Shocks and Technology Adoption: Evidence from Electronic Payment Systems

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We provide evidence on the importance of coordination frictions in technology adoption, using data from a large provider of electronic wallets during the Indian demonetization. Exploiting geographical variation in exposure to the demonetization, we show that adoption of the wallet increased persistently in response to the large but temporary cash contraction, consistent with the predictions of a technology adoption model with complementarities. Model estimates indicate that adoption would have been 45% lower without complementarities. Our results illustrate how large but temporary interventions can help overcome coordination frictions, though we caution that such interventions may also exacerbate initial differences in adoption.

I. Introduction

A rich literature in economics has argued that coordination failures could be an important obstacle to the adoption of new technologies (Rosenstein-

Rodan 1943; Carlton and Klamer 1983). Coordination failures arise when decisions to adopt a new technology are complements across users—that is, when the private value of adoption for each single user depends positively on adoption by other users (Katz and Shapiro 1985, 1986).[1] In these situations, expectations of low adoption can become self-fulfilling. While the possibility of coordination failures is theoretically well understood (Murphy, Shleifer, and Vishny 1989; Matsuyama 1995), direct evidence of their importance is scarce. Using data on the adoption of a digital wallet technology during the 2016 Indian demonetization, our paper provides novel evidence on coordination failures in technology adoption and studies the role that policy can play in addressing them.

There are two reasons why documenting the role of coordination failures in adoption of the digital wallet technology we study is useful. First, this product provides a clean test case for the general proposition that coordination failures can slow down technology adoption. Digital wallets are network goods; this makes adoption decisions complements across users and creates scope for coordination failures (Katz and Shapiro 1994; Rysman 2007). Relative to other network goods, digital wallets are generally cheap and simple to adopt, which helps isolate the role of coordination problems. Second, digital wallets are a canonical example of financial technology ("fintech") products. The rapid diffusion of information technology over the past two decades has raised expectations about the potential for fintech to improve financial inclusion, particularly in developing countries, where

[1] Katz and Shapiro (1985, 1986) highlight how externalities can arise both directly—when the number of users affects the quality of the product—or indirectly—in situations where the number of users affects the value of other add-on products because of compatibility (e.g., hardware/software) or postpurchase services (e.g., cars).

fostering access to financial services remains a key goal for policy makers. Understanding the obstacles to their adoption is therefore also relevant to policy.

To better identify the role of coordination failures, we study adoption of the electronic wallet technology by retailers after the 2016 Indian demonetization. This unexpected policy shock resulted in a large but temporary reduction in the availability of cash, leading to a temporary incentive to adopt the technology. Our analysis is organized into three parts. First, we develop a dynamic model of technology adoption with complementarities and use it to characterize the key features of the response of adoption of digital payments to a temporary shock to the availability of traditional means of payment. Second, we use merchant-level data from the leading fintech payment system in India and quasi-exogenous variation in exposure to the demonetization to test the model's predictions. Third, we quantify the contribution of complementarities to the overall adoption response by structurally estimating our model.

Our main findings are the following. First, the demonetization caused an adoption wave among merchants, characterized by three features: a persistent increase in the *size* of the platform, that is, the total number of merchants using it; a persistent increase in the platform's *adoption rate*, that is, the number of new merchants adopting the platform each month; and *state dependence* in adoption, meaning that the long-run adoption response depends on the initial (preshock) strength of complementarities. The latter two features are important, as we show that they are distinctive predictions of the model when complementarities are present. Second, our quantitative estimation of the model shows that complementarities not only were present but also played a large role: they account for approximately 45% of the long-run adoption response.

Taken together, our results suggest that coordination problems could be an important obstacle to the diffusion of fintech payment systems. The results also indicate that temporary interventions can be sufficient to overcome these coordination problems.[2] However, on this point, we offer an important caveat: interventions that are very brief can also exacerbate long-run differences in adoption across markets or regions. In fact, the state dependence created by adoption externalities is key to this insight. In markets or regions where some core of users already have adopted the technology, a short-lived intervention can durably spread adoption to new users. Instead, if the initial penetration of the technology is low, a short-lived

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[2] This statement should not be interpreted as suggesting that the demonetization generated net benefits for the Indian economy. As we discuss later, the policy had significant economic costs. A full policy evaluation of this event—which is outside the scope of this paper—should clearly weigh any benefits related to technology adoption against these costs.

intervention is unlikely to have any persistent effects. We find evidence for this mechanism in the data and explore its policy implications, using counterfactual experiments in our estimated model. This analysis suggests that the duration of a policy intervention has a first-order economic effect on not only the level but also the dispersion of the adoption response.

The empirical setting of the paper is the Indian demonetization of 2016. On November 8, 2016, the Indian government announced that it would void the two largest denominations of currency in circulation and replace them with new bills. At the time of the announcement, the voided bills accounted for 86.4% of the total cash in circulation. The public was not given advanced warning, and the bills were voided effective immediately. A 2-month deadline was announced for exchanging the old bills for new currency. In order to do so, old bills had to be deposited in the banking sector. However, withdrawal limits combined with frictions in the creation and distribution of the new bills meant that immediate cash withdrawal was constrained. As a result, bank deposits spiked but cash in circulation fell. Cash transactions became harder to conduct, but funds remained available for use in electronic payments. Importantly, though the shock was very large, it was also temporary, as cash availability had normalized shortly after January 2017.

In section II, we start by showing that the demonetization led to a large aggregate increase in the use of electronic payments. We focus primarily on data from the largest provider of non–debit card electronic payments in India. The provider offers a digital wallet consisting of a mobile app that allows customers to pay at stores using funds deposited in their bank accounts. Payment is then transferred to retailers' bank accounts via the app. The pecuniary costs associated with the adoption of this technology for retailers are small; in fact, there are no usage fees, and all that is required to join the platform is to have a bank account and a mobile phone, both of which were common in India by 2016 (Agarwal et al. 2017). Aggregate activity on the platform increased dramatically during the 2 months immediately following the demonetization announcement. Additionally, this increase in activity was persistent, though as highlighted above, the shock was not. There was no significant mean reversion in the aggregate number of retailers using the technology or in aggregate transaction volumes once cash withdrawal constraints were lifted.

The aggregate evidence thus suggests that the temporary contraction in cash led to a persistent increase in adoption of fintech payments. However, this finding alone does not necessarily establish that complementarities played a role in this process. To further investigate this aspect, in section III, we study a dynamic technology adoption model and characterize the testable key implications of adoption complementarities. The model builds on the frameworks of Burdzy, Frankel, and Pauzner (2001), Frankel and Burdzy (2005), and Guimarães, Machado, and Pereira (2020). Firms

face a choice between two payment technologies (cash and the electronic wallet), one of which (the wallet) is subject to positive adoption externalities—the flow profits from operating under this technology increases with its rate of use by other firms.[3] Additionally, the amount of cash available for transactions is subject to aggregate shocks, which affect the relative benefits of adopting one payment technology over the other.

The model predicts that following a large temporary shock to the availability of cash, the total number of firms using the platform increases persistently, consistent with the aggregate evidence of section II. However, it delivers two additional predictions. First, with complementarities, the shock—on top of durably increasing the size of the platform—also increases its adoption rate in a persistent way. In other words, the number of new firms joining the platform every period remains higher even after the shock has dissipated. The reason is that with complementarities, the initial adoption triggered by the shock by temporarily expanding the platform increases the relative future value of adoption for other firms. This snowball effect can generate endogenous persistence in the increase in adoption rates. Second, the model predicts that adoption responses exhibit state dependence: the long-run adoption response depends on the preshock adoption rate. The intuition for this result is simple: all other things equal, higher preshock adoption rates increase the strength of adoption externalities, making it easier to reach the tipping point beyond which the platform has sufficient critical mass to continue growing even after the initial shock dissipates.

In section IV, we then show that the empirical predictions of the model with complementarities highlighted above are consistent with the adoption responses observed in the data after the demonetization. In order to do this, we provide an empirical design to estimate the causal impact of the cash contraction on adoption. Our empirical design exploits variation across districts in the importance of chest banks—local bank branches in charge of the distribution of new currency—to identify variation in exposure to the shock. This design allows us to isolate the effect of the cash contraction from other effects of the demonetization, therefore overcoming the limitations of the aggregate evidence. We show that the districts that were more exposed to the cash crunch also experienced a larger and more persistent increase in total adoption following the demonetization, the first prediction of the model. Crucially, higher exposure also predicts a larger increase

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[3] Different mechanisms could account for this relationship: e.g., the more merchants are on the platform, the more valuable it is for consumers to use it, which in turn increases new merchants' incentive to join the platform. We discuss possible microfoundations for externalities in sec. III.A.3 and provide a specific example with a two-sided market in app. sec. B.5 (apps. A–H are available online), which we show has an isomorphic representation to our baseline model.

in the number of new firms joining the platform, even after restrictions on cash withdrawals are lifted—the second prediction of the model.[4]

Finally, we find evidence consistent with state dependence, the third prediction of the model with complementarities. Our main test exploits variation among districts in their distance to "payment hubs"—cities where the penetration of the technology was already high before the demonetization—as a way to identify areas where the presence of complementarities should generate higher marginal benefits of joining the platform.[5] We find that districts located closer to payment hubs displayed a statistically and economically stronger response to the shock in both the short run and the long run, as the model would predict. The importance of state dependence is also confirmed by firm-level tests that examine at the propagation of adoption between merchants within the same narrow geography and industry (Goolsbee and Klenow 2002; Munshi 2004).

In section IV.E, we also examine which economic mechanism is more likely to generate the externalities evident in our reduced-form estimates. While an exact quantification of the different mechanisms is outside the scope of the paper, our evidence supports the idea that network effects, as opposed to learning, are more likely to play a central role in our setting. To support this claim, we study three pieces of evidence: the long-run, intensive margin response of retailers that adopted the wallet either before or immediately at the onset of the demonetization; the heterogeneity of the effects across proxies for social learning; and the results from a new survey of Indian consumers and retailers that adopted electronic payments during the demonetization. As we argue in the paper, the results from all three approaches are consistent with network effects among retailers being economically important.

Altogether, this reduced-form evidence shows that a model with adoption complementarities can account for the qualitative features of the adoption response caused by the demonetization. However, it is silent about the quantitative contribution of complementarities to the adoption response. In

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[4] We provide a number of robustness tests that confirm the causal interpretation of these results. Among other things, we use our empirical design to show that consumption also temporarily declined following the shock. This evidence helps to reinforce the notion that our results capture the effects of a temporary shock to cash rather than the effects of a demand shock.

[5] The model has the more direct prediction that district-level preshock adoption rates should positively predict the long-run adoption response, which we confirm in the data. But this empirical approach is subject to the standard reflection problem (Manski 1993): independent of network effects, preshock adoption rates may be determined by common unobserved characteristics of local retailers that also determine their adoption choices in the long run. We discuss this issue in more detail in sec. IV.C and explain why the distance to hubs analysis helps address this issue. Among other results, we show that distance to hubs does not predict adoption of other related technologies, such as mobile phones or fintech loans, either before or during demonetization, as one might have expected under alternative interpretations of this test.

order to address this issue, in section V, we estimate the dynamic adoption model of section III via simulated method of moments, using our data on fintech payments. The key parameter of interest is the size of adoption complementarities. Following the intuition described above, we show that this parameter can be identified using the difference between short- and long-run adoption rates following the shock.

Using the estimates of the model, we provide two main results. First, we show that complementarities are quantitatively important in understanding the total adoption response: they account for approximately 45% of the total response of adoption to the demonetization, in the sense that the medium-run adoption rate would have been 45% lower (and declining), had the technology featured no complementarities in adoption. Second, we show that the persistence of the shock is crucial to understanding its effects in terms of both average adoption and the variance of adoption across regions. As discussed earlier, temporary interventions may increase overall adoption. However, because of state dependence, they can also exacerbate initial differences in adoption. Consistent with this intuition, we show that, keeping the present value of the decline in cash constant, a cash crunch with a smaller initial magnitude (by around 50%) but a longer half-life (by a factor of 2) would have led to higher long-run adoption rates (by about 20%) and lower dispersion. Thus, an implication of our model is that policy makers with a preference for uniform adoption across regions or sectors should generally favor smaller but more persistent interventions.

Contribution to the literature.—We contribute to three areas of research. First, our work relates to the literature studying the role of strategic complementarities in technology adoption (Katz and Shapiro 1985; Farrell and Saloner 1986; Arthur 1989; Sakovics and Steiner 2012).[6] Our specific contribution is to test the dynamic implications of strategic complementarities, using electronic payments during the demonetization as our laboratory. In particular, we quantify the extent to which strategic complementarities allow temporary shocks to have long-lasting effects on adoption.[7] In related work, Björkegren (2018) studies adoption of mobile phones (a network

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[6] There is an extensive literature on slow adoption of new technologies (Rosenberg 1972; Hall and Khan 2003), which offers several examples of firms failing to use efficiency-enhancing technologies (Mansfield 1961) or processes (Bloom et al. 2013) for reasons ranging from the presence of organizational constraints (Atkin et al. 2017) to slow learning and information frictions (Munshi 2004; Young 2009; Conley and Udry 2010; Gupta, Ponticelli, and Tesei 2020) to lack of financial development (Bircan and De Haas 2019; Comin and Nanda 2019). For a review of this literature, see Foster and Rosenzweig (2010). Within this literature, we focus on coordination failures as a reason for the slow adoption of new technologies.

[7] A related empirical analysis of dynamic coordination problems is Foley-Fisher, Narajabad, and Verani (2020), who study self-fulfilling runs in the US life insurance market. Their analysis uses a different framework, where actions are substitutes, not complements, and largely abstracts from the persistence of responses to temporary shocks.

good exhibiting adoption complementarities) in Rwanda. Using a structural approach, the paper quantifies the net welfare effects of handset taxes, a form of permanent and targeted intervention. By contrast, we focus on the effects of an untargeted but temporary intervention, which improves our understanding of the conditions under which this type of shock may have durable effects.[8] Our work also relates to Fafchamps, Soderbom, and van den Boogaart (2021), who provide an empirical framework to disentangle whether positive externalities in adoption arise from network effects or learning. While our structural model does not allow us to explicitly quantify different potential sources of externalities, we leverage the framework in Fafchamps, Soderbom, and van den Boogaart (2021) in section IV.E and argue that in the specific empirical context where we test the model, the demonetization, network effects—consistent with the two-sided nature of the technology we analyze—appear to be more likely to drive externalities.[9] However, we recognize that learning may play a more important role in other contexts, as also argued by Munshi (2004) and Suri (2011).[10]

Within the literature on technology adoption, electronic payment systems have often provided a natural example of a technology exhibiting adoption complementarities (Katz and Shapiro 1994; Gowrisankaran and Stavins 2004; Rysman 2007), and for which coordination problems may be an important obstacle to adoption (Crowe, Rysman, and Stavins 2010). In this context, the idea that large, temporary events could be instrumental in generating a persistent shift in adoption has occasionally entered the policy discussion.[11] However, despite the frequency of such events, there

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[8] We discuss the differences between our structural framework and that of Björkegren (2018) in sec. III.A.3.

[9] In sec. IV.5, we contrast in more detail the results with those of Fafchamps, Soderbom, and van den Boogaart (2021), who find a more important role for learning in the empirical context of airtime transfers in Rwanda.

[10] Other papers providing related evidence include Saloner and Shepard (1995), who examine the role of potential network size in banks' decisions to develop ATM networks but do not study how dynamic decisions to adopt by users are influenced by network size; Tucker (2008), who studies how different types of adopters may influence the expansion of the network but also abstracts from the dynamic nature of adoption choices; and Ryan and Tucker (2012), who study the adoption of video calling by firms, using a structural model where coordination problems may create equilibrium multiplicity. Relative to these papers, our empirical strategy is more specifically focused on documenting endogenous persistence; moreover, we use a model where endogenous persistence is an equilibrium outcome, and the equilibrium is unique, lending itself more easily to counterfactuals. Finally, recent theoretical work by Buera et al. (2020) studies how coordination failures in technology adoption can amplify the effects of other steady-state distortions in a general equilibrium setting but also how changes in these distortions can spur adoption. Relative to that paper, while we abstract from general equilibrium considerations, our empirical setting allows us to identify precisely the magnitude of adoption externalities.

[11] For instance, the disruption following the February 2008 earthquake around the Lake Kivu region in Rwanda is considered to have contributed to a significant increase in the use of the credit held on mobile phones (Blumenstock, Eagle, and Fafchamps 2016).

is little work actually quantifying the size and persistence of the effects they might have on adoption. Our paper combines a unique empirical setting, the demonetization, with an explicit model of adoption dynamics, to address this question.

