# Taxing the untaxed? The Lasting Effects of India's 2016 Demonetization on Tax Collection

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Working Paper Series Vol. 2025-16

September 2025



**Asian Growth Research Institute**

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**Abstract**

*This paper evaluates the lasting impact of India's 2016 demonetization on tax collection. Using the synthetic control method and cross-country panel data from 2005–2022, we find a statistically significant and persistent increase in sales and production tax revenues, but no comparable change in income and profits tax collections. Mechanism tests suggest that cash usage fell and digital payments rose during 2016 and 2017, but both returned to pre-demonetization levels from 2018 onward. The size of the informal economy remained unchanged. These findings cast doubt on demonetization's effectiveness as a tool for broadening the tax base and reducing informality. The sustained rise in indirect tax revenues is more plausibly attributable to concurrent structural reforms, particularly the Goods and Services Tax, rather than to demonetization itself. By distinguishing between short-term payment disruptions and long-term revenue effects, this study provides new evidence on the limits of one-off policy shocks in achieving lasting tax compliance.*

Keywords: India demonetization; tax collection; tax compliance; digital payment; synthetic control method

JEL classification: E02, H26, O23

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## 1. Introduction

Mobilizing sufficient fiscal resources remains a central challenge for many low- and middle-income countries. Persistently low tax-to-GDP ratios reflect narrow tax bases and weak enforcement, which in turn limit the governments' ability to finance public goods and long-term growth. In 2020, average tax-to-GDP ratios stood at 32.5% in advanced economies, 19.7% in emerging markets, and only 13.8% in low-income developing countries (Benitez et al., 2023), underscoring substantial disparities in fiscal capacity across income groups. In response, an increasing number of low- and middle-income countries have initiated institutional reforms to strengthen enforcement and broaden the tax base (Naritomi, 2019; Brockmeyer and Sáenz Somarriba, 2025; Kotsogiannis et al., 2025).

India's 2016 demonetization represents one of the most ambitious and controversial tax enforcement experiments in recent history. The policy abruptly withdrew 86% of the currency in circulation by invalidating 500- and 1,000-rupee banknotes, with the stated objectives of combating black money, reducing counterfeit currency, and promoting digital payments. Given India's heavy reliance on cash, the withdrawal compelled holders to either deposit cash into the banking system or to shift toward digital payments, thereby creating financial records and enhancing transaction traceability.[1] This sudden restriction was expected to push informal activities into the formal sector and expand the tax base. Unlike gradual and institution-based reforms, demonetization was a one-time, large-scale intervention, offering a rare opportunity to evaluate the effectiveness of an unconventional tax enforcement tool. However, earlier

[1] India's cash-to-GDP ratio, estimated at approximately 11–13%, was considerably higher than that of comparable emerging economies such as Brazil (about 4%) and Mexico (about 6%) (Mazzotta et al., 2014). In addition, PricewaterhouseCoopers (2015) reports that 98% of consumers' payments by volume, and 68% by value, are made in cash India.

empirical evidence finds mixed results, leaving open the question of whether demonetization produced sustained improvements in tax compliance.[2]

This study investigates the impact of India's 2016 demonetization on tax revenue from 2005 to 2022. We employ the synthetic control method (SCM) to construct a counterfactual India by weighting a set of Asia-Pacific countries that did not experience similar monetary disruptions. This approach enables us to account for time-varying confounders and isolate the policy's fiscal effects from broader regional or global trends. We then compare India's actual tax revenues with this synthetic counterfactual to estimate the effects of demonetization on tax collection. In addition, we investigate mechanisms emphasized in policy discourse and prior research, including cash usage, digital payment adoption, and the size of the informal economy. This analysis examines whether observed shifts in payment behavior translated into lasting compliance gains.

Our results suggest a persistent rise in sales and production tax (SPT) revenues following demonetization, but no comparable increase in income and profits tax (IPT) revenues. If demonetization had effectively formalized the informal sector, we would expect to observe a parallel increase in IPT alongside SPT. The absence of such evidence indicates that the policy's impact on broad-based tax compliance was limited. In examining the underlying mechanisms, we find that cash usage declined and digital payments surged in 2016–2017, but both reverted to pre-demonetization levels by 2018, and the informal economy showed no lasting contraction. These findings imply that the

[2] Some studies document improved compliance and tax collection through reduced cash use and increased digital transactions (Das et al., 2023; Hosain, 2019; Singh, 2019). Other research, however, argues that observed gains were temporary or merely reflected pre-existing upward trends in tax revenue rather than the policy itself (Ashwani and Nataraj, 2018). Furthermore, the Reserve Bank of India (RBI) reported that 99% of demonetized currency returned to the banking system, casting doubt on the policy's success in uncovering unreported wealth. Since a central objective of demonetization was to flush out illicit or undeclared cash holdings, the expectation was that a substantial share of black money would not be returned. The fact that nearly all the currency was ultimately deposited or exchanged suggests that the policy fell short of its intended aim.

rise in SPT is more plausibly attributable to the July 2017 rollout of the Goods and Services Tax (GST), whose input tax credit (ITC) mechanism strengthened compliance across supply chains.[3]

This study contributes to the literature in two ways. First, we provide new evidence on the fiscal effects of India's 2016 demonetization by applying the SCM to construct a credible counterfactual. Unlike much of the existing work, which relies on short post-policy periods and descriptive comparisons (Ashwani and Nataraj, 2018; Singh, 2019; Lahiri, 2020), our approach employs a rigorous identification strategy to examine national trends in tax revenue, cash usage, digital payments, and the size of the informal economy through 2022. This longer horizon and multi-dimensional outcomes allow for a more systematic assessment of whether demonetization produced lasting gains in tax collection in a highly cash-dependent economy.

Our findings also complement micro-level research by offering a macro-level perspective. A related study is Das et al. (2023), which uses firm-level tax data from West Bengal to show short-term increases in digitalization and tax payments following demonetization. While their study provides credible identification, its narrow geographic scope and limited post-policy window (to early 2017) restrict generalizability.[4] Given substantial regional variation in cash reliance, digital infrastructure, and enforcement capacity, such results may not reflect aggregate patterns. Rural and agricultural areas, for example, often rely more heavily on cash, and have limited access to formal financial services (Zhu et al., 2018; Hosain, 2019; Rasel et al.,

[3] ITC allow firms to offset the tax paid on business inputs against the tax due on outputs. As noted in a guide published by the GST Council, the mechanism enables "any registered person to avail credit of tax paid on the inward supply of goods or services or both which is used or intended to be used in the course or furtherance of business," provided that suppliers are also compliant and properly documented.

[4] West Bengal accounts for roughly 5.6% of India's GDP in 2023–24 (EAC-PM, 2024) and only 3.7% of RuPay credit card transaction volume and 3.5% of value nationally (Government of India, 2024). Combined with substantial structural, administrative, and sectoral differences across states, this underscores the limited external validity of extrapolating its experience to the country as a whole.

2019; Aggarwal et al., 2023), constraining their ability to adopt digital payments.[5] In contrast, we use national data over a longer horizon and examine multiple outcome domains. Using synthetic control estimates, we find that digital payments would have grown even without demonetization, driven by structural trends. This broader assessment not only calls into question the effectiveness of demonetization in improving tax compliance at the aggregate level but also suggests that compliance gains documented in prior studies may be context-specific or short-lived.

Second, this study contributes to the broader tax enforcement literature by evaluating one of the most abrupt and ambitious compliance interventions in recent history: India's 2016 demonetization. Whereas most low- and middle-income economies have relied on gradual institution-building reforms such as third-party reporting or digitalization of transactions (Carrillo et al., 2017; Naritomi, 2019; Brockmeyer and Sáenz Somarriba, 2025), demonetization represents a rare case of a sudden, large-scale currency reform. By showing that the policy produced no substantial or lasting gains in tax collection, our findings highlight the limitations of one-off monetary shocks in curbing informality and underscore that durable improvements in compliance are more likely to emerge from sustained institutional changes.

[5] Rural India remains considerably less integrated into digital payments compared to urban areas. According to Global Findex data in 2021, around 40% of urban adults use accounts for digital payments, while the figure is only about 30% among rural adults (World Bank, 2022). Research has further examined the determinants of digital adoption in rural India (Sindakis and Showkat, 2024). These studies point to structural disparities between rural and urban areas that shape how policies such as demonetization play out, since reduced cash availability affects these contexts in very different ways. Such disparities are also evident in access to basic infrastructure: Kar et al. (2018) reported that in 2017, 64% of the urban population had internet connectivity, compared to just 20% of rural residents.

The remainder of this paper is structured as follows. Section 2 provides the institutional background and literature review. Section 3 outlines the research design. Section 4 presents the empirical results. Section 5 concludes.

## 2. Institutional background and literature review

### 2.1 India's earlier demonetization episodes (1946 and 1978)

Demonetization in India refers to the withdrawal of a currency unit's legal tender status, a significant economic measure often used by the government to combat corruption and curb black money. India has implemented demonetization three times: first in January 1946, Reserve Bank of India (RBI) demonetized rupees (Rs.) 1,000 and Rs. 10,000 notes; and again in 1978, under the Morarji Desai administration, when Rs. 1,000, Rs. 5,000, and Rs. 10,000 notes were withdrawn from circulation. Both instances aimed to eradicate black money and promote formalization. However, official data suggest that their effectiveness was limited: in 1946, over 93% of the demonetized currency was returned to the banking system, while in 1978, approximately 85% was exchanged (Supreme Court of India, 2023).

### 2.2 Demonetization in November 2016: objectives and implementation

On November 8, 2016, Prime Minister Narendra Modi announced that banknotes of Rs. 500 and Rs. 1,000 would cease to be legal tender effective midnight, thereby invalidating approximately 86% of the currency in circulation. This unprecedented move immediately disrupted cash liquidity and significantly affected daily transactions across the country.

The initial objectives of demonetization were twofold: to curb black money and to eliminate counterfeit currency. Holders of unaccounted cash faced a dilemma: depositing such funds in banks risked attracting scrutiny from tax authorities, whereas

failing to deposit them rendered the currency invalid and unusable. This situation reflected recognition that a large portion of the Indian economy operated predominantly in cash, often outside the formal tax net. Greater digitalization was thus expected to formalize economic activity, enhance transparency, and improve tax compliance.

To facilitate the currency exchange process, the public was given until the end of December 2016 to deposit invalidated notes into bank accounts or exchange them for newly issued currency. Over-the-counter exchanges were initially capped at Rs. 4,000 per person per day, but this limit was reduced to Rs. 2,000 on November 18, 2016. Cash withdrawals from bank accounts were restricted to Rs. 10,000 per day and Rs. 20,000 per week during the first two weeks after demonetization, compelling most individuals to deposit their cash holdings rather than withdraw large sums (RBI, 2016). These large deposits often triggered additional scrutiny, as the Income Tax Department analyzed inconsistencies between cash inflows and declared income.

In response, the government launched Operation Clean Money in January 2017 to examine deposit patterns and identify tax evasion. Individuals unable to justify the source of their deposits faced punitive taxes of up to 75%. In parallel, the government introduced the Income Disclosure Scheme II, offering another opportunity for individuals to declare previously undisclosed income at a tax rate of 50%, thereby incentivizing voluntary compliance.

The demonetization exercise imposed significant pressure on India's banking system, as commercial banks struggled to handle the unprecedented surge in currency exchanges and withdrawals. This system-wide stress was further compounded by the RBI's limited capacity to print and distribute new notes, which lagged behind the pace of demonetized currency withdrawal. The resulting cash shortage created severe liquidity constraints, which in turn acted as a catalyst for the increased adoption of

digital payment platforms, including mobile wallets, Unified Payments Interface, and debit card transactions.

The RBI later reported that approximately 99% of the invalidated currency had returned to the banking system. This outcome contrasted sharply with the government's initial expectation that a substantial portion would remain outside circulation to reduce black money. It highlighted the limited effectiveness of demonetization in curbing illicit cash holdings and framed the subsequent debate on its broader economic and social impacts.

## 2.3 Impacts of demonetization: review of related studies

The 2016 demonetization was framed by the Indian government as a bold initiative to combat corruption and eliminate black money. However, given the economy's high dependence on cash-based transactions, the sudden withdrawal of legal tender created a severe liquidity crunch and widespread disruption. A growing body of literature has examined the economic consequences of this policy across multiple dimensions, but the findings remain mixed and context-dependent.

### 2.3.1 Impact on GDP and economic activity

One of the most immediate and visible effects of demonetization was a contraction in economic activity, particularly in cash-dependent sectors. Studies report declines in GDP growth, employment, and trade during the initial quarters following the policy's announcement. Chodorow-Reich et al. (2020), using district-level data (meso level), find that regions experiencing more severe cash shortages faced sharper reductions in nightlight intensity and employment, suggesting a real economic contraction of around two percentage points in Q4 2016. At the sectoral level, cash-intensive domains such as agriculture and informal trade were especially hard hit. Aggarwal and Narayanan

(2023) estimate that agricultural trade fell by over 16% in the short run, driven primarily by falling prices, while Singh (2019) observes declines across agriculture, manufacturing, and small and medium-sized enterprises, based on descriptive macro trends. Export performance was also adversely affected, with Lahiri and Deb (2022) reporting significant short-term losses, though no evidence of long-term deterioration.

Micro-level evidence reinforces these meso and macro findings. Using household-level data, Karmakar and Narayanan (2020) show that households without bank accounts experienced sharp declines in expenditure during the initial months of demonetization, although income losses were more transitory. Recovery in consumption was partly enabled through increased borrowing, particularly from informal sources such as moneylenders and shops. Zhu et al. (2018) examine the short-term effects of demonetization on rural households, finding average income losses of roughly USD 20 during the two months after the policy. The shock also disrupted local incomes and remittances unevenly, with female-headed households and those facing higher unemployment experiencing sharper declines. Together, these patterns highlight the uneven burden of liquidity shocks across asset groups and underscore the vulnerability of financially excluded households.

Despite these disruptions, some analysts suggest that the economy began to stabilize within a few quarters. Ashwani and Nataraj (2018), for instance, observe a return to positive macroeconomic trends shortly after demonetization. Overall, while the policy delivered a sharp but short-lived economic shock, its long-term macroeconomic effects appear limited and remain subject to debate.

### 2.3.2 Black money and digital payments

A core objective of the 2016 demonetization was to eliminate black money, which refers to cash holdings outside the formal financial system. However, the RBI later reported that approximately 99% of the demonetized currency was returned to banks. This outcome challenged the government's initial expectation that a significant portion of unaccounted wealth would be extinguished. Scholars such as Singh (2019) argue that demonetization disrupted cash-based illicit assets only temporarily and had little impact on black money stored in other forms, such as real estate, gold, or foreign assets.

On the digital front, demonetization appeared to induce a short-term behavioral shift toward cashless payment, with Ashwani and Nataraj (2018) documenting a notable increase in mobile banking, card usage, and the deployment of Point-of-Sale (PoS) machines in the months following the policy announcement. However, they also note that these gains were not sustained beyond early 2017. Aggarwal et al. (2023) similarly show that while digital payments rose in areas more severely affected by the cash shortage, sustained adoption occurred only where adequate financial infrastructure was already in place. Taken together, this suggests that the surge in digital activity was largely a temporary response to the liquidity shortage, and any persistence depended critically on pre-existing infrastructure.

