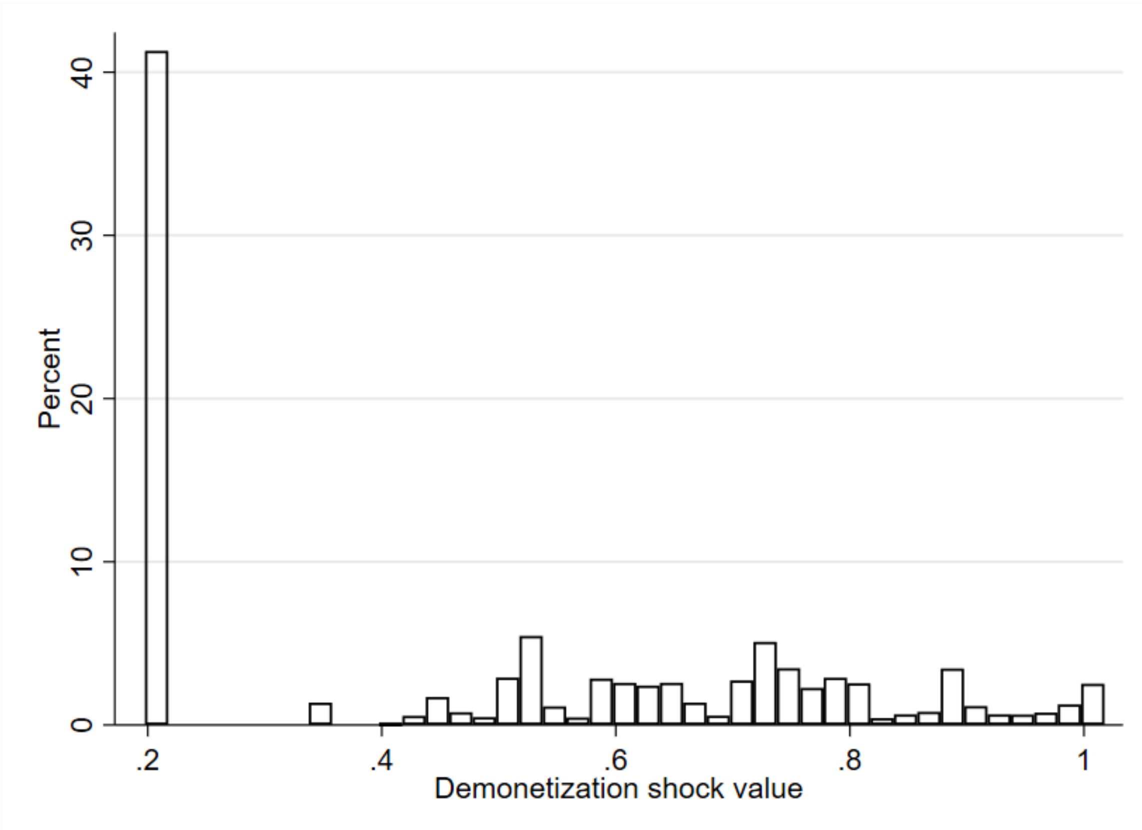
(b) Reported purchases (millions)

(c) Positive tax liability

(d) 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. All monetary values are shown in 2015 terms using the all-India quarterly CPI. The vertical line indicates the first quarter of demonetization. All four outcomes are measured at the firm level; see the text for a description of the variables and data sources.

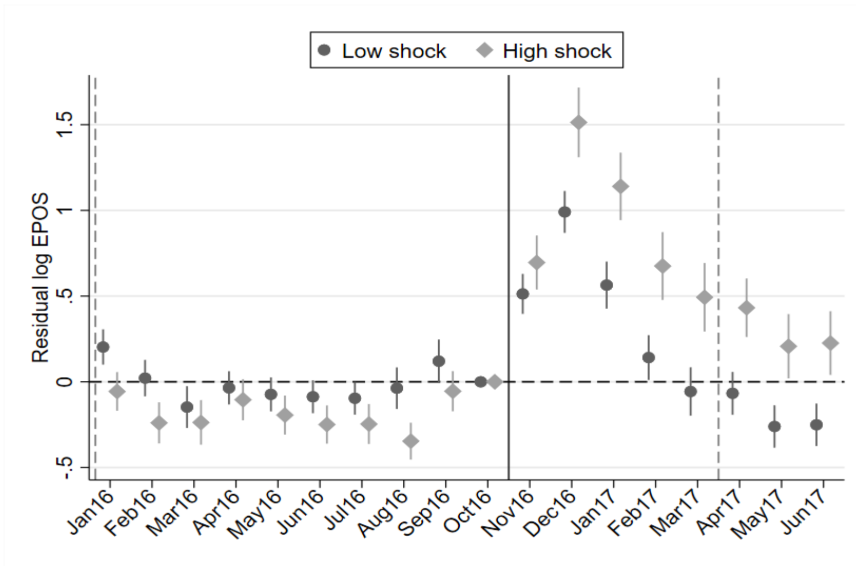
Figure A2: Distribution of demonetization shock