Second, our paper relates to work in monetary economics on the substitutability between payment instruments (Prescott 1987; Kiyotaki and Wright 1992; Aiyagari and Wallace 1997) and on the costs and benefits of cash versus electronic payments in modern economies (Alvarez and Lippi 2017; Rogoff 2017; Engert, Fung, and Segendorf 2019; Shy 2020; Alvarez et al. 2022; Williamson 2022). Our contribution is to provide evidence that strategic complementarities can change the elasticity of substitution between payment instruments. A closely related theoretical contribution is Lotz and Vasselin (2019), who introduce electronic money in a canonical monetary search model (Nosal and Rocheteau 2011).[12] That paper also highlights strategic complementarities as a potential driver of adoption, though the focus is on the theoretical conditions under which electronic payments might coexist with cash and not on the dynamic effects of aggregate shocks on adoption.[13] Within the monetary literature, our paper also relates to Chodorow-Reich et al. (2019), who quantify the welfare effects of the demonetization using a cash in advance model in which cash and electronic payments are assumed to have an elasticity of substitution that is fixed and smaller than unity. Our paper complements this analysis by studying a mechanism amplifying the increase in electronic payments after the shock to cash: the elasticity of substitution between means of payment is endogenous and reflects changes in the strength of adoption externalities.[14] We also note that unlike Chodorow-Reich et al. (2019), our analysis does not aim to provide a broad welfare evaluation of the demonetization but only to use it as a laboratory to identify the frictions that determine the adoption of electronic payments. As a result, the adoption effects should be considered in the context of the broader negative consequences of cash shortages.[15]

Third, our paper relates to the growing literature on fintech (Bartlett et al. 2018; Buchak et al. 2018; Fuster et al. 2018; Howell, Niessner, and Yermack

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[12] The new monetarist literature has explored models in which another payment instrument beside currency can be used, including He, Huang, and Wright (2008), Kim and Lee (2010), Li (2011), and Wang, Wright, and Liu (2017). In these models, the trade-off between currency and other payment instruments is not related to strategic complementarities but to other intrinsic features of the alternative payment instruments, such as risk of theft, record keeping, or interest rate earned.

[13] We compare our framework and Lotz and Vasselin (2019) in more detail in sec. III.A.3.

[14] Additionally, we provide a different research design for identifying quasi-exogenous exposure to the demonetization, which can be easily replicated using publicly available data.

[15] Relatedly, Alvarez and Argente (2020) analyzes evidence from an experiment banning cash (over electronic payments) for Uber riders in Mexico. They find a large reduction in consumer surplus from banning cash.

2018; Vallee, Ghosh, and Yao 2021). Our contribution is to establish that externalities can be a quantitatively important obstacle to adoption of digital payments systems beyond traditional pecuniary costs, such as setup or transaction fees, which are virtually absent for the technology we study. This finding is important because of the benefits of electronic payment systems documented in the literature (Jack and Suri 2014; Suri and Jack 2016; Beck et al. 2018; Yermack 2018; Agarwal et al. 2019).[16] Closely related work by Higgins (2019) explores how a permanent increase in the availability of debit cards in Mexico affected payment choices of consumers and retailers. In that environment, other frictions, such as fixed adoption costs, can impede adoption, while the features of the technology we study help us pinpoint the role of the coordination frictions created by externalities.

The rest of the paper is organized as follows. Section II provides some background on the demonetization and documents aggregate adoption effects. Section III analyzes our dynamic adoption model and derives key predictions. Section IV tests these predictions in the electronic wallet data. Section V estimates the model and provides counterfactuals. Section VI concludes.

II. Background

This section describes the demonetization and its broad effects on traditional and fintech payment systems.

A. The Demonetization

On November 8, 2016, at 8:15 p.m., Indian Prime Minister Narendra Modi announced the demonetization of Rs 500 and Rs 1,000 notes during an unexpected live television interview. The announcement was accompanied by a press release from the Reserve Bank of India (RBI), which stipulated that the two notes would cease to be legal tender in all transactions at midnight on the same day. The voided notes were the largest denominations at the time, and together they accounted for 86.4% of the total value of currency in circulation. The RBI also specified that the two notes should be deposited with banks before December 30, 2016. Two new bank notes—Rs 500 and Rs 2,000—were to be printed and distributed to the public through the banking system. The policy's stated goal was to identify

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[16] We also show that traditional payment technologies—credit or debit cards—were not widely adopted by new users during the demonetization, though they were more actively used by existing users. Our paper thus also relates to empirical work on debit cards and household behavior (Bachas et al. 2017; Schaner 2017).

individuals holding large amounts of black money and remove fake bills from circulation.[17]

However, the swap between the new and old currency notes was not immediate: the public was unable to withdraw new notes at the same rate as they were depositing the old ones. As a result, the amount of currency in circulation dropped precipitously during the first 2 months of the demonetization period. This can be seen in figure H.1 (figs. H.1–H.18 are available online), which plots the monthly growth rate of currency in circulation.[18] Overall, it declined by almost 50% during November and continued declining in December.

This cash crunch partly reflected limits on cash withdrawals put in place by the RBI in order to manage the transition. But it was also driven by the difficult logistics of the swap itself. In order to ensure that the policy remained undisclosed prior to its implementation, the RBI had not printed and circulated large amounts of new notes beforehand. This caused many banks to be unable to meet public demand for cash, even under the withdrawal limits (see app. sec. A.1).

Importantly, the demonetization did not lead to a reduction in the total money supply, defined as the sum of cash and bank deposits. The total money supply was stable over this period, as figure H.1 shows. In its press release, the RBI highlighted that bank deposits could be freely used through "various electronic modes of transfer." The public was thus still allowed to transact using any form of noncash payment, such as cards, checks, or any other electronic payment method; cash transactions were the only ones to be specifically impaired.

Despite its magnitude, the cash crunch was a temporary phenomenon. Overall, cash availability significantly improved in January and essentially normalized in February. Consistent with slacker constraints on cash availability, in January, cash in circulation resumed significant growth. The government lifted most remaining limitations on cash withdrawals by January 30, 2017, in particular removing any ATM withdrawal limit. As discussed extensively in appendix section A.1, several stylized facts confirm this timing. We find that the amount of ATM withdrawals was back to preshock levels shortly after January (fig. H.2). Furthermore, using data on online searches, figure H.3 shows that public perception of constraints on cash availability significantly improved with the new year, with searches of cash-related keywords back to October 2016 levels by February 2017.[19]

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[17] In its annual report for 2017–18, the RBI reported that 99.3% of the value of voided notes had been deposited in the banking system during the demonetization.

[18] The time series for currency in circulation reported in this graph does not mechanically drop with the voiding of the two notes; it declines only as these notes are deposited in the banking sector.

[19] More details are provided in app. sec. A.1. In this section, we also provide a separate discussion of news articles about the demonetization, which also confirms that the likelihood of another round of cash restrictions was perceived as low.

The demonetization thus had three features that will matter for our analysis. First, it led to a significant contraction of cash in circulation. Second, once old notes had been deposited, the public could still access and use money electronically. Third, the demonetization was relatively short-lived: the effects on cash were particularly acute in November and December, but cash availability improved with the new year and had generally normalized by February. These features make the demonetization a particularly suitable laboratory to study how a temporary shock to cash availability can affect adoption of electronic payments.[20]

B. Fintech Payment Systems during the Demonetization

Overall, the demonetization was associated with a large uptake in electronic payments. We start by illustrating this fact using data from the leading digital wallet company in the country. The company allows individuals and businesses to undertake transactions with each other using only their mobile phone. To use the service, a customer needs to download an application and link their bank account to the application. Merchants can then use a uniquely assigned QR code to accept payments directly from the customers into a mobile wallet. The contents of the mobile wallet can then be transferred to the merchant's bank account.[21]

The nature of this technology is similar to other forms of electronic payments studied in the literature (e.g., Rysman 2007; Jack and Suri 2014; Suri and Jack 2016), in particular when considering the importance of adoption externalities. However, it differs from traditional forms of electronic payments—for instance, debit cards (e.g., Higgins 2019; Alvarez et al. 2022)—because its adoption and usage costs are very low. In particular, the activation process is extremely short and no monetary cost is involved. Furthermore, no investment in a point of sale terminal is required; the retailer simply needs a cellphone and a bank account, which are both very common in India (Agarwal et al. 2017). Last, for small and medium-sized merchants—who make up the bulk of our data—transactions using the technology do not involve fees.

Figure 1 reports weekly data on the total number and value of transactions executed by merchants through this platform. In the months before the demonetization, the weekly growth in the usage of the technology had

[20] As it is clear as we introduce the model, the shock will potentially incentivize adoption directly by increasing the need of transacting electronically and indirectly by increasing the value of using electronic payments by increasing the users' network.

[21] Appendix sec. A.3 provides more details on the technology, arguing that the requirements to use the technology were not particularly stringent in our context, and therefore a large part of the population could have accessed this option easily. Last, we want to point out that a smartphone or access to the internet is not necessary: in 2016, the company introduced a service that allows customers to make payments by calling a toll-free number.
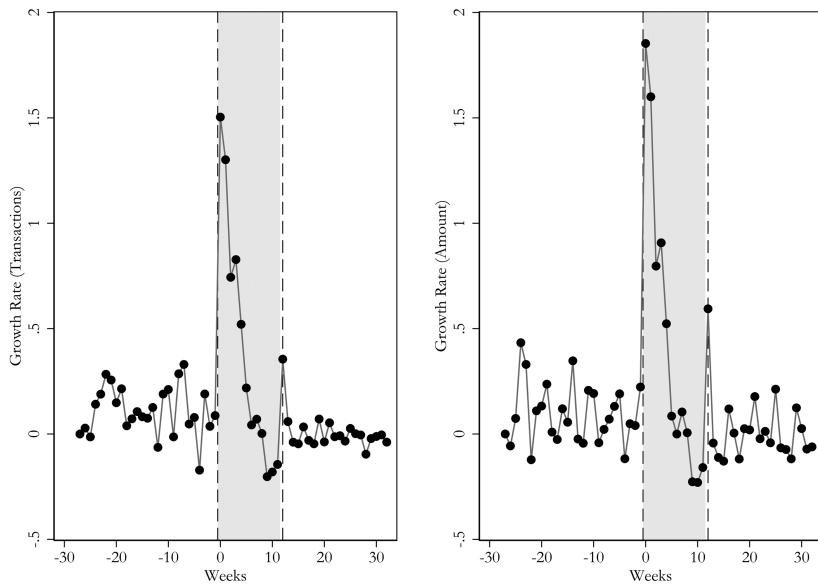
Fig. 1.—Growth in transactions for mobile payment platform. The figure shows the weekly growth rate in the number of transactions (*left*) and the total value of transactions (*right*) conducted through the electronic wallet platform. Dashed line indicates the week of November 8, 2016. More details on the data are provided in appendix section A.3.

been positive on average but relatively modest. However, after the demonetization, the shift toward this payment method was dramatic. In particular, in the first week after the shock, the number of transactions grew by more than 150%, and the value of transactions increased by almost 200%. For the first month after the shock, weekly growth rates were consistently around 100%.

Crucially, this initial positive effect on adoption did not dissipate, even after constraints on cash availability were relaxed. The data show a slowdown in aggregate growth starting in January, which is when the limits on the circulation of new cash started to be lifted. However, after a small negative adjustment in early February, the average growth rate over the next 2 months remained positive, indicating that users did not abandon the platform as cash became widely available again.[22] In other words, a temporary decline in the availability of cash led to a permanent increase in the usage of the platform.

The data shared with us by the electronic wallet company end in June 2017. However, it is important to point out that while the time window we

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[22] We believe that the February decline may be related to the announcement of a small fee, which was later canceled.

study in this paper captures the most important stage of the development of mobile wallets in India, the adoption wave persisted beyond this window, as discussed in more detail in appendix section A.3. To examine this issue, we collected aggregate data on the use of mobile wallets from the RBI (fig. H.4). This aggregate series features the same two regimes found the company's data: an initial large increase in mobile wallet activity in November 2016, followed by high growth for the next several months and a subsequent adjustment period with somewhat lower growth.[23] Besides coinciding with the company's data where they overlap, the RBI data confirm that the effects of the demonetization persisted beyond the window for which we have access to the company data. While less dramatic than in the immediate aftermath of the policy shock, the growth in mobile wallet transactions continued at a high pace until at least the end of 2019. While the mechanism we will propose may explain this persistent increase in growth rates, it is also important to recognize that over very long periods of time, total adoption will likely be affected by a variety of other factors that are outside the scope of our paper.

C. *Traditional Electronic Payment Systems during*
    the Demonetization

Aside from the payments platform that is the focus of our analysis, other more traditional electronic payment technologies were also available to the public. We collected publicly available data on monthly debit and credit card activity aggregated at the national level by the RBI. Figure H.5 presents these data. The top four panels report monthly growth rates in the number of transactions for both credit and debit cards across ATMs and points of sale (stores). The bottom two panels report monthly growth rates in the number of cards, again divided between debit and credit cards.

Two findings are important to highlight. First, the permanent increase in electronic payments is not unique to electronic wallet technologies. In particular, the growth rate of transactions at point of sales increases dramatically in both November and December before returning to levels similar to the preshock period. This suggests that the demonetization also led to a permanent increase in debit card transactions. Second, the short-run increase is completely driven by the intensive margin, unlike with the electronic wallet. The overall number of debit card transactions increases only because debit card holders start to use them more frequently, not because households newly adopt debit cards. In fact, the second panel

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[23] The aggregate data from the RBI show a temporary slowdown in adoption between May and July 2017. This decline could be related to the introduction of a goods and services tax on July 1, 2017, which may have affected the incentive to use cash in transactions.

of figure H.5 shows no clear growth rate in the number of new cards during either November and December.

These findings speak to the differences between traditional and fintech electronic payments. Relative to the electronic wallet technology, cards involve both larger fixed adoption costs—for retailers, the point of sales terminals—and flow use costs (transaction fees). The former, in particular, could explain why the extensive margin response was more limited for traditional must wait electronic payment methods.

III. Theory

In this section, we analyze a dynamic model of technology adoption with complementarities. Our objective is to derive empirical predictions that can help identify these complementarities in our data on electronic wallets. We test these predictions in section IV. The model can also be estimated and therefore provides a useful laboratory for quantification and counterfactuals, an approach we pursue in section V. The model is a variant of the dynamic coordination framework first proposed by Frankel and Pauzner (2000) and further analyzed in Burdzy, Frankel, and Pauzner (2001), Frankel and Burdzy (2005), Guimarães and Machado (2018), and Guimarães, Machado, and Pereira (2020); we leverage the results from these papers in our analysis.

A. Model

This section describes the model. We first lay out its key elements, then characterize equilibrium adoption strategies, and finally, we discuss the key assumptions implicit in the description of the economic environment.

1. Model Description

Fundamentals.—The model is in continuous time. It describes a continuum of retail firms, indexed by $i \in [0, 1]$. Each firm must choose between using one of two payment technologies, $\{e, c\}$, where e stands for electronic money, and c stands for cash. $x_{i,t} \in \{e, c\}$ is the technology choice of firm i at time t. For each firm, flow profits per unit of time are given by

$$\Pi(x_{i,t}, M_t, X_t) = \begin{cases} M_t & \text{if } x_{i,t} = c, \\ M^e + CX_t & \text{if } x_{i,t} = e, \end{cases} \qquad (1)$$

where M_t—"cash"—is an exogenous process described below, $M^e > 0$ and $C \geq 0$ are parameters characterizing the electronic payments technology, and X_t—the "user base"—is an endogenous variable given by

$$X_t = \int_{i \in [0,1]} \mathbf{1}\{x_{i,t} = e\} \, di. \tag{2}$$

Since $C \geq 0$, flow profits to technology e for an individual firm are increasing in the number of other firms using e. The magnitude of C controls the strength of this effect. We discuss below what could explain the positive external returns associated with electronic payments in the case of the wallet technology. We also provide a simple microfoundation for them in a two-sided market, where firms interact with consumers.

Cash-based demand $\{M_t\}_{t \geq 0}$ is exogenous and follows

$$dM_t = \theta_t (M^c - M_t) \, dt + \sigma dZ_t, \ t \geq 0, \tag{3}$$

where M^c is the long-run mean of cash demand, $\{Z_t\}_{t \geq 0}$ is Brownian motion driving innovations to cash, σ is the instantaneous volatility of innovations, and $\theta_t \geq 0$ is the (deterministic) speed of mean reversion, about which we make the following assumption.