Other studies present a more optimistic perspective. Agarwal et al. (2024), using consumer-level data from 171 retail stores across five Indian states, find that greater digital payment usage, driven by cash scarcity, resulted in a 2.38% increase in monthly spending for every 10-percentage point rise in pre-demonetization cash dependence. Moreover, they observe that spending levels remained elevated even after the cash supply stabilized, suggesting that digital payment adoption can persist beyond the immediate liquidity shock. However, because the study covers only the year following

demonetization, it is unclear whether this change represents a lasting, permanent shift in consumer behavior.

Overall, the evidence suggests that while demonetization fell short of eliminating black money, it did succeed in triggering a short-term shift toward digital transactions. Whether this digital transformation can lead to sustained financial formalization remains an open question in the literature.

### 2.3.3 Tax compliance and formalization

Another key rationale behind demonetization was to expand the formal tax base by encouraging tax compliance and discouraging unaccounted income. The policy was expected to bring more individuals and businesses under the tax net, both by scrutinizing large cash deposits and by promoting digital transactions that are more easily traceable.

However, the empirical evidence on demonetization's impact on tax collections is mixed. Ashwani and Nataraj (2018) find no substantial increase in total tax revenue in the immediate aftermath of the policy. In contrast, Singh (2019) reports a sharp rise in the number of income tax returns filed—17.3% in 2016–17 and 24.7% in 2017–18 relative to the previous years—and attributes this increase largely to demonetization. Yet Singh also acknowledges that direct tax collections were already on an upward trajectory prior to the note ban, complicating causal attribution.

Recent empirical work by Das et al. (2023) provides more targeted evidence on the link between demonetization, digital payments, and tax compliance. Using administrative tax return data from the state of West Bengal, the authors exploit regional variation in cash shortages to estimate the policy's effects. They find that demonetization led to a substantial increase in reported sales and tax payments,

particularly in areas that experienced larger cash disruptions. This compliance effect appears to be mediated through greater use of digital payments, which enhanced the tax authority's ability to monitor transactions and enforce reporting. While these results suggest that cashless transactions can support improved tax compliance, the findings are limited to one state and may not generalize across the country.

In sum, the literature presents a nuanced picture. While demonetization may have encouraged greater formalization and improved tax reporting in some settings, its broader impact on tax compliance remains uncertain. Outcomes appear to depend heavily on the interplay between digital infrastructure, administrative capacity, and taxpayer behavior.

Yet, much of the existing research relies on short-term data, localized samples, or descriptive approaches that limit external validity. This study addresses these limitations by applying a macro-level synthetic control approach to evaluate whether demonetization led to sustained improvements in tax revenue, formalization, and digital payment usage at the national level.

### 2.4 GST reform in 2017

A major structural reform closely following demonetization was the introduction of the GST on July 1, 2017, after more than a decade of policy deliberation.[6] GST replaced India's fragmented and complex indirect tax regime, which had comprised multiple overlapping central and state levies such as value added tax (VAT), excise duty, service tax, and entry taxes. Under the old system, states often imposed unilateral

[6] For a detailed discussion of GST structure, rates, exemptions, and compliance mechanisms, see World Bank (2018).

taxes on goods entering their borders, creating inefficiencies, compliance burdens, and significant barriers to the formation of a single national market.

GST unified the taxation of most goods and services under a nationwide, destination-based, value-added framework. Intra-state sales are jointly taxed by the central and state governments under the Central GST (CGST) and State GST (SGST), while inter-state transactions are taxed by the center under the Integrated GST (IGST). The system applies harmonized tax slabs of 0%, 5%, 12%, 18%, and 28%, with special rates for certain goods such as gold (3%) and precious stones (0.25%). Certain categories, such as alcohol for human consumption, petroleum products, stamp duties on real estate transactions, and electricity duties, remain outside the GST and continue to be taxed by state governments.

A central compliance-enhancing feature is the ITC mechanism: businesses may deduct GST paid on inputs from their GST liability on outputs only if their suppliers are registered and compliant. This built-in requirement creates incentives for accurate transaction reporting along the supply chain, thereby broadening the tax base and strengthening enforcement. Small firms with annual turnover below Rs. 2 million are exempt, while those between Rs. 2 million and Rs. 15 million may opt for a simplified "composition scheme," paying a turnover-based tax without ITC claims.

Administrative efficiency is further supported by the GST Network (GSTN), a nationwide IT backbone through which all returns are filed, ITC claims are cross-verified, and audits are selected using risk-based methods. By standardizing tax policy and administration across states, GST is expected to reduce trade frictions, expand the formal tax base, and improve indirect tax compliance, even in sectors that were previously underreported.

## 3. Research design

### 3.1 Data

Because our objective is to identify the aggregate effects of demonetization, we require an appropriate counterfactual benchmark. Since no untreated control group exists within India, we construct synthetic controls using comparable countries that did not undergo similar monetary disruptions. This approach necessitates the use of harmonized cross-country datasets.

Our empirical analysis therefore draws on multiple international sources, including tax revenue indicators, measures of informal economic activity, macroeconomic controls, cash usage, and digital payment data. By integrating these datasets, we assess not only aggregate tax revenue impacts but also the channels through which demonetization may have shaped tax compliance.

#### 3.1.1 Tax collections

We obtain country-level tax revenue indicators from the IMF's World Revenue Longitudinal Database (WoRLD). Our sample spans 2005 to 2022, providing eleven pre-treatment years and seven post-treatment years, which allows for credible identification of pre-trends and medium-term policy effects.

WoRLD compiles harmonized data on government tax and non-tax revenues for over 190 IMF member countries, following the Government Finance Statistics Manual (GFSM) framework. By closely coordinating with national authorities, the database ensures consistency and comparability across countries and over time. It offers detailed disaggregation of tax categories, including taxes on income and profits, taxes on sales and production, property taxes, international trade taxes, and other minor tax categories.

WoRLD has been increasingly used in empirical research on fiscal policy and taxation (Hebous et al., 2020; Benedek et al., 2022).

In this study, we focus on two major tax categories. The first is "taxes on sales and production," a broad measure including general sales taxes, excise taxes, and other levies on the production and sale of goods and services. Since WoRLD aggregates all major consumption-related taxes under a consistent definition, this indicator helps avoid definitional inconsistencies before and after the 2017 GST reform. The "taxes on sales and production" category thus serves as a comprehensive indicator of tax activities related to goods and services throughout the sample period. The second category is "taxes on income and profits," which encompasses personal income taxes, corporate income taxes, and other non-classified income-related taxes, including capital gains and withholding taxes. Together, these two tax measures allow us to assess whether demonetization had differential impacts on consumption-based and income-based tax collections.

All tax variables are expressed as a percentage of GDP. We smooth them using a five-year moving average to reduce annual fluctuations and better capture underlying trends. As a robustness check, we re-estimate the SCM using raw annual series and find qualitatively similar results, confirming that our findings are not sensitive to the smoothing choice.

### 3.1.2 Informal economy estimates

We obtain data on the size of the informal economy from the World Bank's Informal Economy Database (Elgin et al., 2021), which provides harmonized cross-country estimates of informal economic activity as a share of GDP for 196 countries between 1990 and 2020. The estimates are based on a two-sector dynamic general

equilibrium model that captures the interaction between the official and informal sectors. Elgin's data have been increasingly adopted in recent empirical work (e.g., Bogetić and Naeher, 2024; Colombo et al., 2024). Since the latest year available is 2020, we limit our analysis of the informal economy to the 2005–2020 period.

As an alternative, Medina and Schneider (2018) provide estimates based on the and MIMIC (multiple indicators multiple causes) approach. While influential, their dataset ends in 2015 and thus cannot capture post-demonetization dynamics. To evaluate the consistency across data sources, we compare their estimates with those from the World Bank over the overlapping period from 2005 to 2015. As shown in Figure A1, although Medina and Schneider report generally higher levels, both series follow similar trends and converge by 2015. This similarity reassures us that our findings are not sensitive to the choice of data source.

### 3.1.3 Control variables

Macroeconomic predictors used to construct the synthetic control are primarily obtained from the World Bank's World Development Indicators (WDI). These include the logarithm of GDP per capita, the share of consumption in GDP, the age dependency ratio, the population growth rate, and the tax-to-GDP ratio. To better capture country-specific dynamics, we incorporate pre-treatment values of the outcome variables.

### 3.1.4 Cash usage and payment behavior

To examine potential mechanisms behind the observed changes in tax revenue, we analyze trends in cash usage and digital payment behavior using data from the Bank for International Settlements (BIS). The BIS dataset offers harmonized cross-country indicators on retail payment systems and monetary aggregates. Due to data availability,

our analysis covers the period from 2012—the earliest year available in the BIS dataset—to 2022.

We construct three indicators of payment behavior: (1) the cash-to-GDP ratio, defined as the total banknotes and coins in circulation divided by nominal GDP; (2) the total number of card and e-money transactions (in natural logarithm); and (3) the annual growth rate of the total value of card and e-money payments.

According to BIS (2017), card payments include transactions made with debit and credit cards, whether stored on physical cards, mobile devices, or virtual formats.[7] E-money refers to prepaid monetary value stored electronically, issued by a licensed entity such as a bank or an e-money institution.[8] The dataset excludes cash withdrawals and deposits, intra-account transfers, automatic disbursements (e.g., dividends, interest), and money remittances.

### 3.2 Donor pool

To ensure credible counterfactual estimation, countries included in the donor pool should exhibit similar pre-treatment trends in the outcome variable as the treated unit. Including dissimilar countries may lead to interpolation bias and undermine the credibility of the synthetic control estimates (Abadie et al., 2015). Following the literature, for our analysis of tax collections and the informal economy, we restrict the donor pool to Asia-Pacific countries.[9] This regional focus reflects evidence that geographic proximity is associated with similar economic structures, institutional

[7] Payments initiated using physical devices such as mobile phones are classified based on the underlying instrument used to execute the transaction. For example, if a payment is made at a store by transmitting card information stored on a mobile phone, it is recorded as a card payment. For detailed definitions and classifications, see BIS (2017).

[8] Country attribution is determined by the residency of the card-issuing institution in the case of card payments, and by the residency of the e-money issuer in the case of e-money payments.

[9] The classification of Asia-Pacific countries follows the regional grouping adopted by the IMF.

frameworks, and regional business cycles (Billmeier and Nannicini, 2013; Adhikari and Alm, 2016; Breinlich et al., 2020; Bouvet, 2021; Adu and Hartarska, 2025). We further exclude countries with missing values for any outcome or predictor variables over the sample period, resulting in a final donor pool of 14 Asia-Pacific countries.[10]

In contrast, our analysis of cash usage and payment behavior draws on BIS data, which cover fewer Asia-Pacific economies. Only seven countries—Australia, China, Hong Kong, Indonesia, Japan, Singapore, and South Korea—overlap with our main donor pool. To ensure an adequate sample size, we instead construct a separate donor pool of 17 countries with complete BIS data.[11] In robustness checks, we re-estimate the results using only the seven overlapping Asia-Pacific countries to test the sensitivity of our findings to alternative donor pool composition.

### 3.3 Synthetic control method

We apply the SCM, originally developed by Abadie and Gardeazabal (2003) and extended by Abadie et al. (2010), to estimate the causal impact of demonetization on tax revenue, cash usage, digital payments, and the size of the informal economy in India. The SCM is designed for comparative case studies where a single unit (e.g., country, region) is exposed to an intervention, and no clear untreated control group exists. This method constructs a counterfactual scenario—the synthetic control unit—by assigning optimal weights to a set of untreated units, ensuring that the synthetic unit closely matches the treated unit in the pre-treatment period across a range of predictors and outcome variables.

[10] Donor pool countries include Australia, Bangladesh, Cambodia, China, Hong Kong, Indonesia, Japan, South Korea, Malaysia, Nepal, Philippines, Singapore, Thailand, and Vietnam.
[11] Donor pool countries include Australia, Belgium, Brazil, Canada, China, France, Germany, Hong Kong, Indonesia, Italy, Japan, Saudi Arabia, Singapore, South Korea, Spain, United Kingdom, and United States.

An important identifying assumption of the SCM is that the policy under study does not induce anticipatory responses prior to its formal implementation. This condition is well satisfied in the case of demonetization, since both the policy and its exact timing came as a complete surprise to citizens and businesses (Lahiri and Deb, 2022; Aggarwal et al., 2023). The sudden announcement on November 8, 2016 left no scope for agents to adjust their behavior in advance, thereby ensuring that pre-treatment dynamics remain uncontaminated by the intervention.

The SCM offers several key advantages that make it particularly well-suited for evaluating the effects of demonetization in our empirical context. First, it constructs the control unit through a data-driven process, assigning weights based on pre-intervention similarity rather than on researcher discretion. This enhances the objectivity and transparency of control selection. Second, unlike regression-based approaches that rely on large samples and asymptotic properties for valid inference, the SCM is designed for case-specific causal analysis, making it especially appropriate for studying a policy intervention in a single country. Third, whereas difference-in-differences estimators assume that unobserved confounders are time-invariant, the SCM allows for time-varying unobserved heterogeneity and relaxes the parallel trends assumption, thereby improving the credibility of counterfactual estimation.

Let $Y_{it}$ denote the outcome of interest for unit $i$ at year $t$, where $i = 1$ represents the treated unit (India), and $i = 2,\ldots, J + 1$ are the control units in the donor pool. Let $T_0$ denote the year of intervention (2016 in our case), and $T$ the total number of years in the sample period. The goal is to estimate the treatment effect for India in each post-intervention year:

$$\alpha_{1t} = Y_{1t} - Y_{1t}^{SC} \qquad \text{for } t > T_0,$$

where $Y_{1t}$ is the observed outcome for India, and $Y_{1t}^{SC}$ is the outcome of a synthetic India that approximates the counterfactual scenario for India in the absence of demonetization. This synthetic control outcome is constructed as a weighted average of the outcomes of the control units:

$$Y_{1t}^{SC} = \sum_{j=2}^{J+1} w_j Y_{jt},$$

where $w_j$ is the non-negative weights assigned to each country in donor pool, subject to $\sum_{j=2}^{J+1} w_j = 1$. These optimal weights are selected to minimize the pre-treatment discrepancy between India and its synthetic counterpart across a set of predictors. Formally, the weights solve:

$$\min_{w} \sum_{k=1}^{K} v_k \left( X_{1k} - \sum_{j=2}^{J+1} w_j X_{jk} \right)^2,$$

where $X_{ik}$ denotes the value of predictor $k$ for unit $i$, and $v_k$ is a predictor-specific weight reflecting the relative importance of predictor $k$ to the fit of the pre-treatment outcomes.

This procedure yields a synthetic India that closely approximates the country's trajectory in the absence of demonetization. The difference between India's actual and synthetic outcome variable in the post-intervention period provides an estimate of the causal effect of demonetization.