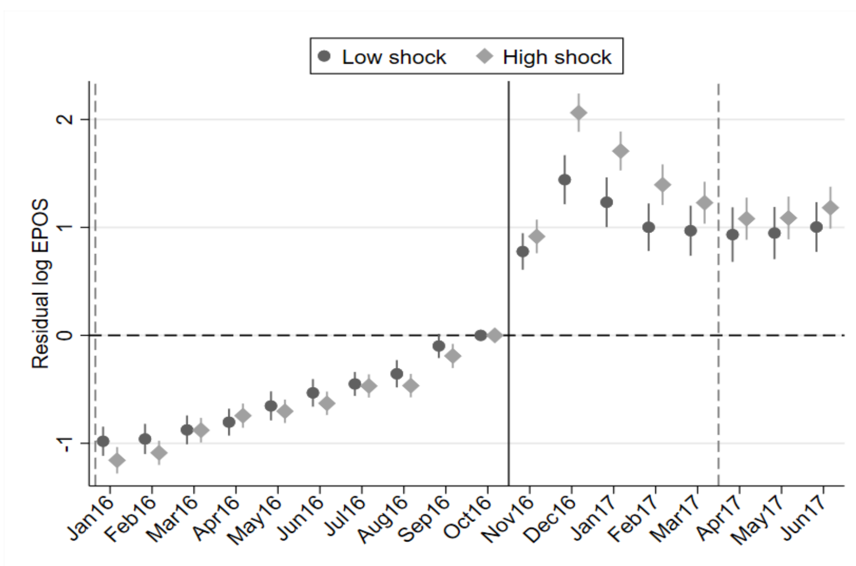
Notes: The graph shows the distribution of the demonetization shock variable with each firm in our main estimation sample appearing once. The variable is defined as one minus the market share of local banks (at the postcode level) with a currency chest and is constructed as described in Appendix A.2. The mode corresponds to firms in Kolkata.

Figure A3: Alternative illustrations of electronic payments in low and high demonetization shock areas

(a) Weighted by number of firms



(b) No time trend



Notes: These graphs show similar estimates to Figure 2; see the notes below that Figure for details. Here, in the top panel postcodes are weighted by the number of firms observed in our main estimation sample. In the bottom panel, no time trend is used to generate the plotted residuals.

Table A4: Time series evidence

A. Tax return variables	Log(Reported Sales)	Taxes>0	Log(Taxes)
Demonetization	0.094***	0.679***	0.088***
	(0.005)	(0.177)	(0.007)
Log(Nightlights)	-0.046***	1.484***	-0.086***
	(0.010)	(0.431)	(0.016)
Log(Income)	0.159***	-7.509***	0.045
	(0.024)	(1.012)	(0.043)
Observations	566745	566745	405345
Pre-demonetization mean	14.02	70.87	9.70

B. EPOS variables	Log(EPOS)	Log(Machines)	Log(Transactions)
Demonetization	1.909***	0.834***	1.768***
	(0.046)	(0.018)	(0.055)
Log(Nightlights)	1.572***	0.759***	1.468***
	(0.126)	(0.043)	(0.136)
Log(Income)	-1.020***	0.135	-0.734**
	(0.320)	(0.101)	(0.323)
Observations	566745	566735	566745
Pre-demonetization mean	14.70	5.09	7.03

Notes: The table presents estimates of a regression of the outcome variable in the column header on log nightlights, log household income and an indicator for the demonetization period (the last quarter of 2016 and the first quarter of 2017) for firms in our estimation sample. Observations are at the firm×quarter level, though EPOS variables are defined at the postcode×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 A5: First stage results

Outcome: Log(EPOS)	(1)	(2)	(3)
Demonetization shock	1.163***	1.147***	1.351***
	(0.167)	(0.175)	(0.162)
Log(Nightlights)	0.743***	0.770***	
	(0.258)	(0.259)	
Log(Income)	-0.234		-0.374
	(0.378)		(0.370)
2016Q4	1.449***	1.452***	1.323***
	(0.0715)	(0.0720)	(0.0597)
2017Q1	1.340***	1.302***	1.437***
	(0.0913)	(0.0658)	(0.0847)
N	566745	566745	566745
F-stat	48.71	43.12	69.33

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 measures total sales via electronic payments at the postcode level in a quarter, 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. The F-statistic reported pertains to only our instrumental variable. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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

Sales quartile	(1)	(2)	(3)	(4)
A. Outcome: Log(Reported Sales)				
Log(EPOS)	0.057*	0.027	-0.003	-0.033
	(0.032)	(0.020)	(0.014)	(0.023)
Observations	90317	146591	163129	166660
First stage F-stat	54.96	48.49	45.13	41.08
Pre demonetization mean	11.27	12.93	14.25	16.25
B. Outcome: Taxes>0				
Log(EPOS)	0.005	0.002	0.019**	0.014*
	(0.008)	(0.008)	(0.009)	(0.007)
Observations	90317	146591	163129	166660
First stage F-stat	54.96	48.49	45.13	41.08
Pre demonetization mean	0.77	0.73	0.68	0.68
C. Outcome: Log(Taxes)				
Log(EPOS)	-0.035	-0.016	-0.003	0.065**
	(0.032)	(0.026)	(0.025)	(0.028)
Observations	69541	107651	113349	114779
First stage F-stat	53.96	46.99	42.79	38.79
Pre demonetization mean	7.69	8.91	9.85	11.55

Notes: The table shows results from 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) shows 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 effects and control for our two proxies for economic activity (nightlights and income). 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. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.