ASSUMPTION 1. The speed of mean reversion is given by

$$\theta_t = \begin{cases} \theta & \text{if } t \leq T, \\ 0 & \text{if } t > T. \end{cases} \tag{4}$$

T is a fixed horizon after which mean reversion vanishes. The value of T can be arbitrarily large. Following Frankel and Burdzy (2005) and Guimarães, Machado, and Pereira (2020), this assumption is made in order to ensure unicity of the equilibrium, as we explain below. Finally, we will assume that $M^e < M^c$, so that without adoption ($X_t = 0$), the electronic payments technology is dominated (on average) by cash.

Individual firm problem.—Firms discount the future at rate r. The value of a firm is given by

$$V_{i,t}(x_{i,t}, M_t, X_t) = \mathbb{E}_{i,t} \left[\int_{s \geq 0} e^{-rs} \Pi(x_{i,t+s}, M_{t+s}, X_{t+s}) \, ds \right].$$

The expectations operator is indexed by i because firms may, in principle, form different expectations about the future path of X_t. Over time, a firm may change the technology it uses to accept payments. This change is governed by a Poisson process with controlled intensity $\tilde{k}$ per unit of time—the "switching rate." In an infinitesimal period $(t, t + dt)$, a firm changes its payment technology with probability $\tilde{k} dt$ and keeps using the same technology with probability $(1 - \tilde{k} dt)$. The switching rate $\tilde{k}$ can be continuously adjusted by the firm at no cost, subject to the constraint that $\tilde{k} \in [0, k]$, where k is an exogenous and fixed parameter common to all firms. The following result about the choice of switching rate holds.

LEMMA 1 (Adoption rule). Define adoption benefits, $B_{i,t}$, and the adoption rule, $a_{i,t}$ as

$$B_{i,t}(M_t, X_t) \equiv V_{i,t}(e, M_t, X_t) - V_{i,t}(c, M_t, X_t),$$

$$a_{i,t}(M_t, X_t) \equiv 1\{B_{i,t}(M_t, X_t) \geq 0\}. \tag{5}$$

Then, the optimal switching rate is given by

$$\tilde{k}_{i,t}(x_{i,t}, M_t, X_t) = \begin{cases} ka_t(M_t, X_t) & \text{if } x_{i,t} = c, \\ k(1 - a_t(M_t, X_t)) & \text{if } x_{i,t} = e, \end{cases} \tag{6}$$

and moreover, adoption benefits are given by

$$B_t(M_t, X_t) = \mathbb{E}_t\left[\int_{s \geq 0} e^{-(r+k)s}\Delta\Pi(M_{t+s}, X_{t+s})ds\right], \tag{7}$$

where $\Delta\Pi(M_t, X_t) \equiv M^e + CX_t - M_t$.

This result is proven in appendix section B.1. Firms with $x_{i,t} = e$ choose the lowest feasible switching rate, $\tilde{k} = 0$, when the benefits of adopting electronic money are positive, and the highest possible one, $\tilde{k} = k$, otherwise. Firms with $x_{i,t} = c$ act symmetrically.[24]

Aggregate law of motion for user base.—Given the optimal choices of firms, the user base X_t follows

$$dX_t = \left(\int_i \tilde{k}_{i,t}di\right)dt = \left(\int_i a_{i,t}(M_t, X_t)di - X_t\right)kdt. \tag{8}$$

Here, we allowed for the adoption rule $a_{i,t}$ to potentially differ across firms $i \in [0, 1]$, and we used equation (6) in order to express the law of motion as a function of the adoption rules.

Equilibrium.—Suppose that an individual firm $i \in [0, 1]$ believes that other firms follow adoption rules given by $\tilde{a}_{-i} \equiv \{\tilde{a}_{j,t}\}_{t \geq 0, j \in [0,1] \setminus \{i\}}$, where each $\tilde{a}_{j,t}$ is a mapping $\mathbb{R} \times [0, 1] \to \{0, 1\}$. That firm then forecasts the adopter share, using the law of motion $dX_t = (\int_i \tilde{a}_{i,t}(M_t, X_t)di - X_t)kdt$. Define

$$B_t(M_t, X_t; \tilde{a}_{-i|t}) = \mathbb{E}_t\left[\int_{s \geq 0} e^{-(r+k)s}\Delta\Pi(M_{t+s}, X_{t+s})ds \,\Big|\, \tilde{a}_{-i|t}\right], \tag{9}$$

where $\tilde{a}_{-i|t} \equiv \{\tilde{a}_{j,t+s}\}_{s \geq 0, j \in [0,1] \setminus \{i\}}$. This mapping gives the value of adoption for an individual firm that believes that other firms will follow adoption

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[24] Note that if $B_t(M_t, X_t) = 0$, a firm is in principle indifferent across any $\tilde{k} \in [0, k]$, so that the optimal arrival rate is a correspondence, not a function. We simplify the expression for the optimal arrival rate by assuming that $\tilde{k}(c, M_t, X_t) = k$ and $\tilde{k}(e, M_t, X_t) = 0$ when $B_t(M_t, X_t) = 0$. This is without loss of generality because, in equilibrium, $B_t(M_t, X_t)$ is equal to 0 only on a measure 0 set of states.

rules $\tilde{a}_{-i|t}$ from t onward. Lemma 1 implies that the best response of the firm is

$$\forall\ t \geq 0, \quad \hat{a}_{i,t}(M_t, X_t; \tilde{a}_{-i|t}) = 1\{B_t(M_t, X_t; \tilde{a}_{-i|t}) \geq 0\}. \tag{10}$$

We can then define an equilibrium as follows.

DEFINITION 1 (Equilibrium). An equilibrium is a set of adoption rules $\{a_{i,t}\}_{t \geq 0, i \in [0,1]}$, where each $a_{i,t} : \mathbb{R} \times [0,1] \to \{0,1\}$ satisfies $\forall\ (t, M_t, X_t) \in \mathbb{R}^+ \times \mathbb{R} \times [0,1]$, $\hat{a}_{i,t}(M_t, X_t; a_{-i|t}) = a_{i,t}(M_t, X_t)$.

We focus on Markov perfect equilibria in pure strategies. The adoption rules are an equilibrium when at each time t, the adoption rule $a_{i,t}$ is the best response of firm i to other firms $j \neq i$ using the adoption rules $a_{-i|t}$ from that period onward.

2.   Equilibrium Characterization

Existence, unicity, and adoption rule.—Our model is a special case of the more general framework of Frankel and Burdzy (2005).[25] Appendix section B.1 shows that our model satisfies the sufficient conditions for existence, unicity, and monotonicity of the equilibrium derived in that paper. We therefore have the following result.

RESULT 1 (Uniqueness, continuity, and monotonicity [Frankel and Burdzy 2005]).   There exists a unique equilibrium set of adoption rules, a, which is symmetric across firms: $a_{i,t} = a_t$ for all $i \in [0,1]$ and all $t \geq 0$. The value of an individual firm, $V_t(x_{i,t}, M_t, X_t)$, is also symmetric across firms and is a continuous function of t, M_t, and X_t. The value of adoption, $B_t(M_t, X_t)$, is also symmetric across firms; is a continuous function of t, M_t, and X_t; and is strictly decreasing in M_t and weakly increasing in X_t (strictly so if $C > 0$).

In order to guarantee the unicity of the equilibrium in the model, Frankel and Burdzy (2005) show that a sufficient condition is that the rate of mean reversion, θ_t, goes to zero asymptotically. Assumption 1 guarantees that this is true. Note that because θ_t is (deterministically) time varying, all policy and value functions depend on time. Additionally, result 1 states that adoption benefits are continuous and monotone. We can use this fact to show that adoption follows a threshold rule.

RESULT 2 (Threshold rule for adoption).   For all $t \geq 0$ and $X_t \in [0,1]$, there exists a unique $\Phi_t(X_t)$ such that $B_t(\Phi_t(X_t), X_t) = 0$. The mapping $(t, X_t) \to \Phi_t(X_t)$ is continuous in t and X_t, increasing in X_t (strictly so when

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[25] Table H.19 (tables H.1–H.23 are available online) describes the mapping between Frankel and Burdzy (2005) and the model of this paper.

$C > 0$), and satisfies $\underline{\Phi}_t(X_t) \leq \Phi_t(X_t) \leq \bar{\Phi}_t(X_t)$ (with strict inequality when $C > 0$), where $\underline{\Phi}_t$ and $\bar{\Phi}_t$ are strict dominance bounds with expressions given in appendix section B.1. The user base follows

$$dX_t = \begin{cases} (1 - X_t)kdt & \text{if } M_t \leq \Phi_t(X_t), \\ -X_t kdt & \text{if } M_t > \Phi_t(X_t). \end{cases} \tag{11}$$

Finally, for any $C > 0$ and all $t \geq 0$, $X_t \in [0, 1]$, $\Phi_t(X_t) > \Phi_t^{(0)}$, where $\Phi_t^{(0)}$ is the adoption threshold when $C = 0$, which is independent of X_t.

The proof is in appendix section B.1. Figure 2 illustrates two cases: $C = 0$ and $C > 0$.

When $C = 0$, the two strict dominance bounds coincide, and the threshold satisfies $\underline{\Phi}_t(X_t) = \bar{\Phi}_t(X_t) = \Phi_t(X_t) = \Phi_t^{(0)}$ for all X_t; in other words, the threshold is independent of X_t. When cash is sufficiently low, firms switch with intensity k to electronic money, while when cash is sufficiently high, firms switch with intensity k to cash, regardless of the number of other firms operating with electronic money. (In fig. 2, the two regions $M_t < \Phi_t^{(0)}$ and $M_t > \Phi_t^{(0)}$ are light gray and dark gray, respectively.) Thus, adoption dynamics are independent of the user base X_t.

On the other hand, when $C > 0$, the adoption threshold $\Phi_t(X_t)$ is strictly increasing with the user base X_t, as illustrated in figure 2A2. Moreover, result 2 shows that for any $C > 0$, $\Phi_t(X_t) > \Phi_t^{(0)}$. In other words, with positive external returns, the threshold for adoption is everywhere higher than without positive external returns.

These observations have two implications. First, when $C > 0$, for a given size of the user base X_t, firms choose electronic money at higher levels of cash compared with when $C = 0$. Second, for a given level of cash M_t, firms are more likely to choose electronic money if the user base X_t is higher. As we explain below, the former mechanism generates endogenously persistent adoption dynamics following a transitory shock, while the latter mechanism implies positive state dependence with respect to the size of the user base.

3. Discussion of Modeling Choices

We make two key assumptions in this model. First, electronic payments feature positive external returns with respect to adoption by other firms; that is, $C \geq 0$. External returns could arise in a two-sided market with both consumers and firms, where a high level of adoption among firms creates an incentive for customers to adopt the platform and, conversely, a high participation by customers on the platform raises the benefits of adoption for firms. Appendix section B.5 describes such a model and shows
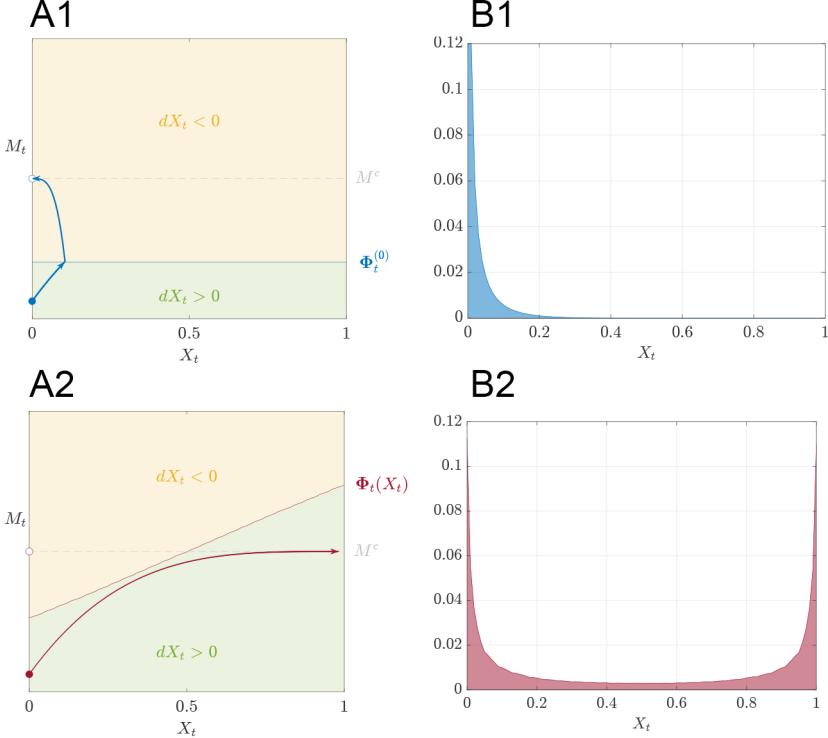
FIG. 2.—Adoption dynamics in model of section III. *A1* and *A2* report the phase diagram of the model, while *B1* and *B2* report the ergodic distribution of the user share. The top line reports results for the model when $C = 0$, while the bottom line reports results for the model when $C > 0$. In the phase diagrams (*A1, A2*), solid lines represent the adoption thresholds $\Phi_t^{(0)}$ and $\Phi_t(X_t)$ described in result 2, and dashed lines indicate the long-run level of cash demand, M_c. Dark gray regions correspond to the state of the economy where firms adopt electronic money at rate $k(dX_t = (1 - X_t)kdt)$, while light gray regions correspond to the state of the economy where firms adopt cash at rate $k(dX_t = -X_tkdt)$, as described in result 2. Solid arrows illustrate the perfect foresight response trajectories of the economy following a large drop in cash demand from $M_{0^-} = M_c$ (open circle in both phase diagrams) to $M_0 = (1 - S)M^c$ (solid circle in both diagrams). In the perfect foresight response, innovations to cash demand for $t > 0$ are assumed to be exactly zero. The size of the shock is chosen so that in the case where $C > 0$, the economy does not hit the adoption threshold at any $t > 0$. In both cases, the calibration used is $r = -\log(0.70)/12$ (the calibration is monthly); $k = 0.200$; $M_c = 1$; $M_e = 0.970$; $\theta = -30\log(1 - 0.90)/120$; $\sigma = 0.06$; $T = 1,200$. Additionally, in the bottom line, we use $C = 0.060$. The definition and computation of the ergodic distribution of the user share are described in appendix C.

that it is isomorphic to our baseline model, which focuses on firms.[26] Alternatively, external returns could arise from spillovers across firms in

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[26] Our baseline model focuses on firms primarily because our data allow us to see only the firm side of the payments network.

learning how to use the technology. We discuss this issue in more detail in section IV.E, where we provide evidence that external returns arising from learning are unlikely to provide a complete explanation of the adoption patterns we observe in the data.[27]

The second key assumption is that firms do not instantly and continuously adjust their technology choice. That is, the controlled switching intensity $\bar{k}$ is bounded from above by some $k > 0$, where $1/k$ gives the minimum (expected) time for firms to switch technologies. This assumption captures the possibility that firms have heterogeneous (unobservable) abilities to adjust to market conditions as they change, because of behavioral or informational frictions that we leave unmodeled.[28] It makes technology adjustment sluggish and allows for persistent deviations from the static optimal technology choice even if fixed pecuniary costs of adoption are small, which we have argued is likely the case for the technology we study.

Aside from these two assumptions, two remarks about the model are in order. First, in the baseline model, firms must choose between accepting cash and accepting electronic payments instead of being able to accept both cash and electronic payments. However, this is without loss of generality. In the two-sided market model of appendix section B.5, we allow firms to choose between accepting only cash and accepting either cash or electronic payments ("multihoming") and show that the model remains isomorphic to our baseline model.[29] More generally, it suffices that the relative flow profits from accepting electronic payments (whether as a stand-alone or as an add-on) compared with accepting cash increase with the user base X_t for the qualitative features of the model to remain unchanged (a point we expand on in app. sec. B.5).[30]

Second, the model does not explore the possibility that several platforms compete in offering electronic payments to the household. As we discuss in section IV.D and in appendix section A.3, the assumption of a single platform is empirically reasonable in our setup. While an analysis of the effects

[27] The linearity assumption we maintain is useful to derive some closed-form results, in particular those described in sec. III.B.1. However, results 1 and 2 would also hold under more general functional forms for returns to adoption, including the case of increasing returns to adoption, so long as these functional forms satisfy technical assumptions reported in app. sec. B.1.