## 4. Empirical results

### 4.1 Sales and production tax revenue

Table A1 reports the weights assigned to donor countries for tax collections. Table A2 illustrates the pre-treatment balance between India and its synthetic counterpart

across all predictor variables. The close alignment in macroeconomic fundamentals and prior fiscal outcomes reinforces the credibility of the synthetic control as a counterfactual for India.Figure 1 illustrates the trends in SPT revenue for actual India and its synthetic counterpart. The synthetic series closely tracks India in the pre-intervention period, confirming that the SCM reproduces the underlying outcome dynamics. However, starting in 2016, a noticeable divergence arises between the two series, with actual India exhibiting higher tax revenues than its synthetic control. To quantify this divergence, we present in Column (1) of Table 1 the annual differences in SPT revenue between actual India and its synthetic counterpart from 2016 to 2022. The estimated gap rises steadily, from 0.304 percentage points in 2016 to 1.94 percentage points in 2022.

[Insert Figure 1 here]

[Insert Table 1 here]

To assess the statistical significance of the estimated policy effect, we implement three empirical tests. First, following Abadie et al. (2010), we conduct in-space placebo tests. Specifically, we iteratively assign each donor country as a placebo-treated unit, re-estimate the synthetic control, and compute the corresponding placebo effects. To improve comparability, we exclude countries with a pre-treatment mean squared prediction error (MSPE) more than ten times greater than that of India, since a poor pre-treatment fit can inflate the placebo effect and bias inference. A policy effect can be considered statistically significant only if it exceeds the vast majority of placebo effects observed among countries that did not experience the intervention.

Panel A of Figure 2 illustrates the results for SPT revenue. Each gray line represents the gap between a donor country and its synthetic counterpart under the placebo assignment, while the black line denotes the gap between actual India and its

synthetic control. Prior to 2016, the gap between India and synthetic India was close to zero, indicating a strong pre-treatment match. After 2016, however, the gap widens steadily. Compared to the placebo distribution, India's estimated effect is greater than that for most donor countries.

[Insert Figure 2 here]

Second, we assess statistical significance using the post-treatment to pre-treatment MSPE ratio across countries, as proposed by Abadie et al. (2010). A higher MSPE ratio indicates a larger deviation from the counterfactual trajectory following demonetization, conditional on the quality of pre-treatment fit. Unlike in-space placebo tests, which require arbitrary thresholds to exclude units with poor fit, the MSPE ratio provides a continuous measure that accounts for pre-treatment match quality. Panel A of Figure 3 reports these ratios for SPT sorted in descending order. India exhibits the highest MSPE ratio among all countries in the donor pool, suggesting that the divergence between actual and synthetic India is particularly pronounced relative to the pre-intervention fit.

[Insert Figure 3 here]

Finally, we evaluate the statistical significance by computing pseudo two-sided *p*-values for each post-demonetization year. These *p*-values are calculated as the proportion of donor countries whose standardized treatment effects exceed that of India, where the standardized effect is defined as the post-treatment gap divided by the pre-treatment root mean squared prediction error (RMSPE). This standardization ensures that inference accounts for pre-treatment match quality and reduces the influence of poorly fitted synthetic controls (Boxell and Steinert-Threlkeld, 2022). A smaller pseudo *p*-value thus indicates that India's estimated effect is unusually large relative to the

placebo distribution, providing stronger evidence that the observed treatment effect is unlikely to be due to chance.

The results are reported in parentheses in Table 1. In each post-treatment year, the pseudo *p*-value falls below 0.01. If interpreted in line with conventional statistical inference, these results imply statistical significance at the 1% level. Whether these sustained effects can be attributed to demonetization itself, however, requires further examination, which we address in the subsequent analysis.

### 4.2 Income and profits tax revenue

We next examine the effects of demonetization on IPT revenue. The donor pool weights and predictor balance are reported in Table A1 and Table A2, respectively. Column (2) of Table 1 presents the estimated treatment effects from 2016 to 2022. Although post-treatment estimates are uniformly positive, the corresponding pseudo *p*-values all exceed 0.1, indicating no statistical significance at conventional levels. This finding is reinforced by Panel B of Figure 2, where India's treatment effect falls well within the distribution of placebo effects, and by Panel B of Figure 3, where India registers the lowest post/pre-MSPE ratio among all donor countries.

The divergence between IPT and SPT outcomes points to a more limited effect of demonetization on overall tax compliance. While SPT collections rose significantly, IPT revenues exhibited only modest and statistically insignificant increases. This asymmetry raises doubts about whether demonetization succeeded in expanding the tax base by bringing informal activities into the tax net.

### 4.3 Cash usage and digital payment

To better understand the channels through which demonetization may have influenced tax outcomes, we investigate changes in cash usage and digital payment

behavior, mechanisms explicitly emphasized in the official policy discourse surrounding the reform. If demonetization had permanently expanded the tax base and improved tax compliance, one would expect a sustained decline in cash usage accompanied by a persistent increase in the adoption of digital payments. Such shifts in transaction patterns are argued to enhance the traceability of economic activity, thereby reducing opportunities for tax evasion and bringing more agents into the formal tax system.

Before presenting the results, we provide an overview of the synthetic control construction. Table A3 reports the donor weights used to construct synthetic India. Although the BIS dataset includes countries beyond the Asia-Pacific region, most of the donor weights derive from Asia-Pacific countries. For instance, in the case of the cash-to-GDP ratio, 94% of the total weight is drawn from China (44.3%), Indonesia (19.4%), and Japan (30.3%). This concentration of weights indicates that the influence of non-Asia-Pacific countries on the empirical results is limited. Table A4 presents the predictor balance between actual and synthetic India. Actual India shares similar characteristics with synthetic India during the pre-treatment period.

Figure 4 presents the SCM estimates for cash usage and digital payment behavior. We begin with the cash-to-GDP ratio. Prior to demonetization, India's ratio remained stable at around 11–12 percent and closely aligned with its synthetic counterpart. In 2016, however, the ratio for India dropped sharply to roughly 9 percent, while the synthetic unit continued on its pre-existing path. The ratio partially rebounded in 2017 but stayed below that of synthetic India. From 2018 onward, except for a temporary increase in 2020, India's trajectory converged with the synthetic India and returned to its pre-demonetization level.[12] Figure A2 presents the in-space placebo tests and

[12] The temporary increase in 2020 is plausibly attributable to the COVID-19 pandemic, which disrupted

confirms these patterns. India experienced a pronounced decline in 2016 that exceeded all placebo effects from donor countries, while the 2017 effect, though still negative, was less distinct relative to the placebo distribution.

[Insert Figure 4 here]

Column (1) of Table 2 reports the estimated treatment effects of demonetization on the cash-to-GDP ratio for each post-treatment year. The statistically significant declines in 2016 and 2017 indicate a sharp contraction in cash usage immediately following the policy. In particular, the estimated effect in 2016 is –3.192 percentage points, equivalent to roughly 27 percent of India's pre-treatment mean. Starting in 2018, however, we no longer observe any statistically significant negative effects. Overall, these results suggest that the impact of demonetization on cash usage was transitory, with little evidence of a lasting decline in cash reliance in the medium term.

[Insert Table 2 here]

In addition, Panel B of Figure 4 presents the synthetic control estimates for the total number of card and e-money transactions (in logarithms). Prior to demonetization, the trends for India and synthetic India closely aligned, suggesting a strong pre-treatment fit. After 2016, the synthetic series continued on its upward trajectory, indicating that digital payment adoption in India would likely have grown even in the absence of demonetization. In contrast, actual India experienced a sharper surge during 2016–2017, reflecting a behavioral shift in consumer payment preferences triggered by the cash shortage. From 2018 onward, however, the gap between India and its synthetic counterpart narrows, and the two trajectories begin to converge. This pattern suggests

both cash and digital transaction patterns. Mobility restrictions, supply-chain disruptions, and precautionary cash hoarding altered normal payment behavior, leading to a short-lived deviation from the pre-existing trajectory before convergence resumed in subsequent years.

that demonetization induced only a temporary boost in digital transactions. These findings are further supported by the in-space placebo test results in Panel B of Figure A2 and the policy effect estimates in Table 2, both of which reveal that India's treatment effects were statistically significant only in the immediate post-demonetization years of 2016 and 2017.

Finally, Panel C of Figure 4 presents the synthetic control estimates for the annual growth rate of the total value of card and e-money payments. Prior to demonetization, the growth rates of actual India and synthetic India moved in close alignment. In 2016, however, a sharp divergence emerged. India's growth rate surged to nearly 60 percent, which represents the highest level observed during the sample period, while the synthetic control continued along a trajectory consistent with its pre-existing trend. From 2018 onward, the two trajectories largely converged, indicating that the surge in digital payments was short-lived.

This short-lived effect is corroborated by the in-space placebo results in Panel C of Figure A2. In 2016, India's treatment effect clearly exceeds those of all placebo countries. From 2018 onward, no discernible effect is observed. Table 2 further confirms these findings: the estimated treatment effects are statistically significant only in 2016–2017, with no evidence of significance in later years.

In summary, the evidence on cash usage and digital payment behavior suggests that the effects of demonetization were temporary. We find a statistically significant decline in the cash-to-GDP ratio and a notable increase in both the volume and value of digital payments in 2016–2017, but these changes dissipated from 2018 onward, when actual India no longer differed from its synthetic counterpart. If demonetization had a lasting impact on tax collections through changes in payment behavior, we would expect continued reductions in cash usage and prolonged growth in digital payments.

The absence of such patterns indicates that the persistent rise in SPT revenue cannot be attributed to demonetization alone.

### 4.4 Informal economy

The estimated policy effects on the size of the informal economy are presented in Table 3, with the corresponding in-space placebo tests shown in Figure A3. Donor pool weights and predictor balance are reported in Tables A5 and A6, respectively. We find no statistically significant contraction of India's informal economy after demonetization, a result consistent with our earlier evidence on digital payments. Had demonetization curtailed tax evasion and shifted activity into the formal sector, a pronounced decline in informality would be expected after 2016. The absence of such a pattern casts doubt on the policy's effectiveness in achieving its stated goals.[13]

[Insert Table 3 here]

### 4.5 Robustness checks

#### 4.5.1 Raw series of tax collections

As a first robustness check, we re-estimate the policy effects using the raw tax revenue series, rather than the five-year moving average employed in the main analysis. While the moving average approach helps smooth short-term volatility and reduce noise in the data, it may also attenuate treatment effect estimates by diluting year-specific policy responses. To assess the sensitivity of our results to this smoothing procedure, we re-implement SCM using unsmoothed tax revenue series.

[13] Our main measure of the informal economy is based on the IMF's two-sector dynamic general equilibrium model. As a robustness check, we also use the IMF's alternative estimates derived from the MIMIC approach. In untabulated results, we still find that the estimated policy effects remain statistically insignificant using the MIMIC-based measure.

Panel A of Figure 5 presents the in-space placebo tests for SPT revenue based on the raw series. As expected, the pre-demonetization gap between India and its synthetic counterpart shows greater volatility than in the smoothed series. More importantly, in the post-demonetization period, India's treatment effects exceed those of all placebo countries in most years, reinforcing the robustness of our baseline findings. Column (1) of Table A7 reports the corresponding annual estimates and pseudo *p*-values. We continue to observe consistent increases in SPT revenue, with most estimates significant at conventional levels. In terms of magnitude, the raw-series estimates are generally larger than those from the smoothed series, suggesting that smoothing may indeed dilute the measured policy impact. By contrast, Panel B of Figure 5 and Column (2) of Table A7 show no evidence of a significant increase in IPT revenue, fully consistent with our baseline results.

[Insert Figure 5 here]

### 4.5.2 Alternative donor pool in tax collections analysis

The selection of an appropriate donor pool is crucial for ensuring the reliability and credibility of SCM estimates. Poorly matched donor units may lead to implausible counterfactuals and distort the estimated treatment effects. To assess the sensitivity of our results to alternative donor pool specifications, we conduct two robustness checks that alter the set of donor countries used in the tax collections analysis.

First, we broaden the Asia-Pacific donor pool by incorporating additional countries from Central and Western Asia. After excluding countries with incomplete data, the additional donor countries include Georgia, Kyrgyzstan, Saudi Arabia, and Pakistan. Second, we adopt an alternative donor pool construction following Lahiri and Deb (2022), who use the SCM to evaluate the short-run effects of demonetization on

India's export performance. Their donor pool is restricted to countries whose average export values during the pre-intervention period are at least half that of India's, thereby ensuring greater comparability in terms of global economic integration and market size. While their focus is trade outcomes, we view this criterion as relevant for fiscal analysis as well, since countries with larger trade volumes typically also possess larger taxable economic bases. After excluding countries with missing data, this approach yields a donor pool of 26 countries.[14]

Figure 6 presents the in-space placebo tests under these alternative donor pool specifications. In Panel A, which incorporates Central and West Asian economies, the estimated effects of demonetization on India's SPT revenue remain clearly distinguishable from most placebo countries in the post-treatment period. Likewise, in Panel B, based on the Lahiri and Deb (2022) donor pool, India's SPT effects continue to lie well above the distribution of placebo effects, reinforcing the robustness of our main findings. By contrast, the IPT effects remain well within the placebo distribution across both specifications, indicating no statistically significant response.

[Insert Figure 6 here]

Table A8 reports the corresponding annual treatment effects and pseudo *p*-values. Across both donor pool constructions, India's SPT revenue exhibits consistently positive and statistically significant effects in most post-treatment years, whereas IPT effects remain statistically insignificant throughout. Taken together, these robustness checks demonstrate that our baseline findings are not driven by donor pool selection, thereby further strengthening the credibility of our conclusions.

[14] Donor pool countries include Australia, Austria, Belgium, Canada, Chile, China, Costa Rica, Finland, France, Greece, Hong Kong, Hungary, Indonesia, Italy, Japan, Luxembourg, Malaysia, Norway, Paraguay, Peru, Philippines, Poland, Portugal, Singapore, Thailand, and United Kingdom.

### 4.5.3 Alternative donor pool in cash usage and digital payments analysis

As discussed in section 3.2, we re-estimate the SCM for cash usage and digital payments using a restricted donor pool comprising only the seven Asia-Pacific economies available in the BIS dataset. The in-space placebo tests are presented in Figure 7. The estimated effects for the cash-to-GDP ratio and the growth rate of card and e-money payments in 2016 remain distinguishable from the distribution of placebo effects, lending support to our main findings. By contrast, the estimated effect for the total number of card and e-money transactions does not appear to deviate from the placebo distribution, which may reflect weaker pre-treatment fit due to the limited donor pool size.

[Insert Figure 7 here]

Table A9 reports the estimated treatment effects and pseudo *p*-values for each post-treatment year. We continue to find a statistically significant decline in the cash-to-GDP ratio in 2016 and 2017, with magnitudes similar to those reported in the baseline analysis. The total number of card and e-money transactions shows a significant increase in 2016 and 2017, followed by a return to baseline levels in later years. For the growth rate of card and e-money payments, we again observe a significant positive effect in 2016–2017. Overall, these results suggest that our main findings are qualitatively robust to donor pool restrictions.

### 4.5.4 Leave-one-out tests

We conduct leave-one-out tests to assess the extent to which our findings are driven by any single donor country. Specifically, we iteratively re-estimate the SCM for each outcome variable, omitting one donor country that received a positive weight in the baseline specification, and compute the corresponding treatment effects.