[28] From a theoretical standpoint, assuming that $k < +\infty$ creates sluggishness that helps neutralize the potential for complementarities to generate multiple equilibria, as emphasized by Frankel and Pauzner (2000).

[29] Even in the multihoming model, firms may prefer to accept only cash. This is because we assume that there is always a positive (though potentially small) probability that when they meet a customer who has the wallet, it will be used for payment. When cash is sufficiently high (or, alternatively, if wallet-based demand is sufficiently weak), this creates an opportunity cost of multihoming, justifying why firms may choose to go back to accepting only cash.

[30] The model in app. sec. B.5 also illustrates how customer payment choices at the point of purchase could create adoption incentives for firms, providing a microfoundation for how consumer-side forces could explain the network effects we focus on.

of platform competition is beyond the scope of our model, related theoretical work suggests that competing platforms, if any, may have reacted to the shock by offering incentives for retailers to switch platforms. This would weaken any adoption response in the model (relative to the single-platform case), generally biasing our analysis toward estimating weaker externalities for our platform.[31]

Finally, it is useful to contrast our framework to other models in which strategic complementarities play an important role. A key departure from existing work is that, as highlighted above, we remain agnostic to the source of externalities, imposing only that adoptions are (weak) strategic complements across firms (that is, $C \geq 0$). The main drawback is that the model cannot speak to certain specific counterfactuals that might be relevant in other contexts (e.g., the effect of policies to improve awareness of the technology). However, the main advantage of using this relatively simple specification is that we can study the effects of aggregate shocks. Aside from helping ensure equilibrium unicity—as highlighted by Frankel and Pauzner (2000)—aggregate shocks are central to both our positive analysis (as they help us model the transitory nature of the demonetization) and to our counterfactuals (since they allow us to study the role of shock persistence in fostering adoption). By contrast, the framework of Björkegren (2018) has a much richer specification of consumer utility (so that the model can speak to welfare questions) but no aggregate shocks, and it has a fixed set of social links for potential users of the cellphone networks. Instead, our framework allows for aggregate shocks, and moreover, while we do not explicitly model retailers' individual networks, we let the number of network users a retailer has access to vary with changes in the aggregate state of the economy, so that we can speak to changes in adoption incentives following a shock. Similarly, the framework of Lotz and Vasselin (2019) provides a precise microfoundation for the existence of money (in the tradition of Lagos and Wright 2005) and is well suited to studying the theoretical conditions allowing for the coexistence of multiple means of payments. However, the absence of aggregate shocks makes it difficult to use in order to study the dynamic effects of temporary shocks, such as the demonetization. It also leads to equilibrium multiplicity, making it more difficult to use it for counterfactual analysis. Finally, we note that contrary to some existing work on network effects, our model is fully dynamic. The reason for this choice is twofold. First, as established by Frankel and Pauzner (2000), the dynamic nature of the adoption choice, in combination with sluggish adjustment by firms, helps resolve the multiplicity issue inherent in models with adoption complementarities, such as network models. Second, in the particular

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[31] For models of platform competition in which pricing and investment strategies are endogenized (but dynamic adoption decisions are generally not), see Rochet and Tirole (2006), Weyl (2010), and Chen (2020).

context of the shock we study, firms' expectations about how long the cash crunch would last likely played an important role in shaping aggregate adoption dynamics, a point we come back to in section V.

B. The Response to Large Shocks: Empirical Predictions

We now characterize how the use of electronic money responds to a large, unexpected, but temporary decline in cash. We highlight three key predictions of the model when $C > 0$. First, the shock leads to a persistent response of the *level* of the user base, even though the shock itself is temporary. Second, the shock also leads to a persistent response of the *growth rate* of the user base. Finally, the response to the shock exhibits positive state dependence with respect to the initial user base.

Let cash on impact be given by $M_0 = (1 - S)M^c$. We assume that the shock S is large in the sense that

$$S > \left(1 + \frac{\theta}{r + k}\right)\left(\frac{M^c - M^e}{M^c}\right), \tag{12}$$

which, using result 2, is sufficient to ensure that the shock triggers adoption at $t = 0^+$ regardless of the initial size of the user base, X_0.

To establish the three predictions, we consider two cases: the case of perfect foresight, in which there are no subsequent disturbances to cash other than the initial contraction; and the general case, in which there can be disturbances to cash after $t = 0^+$. We use the perfect foresight case as a way to illustrate the underlying mechanisms that generate persistence, leveraging the analytical solutions we can obtain in that case. Furthermore, to the extent that it captures the idea that the demonetization was a discretely large policy shock, the perfect foresight case may be interesting in its own right. However, as we discuss below, while the perfect foresight case leads to a stronger version of our three predictions, our results still hold in the more general case, when subsequent disturbances to cash are allowed.

1. The Case of Perfect Foresight

We start with the perfect foresight response of the economy to a shock, which we define formally as follows.

DEFINITION 2. The perfect foresight response of the economy is defined as the sample path $\{\tilde{M}_t, \tilde{X}_t\}_{t \geq 0}$ corresponding to a sequence of innovations to cash demand that are exactly equal to zero for all $t > 0$.

The perfect foresight response can be constructed for arbitrary values of T, the horizon after which mean reversion vanishes. For each value of T, result 2 guarantees the existence and unicity of a unique set of adoption

thresholds $\mathbf{\Phi}^{(T)} = \{\Phi_t^{(T)}\}_{t \geq 0}$, from which the perfect foresight response can be constructed.

For the discussion in this section, we will focus on the limit $T \to +\infty$. We make the assumption that the thresholds $\mathbf{\Phi}^{(T)}$ converge to a unique limit $\mathbf{\Phi} = \{\Phi_t\}_{t \geq 0}$ as $T \to +\infty$ and that this limit is time invariant: $\Phi_t = \Phi$ for all $t \geq 0$.[32] Note that when $T \to +\infty$, the perfect foresight response of cash is simply $\tilde{M}_t = (1 - Se^{-\theta t})M_t$. Appendix section B.2 characterizes completely the perfect foresight response of the economy in this case. Here, we summarize key predictions.[33]

Consider first the case where $C = 0$, and let $\{\tilde{X}_t^{(0)}\}_{t \geq 0}$ be the response of the user base, starting from some initial level $\tilde{X}_0^{(0)} = X_0$. As shown in appendix section B.2, in this case, $\lim_{t \to +\infty} \tilde{X}_t^{(0)} = 0 < X_0$. Moreover, firms adopt electronic money for $0 \leq t \leq \hat{t}^{(0)}$ and move back to cash for $t > \hat{t}^{(0)}$, where

$$\hat{t}^{(0)} = \frac{1}{\theta} \log\left(\frac{r+k}{r+k+\theta} \frac{SM^c}{M^c - M^e} \right) < +\infty. \tag{13}$$

Thus, when $C = 0$, the user base always mean reverts back to zero, and adoption stops at time $\hat{t}^{(0)}$. Moreover, there is no state dependence in the response of the economy, in the sense that the time $\hat{t}^{(0)}$ after which firms stop adopting is independent of the initial user base.

PREDICTION 1a (Persistent response of the user base). When $C > 0$, the response of the user base satisfies $\tilde{X}_t \geq \tilde{X}_t^{(0)}$ for all $t \geq 0$. Moreover, when $C > \bar{C}(X_0)$, $\lim_{t \to +\infty} \tilde{X}_t = 1 > X_0$, where the expression for $\bar{C}(X_0)$ is reported in appendix section B.2.

PREDICTION 2a (Persistent response of the adoption rate). When $C > 0$, the adoption decision, $\tilde{a}_t$, is given by $\tilde{a}_t = \mathbf{1}\{t \leq \hat{t}(X_0)\}$, where $\hat{t}(X_0) \geq \hat{t}^{(0)}$. Moreover, when $C \geq \bar{C}(X_0)$, $\hat{t}(X_0) = +\infty$.

Predictions 1a and 2a highlight how the magnitude of C shapes the persistence of the adoption response. First, when $C > 0$, the response of both the user base and the adoption decision are more persistent than when $C = 0$, in the sense that the time at which adoption stops and the user base peaks, $\hat{t}(X_0)$, is always larger. Second, if externalities are sufficiently strong, the shock may have permanent effects both on the user base and on the adoption decision.

These predictions are illustrated in figures 2A1 and 2A2, which show the perfect foresight response of the economy when $C = 0$ and $C > 0$. When $C = 0$, the economy moves from its initial point ($X_0 = 0$, $M_{0^-} = M^c$; open

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[32] In app. sec. B.3, we study perfect foresight responses with finite T. So long as $T > (1/\theta)(SM^c/(M^c - M^e))$, analog predictions to 1a, 2a, and 3a hold. However, we cannot characterize analytically the values of (X_0, C) for which the perfect foresight response trajectory satisfies $\lim_{t \to +\infty} X_t = 1$, as we do in fig. 3.

[33] Proofs of the predictions are also reported in app. sec. B.2.

circle in fig. 2A1) to a point located in the region of the phase diagram where the user base in growing ($X_0 = 0$, $M_{0^+} = (1 - S)M^c$; solid circle in fig. 2A1). After that, it moves up (because of mean reversion in cash) and to the right (because of adoption) on the phase diagram. However, because the adoption threshold is flat, the economy will reach it in finite time. After that, the economy will continue moving up but this time to the left (as firms now abandon the electronic wallet), returning to its initial state in the long run and implying the hump-shaped dynamics described above.

By contrast, when $C > 0$, this tendency toward mean reversion may be overturned by the effect of positive external returns. Immediately after a large shock to cash, the economy moves to the adoption region, as in the case where $C = 0$. However, the adoption threshold is now upward sloping. If C is sufficiently high, the threshold is also steep relative to the curvature of the perfect foresight trajectory. In that case, the economy never reaches the adoption threshold again and therefore converges to $X_t = 1$ as t becomes large, as indicated by predictions 1a and 2a.

We note that relative to prediction 1a, prediction 2a highlights the fact that complementarities create a persistent incentive for firms to keep adopting electronic money, even after the shock to cash has dissipated. This will help to distinguish positive external returns from other mechanisms that can generate persistent level responses of the user base (such as fixed costs) but generally do not imply persistent growth rate responses. We come back to this point in section IV.D, where we consider alternative mechanisms that could account for our empirical findings.

PREDICTION 3a (Positive state dependence with respect to the initial user base).   When $C > 0$, the persistence of the response increases with the size of the initial user base: $\hat{\imath}(X_0)$ is increasing with X_0.

When it is finite, the time $\hat{\imath}(X_0)$ at which firms stop adopting can be bounded from below as follows:

$$\hat{\imath}(X_0) \geq \hat{\imath}^{(0)} + \frac{1}{\theta}\log(1 + h(X_0)), \qquad (14)$$

where $h(X_0) \geq 0$ is a function that is identically zero if $C = 0$ and positive and strictly increasing if $C > 0$ and whose expression is reported in appendix section B.2. Therefore, when $C > 0$, complementarities create endogenous persistence in the response of adoption, in the sense that they imply $\hat{\imath}(X_0) > \hat{\imath}^{(0)}$. The degree to which they do is stronger, the larger the value of the initial user base X_0. Intuitively, in the phase diagram reported in figure 2A2, all other things equal, a higher initial user base shifts the trajectory of the economy to the right. This makes the time needed to reach the adoption threshold longer, leading to a more persistent response of the economy to the shock. This state dependence does not arise when $C = 0$, because the adoption threshold is flat in that case, as illustrated by figure 2A1.
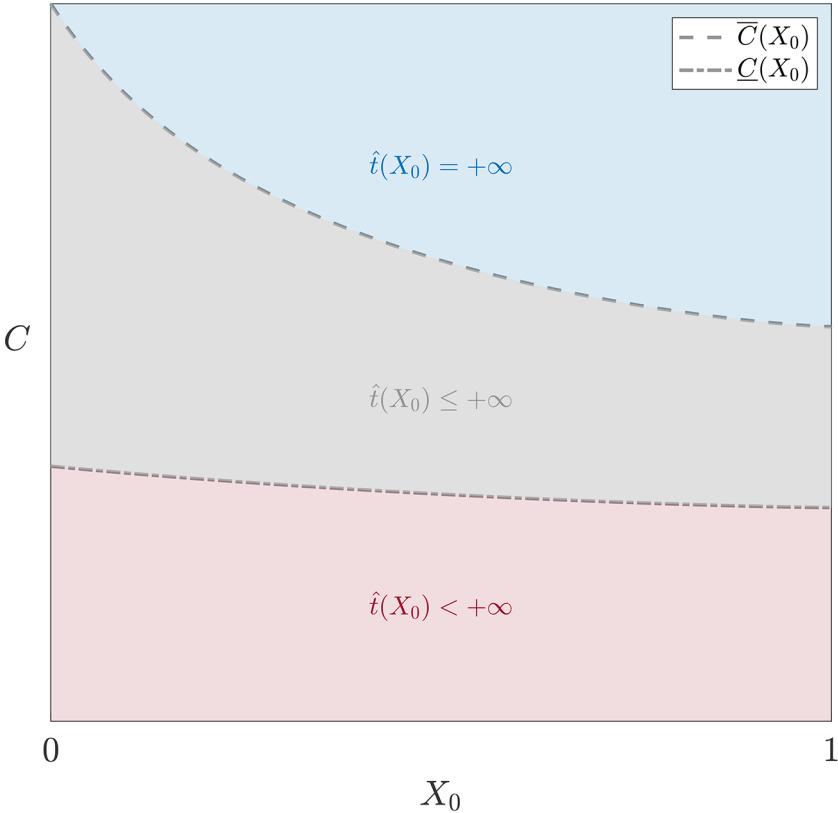
FIG. 3.—Summary of perfect foresight response to large shock. The shock size S is assumed to satisfy $S > M_c^{-1}(1 + k/(r + k))(M^c - M^e)$. Dark gray region corresponds to values of (X_0, C) such that adoption stops at a finite time in the perfect foresight response to a shock of size S. Light gray area corresponds to values of (X_0, C) such that adoption continues at all dates $t \geq 0$. In the medium gray area, the bounds on the equilibrium adoption threshold are not sufficiently tight to determine whether adoption stops at a finite horizon or whether the shock leads to adoption at all future dates. The parameter values used to construct the graph are $r = -\log(0.70)/12$ (the calibration is monthly); $k = 0.200$; $M_c = 1$; $M_e = 0.970$; $\theta = -30\log(1 - 0.90)/80$; $\sigma = 0.06$; $T = 1,200$. In this calibration, $\theta > k$ (the shock is mean reverting quickly). Figure H.16 summarizes the perfect foresight response when $\theta \leq k$. See appendix section B.2 for derivations of $\hat{t}(X_0)$ and the boundaries $\underline{C}(X_0)$ and $\bar{C}(X_0)$.

Figure 3 summarizes the adoption dynamics in perfect foresight by partitioning the initial user base X_0 and the strength of external returns C into three regions.[34] The top region (light gray) corresponds to combinations of (X_0, C) where the economy moves to full adoption after the

[34] The derivation of the frontiers of these regions is reported in app. sec. B.2. The plot in fig. 3 is for the case of rapidly mean-reverting shocks ($\theta > k$); fig. H.16 reports the same plot for the case of slowly mean-reverting shock ($\theta \leq k$).

shock, while the bottom region (dark gray) corresponds to combinations where adoption stops at a finite date after the shock.[35] Consistent with predictions 1a and 2a, as C increases, adoption is more likely to respond permanently to the shock. Consistent with prediction 3a, the adoption response is also more likely to be permanent if X_0 is higher.

2. The General Case

We now go back to the general model to develop analogs to predictions 1a–3a. We characterize the properties of the model in terms of the impulse response function (IRFs) of the user base X_t and the adoption rule a_t, defined as

$$\mathcal{I}_X(t; X_0, C) \equiv \mathbb{E}_0[X_t \mid M_0 = (1 - S)M^c, X_0],$$
$$\mathcal{I}_a(t; X_0, C) \equiv \mathbb{E}_0[a_t \mid M_0 = (1 - S)M^c, X_0].$$

The former IRF characterizes the expected size of the user base at horizon t following the shock, while the latter characterizes the probability that at horizon t, firms will still be actively switching from cash to electronic money, and the user base will still be growing. We start by stating the three main predictions in the general case and then discuss the intuition for each. These predictions are somewhat weaker than in the perfect foresight case, as they do not give us a characterization of the full distribution of the user base at long horizons. The proofs for these predictions are reported in appendix section B.1. The solution algorithm used to construct the numerical examples discussed below is described in appendix C.

PREDICTION 1b (Persistent response of the user base).   At any horizon $t > 0$, the IRF of X_t is strictly larger when $C > 0$ than when $C = 0 : \forall \, C > 0, t > 0, X_0 \in [0, 1], \mathcal{I}_X(t; X_0, C) > \mathcal{I}_X(t; X_0, 0).$

Figure 4A1 illustrates numerically the IRF of X_t when $C = 0$ and when $C > 0$ at all horizons up to $t = 12$ months. In the underlying calibration, the half-life of innovations to cash is approximately half a month. Similar to the perfect foresight case, the IRF for $C = 0$ is hump shaped and exhibits rapid mean reversion. As per prediction 1b, the IRF for $C > 0$ is everywhere above the IRF for $C = 0$. Moreover, it does not exhibit rapid mean reversion, even horizons an order of magnitude larger than the half-life of the shock.[36]

Figure 4B1 provides a further numerical illustration of this endogenous persistence by plotting the IRF $\mathcal{I}_X(t; 0, C)$ at horizon $t = 12$ months as a

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[35] The intermediate gray region corresponds to cases where the strict dominance bounds are not sufficiently tight to determine whether the equilibrium adoption threshold implies a permanent response to the shock.

[36] Note that fig. 4A1 uses $X_0 = 0$ as the initial condition for adoption. We choose this example because in our data, adoption was generally close to zero before demonetization.
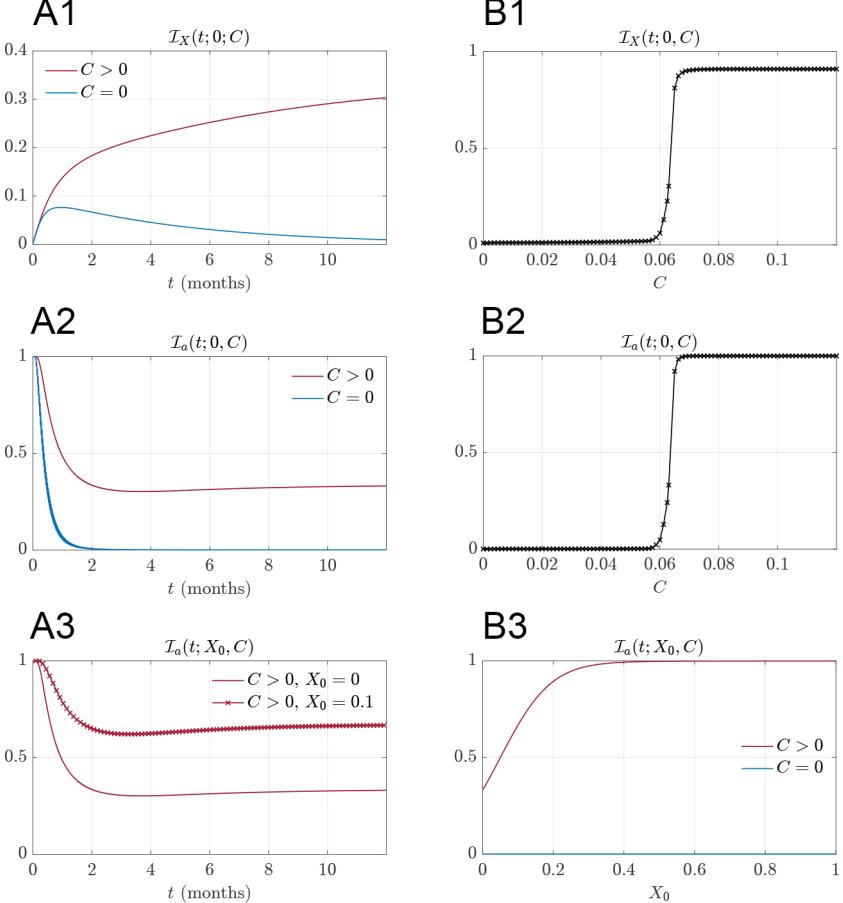
Fig. 4.—Predictions 1b, 2b, and 3b. *A1* and *B1* report the IRF of X_t as a function of *C. A2* and *B2* report the IRF of a_t as a function of *C. A3* and *B3* report the IRF of a_t as a function of X_0, the initial size of the user base. In *B1–B3*, we use a horizon of $t = 12$ months. Across all panels, the calibration used is $r = -\log(0.70)/12$ (the calibration is monthly); $k = 0.200$; $M_t = 1$; $M_e = 0.970$; $\theta = -30 \log(1 - 0.90)/80$; $\sigma = 0.06$; $T = 1,200$. In *A1–A3*, the positive value of *C* used is $C = 0.06$. The procedure for the numerical computation of IRFs is described in appendix C.

function of *C.* At the parameters chosen for the calibration, the IRF at horizon t is strictly increasing with *C.* The higher IRF of the user base when $C > 0$ is the analog to prediction 1a in the perfect foresight case.

Prediction 2b (Persistent response of the adoption rate). At any horizon $t > 0$, the IRF of a_t is strictly larger when $C > 0$ than when $C = 0$: $\forall \ C > 0, t > 0, X_0 \in [0, 1], \mathcal{I}_a(t; X_0, C) > \mathcal{I}_a(t; X_0, 0)$.

Figure 4A2 provides numerical examples of two IRFs of the adoption decision, when $C = 0$ and when $C > 0$. While the IRF when $C = 0$ reverts to the long-run average adoption rate, when $C > 0$, it exhibits a persistent response and remains significantly above zero even at long horizons. If there are external returns, the probability that, at horizon t, the economy is still in the adoption region, so that the platform is still growing, is strictly higher than when there are no external returns. Figure 4B2 further illustrates this property by plotting $\mathcal{I}_a(t; 0, C)$ as a function of C. In this numerical example, other things equal, the IRF of the adoption decision, a_t, is increasing with C. These implications of the model are similar to (though weaker than) prediction 2a in the perfect foresight case, which states that the economy will remain in the adoption region for a longer period of time when $C > 0$.

Figures 2B1 and 2B2 further illustrate how the presence of external returns shapes the response of the economy to large shocks. These figures plot the ergodic distribution of X_t in the model.[37] When $C = 0$, the ergodic distribution has most of its mass concentrated around 0, indicating that the user base tends to mean revert toward zero adoption. By contrast, when $C > 0$, the ergodic distribution is bimodal, with mass concentrated around 0 and around 1. When the user base is small, shocks to cash generally produce locally mean reverting responses, as in the case $C = 0$. But occasional large negative shocks may push the user base away sufficiently far away from zero that its growth becomes self-perpetuating. The user base becomes large and remains so until a large positive shock generates opposing dynamics.

PREDICTION 3b (Positive state dependence with respect to the initial user base). When $C > 0$, at any horizon t, the IRF of a_t is strictly increasing in $X_0 : \forall\ X_0^{(a)}, X_0^{(b)} \in [0, 1]^2, X_0^{(a)} < X_0^{(b)}, \mathcal{I}_a(t; X_0^{(a)}, C) < \mathcal{I}_a(t; X_0^{(b)}, C)$. When $C = 0$, the IRF of a_t is independent of X_0.

Figure 4B3 shows that following the shock, when $C > 0$, as the initial user base X_0 increases, the probability that the economy is still in the adoption region at horizon t also increases. By contrast, when $C = 0$, this probability is independent of X_0. In this sense, the response of the economy to the shock is more persistent when the initial user base is larger, similar to prediction 3a in the perfect foresight case.

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[37] We define the ergodic distribution as the distribution over states (M_t, X_t) that is invariant, given the law of motion for M_t and the optimal policy functions. For $t > T$, since the model becomes stationary, there is a unique such distribution. For $t < T$, because of the time dependence in θ_t, there is in principle no uniquely defined stationary distribution. However, as described above, when $t \ll T$, policy functions are stationary up to numerical tolerance, so that we can derive the unique distribution that is invariant under these policy functions. Appendix C reports the numerical details of this computation.

IV.   Adoption Dynamics in the Data

This section uses micro data from a leading electronic wallet provider in India to test the three empirical predictions of the adoption model, with externalities described in section III. We test the first two predictions—on the long-run increase in both the size of the platform and its adoption rate—by using quasi-random variation in the exposure to the shock. Additionally, we provide evidence consistent with the third prediction, the positive dependence of adoption responses with respect to baseline adoption rates.

A.   Data

The main data we use in our analysis are merchant-level transactions from a leading digital wallet company.[38] We observe weekly-level data on the sales amount and number of transactions happening on the platform for anonymized merchants between May 2016 and June 2017.[39] For each merchant, we also observe the location of the shop at the district level as well as the store's detailed industry. For a random subsample of shops, the location is provided at the more detailed level of a six-digit postal index number (PIN) code.[40] There are two key features of these data. First, since the information is relatively high frequency, we can aggregate it to weekly or monthly levels. Second, since the transactions are geolocalized, we can aggregate them up at the same level as other data sources used in this study.

We obtain data on district-level banking information from the RBI. This includes three pieces of information for each district: first, the number of bank branches; second, the number of currency chests and the identity of the banks operating the chests; third, quarterly bank deposits at the bank-group level. Finally, we complement this data with information from the 2011 Indian Population Census to obtain a number of district-level characteristics, including population, quality of banking services (share of villages with an ATM and banking facility, number of bank branches and agricultural societies per capita), socioeconomic development (sex ratio, literacy rate, growth rate, employment rate, share of rural

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[38] During the period we study, the company was the largest provider of mobile transaction services in the country. After March 2017, some competitors emerged, in part as a result of the government's initiative (see app. sec. A.2 for more details).

[39] The company shared with us information on the 1 million largest firms by activity, using the QR code–based payment product designed for small and medium-sized retailers. This sample represents more than 95% of all transactions—in both number and value—conducted using this payment product. See app. sec. A.3 for more details on the technology.

[40] A PIN code in India is the approximate equivalent of a five-digit zip code in the United States. PIN codes were created by the postal service in India. India has a total of 19,238 PIN codes, of which 10,458 are covered in our dataset.

population), and other administrative details, including distance to the state capital.[41] For some robustness tests, we also use data from the Centre for Monitoring Indian Economy survey, which is described in appendix D.

B.   The Effects of the Demonetization on Adoption

Next, we test the first two predictions of the model: the long-run increase in both the size of the platform and its adoption rate. The aggregate event study evidence discussed in section II is qualitatively consistent with these predictions. At the same time, this aggregate event study evidence may not properly capture the long-run causal response of adoption to the shock. One particularly important confounding factor is national government policies that may have affected the subsequent adoption of electronic payments for reasons unrelated to externalities, as we describe in appendix section A.2. We overcome this concern by using quasi-random variation across different districts in exposure to the cash contraction. This approach allows us to recover the causal effect of the temporary cash contraction on adoption of electronic payments independent of any other aggregate shocks after the demonetization.

Exposure measure.—To identify heterogeneity in the exposure to the cash contraction, we exploit the heterogeneity across districts in the relative importance of chest banks—defined as banks operating a currency chest in the district—in the local banking market.[42] In the Indian system, currency chests are branches of commercial banks that are entrusted by the RBI with cash management tasks in the district. Currency chests receive new currency from the central bank and are in charge of distributing it locally. While the majority of Indian districts have at least one chest bank, districts differ in the total number of the chest banks as well as in chest banks' share of the local deposit market. Importantly, this institution was not created in response to the demonetization, but instead it was active in India for decades before 2016. Furthermore, the list of currency chests has been largely stable over time, with the revision of participating branches happening only partially and infrequently.

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[41] We always exclude sparsely populated northeastern states and union territories from the analysis because of missing information on either district-level characteristics or banking variables. The seven northeastern states include Arunachal Pradesh, Manipur, Meghalaya, Mizoram, Nagaland, Sikkim, and Tripura, while union territories include Anadaman and Nicobar Islands, Chandigarh, Dadra and Nagar Haveli, Daman and Diu, and Lakshadweep and Pondicherry. Altogether these regions account for 1.5% of the Indian population. For consistency with the state dependence analysis (sec. IV.C), we also always exclude the five major electronic payment hubs. The results that include the hubs are, if anything, stronger. Last, to keep the panel balanced, we also add one when log transforming outcomes throughout the paper.

[42] Following our paper, other works (Aggarwal, Kulkarni, and Ritadhi 2020; Vallee, Ghosh, and Yao 2021; Das et al. 2022)—which have leveraged our proposed strategy of exposure to chest bank to study the impact of the demonetization on other margins of economic activity—provided complementary evidence that validates our approach.

Consistent with anecdotal evidence, we expect that districts where chest banks account for a larger share of the local banking market should experience a smaller cash crunch during the months of November and December.[43] On some level, this relationship is mechanical. Chest banks were the first institutions to receive new notes, so in districts where chests account for a larger share of the local banking market, a larger share of the population can access the new bills faster. Furthermore, the importance of chest banks may be an even more salient determinant of access to cash if these institutions were biased toward their own customers or partners. Indeed, concerns of bias in chest bank behavior were widespread in India during the demonetization.[44] In any case, we will show that this connection between chest bank presence and the cash contraction is supported by data.

To measure the local importance of chest banks, we combine data on the location of chest banks with information on overall branching in India and data on bank deposits in the fall quarter of the year before demonetization (2015:4). Ideally, we want to measure the share of deposits in a district held by banks operating currency chests in that district. However, data on deposits are not available at the district level for each bank. Instead, the data are available only at the bank-type level (G_d).[45] Since we have information on the number of branches for each bank at the district level, we can proxy for the share of bank deposits of each bank by scaling the total deposits of the bank type in the district by the banks' share of total branches in that bank type and district.[46] We can then can compute our score as

$$\text{Chest}_d = \frac{\sum_{b \in C_d} \sum_j D_{jbd}}{\sum_{b \in B_d} \sum_j D_{jbd}} \approx \frac{1}{D_d}\left(\sum_{g \in G_d}\left(D_{gd} \times \frac{N_{gd}^c}{N_{gd}}\right)\right),$$

where D_d is the total amount of deposits in district d; D_{gd} and N_{gd} are, respectively, the amount of deposits and the number of branches in bank-type g

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[43] In the popular press, several articles argue that proximity—either geographical or institutional—to chest banks contributed to the public's ability to have early access to new cash (e.g., see https://www.thehindubusinessline.com/opinion/columns/all-you-wanted-to -know-about-currency-chest/article9370930.ece).

[44] In a report in December, the RBI has discussed this issue extensively. In one comment, they report how "these banks with currency chests are, therefore, advised to make visible efforts to dispel the perception of unequal allocation among other banks and their own branches" (https://economictimes.indiatimes.com/news/economy/finance/banks-with-currency -chest-need-to-boost-supply-for-crop-rbi/articleshow/55750835.cms?from=mdr).

[45] The RBI classifies banks into six bank groups: State Bank of India and its associates (26%), nationalized banks (25%), regional rural banks (25%), private sector banks (23%), and foreign banks (1%).

[46] A simple example may help. Assume that we want to figure out the local share of deposit by two rural banks, A and B. From the data, we know that rural banks in aggregate represent 20% of deposits in the district and that bank A has three branches in the district, while bank B only has one. Our method will impute the share of deposits to be 15% for bank A and 5% for B.

and district d; and N_{gd}^c is the number of branches of banks of type g with at least one currency chest in the district.[47] Since we want to interpret our instrument as a measure of exposure to the shock, our final score, Exposure$_d$, is simply the converse of the above chest measure, that is, Exposure$_d = 1 - $ Chest$_d$. The score is characterized by a very smooth distribution centered on a median around 0.55, with large variation at both tails (fig. H.6). Overall, exposure appears to be evenly distributed across the country, as very high and very low exposure districts can be found in every region (fig. H.7). Consistent with this idea, in section IV.B.3, we show that results do not depend on any specific part of the country.

According to the logic of our approach, we expect areas where chest banks are less prominent—or have higher exposure according to the index—to have experienced a higher cash contraction during the months of November and December. While we cannot directly observe the cash contraction at the local level, we can use deposit data to proxy for it. Cash declined because old notes had to be deposited by the end of the year, but withdrawals were severely limited. Therefore, the growth in deposits during the last quarter of 2016 should proxy for the cash contraction in the local area. Figure H.8 provides evidence consistent with this intuition by plotting deposit growth across districts for the last quarter of both 2016 and 2015. In normal times (2015), the growth distribution is relatively tight around a small positive growth. During the demonetization, the distribution looks very different. First, almost no district experienced a reduction in deposits. Second, the median increase in deposits was one order of magnitude larger than during normal times. Third, there is a lot of dispersion across districts, suggesting that the effect of the demonetization was likely not uniform across Indian districts.