Figure 8 presents the leave-one-out results for tax collections, cash usage, digital payments, and the size of the informal economy. For SPT revenue, the post-demonetization upward trend remains stable across all iterations. The IPT revenue and the size of the informal economy continue to show no discernible change. Regarding cash usage and digital payment indicators, we still observe a sharp decline in the cash-to-GDP ratio and a temporary surge in digital transactions in 2016. Taken together, these results demonstrate that the baseline findings remain robust to the exclusion of any particular donor country with a positive weight from the sample.

[Insert Figure 8 here]

**4.6 Discussion**

Taken together, our results indicate that demonetization did not deliver the sustained improvements in tax collection that were central to the policy's stated objectives. The increase in SPT revenue observed after 2016 is robust across specifications and donor pool choices, but no comparable pattern emerges for IPT revenue. These inconsistent results are difficult to reconcile with the claim that demonetization broadened the tax base by formalizing unreported income.

Demonetization triggered an immediate and statistically significant drop in the cash-to-GDP ratio and a surge in card and e-money transactions, consistent with a forced shift toward digital payment channels during the cash shortage of late 2016 and 2017. However, these changes proved temporary: by 2018, both cash usage and digital payment volumes had converged with their synthetic counterfactuals, suggesting that transaction behavior quickly reverted to pre-reform trends once currency availability normalized. In other words, the payment-channel changes induced by demonetization were short-lived and therefore unlikely to explain the persistent gains in SPT revenue.

One likely reason for this short-lived impact on digital payments is the entrenched reliance on cash in India. Once currency supply stabilized, individuals and firms reverted to familiar cash-based practices (Lahiri and Deb, 2022). This tendency was especially pronounced in rural and low-income regions, where financial infrastructure remains underdeveloped and digital literacy is limited. Existing studies confirm that the positive effects of digitalization were concentrated in districts with fewer rural households and with infrastructure for digital payments already in place (Aggarwal et al., 2023). Together, these factors help explain why the initial surge in digital payments could not translate into lasting behavioral change.

Our findings on payment behavior broadly align with those of Lahiri (2020). Using descriptive national-level data, the study shows that money circulation fell sharply in 2016–17 but reverted to its pre-demonetization trajectory by 2018, consistent with our evidence on cash usage. The study further emphasizes that digital payments were already growing exponentially prior to demonetization, a view that complements our SCM results showing that the post-2016 surge largely reflected a temporary deviation from an ongoing upward trend rather than a permanent policy effect.

On the other hand, our evidence contrasts with Das et al. (2023), who analyze RuPay electronic point-of-sale (EPOS) transactions in West Bengal and report that digital payments remained elevated through 2019Q3. Their identification strategy relies on variation in access to currency chests as a proxy for exposure to demonetization shocks. However, regions that adopt digitalization more aggressively following demonetization were also those with higher baseline adoption of digital payments or closer ties to financial hubs (Lahiri, 2020). Hence, the persistence observed in West Bengal may not generalize nationwide but instead represents a region-specific outcome.

By contrast, our study draws on nationwide BIS data covering all card and e-money transactions through 2022, offering broader coverage and a longer horizon. By applying the SCM, we are able to construct a counterfactual trajectory for India's digital payments in the absence of demonetization. This counterfactual reveals that, even without the policy, digital transactions would have continued to grow due to structural trends such as rising smartphone penetration and expanding financial infrastructure. Demonetization generated only a short-term spike in 2016–17, after which digital payments converged back to the pre-existing growth path by 2018. This perspective shows that the initial boost in digital payments was temporary and not sustained at the aggregate level—a pattern less evident in the shorter, region-specific RuPay series analyzed by Das et al. (2023).

Our results on the informal economy provide further support for this interpretation. Despite the sharp contraction in cash usage, we find no statistically significant reduction in informality. This outcome is consistent with prior evidence that much of India's shadow wealth is held in non-cash assets such as gold or real estate (Rasel et al., 2019; Lahiri, 2020), which are largely unaffected by currency withdrawal. It also aligns with the RBI report that 99% of the demonetized currency eventually returned to the banking system, underscoring that cash holdings were not permanently eliminated but merely redeposited.

We therefore interpret the sustained increase in indirect tax collections as more plausibly linked to the introduction of the GST on July 1, 2017. GST replaced a fragmented indirect tax system with a nationwide value-added tax structure featuring a destination-based framework, harmonized rates, and a mandatory ITC mechanism that ties credit eligibility to supplier registration and verified tax payment. This design fosters transaction formalization and eliminates the cascading tax effects of the pre-

GST regime. The GSTN's centralized digital filing, verification, and audit platform further reduces evasion opportunities, providing a structural explanation for the persistence of SPT gains.

Evidence from Sureka and Bordoloi (2024) demonstrates that effective utilization of the GST's ITC mechanism significantly enhances the profitability and competitiveness of micro, small, and medium-sized enterprises, while also drawing attention to the compliance challenges firms face in navigating the system. This suggests that by linking tax benefits to formalized transactions, the ITC framework holds the potential to strengthen compliance incentives and foster greater formalization within the business ecosystem. Complementing this, Maheshwari and Mani (2022), using an Analytical Hierarchy Process approach, identify the elimination of cascading tax effects as one of the most significant benefits of GST implementation, underscoring its role in improving efficiency and reducing distortions across the indirect tax system. Together, these findings illustrate how GST's ITC-driven architecture advances both compliance and efficiency objectives.

Overall, the evidence points to a nuanced conclusion: while demonetization created short-term disruptions that temporarily altered payment behavior, its medium- and long-run effect on tax compliance appears limited. The persistent rise in SPT collections is more consistent with the structural and compliance-enhancing changes introduced under the GST reform than with the one-off shock of currency withdrawal. This interpretation aligns with international experience, as seen in Brazil's electronic invoicing reform and Mexico's SAT digital reporting system, where sustained compliance gains typically result from systemic administrative changes rather than temporary cash-reduction measures (Barreix et al., 2018).

## 5. Conclusion

A central policy objective of India's 2016 demonetization was to broaden the tax base by curtailing informal economic activity and fostering formalization. Rigorous empirical evaluation of such large-scale interventions is essential for judging their effectiveness and for informing the design of future policies in cash-reliant developing economies.

Using the SCM and cross-country panel data from 2005 to 2022, this study estimated the counterfactual trajectory of India's tax revenues in the absence of demonetization. We find a statistically significant and persistent increase in SPT revenue after 2016, but no comparable change in IPT revenue. Examination of possible mechanisms shows that demonetization led to a sharp but temporary reduction in cash usage and a surge in digital transactions during 2016–2017, with both indicators returning to pre-reform trends by 2018. No significant contraction in the size of the informal economy is observed.

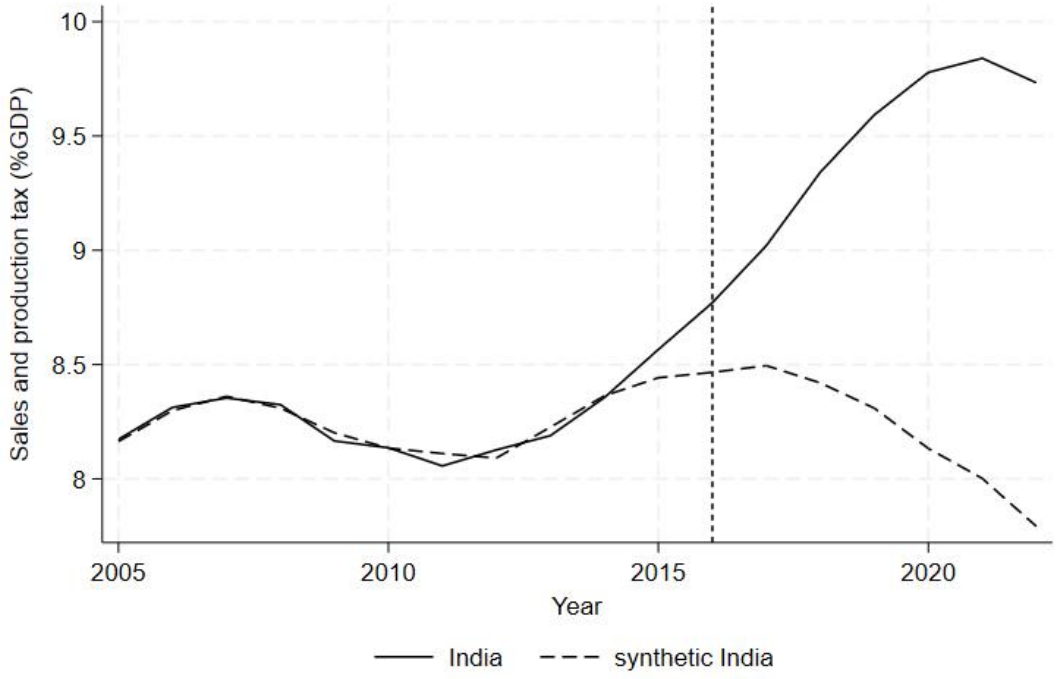
These results cast doubt on the effectiveness of demonetization in achieving its broader fiscal and formalization goals. While the policy temporarily disrupted cash-intensive sectors, there is little evidence that it produced lasting improvements in tax compliance or structural reductions in informality. The persistence of SPT gains is more plausibly linked to the concurrent introduction of the GST in 2017, which created enduring compliance incentives through its VAT structure, ITC mechanism, and centralized digital reporting system.

Policy implications follow directly from these results. The evidence suggests that one-off policy shocks such as demonetization may be unlikely to deliver sustained improvements in tax compliance. Lasting gains in tax compliance are more plausibly achieved through systemic, data-driven approaches that strengthen the capacity of tax administrations to detect and deter evasion on an ongoing basis. Such approaches

integrate information across tax types, use reliable third-party data to cross-verify declarations, and apply analytics to identify non-compliance risks in real time. By embedding continuous monitoring into the structure of tax administration, these measures may address structural enforcement gaps in a way that temporary liquidity shocks cannot.

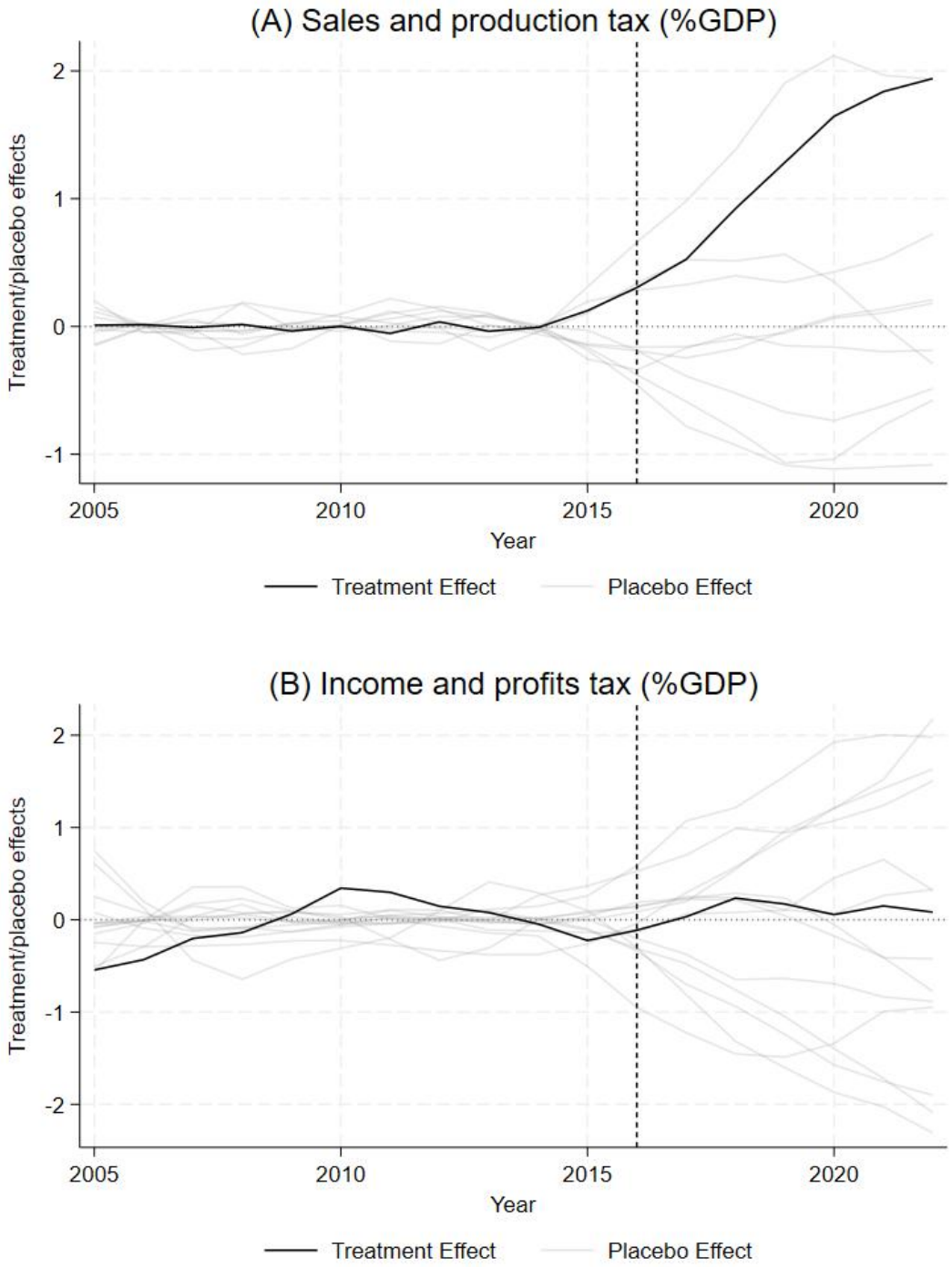
Several limitations should be acknowledged. First, the close timing of demonetization and the GST rollout complicates efforts to fully disentangle their respective effects. Our main strategy is to contrast SPT, which were directly reshaped by GST, with IPT revenues, which were not. This comparison helps distinguish the structural reform effects from demonetization's potential influence on direct tax compliance. As supporting evidence, we also examine indicators such as cash usage and digital transactions, which GST did not directly affect. Nevertheless, complete separation from concurrent reforms is not possible. Second, the reliance on annual tax revenue data limits our ability to capture seasonal fluctuations or short-term disruptions. While consistent with our focus on sustained, multi-year effects, this approach may overlook important adjustment dynamics in the immediate aftermath. Third, our macro-level analysis does not account for regional or sectoral heterogeneity, where impacts may have varied substantially. Future research integrating national evaluation with disaggregated, higher-frequency data could provide a more nuanced understanding of when and where such interventions expand the tax base.