Using this proxy for the cash crunch, we can provide evidence that supports the intuition behind our identification strategy.[48] Figure 5 shows that there is a strong relationship between district-level exposure to the shock and deposit growth. The same relationship holds when using different measures of deposit growth and including district-level controls, as shown in table H.1. Importantly, table H.2 also shows that this strong relationship

[47] In practice, this approximation relies on the assumption that the amount of deposits held by each bank is proportional to the number of branches within each district. The strength of our first-stage analysis suggests that this approximation appears to be reasonable.

[48] Notice that our approach is different from Chodorow-Reich et al. (2019). We use ex ante district characteristics that predict the exposure to the cash contraction, while Chodorow-Reich et al. (2019) exploits a time-varying measure of cash flowing in and out of a district during the demonetization period. While each approach has its own advantage, the two approaches display a similar variation across district, as expected. Using fig. 5 from Chodorow-Reich et al. (2019), we coded their categorization of the intensity of the cash crunch across districts and compared it with our treatment variable. We found a statistically significant positive correlation between the two treatments.
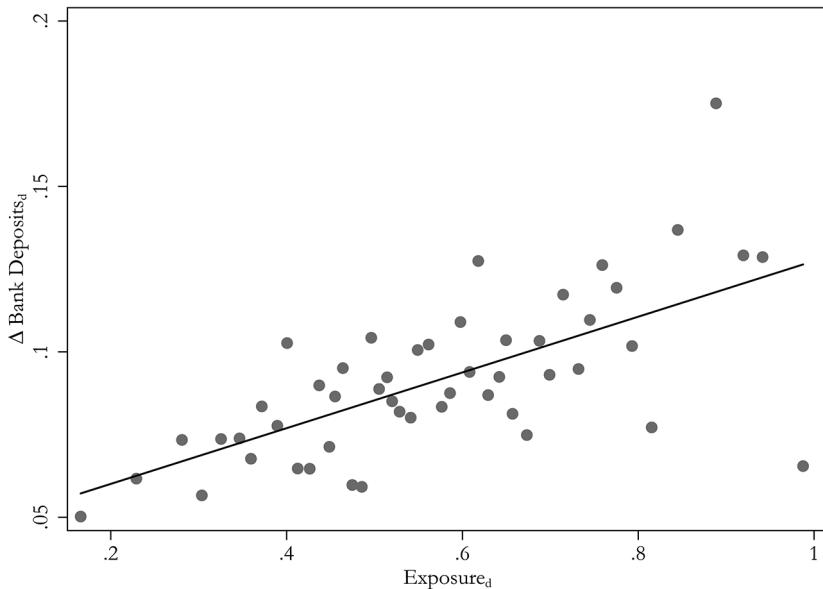
F ɪɢ. 5.—Relation between Exposure and 2016:4 deposit growth. The figure shows the relation between our measure of Exposure$_d$ (as described in sec. IV) and the change in bank deposits in the district between September 30, 2016, and December 31, 2016, that is, during the quarter of demonetization. Source: RBI.

holds only during the quarter of the demonetization, therefore further validating our approach.[49]

1.   Econometric Model

Using this measure of exposure, for different outcome variables of interest, y, we estimate the following difference-in-difference model:

$$\log(y_{d,t}) = \alpha_t + \alpha_d + \delta(\text{Exposure}_d \times 1_{\{t \geq t_0\}}) + \Gamma'_t Y_d + \epsilon_{d,t}, \quad (15)$$

where t is time (month), d indexes the district, t_0 is the time of the shock (November 2016), and Exposure$_d$ is the measure of the district's exposure constructed with chest bank data, as explained above. The equation is estimated with standard errors clustered at the district level, which is the level of the treatment (Bertrand, Duflo, and Mullainathan 2004). Last, the specification is based on the data between May 2016 and June 2017.

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[49] This table shows that, outside of that quarter, the relationship between these two quantities is small and generally insignificant. In the only other quarter when it is significant, this effect is one-fourth of the magnitude of 2016:4.

Importantly, the specification is also augmented with a set of district-level controls (Y_d), which are measured before the shock and interacted with time dummies. The presence of controls is important, because chest exposure is clearly not random. Table 1 examines this issue by showing the difference across characteristics for districts characterized by different exposure. Exposure to chest banks is uncorrelated with several district-level demographic and economic characteristics but not all of them. In particular, higher exposure is found in districts with a smaller deposit base, a smaller population, and a larger share of rural population. However, most of the variation in exposure is absorbed once we control for two observables: the size of the deposit base in the quarter before the shock and the percentage of villages with an ATM (cols. 4 and 5 in table 1). Taking

TABLE 1
EXPOSURE AND DISTRICT CHARACTERISTICS (Balance Test)

| | | UNIVARIATE OLS | | BASELINE CONTROLS | |
	MEAN (1)	Coefficient (2)	R^2 (3)	Coefficient (4)	R^2 (5)
Log(predeposits)	11.053	−1.380***	.061		
	(.046)	(.268)			
% villages with ATM	.031	.087***	.055		
	(.003)	(.019)			
Bank branches per 1,000s	.046	.001	.000	.016	.252
	(.002)	(.012)		(.012)	
Agricultural credit societies per 1,000s	.043	−.015	.001	.029	.078
	(.004)	(.027)		(.021)	
% villages with banks	.081	.135***	.038	.021	.698
	(.006)	(.034)		(.023)	
Log(population)	14.393	−.624***	.028	.100	.472
	(.031)	(.179)		(.177)	
Literacy rate	.620	−.027	.003	.009	.238
	(.004)	(.024)		(.023)	
Sex ratio	.948	.001	.000	−.025	.082
	(.002)	(.015)		(.016)	
Growth rate	.196	−.253*	.023	−.264	.027
	(.014)	(.133)		(.172)	
Working population/total population	.409	.024	.004	.006	.061
	(.003)	(.016)		(.017)	
Distance to state capital (km)	.216	.035	.002	.026	.009
	(.006)	(.031)		(.032)	
Rural population/total population	.758	.160***	.033	.007	.528
	(.007)	(.044)		(.029)	

NOTE.—The table tests for differences in observable district characteristics and Exposure$_d$. Column 1 reports the mean of the district characteristics. The treatment variables is our measure of Exposure$_d$, as described in sec. IV. Columns 2 and 3 report the coefficient of the univariate OLS regression of each variable on the treatment variable. Columns 4 and 5 report the coefficients after controlling for the predemonetization bank deposits in the districts (in logs) and share of villages with an ATM. Robust standard errors are in parentheses.
* $p < .10$.
*** $p < .01$.

TABLE 2
EXPOSURE AND ADOPTION OF DIGITAL WALLET

	log(Amount) (1)	log(Users) (2)	log(Switchers) (3)
Exposure $\times$ 1 ($t \geq t_0$)	3.213***	1.171***	.795***
	(.854)	(.412)	(.306)
Observations	7,168	7,168	7,168
R^2	.851	.869	.818
District fixed effects	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes
District controls $\times$ month fixed effects	Yes	Yes	Yes

NOTE.—The table reports the difference-in-differences estimates of the effect of the shock on the adoption of digital wallet. The estimated specification is eq. (15). Across cols. 1–3, we focus on different measures of activity in the platform. Specifically, we examine the total amount (Rs) of transactions carried out using a digital wallet in district d during month t (col. 1), the total number of active retailers using a digital wallet in district d during month t (col. 2), and the total number of new retailers joining the digital wallet in district d during month t (col. 3). District controls include (log) preshock banking deposits, share of villages with ATM facilities, share of villages with banking facility, share of rural population, and level of population in the district. Standard errors (in parentheses) are clustered at the district level.

*** $p < .01$.

a more conservative approach, our controls include the log of deposits in the quarter before the demonetization, the percentage of villages with an ATM, the log of population, the share of villages with a banking facility, and the share of rural population.[50]

2. Results

Table 2 shows that districts more exposed to the cash contraction also experienced more adoption of the electronic wallet after the demonetization. Column 1 shows that districts that were more exposed to the shock saw a larger increase in the amount transacted on the platform in the months following the demonetization. This result is both economically and statistically significant. Districts with 1 standard deviation higher exposure experienced a 55% increase in the amount transacted on the platform relative to the average. Similarly, the number of firms operating on the platform—our main measure of adoption—increased by 20% more in districts with 1 standard deviation higher exposure to the shock (col. 2).[51]

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[50] We also show that our exposure measure is not correlated with adoption of technologies prior to the demonetization. Specifically, we examine both the level of penetration of our main technology as well as that of mobile phones, bank accounts, and fintech loans (table H.3).

[51] We obtain qualitatively identical results if we define active firms on the platform as firms with at least 50 of transactions in a month.
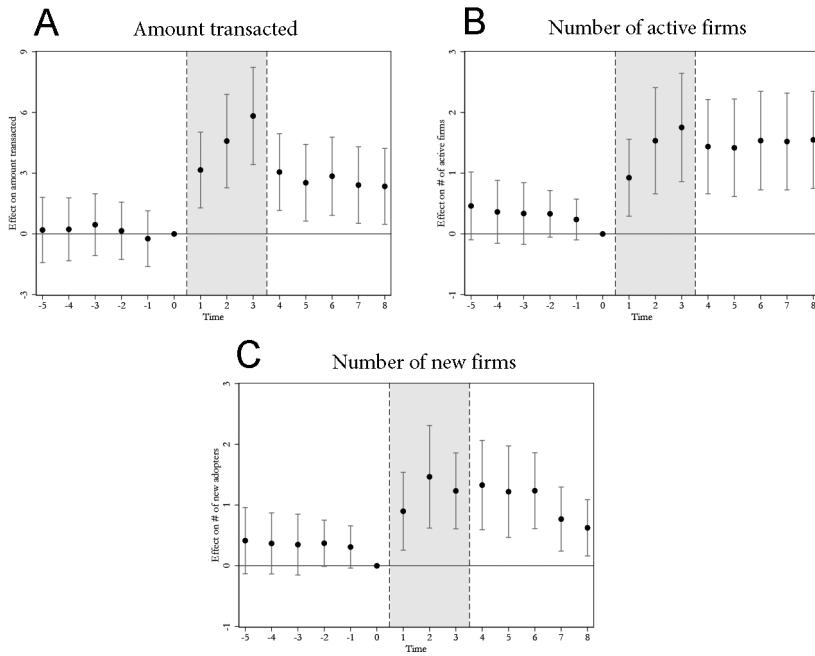
FIG. 6.—District adoption dynamics in electronic payments data based on exposure to shock. The figure plots the dynamic treatment effects of the demonetization shock on technology adoption of electronic payment systems. The graphs report the coefficients δ_t from specification (15); panel A reports the effects for the total amount of transactions, panel B reports the effects for the total number of active firms on the platform, and panel C reports the effect for the total number of new firms on the platform. The x-axis represents the month, where October 2016 is normalized to be zero. Vertical lines indicate 95% confidence intervals; standard errors are clustered at the district level.

In figure 6A and 6B, we plot the dynamics of the main effect, that is, the month-by-month estimates of how districts characterized by different levels of exposure responded to the shock.[52] This figure highlights three main findings. First, it confirms that our main effect is not driven by differential trends across high- versus low-affected areas. Second, the shift in adoption across districts happened as early as November. Third, the difference in the response persists even after cash availability has normalized. In particular, the effects are still large and significant after the month of February. These findings, taken together with the aggregate-level evidence in section II, confirm that the temporary cash contraction led to a persistent increase

[52] The specification is $\log(y_{d,t}) = \alpha_t + \alpha_d + \delta_t(\text{Exposure}_d) + \Gamma'_t tY_d + \epsilon_{d,t}$, and October is the base month.

in size of the user base of the electronic payment technology, consistent with the first prediction of the model.[53]

Next, we test the second prediction of the model, which is that the shock led to a persistent increase in the adoption rate, that is, the flow of new users to the platform. We empirically test this by analyzing whether districts more affected by the shock witnessed a more persistent increase in new adopters. We define new adopters at time t as the firms using the technology for the first time at time t. Figure 6C shows that districts experiencing a larger contraction in cash saw a larger increase in new adopters joining the platform as early as November 2016. Crucially, the relative increase in the number of new adopters continued even after January 2017, the last month during which cash availability was constrained, and persisted for the whole of Spring 2017. This persistent increase in new users is consistent with the second prediction of the model—the persistent effects of the shock on the growth rate of the platform.

3.   Robustness

As stated above, we argue that the relationship between exposure to the cash contraction and adoption of electronic payments is causal. Consistent with this interpretation, we have shown that, conditional on covariates, more exposed areas do not look different than less exposed regions in preshock levels. Additionally, our effects are not driven by pretrends across affected districts. As a further robustness check, we note that our main results are not driven by the response of any particular region in the country: our effects are stable when excluding any of the Indian states from our analysis (fig. H.9).

Given these results, one remaining concern to rule out is the presence of a contemporaneous demand shock that is correlated with our exposure measure but it is unrelated to the cash scarcity. We provide two tests to rule this out, which appendix D expands on. First, we show that the same highly affected districts also experienced a larger decline in consumption during this period. In particular, using the same empirical model and a panel of almost 100,000 households in India around the demonetization, we document that exposure to the cash contraction is associated with a temporary contraction in total consumption. This effect is mostly driven by a reduction of nonessential consumption items (e.g., recreational expenses). This result is interesting on its own but also helps rule out the possibility that unobserved demand shocks could explain our results. Indeed, a demand-side explanation of the increase in electronic

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[53] As a robustness, we address concerns of path dependence and show that our main results are robust to the inclusion of a lagged dependent variable, using both a 1-month lag (cols. 1–3) and a 2-month lag (cols. 4–6; table H.4).

payments would likely require that highly exposed districts receive a positive demand shock. Our findings reject this hypothesis and actually find that—consistent with a supply-side interpretation—highly affected areas saw a reduction in consumption. Second, appendix D also presents a full set of placebos that exploit the longer panel dimension of the consumption data and confirm the quality of our empirical strategy.

Last, we also show that—consistent with model predictions—the effects are also nonlinear and disproportionately stronger in areas with higher shock exposure.[54] We test this prediction by estimating the effect of the shock across five quintiles and report the result in table H.5: as expected, we find that the effect is mostly concentrated in the top two groups, while lower shock groups are statistically indistinguishable from the bottom quintile (i.e., reference group).[55]

C. State Dependence in Adoption

The last key prediction of the model with complementarities is the state dependence of adoption. The model suggests that a temporary shock may lead to a permanent shift in adoption but that this effect will not be uniform across regions: it will crucially depend on the initial strength of complementarities in each region. We now use the data on electronic payments to present evidence that is consistent with this prediction. The objective is not to causally identify a relationship between variables but rather to generate empirical regularities that would support the importance of state dependence. To do so, we will also try to isolate state dependence from other economic forces that might have similar observable implications.

In the model, the strength of complementarities in a district is completely captured by the size of the user base immediately before the shock. As a result, a natural way to test for state dependence is to check whether areas with a high initial level of adoption tend to be characterized by higher growth after the shock. While we find evidence that is consistent with this hypothesis (table H.6), we also recognize that the presence of a standard reflection problem (Manski 1993; Rysman 2019) makes it hard to interpret this solely as evidence of state dependence. Past adoption decisions by firms in the district may reflect unobservable heterogeneity across these

[54] In fact, the model predicts the presence of district-specific thresholds with respect to shock size (i.e., a minimum shock size below adoption is unresponsive). One implication of this prediction is that we should see the response to be minimal or null in areas with low exposure (i.e., where shock size is below the threshold), while the response should be large in areas with high exposure.

[55] We also test for the presence of a threshold, using the approach by Hansen (1999), as implemented by Wang (2015). Consistent with the result discussed above, the model identifies a threshold at a level of the shock of 0.1948, with a 95% confidence interval between 0.1942 and 0.2079.

firms that is unrelated to the strength of complementarities but correlated with subsequent adoption decisions.

To overcome these issues, we test whether the increase in adoption differs depending on the distance between a district and areas in which the usage of the electronic wallets was important prior to November (*hubs*).[56] The mapping between the strength of complementarities and distance to an electronic payment hub is intuitive. In the model, local adoption rates entirely determine the strength of complementarities associated with the technology. In reality, individuals move across districts, and the size of adoption in neighboring districts will therefore also be important. Being located close to a large hub—where electronic payment use is relatively common— may significantly increase the benefits of adoption (Comin, Dmitriev, and Rossi-Hansberg 2012).

This approach also allows us to address the standard reflection issue. Rather than exploiting variation in the size of the network to identify the endogenous response due to externalities, this analysis follows the same logic as used in the empirical literature on indirect network effects (Rysman 2019; Jullien, Pavan, and Rysman 2021) and examines the relationship between two economic variables that should be related only under the assumption that network effects are sufficiently strong. This approach also has the added benefit of allowing a more transparent way to think about confounding factors.

We implement this test by running a simple difference-in-difference model where we compare the usage of wallet technologies around the demonetization period across districts that are differentially close to a digital wallet hub. Despite the clear advantages presented above, there still are two concerns with this approach. First, by sorting on distance, we might capture variation coming from areas that are located in more extreme or remote parts of the country. Second, since the electronic hubs are some of the largest and most important cities in the country, we should expect that being located close to them will have benefits that go beyond the effect of complementarities.[57]

Our specification deals with these limitations in three ways. First, we limit the comparison to districts that are located within the same state, adding state × month fixed effects. In this way, we exploit distance variation only

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[56] In particular, we define a district to be an electronic payment hub if there were more than 500 active firms before demonetization (September 2016). The results are essentially identical if we use a threshold of 1,000 firms to define the hub districts. The nine hubs are spread evenly across the country. In particular, these districts are Delhi, Chandigarh, and Jaipur (north); Kolkata (east); Mumbai and Pune (west); and Chennai, Bangalore, and Rangareddy (south). The distance to the hub is defined as the minimum of the distance between the district and all the hubs.

[57] A third concern is that distance may simply capture variation in exposure to the shock, as defined before. However, we actually find that the two treatment variables are uncorrelated (table H.7).

between areas that are already located in similar parts of the country. Second, we also control for the distance to the capital of the state, also interacted with time effects. This control allows us to isolate the effect of the distance to a major electronic payment hub from the effect of being located close to a large city. Third, as in the previous analyses, we augment the specification with a wide set of district-level covariates interacted with the time dummies. This implies a specification of the following form:

$$X_{d,s,t} = \alpha_{st} + \alpha_d + \delta(D_d \times 1_{\{t \geq t_0\}}) + \gamma(\tilde{D}_{d,s} \times 1_{\{t \geq t_0\}}) + \Gamma'_t Y_d + \epsilon_{d,t}, \quad (16)$$

where t indicates time, defined at the monthly level in this analysis; d indexes the district; and s identifies the state of the district. D_d is the district's distance to the nearest electronic wallet hub, and $\tilde{D}_{d,s}$ is the district's distance to the capital district of the state. As before, standard errors are clustered at district level.[58] The main coefficient of interest is δ, which provides the difference in the level of adoption before and after demonetization, depending on how far the district is from its closest electronic wallet hubs. We first present the results and then come back to discuss further the identification of the model.

These results are reported in table 3. Across all outcomes—the amount of transactions, number of operating firms, and number of new adopters—we find that the districts farther away from major hubs experienced a lower increase after the demonetization. The most conservative of the estimates indicates that a 50-kilometer increase in distance translates into a 19% lower increase in the amount of transactions.[59]

To be clear, interpreting these results as evidence for state dependence requires us to assume that distance affects the change in the use of electronic payments only because of the differences in the size of network effects for locations closer to a hub. A possible violation to this assumption would be if areas closer to electronic hubs are just more familiar with technology products and therefore inclined to use electronic payments. While we recognize that this assumption is fundamentally untestable, several of our results suggest that alternative interpretations are unlikely to play a significant role here.

First, conditional on the controls, areas that are characterized by different distances from a hub do not appear different on observable characteristics, in particular when looking at characteristics that should capture ex ante adoption propensity (table H.7). Second, these effects are

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[58] As we mentioned before, we remove the five major digital wallet hubs. Notice that this exclusion does not affect our results; the results that include the hubs are, if anything, stronger.

[59] Table H.11 shows similar results when with a dichotomous definition of the treatment. In particular, we consider several alternatives, going from 400 kilometers down to 200 kilometers. Across all these tests, the results are stable and significant.

TABLE 3
DISTRICT ADOPTION RATE OF DIGITAL WALLET BASED ON DISTANCE TO HUBS

	log(Amount)		log(Users)		log(Switchers)	
	(1)	(2)	(3)	(4)	(5)	(6)
Distance to hub × 1 ($t \geq t_0$)	−4.907***	−3.795***	−2.238***	−1.637***	−1.675***	−1.082***
	(.812)	(1.144)	(.414)	(.481)	(.320)	(.372)
Observations	7,168	7,168	7,168	7,168	7,168	7,168
R^2	.853	.887	.872	.912	.822	.871
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
District controls × month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
State × month fixed effects		Yes		Yes		Yes

NOTE.—The table reports the difference-in-differences estimate of the effect of initial conditions, using the distance to the nearest hub (defined as districts with more than 500 retailers in September 2016) as a proxy for the initial share of adopters. The specification estimated is eq. (16). Across cols. 1–6, we focus on different measures of activity in the platform. Specifically, we examine the total amount (Rs) of transactions carried out using a digital wallet in district d during month t (cols. 1, 2), the total number of active retailers using a digital wallet in district d during month t (cols. 3, 4), and the total number of new retailers joining the digital wallet in district d during month t (cols. 5, 6). District-level controls include (log) preshock banking deposits, share of villages with ATM facilities, share of villages with banking facility, share of rural population, level of population, and distance to state capital. Standard errors (in parentheses) are clustered at the district level.
*** $p < .01$.

not driven by differential trends in adoption between areas that are closer and farther from hub cities (fig. 7). This is important because several alternative interpretations should have affected adoption trends both before and after. Third, we find that distance from hub does not predict differential adoption of other related technologies, which one might have expected under the assumption that distance from hub proxies for a preference for innovation. Specifically, changes in use of fintech loans, bank accounts, and mobile phones are not differentially affected in areas that are closer to a payment hub (fig. H.10). Fourth, as we discuss in detail in appendix section E.2, we also find evidence that is consistent with state dependence examining the pattern of adoption at the firm level (Goolsbee and Klenow 2002; Munshi 2004). More tests of our hypothesis are presented in appendix E. Altogether, we argue that this evidence is easy to rationalize if we think that being located close to a hub generates higher strength of externalities—which became particularly important as the demonetization hit—and harder to reconcile with other interpretations.
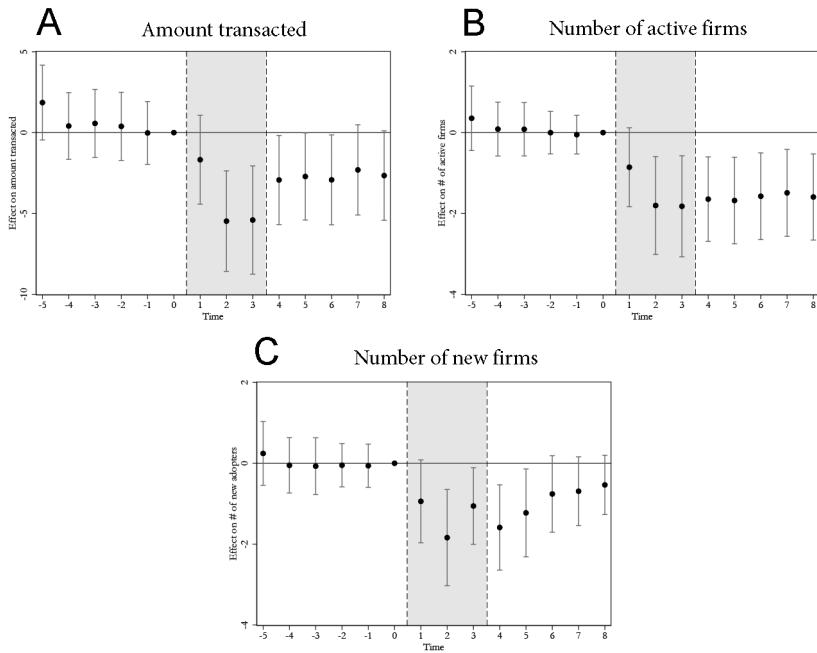
Fɪɢ. 7.—District adoption dynamics in electronic payments data based on distance to electronic hub. The figure plots the dynamic effects of adoption across districts based on a district's initial adoption rates, as proxied by the distance of that district to the closest district with more than 500 active firms before the demonetization. The specification we estimate is the dynamic version of equation (16). Panel A reports the effects for the total amount of transactions, panel B looks at the total number of firms, and panel C reports the effects for the total number of new firms transacting on the platform. The x-axis represents the month, where October 2016 is normalized to be zero. Vertical lines indicate 95% confidence intervals; standard errors are clustered at the district level.

D. Discussion

Overall, the evidence suggests that the demonetization caused an adoption wave with features that are qualitatively consistent with three predictions of the model with externalities: (1) a persistent increase in the size of user base; (2) a persistent increase in the adoption rate, that is, the flow of new users into the platform; and (3) state dependence in responses, that is, a positive relation between initial adoption rates and the initial strength of adoption externalities, broadly defined. In the context of our model, these predictions are specific to the presence of externalities, so these reduced-form results support the notion that externalities played a key role in shaping the adoption response following the shock.

Before moving forward, we discuss some additional factors that may influence the interpretation of our findings. In general, while the contraction

of cash was temporary (sec. II), one may be concerned that the policy changed in a persistent way other aspects of the Indian economy and that these forces may potentially play a role in the persistence of adoption. While we discuss specific concerns below, we also want to highlight that, in general, this type of issue is unlikely to explain our findings. First, the nature of our analysis—which exploits granular variation across districts— implies that aggregate shifts in the economy net out from our empirical models and therefore should not affect our estimates.[60] Second, while other aggregate channels may be able to account for each of our predictions in isolation, they generally cannot generate all three jointly.[61]

Specifically, one concern is that the demonetization may have persistently changed the way the Indian population valued cash for reasons unrelated to the increased value of electronic payments. For example, the policy may have reduced the incentive to use cash as a store of value for households. As we discuss in appendix section A.1, several stylized facts appear to contradict this hypothesis. For instance, we follow Engert, Fung, and Segendorf (2019) and examine whether the propensity to hold cash was permanently affected by the shock. Data reject this hypothesis (fig. H.12): the aggregate amount of cash in circulation relative to measures of total liquid wealth returned to its long-term average relatively quickly after the shock. By the same token, ATM debit card withdrawals go back to their preshock level shortly after February 2017 (fig. H.2). This evidence confirms that the Indian economy did not shy away from cash, therefore suggesting that underlying preferences for cash were not durably affected.[62] Similarly, an increase in uncertainty (Bloom 2009) cannot explain the persistence of the response: while overall uncertainty increased around November 2016, the impact was largely temporary and dissipated with the new year.[63]

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[60] As we discuss in app. sec. A.1, an aggregate shock is a relevant confounding factor only if it has differential effects across districts and if these differential sensitivities are correlated with our treatment variable in a systematic way. We follow this intuition in a few tests discussed later.

[61] For instance, factors that would persistently affect the relative value of cash vs. electronic payment irrespective of complementarities will generally reinforce the persistence in adoption but weaken state dependence.

[62] The increase in electronic payment and the lack of decline in cash in circulation are not facts in conflict. As discussed in Rogoff (2017), majority of the share of cash in circulation is not held for transactions but rather for store of value. Therefore, an increase in electronic payment does not necessarily impact the holding of cash in a significant way (Engert, Fung, and Segendorf 2019).

[63] In app. sec. B.7, we discuss the comparative statics of the model without complementarities ($C = 0$) with respect to the volatility of innovations to cash demand, σ. We highlight two findings: first, the comparative statics of the adoption trajectory with respect to σ do not depend on the initial level of the user base, in contrast to the state dependence we discussed in this section; second, with higher uncertainty, the autocovariance of the adoption decision declines, in contrast to the high persistence of the response to demonetization, which we also documented in this section. We note that these results differ from

Another important dimension to consider is the role of the government. While its initial objective was not to foster a shift toward electronic payments, the need to mitigate the impact of the policy on households led the government to introduce policies that may affect the use of electronic payments (app. sec. A.2). However, these interventions are unlikely to change the interpretation of our findings. First, most of them generally targeted traditional electronic payment technologies, not fintech, and therefore they are—if anything—likely to bias our findings toward no effect on mobile wallets technology. Second, our analysis fails to find any specific evidence that these policies affected the adoption of our mobile wallet technologies. Looking both in aggregate and across districts that were highly affected by the cash shock (table H.9), we find no significant response to policy announcements or implementation. While this evidence does not aim to represent a comprehensive policy evaluation of the government's postdemonetization policies, it does support the idea that these factors are unlikely to drive our empirical results.

Competition is another factor to consider. The policy shock had shaken up the Indian payment industry and potentially affected the nature of competition in this space. A few aspects should be considered. First, within fintech, our partner firm was the largest provider in India and could be considered the de facto monopolist for most of the sample period. Second, other traditional electronic payments did not experience any increase in new adopters at the time of the demonetization (as discussed in sec. II). Last, the nature of our data also implies that competition between platforms should increase measurement error, and therefore—if anything—this would bias our analyses toward finding no effects. Therefore, altogether we do not believe that competition between platforms can explain our results.

We also want to stress that marketing efforts and pricing strategies by the platform should not be important confounding factors. If local marketing spending by the partner company is correlated with our district-level treatment, then our effects would capture responses to such marketing efforts. However, our partner company organizes customer acquisition through national campaigns, and there was no program targeting specific local areas.[64] At the same time, pricing strategies to overcome coordination failure do not play an essential role in our analysis, as the fees to join the platform were zero during our sample period.

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the IRF analysis of sec. III, since they provide comparisons only across steady states. A full treatment of uncertainty shock would require extending the model to allow for stochastic volatility, which raises questions regarding existence and unicity of equilibria that are beyond the scope of this paper.

[64] Furthermore, as we explain in app. sec. A.3, the firm did not systematically change their model around the demonetization. Therefore, if our district exposure would capture areas with higher intensity of marketing efforts (or higher sensitivity), we should find some evidence of pretrend in the analyses, which we excluded before.

Finally, in section II, we argued that fixed, pecuniary adoption costs are unlikely to matter for the technology we are studying, because joining the platform does not involve initial fees, and the technological requirements to use it are very limited (in particular, no point of sale is required). Nevertheless, one may wonder whether, more generally, fixed costs could produce adoption patterns similar to those that we documented in this section. In appendix section B.6, we develop a model analog to section III and in which (1) there are no positive external returns to adoption but (2) adopting electronic money, when a retailer is using only cash, requires a lump sum payment of $\kappa > 0$.

We then study whether predictions 1a–3a hold in this alternative model. We show that while the first one does (the user base increases persistently following a sufficiently large shock), the second and third ones do not (the growth rate of the user base stops increasing at a finite horizon that depends on the persistence of the underlying shock, and there is no state dependence in the response to the shock). An important intuition that helps contrast fixed costs to positive externalities is that fixed costs generate persistent responses in levels because of inaction regions, not because of a growing incentive to join the platform. Following a large shock, firms have a temporary incentive to pay the fixed cost associated with the cash alternative; later on, they do not readjust their technology choice in order to avoid having to pay the sunk adoption cost again in the future (should another large shock arise) but not because adoption has become more attractive. As a result, there is no long-run growth in the platform, contrary to the evidence we discussed above.

The key takeaway from this discussion is that complementarities in adoption decisions are necessary to rationalize simultaneously the persistence and the state dependence in adoption documented in the data. However, these results leave two related questions open. First, they do not indicate how important complementarities are in the data. That is, these results do not allow us to take a stronger stand about the quantitative importance of complementarities in explaining the increase in adoption (i.e., how large C is, in the language of the model of sec. III). In section V, we address this question by structurally estimating the model, using the data on electronic wallet adoption and studying the estimated model's implications for the transmission of policies.

Second, while our results strongly support the idea that complementarities are key to explaining the increase in adoption, we have been so far silent about the exact sources of complementarities in our context. The model of section III does not take a clear stand on this; instead, it captures complementarities in reduced form by assuming that the returns to adoption increase with the number of other adopters. In section IV.E, we empirically examine this issue.