## Figure



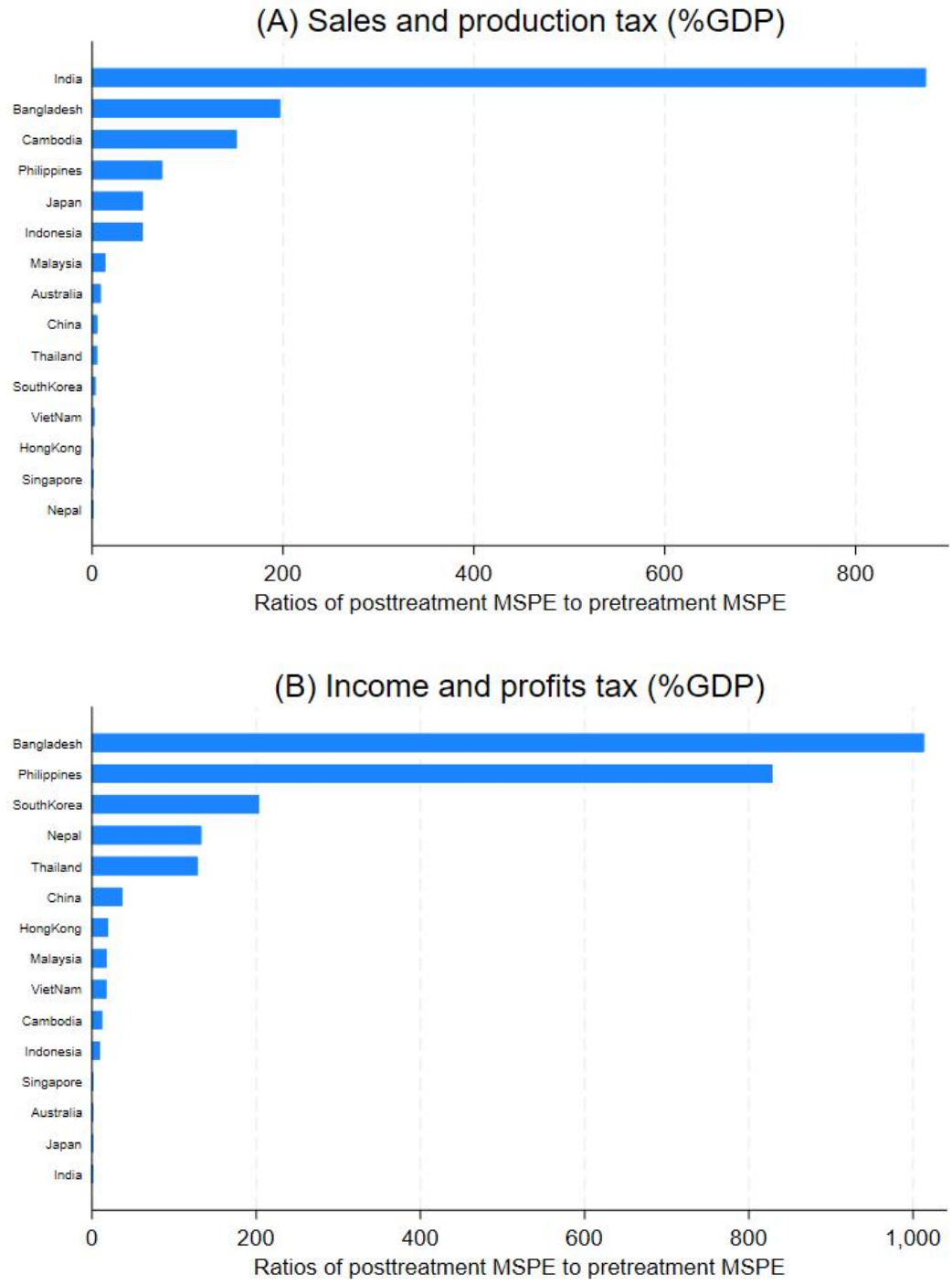

Note: This figure plots the trends in sales and production tax revenue for actual India and its synthetic counterpart. The synthetic India represents the trend in India without demonetization. The vertical dashed line marks the year (2016) when demonetization was announced, separating the sample into pre- and post-intervention periods.

**Fig. 1. Trends in sales and production tax revenue: Actual and synthetic India**



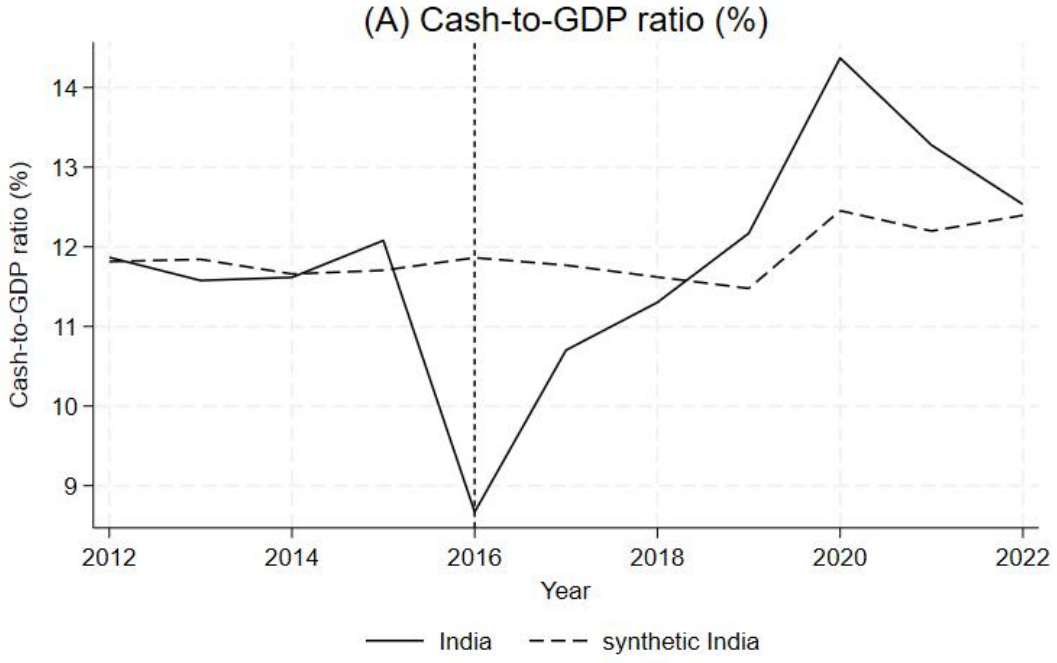

Note: This figure presents the results of the in-space placebo test. The outcome variables include sales and production tax revenue (%GDP) and income and profits tax revenue (%GDP). We smooth the tax variables using a five-year moving average to reduce annual fluctuations and better capture underlying trends. Each gray line represents the difference in outcome variables between a country in the donor pool and its synthetic counterpart. The black line denotes the gap between the actual India and synthetic India. Donor countries with a pre-treatment MSPE more than ten times that of India are excluded to enhance comparability.

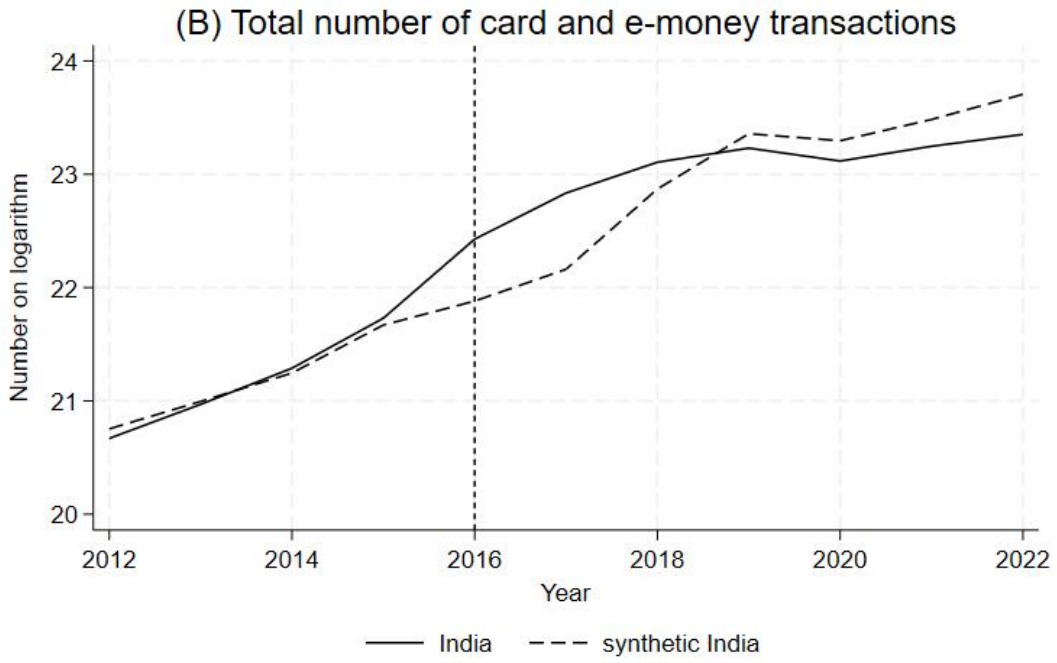
**Fig. 2. In-space placebo tests: Tax collections**

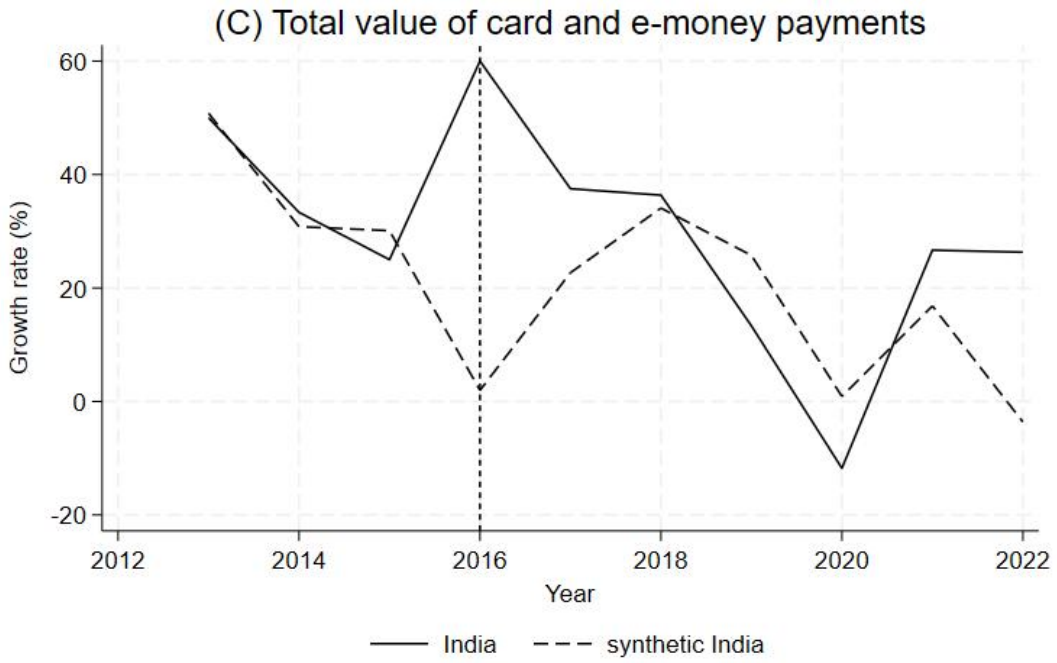



Note: This figure reports the ratio of post-treatment MSPE to pre-treatment MSPE for India and each donor country. The outcome variables include sales and production tax revenue (%GDP) and income and profits tax revenue (%GDP). We smooth the tax variables using a five-year moving average to reduce annual fluctuations and better capture underlying trends. Countries are sorted in descending order based on this ratio.

**Fig. 3. Ratio of post-treatment MSPE to pre-treatment MSPE: Tax collections**

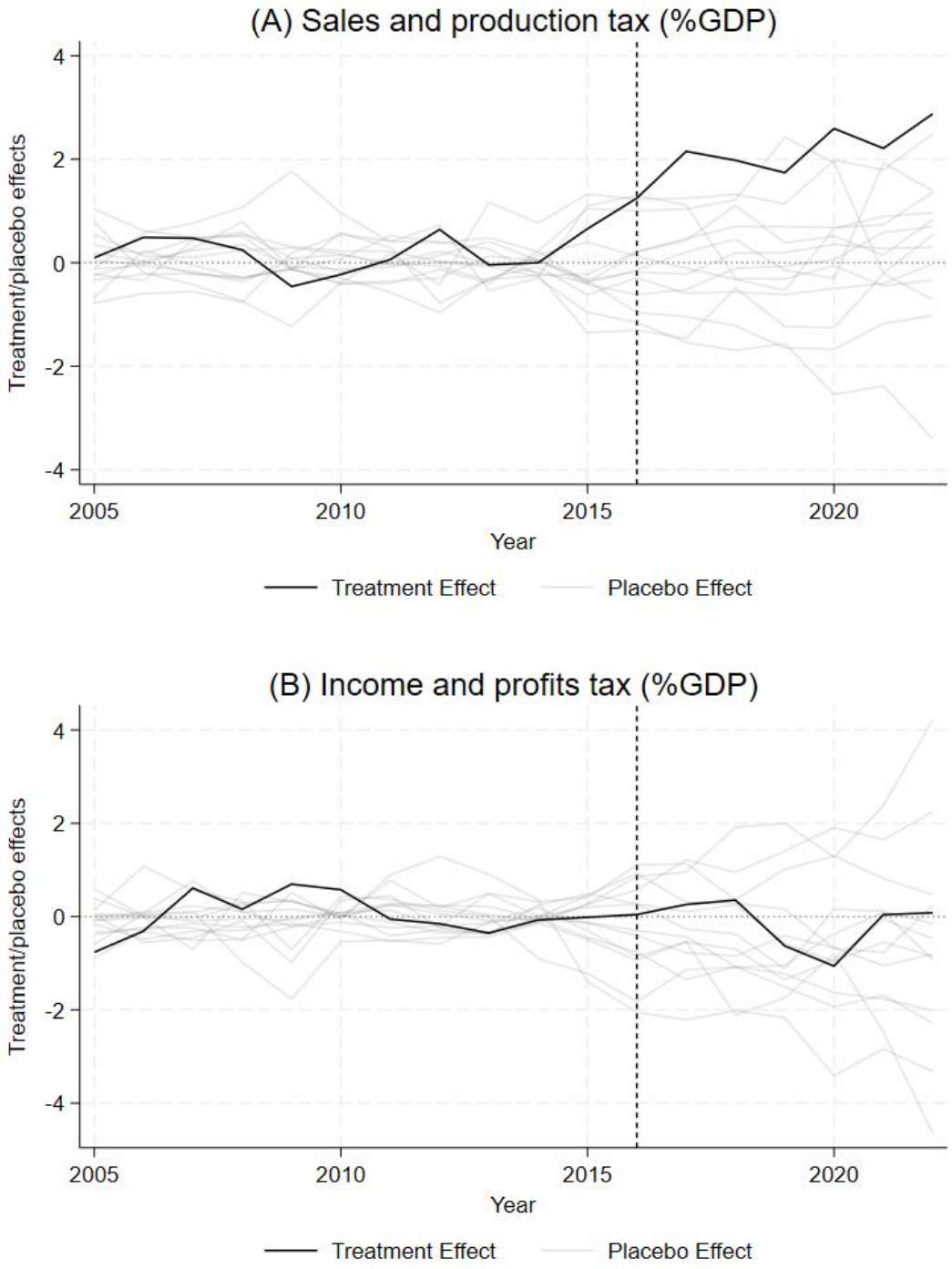

(A) Cash-to-GDP ratio (%)
Cash-to-GDP ratio (%)
14
13
12
11
10
9
2012
2014
2016
2018
2020
2022
Year
India
synthetic India


(B) Total number of card and e-money transactions
Number on logarithm
24
23
22
21
20
2012
2014
2016
2018
2020
2022
Year
India
synthetic India




Note: This figure plots the trends in cash usage and digital payment for actual India and its synthetic counterpart. The synthetic India represents the trend in India without demonetization. Outcome variables include cash-to-GDP ratio, total number of card and e-money transactions in logarithm, and annual growth rate of the total value of card and e-money payments (%). The vertical dashed line marks the year (2016) when demonetization was announced, separating the sample into pre- and post-intervention periods. Due to data availability, the series for the growth rate of card and e-money payments begins in 2013 rather than 2012.

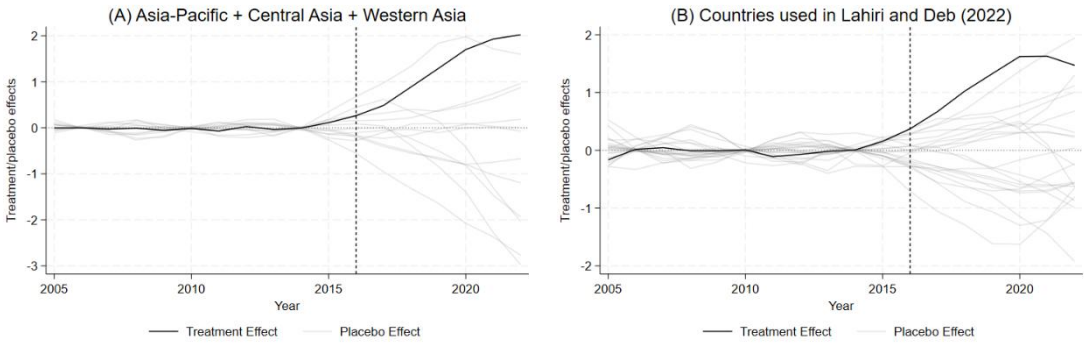
**Fig. 4. Trends in cash usage and digital payment: Actual and synthetic India**



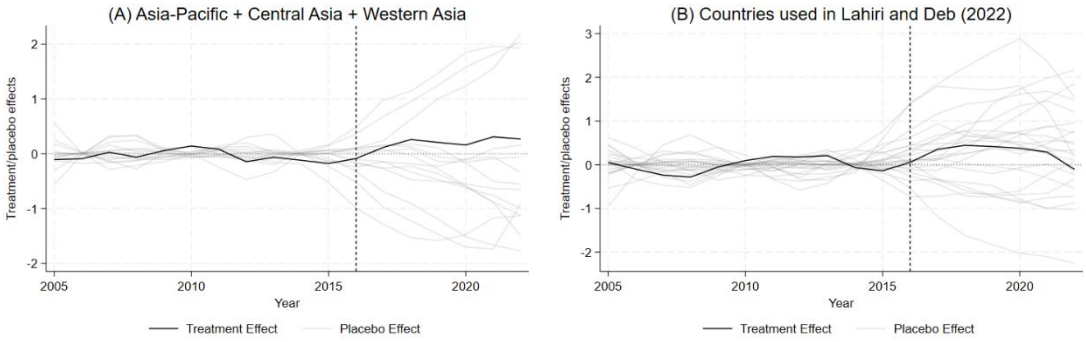

Note: This figure presents the results of the in-space placebo test. The outcome variables include raw series of sales and production tax revenue (%GDP) and income and profits tax revenue (%GDP). Each gray line represents the difference in outcome variables between a country in the donor pool and its synthetic counterpart. The black line denotes the gap between the actual India and synthetic India. Donor countries with a pre-treatment MSPE more than ten times that of India are excluded to enhance comparability.

**Fig. 5. Robustness checks: Raw series of tax collections**

Sales and production tax (%GDP)

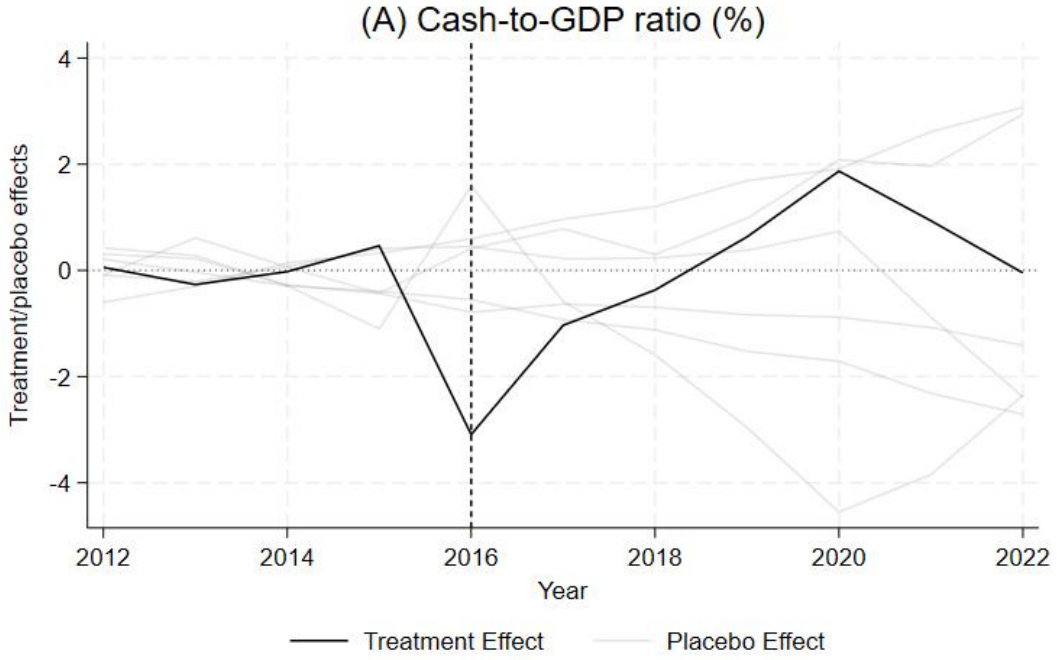


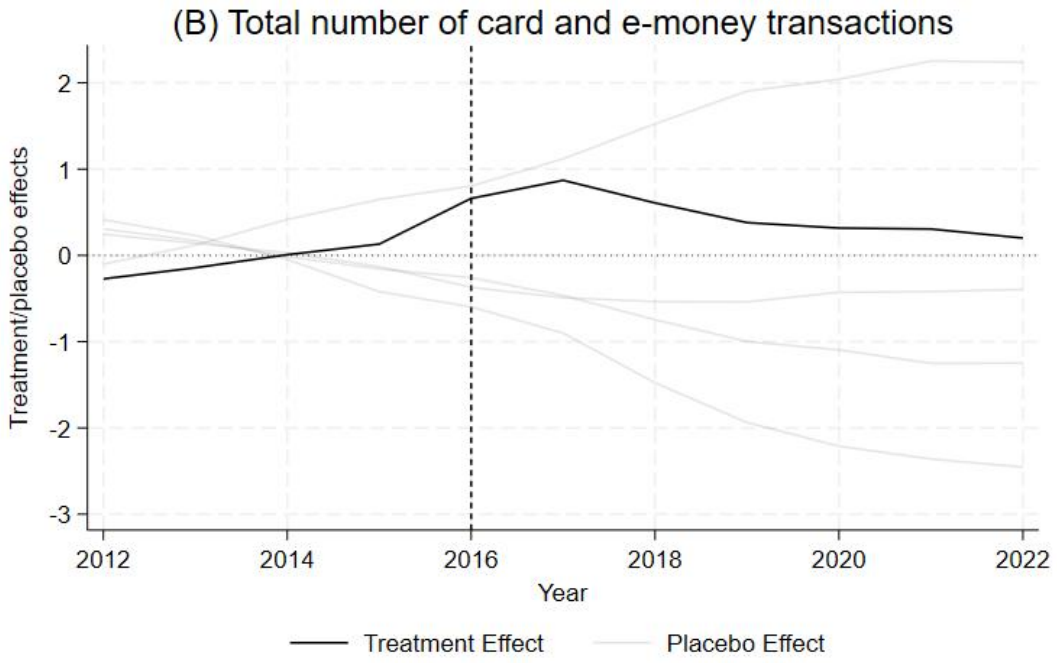

Income and profits tax (%GDP)

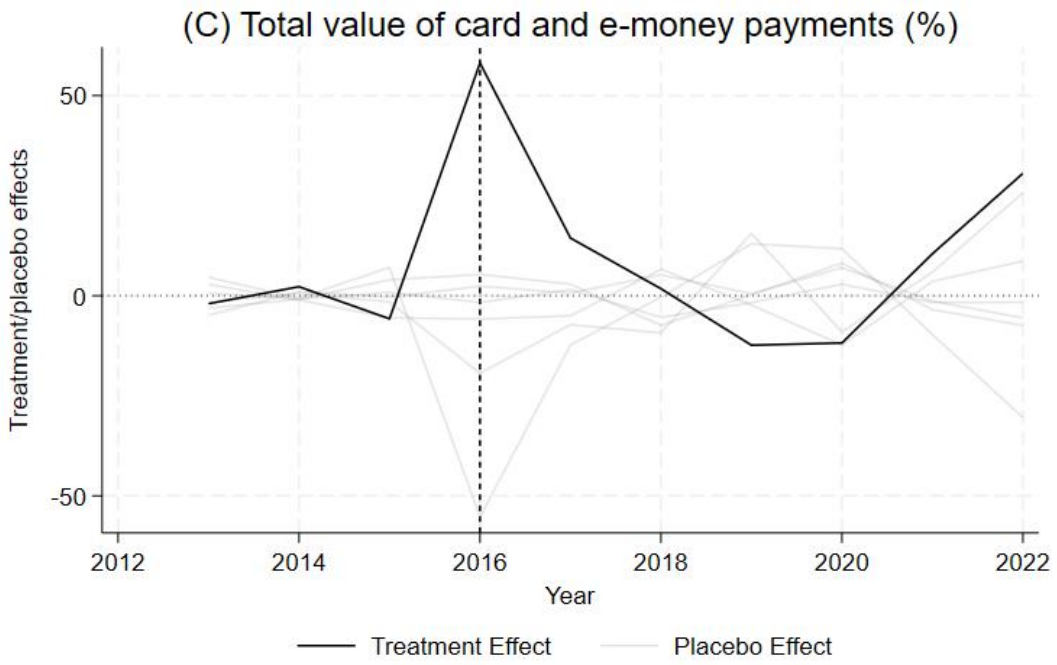



Note: This figure presents the results of the in-space placebo test. The outcome variables include sales and production tax revenue (%GDP) and income and profits tax revenue (%GDP). Panel (A) reports results using a donor pool expanded to include Central and West Asian countries. Panel (B) is based on the donor pool used in Lahiri and Deb (2022). Each gray line represents the difference in outcome variables between a country in the donor pool and its synthetic counterpart. The black line denotes the gap between the actual India and synthetic India. Donor countries with a pre-treatment MSPE more than ten times that of India are excluded to enhance comparability.

**Fig. 6. Robustness checks: Alternative donor pool in tax collections**

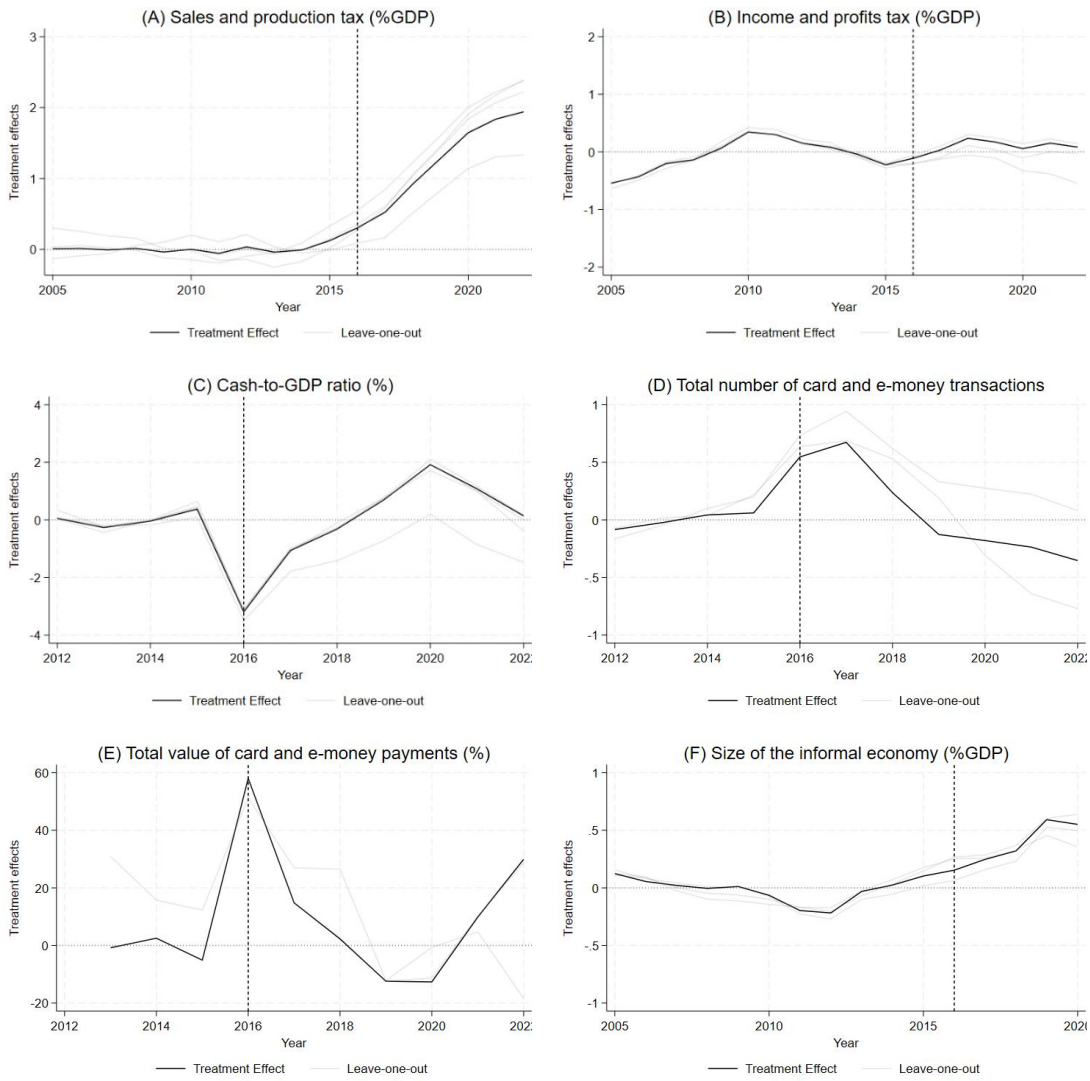

(A) Cash-to-GDP ratio (%)
Treatment/placebo effects
4
2
0
-2
-4
2012
2014
2016
2018
2020
2022
Year
Treatment Effect
Placebo Effect


(B) Total number of card and e-money transactions
Treatment/placebo effects
2
1
0
-1
-2
-3
2012
2014
2016
2018
2020
2022
Year
Treatment Effect
Placebo Effect




Note: This figure presents the results of the in-space placebo test. Outcome variables include cash-to-GDP ratio, total number of card and e-money transactions in logarithm, and annual growth rate of the total value of card and e-money payments (%). The donor pool comprises only the seven Asia-Pacific economies available in the BIS dataset. Each gray line represents the difference in outcome variables between a country in the donor pool and its synthetic counterpart. The black line denotes the gap between the actual India and synthetic India. Donor countries with a pre-treatment MSPE more than ten times that of India are excluded to enhance comparability. Due to data availability, the series for the growth rate of card and e-money payments begins in 2013 rather than 2012.

**Fig. 7. Robustness checks: Alternative donor pool in cash usage and digital payments analysis**




Note: This figure presents the results of the leave-one-out tests. Outcome variables include sales and production tax revenue (%GDP), income and profits tax revenue (%GDP), cash-to-GDP ratio, total number of card and e-money transactions in logarithm, annual growth rate of the total value of card and e-money payments (%), and size of the informal economy. Each gray line represents the estimated gap between actual India and synthetic India when one donor country receiving a positive weight is excluded from the donor pool. The black line corresponds to the baseline estimate using the full donor pool. Due to data availability, the series for the growth rate of card and e-money payments begins in 2013 rather than 2012.

**Fig. 8. Robustness checks: Leave-one-out tests**

## Table

**Table 1: Estimated policy effects on tax collections**

| Year | (1)<br>Sales and production tax (%GDP) | (2)<br>Income and profits tax (%GDP) |
|---|---|---|
| 2016 | 0.304*** | –0.113 |
| | (0) | (0.929) |
| 2017 | 0.525*** | 0.033 |
| | (0) | (1) |
| 2018 | 0.922*** | 0.235 |
| | (0) | (0.929) |
| 2019 | 1.283*** | 0.172 |
| | (0) | (0.929) |
| 2020 | 1.645*** | 0.056 |
| | (0) | (.857) |
| 2021 | 1.838*** | 0.151 |
| | (0) | (1) |
| 2022 | 1.940*** | 0.083 |
| | (0) | (1) |

Note: This table presents the estimated effects of demonetization on tax collections. The outcome variables include sales and production tax revenue (%GDP) and income and profits tax revenue (%GDP). We smooth the tax variables using a five-year moving average to reduce annual fluctuations and better capture underlying trends. The estimated policy effect is defined as the difference between the outcome variable for actual India and that of synthetic India during the post-intervention period. Pseudo two-sided *p*-values (reported in parentheses) are calculated as the proportion of donor pool countries with standardized policy effects (post-treatment effects divided by the pre-treatment RMSPE) that exceed those of India. *** indicates significance at the 1% level (pseudo $p$-values $< 0.01$).

**Table 2: Estimated policy effects on cash usage and digital payment**

| Year | (1)<br>Cash-to-GDP ratio (%) | (2)<br>Total number of card and e-money transactions in logarithm | (3)<br>The growth rate of the total value of card and e-money payments (%) |
|---|---|---|---|
| 2016 | –3.192*** | 0.546*** | 58.035*** |
| | (0) | (0) | (0) |
| 2017 | –1.066*** | 0.673*** | 14.801* |
| | (0) | (0) | (0.063) |
| 2018 | –0.318 | 0.236 | 2.256 |
| | (0.583) | (0.333) | (0.813) |
| 2019 | 0.692 | –0.127 | –12.411 |
| | (0.25) | (0.533) | (0.188) |
| 2020 | 1.917*** | –0.179 | –12.651 |
| | (0) | (0.4) | (0.313) |
| 2021 | 1.079 | –0.237 | 9.826 |
| | (0.583) | (0.533) | (0.25) |
| 2022 | 0.138 | –0.353 | 29.915 |
| | (0.917) | (0.467) | (0.188) |

Note: This table presents the estimated effects of demonetization on cash usage and digital payment. Outcome variables include cash-to-GDP ratio (%), total number of card and e-money transactions in logarithm, and annual growth rate of the total value of card and e-money payments (%). The estimated policy effect is defined as the difference between the outcome variable for actual India and that of synthetic India during the post-intervention period. Pseudo two-sided *p*-values (reported in parentheses) are

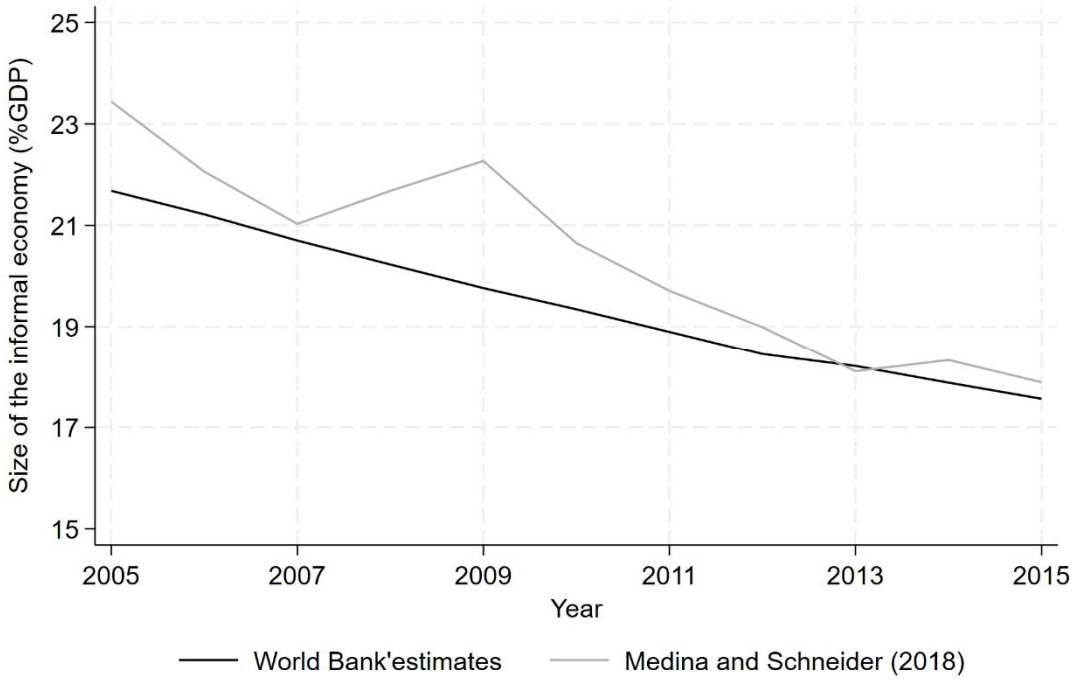
calculated as the proportion of donor pool countries with standardized policy effects (post-treatment effects divided by the pre-treatment RMSPE) that exceed those of India. *** indicates significance at the 1% level (pseudo *p*-values < 0.01).

**Table 3: Estimated policy effects on the informal economy**

| Year | Size of the informal economy (%GDP) |
|---|---|
| 2016 | 0.153 |
| | (0.692) |
| 2017 | 0.247 |
| | (0.308) |
| 2018 | 0.321 |
| | (0.385) |
| 2019 | 0.592 |
| | (0.308) |
| 2020 | 0.551 |
| | (0.462) |

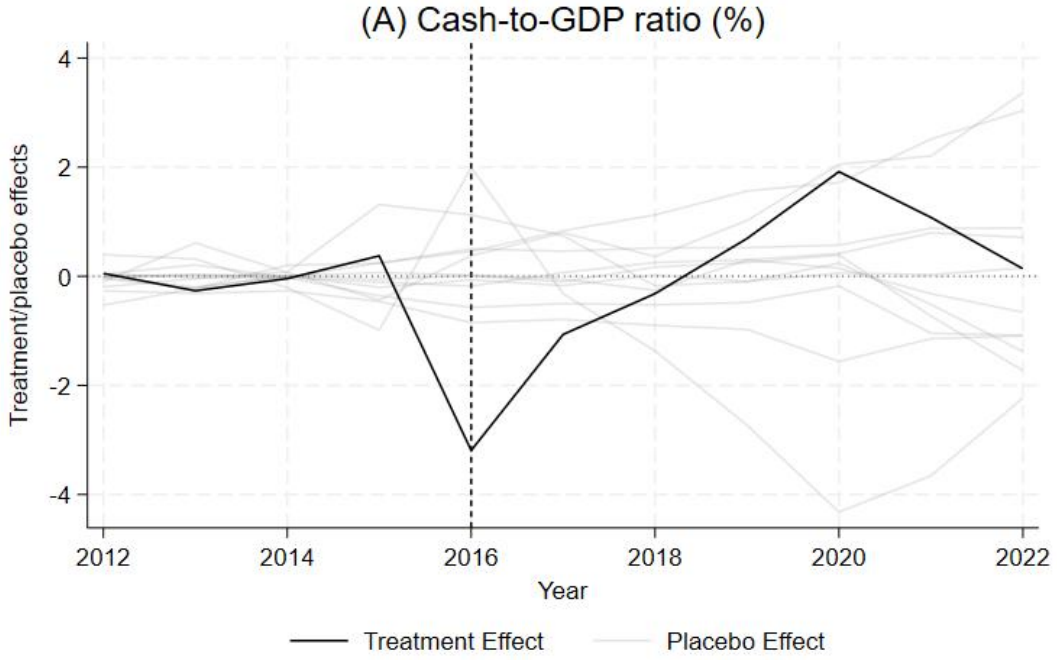
Note: This table presents the estimated effects of demonetization on the size of the informal economy. The outcome variable is the size of the informal economy (%GDP). The estimated policy effect is defined as the difference between the outcome variable for actual India and that of synthetic India during the post-intervention period. Pseudo two-sided *p*-values (reported in parentheses) are calculated as the proportion of donor pool countries with standardized policy effects (post-treatment effects divided by the pre-treatment RMSPE) that exceed those of India.

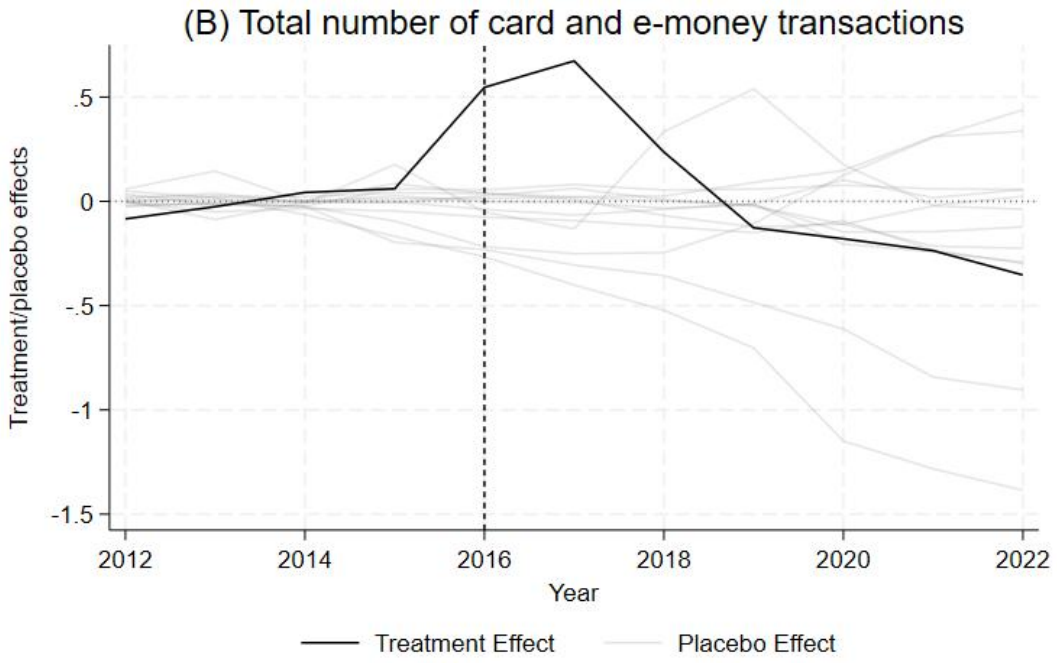
## Appendix

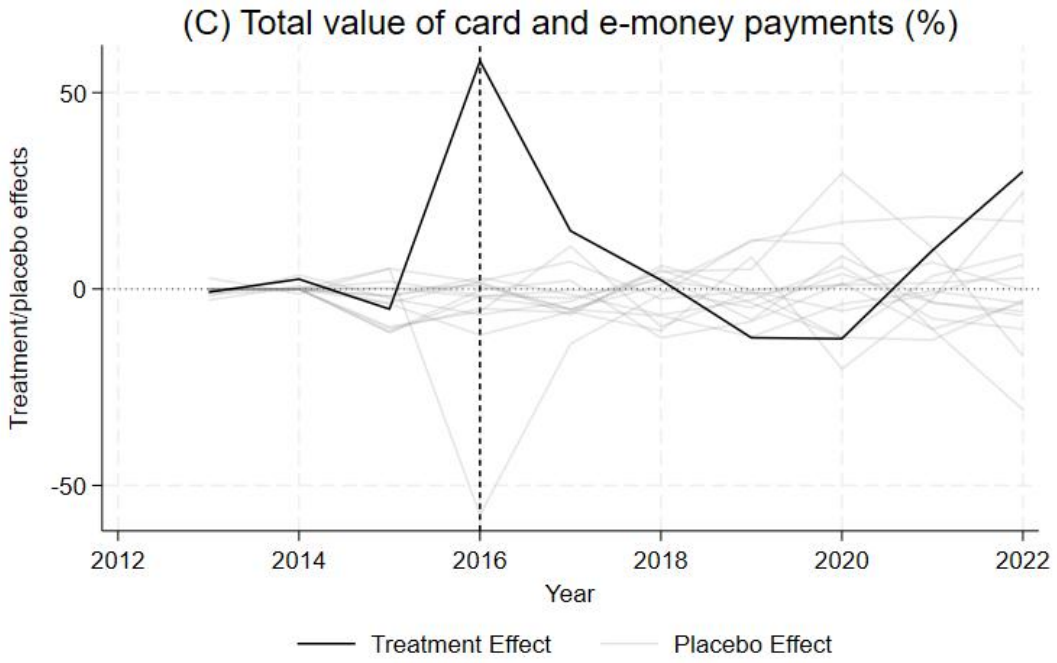



Note: This figure compares two alternative measures of the size of the informal economy (as a share of GDP). The black line reports the World Bank's estimates, while the gray line reports estimates from Medina and Schneider (2018).

**Fig. A1. Estimates of the size of the informal economy in India**

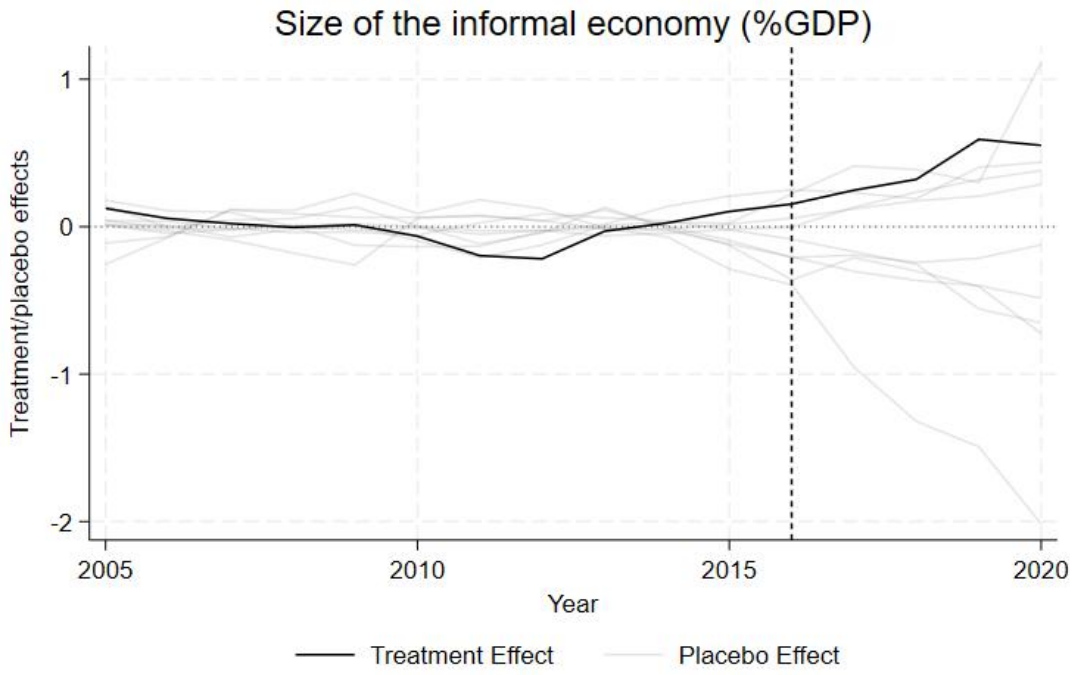

(A) Cash-to-GDP ratio (%)
Treatment/placebo effects
4
2
0
-2
-4
2012
2014
2016
2018
2020
2022
Year
Treatment Effect
Placebo Effect


(B) Total number of card and e-money transactions
Treatment/placebo effects
.5
0
-.5
-1
-1.5
2012
2014
2016
2018
2020
2022
Year
Treatment Effect
Placebo Effect




Note: This figure presents the results of the in-space placebo test. Outcome variables include cash-to-GDP ratio, total number of card and e-money transactions in logarithm, and annual growth rate of the total value of card and e-money payments (%). Each gray line represents the difference in outcome variables between a country in the donor pool and its synthetic counterpart. The black line denotes the gap between the actual India and synthetic India. Donor countries with a pre-treatment MSPE more than ten times that of India are excluded to enhance comparability. Due to data availability, the series for the growth rate of card and e-money payments begins in 2013 rather than 2012.

**Fig. A2. In-space placebo tests: Cash usage and digital payment**




Note: This figure presents the results of the in-space placebo test. The outcome variable is the size of the informal economy. Each gray line represents the difference in outcome variables between a country in the donor pool and its synthetic counterpart. The black line denotes the gap between the actual India and synthetic India. Donor countries with a pre-treatment MSPE more than ten times that of India are excluded to enhance comparability. Due to data availability, the series for the informal economy end in 2020.

**Fig. A3. In-space placebo tests: Size of the informal economy**

**Table A1: Country weights in synthetic India: Tax collections**

| Country | (1)<br>Sales and production tax (%GDP) | (2)<br>Income and profits tax (%GDP) |
|---|---|---|
| Australia | 0.284 | 0 |
| Bangladesh | 0 | 0 |
| Cambodia | 0 | 0.157 |
| China | 0.304 | 0 |
| Hong Kong | 0 | 0 |
| Indonesia | 0 | 0 |
| Japan | 0 | 0 |
| South Korea | 0 | 0 |
| Malaysia | 0 | 0 |
| Nepal | 0 | 0.095 |
| Philippines | 0.11 | 0 |
| Singapore | 0 | 0 |
| Thailand | 0.302 | 0.748 |
| Vietnam | 0 | 0 |

Note: This table reports the weights assigned to each donor country in constructing the synthetic India. The outcome variables include sales and production tax revenue (%GDP) and income and profits tax revenue (%GDP).

**Table A2: Predictor balance: Tax collections**

| | Sales and production tax (%GDP) | | Income and profits tax (%GDP) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | India | Synthetic India | India | Synthetic India |
| Outcome variable in 2014 | 8.355 | 8.363 | 5.715 | 5.761 |
| Outcome variable in 2010 | 8.136 | 8.134 | 5.811 | 5.467 |
| Outcome variable in 2006 | 8.313 | 8.298 | 4.219 | 4.651 |
| Logarithm of GDP per capita | 7.107 | 9.147 | 7.107 | 8.110 |
| Share of consumption (%GDP) | 67.275 | 66.500 | 67.275 | 73.276 |
| Dependency ratio (%) | 57.024 | 44.741 | 57.024 | 46.431 |
| Population growth (%) | 1.420 | 1.036 | 1.420 | 0.803 |
| Tax-to-GDP ratio (%) | 16.614 | 19.505 | 16.614 | 15.049 |

Note: This table presents the pre-treatment averages of predictor variables for India and its synthetic counterpart. The outcome variables include sales and production tax revenue (%GDP) and income and profits tax revenue (%GDP). We smooth the tax variables using a five-year moving average to reduce annual fluctuations and better capture underlying trends. We incorporate lagged outcome variables in 2006, 2010, and 2014 to account for pre-treatment trends.

**Table A3: Country weights in synthetic India: Cash usage and digital payment**

| Country | (1)<br>Cash-to-GDP ratio (%) | (2)<br>Total number of card and e-money transactions in logarithm | (3)<br>The growth rate of the total value of card and e-money payments (%) |
|---|---|---|---|
| Australia | 0 | 0 | 0 |
| Belgium | -- | 0 | 0 |
| Brazil | 0 | 0 | 0 |
| Canada | 0 | 0 | 0 |
| China | 0.443 | 0.248 | 0.963 |
| France | -- | 0 | 0 |
| Germany | -- | 0 | 0 |
| Hong Kong | 0 | -- | -- |
| Indonesia | 0.194 | 0.752 | 0 |
| Italy | -- | 0 | 0 |
| Japan | 0.303 | -- | 0 |
| Saudi Arabia | 0.061 | 0 | 0.037 |
| South Korea | 0 | 0 | 0 |
| Singapore | 0 | 0 | 0 |
| Spain | -- | 0 | 0 |
| United Kingdom | 0 | 0 | 0 |
| United States | 0 | 0 | 0 |

Note: This table reports the weights assigned to each donor country in constructing the synthetic India for the analysis of cash usage and digital payment. Outcome variables include cash-to-GDP ratio (%), total number of card and e-money transactions in logarithm, and annual growth rate of the total value of card and e-money payments (%). "--" indicates that the corresponding country lacks data for the relevant variable and is therefore excluded from the analysis.

**Table A4: Predictor balance: Cash usage and digital payment**

| | (1) Cash-to-GDP ratio (%) | | (2) Total number of card and e-money transactions in logarithm | | (3) The growth rate of the total value of card and e-money payments (%) | |
|---|---|---|---|---|---|---|
| | India | Synthetic India | India | Synthetic India | India | Synthetic India |
| Outcome variable in 2014 | 11.616 | 11.653 | 21.289 | 21.246 | 33.333 | 30.812 |
| Outcome variable in 2013 | | | | | 50 | 50.807 |
| Outcome variable in 2012 | 11.867 | 11.815 | 20.669 | 20.753 | | |
| Logarithm of GDP per capita | 7.277 | 9.286 | 7.277 | 8.258 | 7.305 | 8.987 |
| Share of consumption (%GDP) | 68.272 | 63.176 | 68.272 | 63.035 | 68.648 | 53.395 |
| Dependency ratio (%) | 53.709 | 48.358 | 53.709 | 47.078 | 53.259 | 38.887 |
| Population growth (%) | 1.290 | 0.739 | 1.290 | 1.065 | 1.262 | 0.764 |
| Tax to GDP ratio (%) | 16.624 | 15.847 | 16.624 | 12.893 | 16.475 | 17.768 |

Note: This table presents the pre-treatment averages of predictor variables for India and its synthetic counterpart. Outcome variables include cash-to-GDP ratio (%), total number of card and e-money transactions in logarithm, and annual growth rate of the total value of card and e-money payments (%). Since the earliest available year for BIS payment data is 2012, we incorporate lagged outcome variables in 2012 and 2014 for the cash-to-GDP ratio and transaction volume, and from 2013 and 2014 for the growth rate of payment value.

**Table A5: Country weights in synthetic India: Informal economy**

| Country | Size of the informal economy (%GDP) |
|---|---|
| Australia | 0 |
| Bangladesh | 0 |
| Cambodia | 0.207 |
| China | 0.105 |
| Hong Kong | 0 |
| Indonesia | 0 |
| Japan | 0 |
| South Korea | 0 |
| Malaysia | 0 |
| Nepal | 0 |
| Philippines | 0 |
| Singapore | 0 |
| Thailand | 0 |
| Vietnam | 0.688 |

Note: This table reports the weights assigned to each donor country in constructing the synthetic India. The outcome variables is the size of the informal economy (%GDP).

**Table A6: Predictor balance: Informal economy**

| | Size of the informal economy (%GDP) | |
|---|---|---|
| | (1) | (2) |
| | India | Synthetic India |
| Outcome variable in 2014 | 17.882 | 17.857 |
| Outcome variable in 2010 | 19.346 | 19.411 |
| Outcome variable in 2006 | 21.218 | 21.162 |
| Logarithm of GDP per capita | 7.107 | 7.610 |
| Share of consumption (%GDP) | 67.275 | 71.377 |
| Dependency ratio (%) | 57.024 | 47.957 |
| Population growth (%) | 1.420 | 1.274 |
| Tax to GDP ratio (%GDP) | 16.614 | 14.089 |

Note: This table presents the pre-treatment averages of predictor variables for India and its synthetic counterpart. The outcome variables is the size of the informal economy (%GDP). We incorporate lagged outcome variables in 2006, 2010, and 2014 to account for pre-treatment trends.

**Table A7: Robustness checks: Raw series of tax collections**

| Year | (1)<br>Sales and production tax (%GDP) | (2)<br>Income and profits tax (%GDP) |
|---|---|---|
| 2016 | 1.247* | 0.043 |
| | (0.071) | (1) |
| 2017 | 2.152*** | 0.258 |
| | (0) | (0.857) |
| 2018 | 1.980*** | 0.354 |
| | (0) | (0.929) |
| 2019 | 1.740 | –0.629 |
| | (0.143) | (0.786) |
| 2020 | 2.592*** | –1.061 |
| | (0) | (0.5) |
| 2021 | 2.211*** | 0.041 |
| | (0) | (0.929) |
| 2022 | 2.877*** | 0.081 |
| | (0) | (1) |

Note: This table presents the estimated effects of demonetization on tax collections. The outcome variables include raw series of sales and production tax revenue (%GDP) and income and profits tax revenue (%GDP). We use raw series of tax collections to estimate SCM. The estimated policy effect is defined as the difference between the outcome variable for actual India and that of synthetic India during the post-intervention period. Pseudo two-sided *p*-values (reported in parentheses) are calculated as the proportion of donor pool countries with standardized policy effects (post-treatment effects divided by the pre-treatment RMSPE) that exceed those of India. * and *** indicate significance at the 10% and 1% level (pseudo *p*-values < 0.1 and < 0.01), respectively.

**Table A8: Robustness checks: Alternative donor pool in tax collections**

| | Sales and production tax (%GDP) | | Income and profits tax (%GDP) | |
|---|---|---|---|---|
| Year | (1) Asia-Pacific + Central Asia + Western Asia | (2) Countries used in Lahiri and Deb (2022) | (3) Asia-Pacific + Central Asia + Western Asia | (4) Countries used in Lahiri and Deb (2022) |
| 2016 | 0.265*** | 0.371** | –0.087 | 0.053 |
| | (0) | (0.038) | (0.833) | (0.846) |
| 2017 | 0.486*** | 0.669*** | 0.117 | 0.352 |
| | (0) | (0) | (0.722) | (0.615) |
| 2018 | 0.887*** | 1.026** | 0.261 | 0.443 |
| | (0) | (0.038) | (0.611) | (0.615) |
| 2019 | 1.290*** | 1.327*** | 0.208 | 0.417 |
| | (0) | (0) | (0.667) | (0.692) |
| 2020 | 1.702*** | 1.625*** | 0.161 | 0.378 |
| | (0) | (0) | (0.722) | (0.731) |
| 2021 | 1.932*** | 1.630** | 0.310 | 0.289 |
| | (0) | (0.038) | (0.722) | (0.731) |
| 2022 | 2.024*** | 1.470 | 0.272 | –0.107 |
| | (0) | (0.154) | (0.667) | (0.846) |

Note: This table presents the estimated effects of demonetization on tax collections. The outcome variables include sales and production tax revenue (%GDP) and income and profits tax revenue (%GDP). We smooth the tax variables using a five-year moving average to reduce annual fluctuations and better capture underlying trends. Column (1) and (3) report results using a donor pool expanded to include Central and West Asian countries. Column (2) and (4) are based on the donor pool used in Lahiri and Deb

(2022). The estimated policy effect is defined as the difference between the outcome variable for actual India and that of synthetic India during the post-intervention period. Pseudo two-sided *p*-values (reported in parentheses) are calculated as the proportion of donor pool countries with standardized policy effects (post-treatment effects divided by the pre-treatment RMSPE) that exceed those of India. ** and *** indicate significance at the 5% and 1% level (pseudo *p*-values < 0.05 and < 0.01), respectively.

**Table A9: Robustness checks: Alternative donor pool in cash usage and digital payments analysis**

| Year | (1)<br>Cash-to-GDP ratio (%) | (2)<br>Total number of card and e-money transactions in logarithm | (3)<br>The growth rate of the total value of card and e-money payments (%) |
|---|---|---|---|
| 2016 | –3.092*** | 0.660*** | 58.124*** |
| | (0) | (0) | (0) |
| 2017 | –1.034*** | 0.870*** | 14.405*** |
| | (0) | (0) | (0) |
| 2018 | –0.369 | 0.608 | 1.749 |
| | (0.714) | (0.4) | (1) |
| 2019 | 0.632 | 0.380 | –12.353*** |
| | (0.857) | (0.8) | (0) |
| 2020 | 1.870*** | 0.317 | –11.813 |
| | (0) | (0.8) | (0.333) |
| 2021 | 0.936 | 0.306 | 10.465 |
| | (0.714) | (0.8) | (0.167) |
| 2022 | –0.046 | 0.201 | 30.552 |
| | (1) | (0.8) | (0.167) |

Note: This table presents the estimated effects of demonetization on cash usage and digital payment. Outcome variables include cash-to-GDP ratio (%), total number of card and e-money transactions in logarithm, and annual growth rate of the total value of card and e-money payments (%). The donor pool comprises only the seven Asia-Pacific economies available in the BIS dataset. The estimated policy effect is defined as the difference between the outcome variable for actual India and that of synthetic India

during the post-intervention period. Pseudo two-sided *p*-values (reported in parentheses) are calculated as the proportion of donor pool countries with standardized policy effects (post-treatment effects divided by the pre-treatment RMSPE) that exceed those of India. *** indicates significance at the 1% level (pseudo *p*-values < 0.01).

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