E.   Mechanisms Underlying Complementarities

In our context, the presence of complementarities in the decision of retailers to adopt can arise because of multiple channels. For instance, they may be generated by the presence of the network effects that are typical of a two-sided market (Katz and Shapiro 1994; Rysman 2007), as we illustrate in the two-sided model of appendix section B.5. Alternatively, learning by retailers about the costs and benefits of an uncertain technology—either through social interactions or by observing the experiences of peers—could also make adoption decisions complements (Munshi 2004; Young 2009; Bailey et al. 2019). The main empirical regularities we highlighted above, persistence in adoption after a temporary shock and state dependence, do not depend on the specific mechanism generating complementarities, but some more specific policy implications might.

While quantifying exactly the relative strength of these two mechanisms is outside the scope of this paper, we think that shedding more light on their empirical importance is useful. To examine this issue, we first study how use of the technology differs depending on the timing of the adoption decision. If learning is the main source of externalities in adoption, one should expect a more limited long-run response among users who were well informed about the technology, for instance, retailers that were already using the technology before the shock (Fafchamps, Soderbom, and van den Boogaart 2021). The same prediction should not hold, however, if traditional network effects represent a key determinant of complementarities between retailers. In this case, the cash crunch should affect usage independently from whether a retailer had prior knowledge of the technology.

Empirically, we examine this issue from two angles. First, we focus on firms that were already using electronic payments before November 2016 (preadopters) and had little more to learn about the benefits the technology. We find that this group experienced a persistent increase in the use of electronic payment. In aggregate, the firms that were preadopters (i.e., users in October 2016) saw a 100% increase in the number of transactions between October 2016 and May 2017. Using our analysis exploiting variation across districts, we also find a large persistent effect of the shock in this subsample of firms. In table H.15, we conduct our main analysis, focusing on preadopters. On top of finding that these firms also increased their use of electronic payments on average after the demonetization (col. 1), the results show that the effects are large and significant in both the short and the long run (col. 2).[65]

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[65] In fact, in this specification, we estimate a separate effect for the short run (i.e., November 2016–January 2017) and long run (i.e., February 2017 onward) and find that the long-run effect is very large and statistically similar to the short-run effect.

Second, we follow the same logic as the previous test but now look at a different subset of users in our data: those who adopted electronic payments in the short run during the demonetization period. The idea here is that if complementarities in adoption are completely determined by learning, long-run usage growth in this group (i.e., growth after cash availability has normalized) should not depend on the extent of the initial cash decline.[66] Instead, if network effects are a primary determinant of externalities, the cash shock experienced in November should also affect the growth experienced after January 2017 through the snowball effect generated by the increase in users' activity. The data appear to be more consistent with this second interpretation. As we show in figure H.13, the growth experienced during spring 2017 (i.e., between March and June 2017) is strongly predicted by the November shock.

This evidence suggests that the presence of externalities in adoption extends further than simply facilitating retailers' learning about the technology and that the presence of network effects generated by the two-sided nature of the payment platform represents an important mechanism in explaining our results. This interpretation is also consistent with two other findings.

First, we find no differences in the response to the main shock in areas where learning is easier. We consider two proxies for consumer learning in a region: the degree of language concentration and the extent to which the population of a district is connected to other people from the same district on Facebook (Bailey et al. 2018).[67] In general, if learning were a first-order mechanism, we should expect to find a stronger increase in adoption in districts where learning is easier, like districts with more homogeneous languages or where individuals are more connected with each other through social networks. Examining both aggregate district-level activity (table H.16) and the intensive margin (cols. 3–6 of table H.15), we reject this hypothesis.

Second, we confirm the importance of the two-sided nature market in generating externalities, using a survey of Indian adults who have adopted some form of electronic payment in the aftermath of the demonetization.[68] In the main question of this brief survey, we ask them the main

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[66] As we discuss in app. F, this result follows from two observations. First, cash availability had normalized by the end of January 2017. After this date, the shock should affect the use of electronic payments only indirectly through its impact on total use of mobile wallets in a local market. Second, the businesses we are considering have already adopted it by January 2017, and therefore they have already learned about the technology by then. This implies that if externalities operate only through learning, other contextual factors—e.g., other firms' adoption decisions—should not be relevant anymore.

[67] For the definitions of these measures, see app. F.

[68] The survey is discussed in greater detail in app. F. The data were collected using Mturk, and the final sample was of about 430 adult individuals living in India and having adopted electronic payments during the demonetization.

reason explaining their decision to adopt electronic payment after the demonetization, providing them with three non–mutually exclusive options to choose from. We find that 75% of respondents claim that an increase in the use of electronic payments in the other side of the market (e.g., stores for consumers) was an important aspect in their decision.[69] This number is high in absolute terms, but it is also high relative to the number of individuals who instead identified in the direct effect of cash the main reason for adopting electronic payments (56%). The role of learning appears relevant but less important than the other two options: only 44% of respondents claim that the adoption of electronic payment was affected by having learned about the technology from friends and family. While only suggestive, these results are consistent with our general narrative: both learning and network effects appear to be relevant to understanding the adoption of electronic payment during the demonetization, but the former factor seems to play a larger role.[70]

To conclude, we recognize that separating the different channels that could generate externalities in adoption is a notoriously challenging task. In this context, while the presence of externalities likely reflects a combination of different mechanisms, our evidence supports the idea that the network effects induced by the two-sided nature of the payment market play an important role in explaining our results.[71] Instead, learning from retailers cannot easily rationalize all our findings. While this evidence points to a smaller role played by learning within our context, it does not imply that learning is completely unimportant and that it could play a more central role in other contexts (e.g., Fafchamps, Soderbom, and van den Boogaart 2021).

V. Quantifying the Role of Complementarities

We now combine the model of section III with the data of section IV to estimate the quantitative importance of complementarities in our empirical

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[69] The survey asks whether the respondent is a shopkeeper, and we tailor the exact wording of subsequent questions accordingly. For instance, for consumers, we motivate their adoption of electronic payment as the result of an increase in the use of electronic payment by the shops they commonly use ("Because the shops where I buy things started accepting noncash payments, it was better for me to use this option"). More discussion of the survey instrument is given in app. F.

[70] One concern with our survey is that the sample of individuals recruited on Mturk may not be representative of the Indian population at large. To partially address this concern, in app. F, we discuss how our findings on the relative importance of network effects relative to learning for adoption are consistent with some results in the demonetization survey ran in 2016–17 by Financial Inclusion Insights.

[71] As we clarify in app. F, we purposefully did not discuss the presence of learning between consumers as a separate mechanism explaining our results because in our context, this mechanism would also traditional network effects. In order to affect retailer adoption (which is the object of our study), learning between consumers requires the presence of a feedback loop between the two sides of the market, similar to the standard network effects discussed earlier.

setting. We then use the estimated model to discuss the potential effects of counterfactual policies that can be relevant in other contexts, such as the trade-offs that exist between the size and persistence of interventions targeting adoption as well as the homogeneity of their effects.

A. Estimation

We use the simulated method of moments to estimate the key parameters of the model. We start by describing briefly our approach, focusing on the intuition for how specific moments help identify different model parameters. We then report the results and discuss model fit.

1. Methodology and Identification

We calibrate two parameters. First, we set $r = -\log(0.90)/12$, corresponding to a time discount rate of 0.90 per year. Second, we set $\theta = -\log(1 - 0.90)/(90/30)$, where θ is the (inverse of) the persistence of innovations to the money stock.[72] Additionally, and without loss of generality, we normalize the long-run mean of cash-based demand $M^c = 1$.

   We estimate the remaining $N_p = 5$ parameters of the model, $\Theta = (S, C, k, \sigma, M_e)$. They are, respectively, the size of the demonetization shock (S), the strength of complementarities in adoption (C), the Poisson arrival rate of the technology switching shock (k), the standard deviation of normal innovations to the money stock (σ), and the profits associated with the electronic payments technology when there is no adoption (M^e). In order to estimate those parameters, we use the following set of regressions on a balanced panel of districts:

$$\Delta_{t_0} X_{d,t} = \beta + \gamma \mathbf{1}\{t \geq t_0 + 3\} + \delta X_{d,t_0} + \zeta(\mathbf{1}\{t \geq t_0 + 3\} \times X_{d,t_0}) + \epsilon_{d,t},$$

$$\widehat{\mathrm{var}}_t(\Delta_{t_0} X_{d,t}) = \eta + \kappa \mathbf{1}\{t \geq t_0 + 3\} + \mu_t, \tag{17}$$

$$\widehat{\mathrm{var}}_d(\Delta_{t_0} X_{d,t}) = \nu + \omega_d,$$

and we additionally estimate the average of the squared residuals $\hat{\epsilon}_{d,t}^2$ from the first regression in (17) through $\hat{\epsilon}_{d,t}^2 = \xi + \omega_{d,t}$. In these regressions, d indexes the 512 districts included in our analysis, and t indexes months. The month t_0 is October 2016 (the last month observed prior to the demonetization shock), and $\Delta_{t_0} X_{d,t}$ is the cumulative change in adoption rates: $\Delta_{t_0} X_{d,t} = X_{d,t} - X_{d,t_0}$. We use the 8 months running from

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[72] This choice ensures that it takes on average 90 days for the aggregate shock to be 90% dissipated. The choice of 90 days is approximately equal to the time that elapsed between the announcement of the cash swap (November 8, 2016) and the date at which the government lifted most remaining restrictions on cash withdrawals (January 30, 2017).

TABLE 4
POINT ESTIMATES AND STANDARD DEVIATIONS FOR $\hat{\Theta}$

Parameter	Definition	Estimate	Standard Error
S	Size of aggregate shock	.215	.035
C	Adoption complementarities	.062	.016
k	Speed of technology adjustment	.166	.032
σ	Volatility of idiosyncratic innovations	.042	.016
M^e	Returns to electronic payments when $X_{d,t} = 0$	.974	.001

NOTE.—Parameters are estimated on a balanced panel with 512 districts and 8 months. The estimation procedure uses the simulated method of moments and is described in sec. V. Standard errors (in parentheses) are computed using the bootstrap described in app. G.

November 2016 to June 2017.[73] We compute the participation rate in each district, $X_{d,t}$, as the ratio of the number of monthly users active on the platform during month t, divided by the number of retailers with less than four employees, which we obtain from the 2013 Economic Census.[74] Finally, $\widehat{\text{var}}_t(\cdot)$ denotes cross-sectional variances, while $\widehat{\text{var}}_d(\cdot)$ denotes within-district variances.

In order to estimate our five data parameters, we use $N_m = 8$ data moments from the regressions above: $\hat{\Xi} = (\hat{\beta}, \hat{\gamma}, \hat{\delta}, \hat{\zeta}, \hat{\xi}, \hat{\eta}, \hat{\kappa}, \hat{\nu})$. Appendix section G.1 reports the details of the estimation procedure. We use the bootstrap, clustering by district, in order to construct the variance-covariance matrix of data moments. Appendix section G.2 discusses in more detail the intuition for why the chosen data moments help identify the five estimated parameters. In particular, consistent with the reduced-form approach of section IV.B, the strength of externalities, C, is primarily identified by the difference between the short- and the medium-run response of adoption to the shock, $\hat{\gamma}$.

2. Results

Table 4 reports estimates of the five structural parameters. The point estimate for the size of the shock, S, is 21.5% (with a 90% coverage interval of [13.2%, 29.7%]). The parameter S expresses the decline in profits associated with cash-based transactions relative to their long-run mean. There are two numbers with which this estimate could be compared. First, recall that the cash denominations that were voided by the shock represented

[73] We subtract the initial adoption rate in order to eliminate district-specific fixed effects, but results either in levels or adding explicit fixed effects in the estimation of (17) are similar.

[74] Additionally, we renormalize the census retail counts so that the five districts with the highest adopter share reach full adoption. Appendix G discusses this normalization in more detail and shows that it does not materially affect our results.

86.4% of the total currency in circulation. The shock size we estimate is much smaller than this, but not all of the voided currency was actively used in transactions prior to shock (though it is difficult to measure exactly what fraction was). Second, Chodorow-Reich et al. (2019) estimates that the general equilibrium decline output to the shock was approximately 3%. Aside from being a general equilibrium estimate, this figure expresses the response of value added (not profits), includes the potential effects of substitution into electronic payments technologies, and encompasses all sectors of the economy. For these reasons, it is likely a lower bound on the size of the shock. Our point estimate, however, has a reasonable magnitude compared with theirs: for instance, assuming a labor share of 70% in retail and no adjustment of labor or hours in the short run, the implied decline in profit rates in retail using the 3% figure is $1/0.3 \times 3\% = 9\%$, or a little less than half of our point estimate.

The magnitudes of the point estimates for the level and the slope of the switching frontier are difficult to interpret explicitly, but it is worth making two points about them. First, the point estimate of C is 0.062, with a 90% coverage interval of [0.047, 0.076]. Our findings therefore reject the null of no adoption complementarities. Second, the point estimates imply that relative to cash, profits under the electronic technology are on average 2.6% lower if there are no other adopters and 3.6% higher if there is full adoption. Together with other parameters, these differences imply that the equilibrium switching frontier is such that cash-based demand M_t must fall by 14.2% in a district with $X_t = 0$ adoption, or a little over 4 standard deviations, in order for adoption to start. The estimated size of the shock substantially exceeds this threshold.

Finally, the point estimate of the rate of technology resetting implies that, on average, firms receive the option to adjust their technological choice every 6.0 months, with the 90% coverage interval of the arrival rate corresponding to frequencies between 4.1 and 10.2 months. The estimate of k is fairly imprecise, but it implies that arrival rates higher than 3 months can be rejected at the 1% level. As discussed earlier, this relatively slow technological adjustment rate may reflect learning or cognitive costs associated with the use of the technology.

Table 5 reports measures of goodness of fit. Column 2 reports the empirical value of the moments used in the estimation. Columns 3–5 provide average values, standard deviations, and one-sided p-values obtained from $S_{CI} = 2{,}000$ simulations of the model, with structural parameters set to their estimated values, that is, $\Theta = \hat{\Theta}$. We can reject equality of the empirical and simulated moments at the 1% for two of the eight moments, and overall, the overidentification test cannot reject the null that the model is correctly specified at the 1% level. The moment with the worse fit is the medium-run variance in adoption, which the model tends to underestimate, relative to the data.

TABLE 5
MODEL FIT FOR SIMULATED METHOD OF MOMENTS ESTIMATION

Moment	Definition (1)	Empirical Value (2)	Simulated Value (3)	Standard Error (4)	p (5)
$\hat{\beta}$	Short-run average effect	.030	.027	.003	.19
$\hat{\gamma}$	Medium-run average effect	.038	.029	.004	.02
$\hat{\delta}$	Short-run effect of $X_{d,0}$	.081	.081	.011	.47
$\hat{\zeta}$	Medium-run effect of $X_{d,0}$	.027	.027	.007	.26
$\hat{\xi}$	Mean squared residuals	.083	.091	.006	.11
$\hat{\eta}$	Short-run between-district variance	.126	.142	.010	.06
$\hat{\kappa}$	Medium-run between-district variance	.052	.084	.006	.00
$\hat{\hat{\xi}}$	Within-district variance	.045	.061	.004	.00

NOTE.—Overidentification statistic = 3.391 (df = 3, p = .335). Column 2 shows the empirical values of the moments used in the estimation of the model and described in sec. V. The simulated values are computed using the point estimates reported in table 4. We simulate 2,000 panels consisting of 512 districts each and sample data from each panel at the monthly frequency. We then use each panel to compute the moments described in eq. (17) and used in the estimation of the model. The standard error reported is the simulated sample standard error. The p-values reported for each moment are one sided: they are the fraction of observations for which the simulated moment is at least as far from the average simulated moment as the empirical moment is. In the estimation procedure, we use the square root of all second-order moments; the table reports these standard errors and not the variance. More details on the estimation procedure are reported in app. G.

B. Counterfactuals

Next, we use the estimated model to construct the quantitative answer to three questions about the effects of the shock and the role played by complementarities in the adoption process.

1. How Would Adoption Have Responded in the Absence of Complementarities?

Figure 8 reports empirical and model-based paths of average adoption across districts in the aftermath of the shock. At the point estimates reported in table 4, adoption rises by approximately 4 percentage points by the end of December and 6.5 percentage points by the end of May, in line with the empirical estimates. This result is not surprising, since these moments were explicitly targeted. The figure also reports a counterfactual path of adoption rates, under the assumption that there are no complementarities, that is, when $C = 0$. With respect to the data and to our baseline estimate, the adoption path is similar during the first 3 months, when the cash crunch is still ongoing. After that, it diverges from the data and from the model with complementarities, declining in the medium run. The gap is fairly substantial: the predicted increase in adoption rates without complementarities would have been 3 percentage points (or
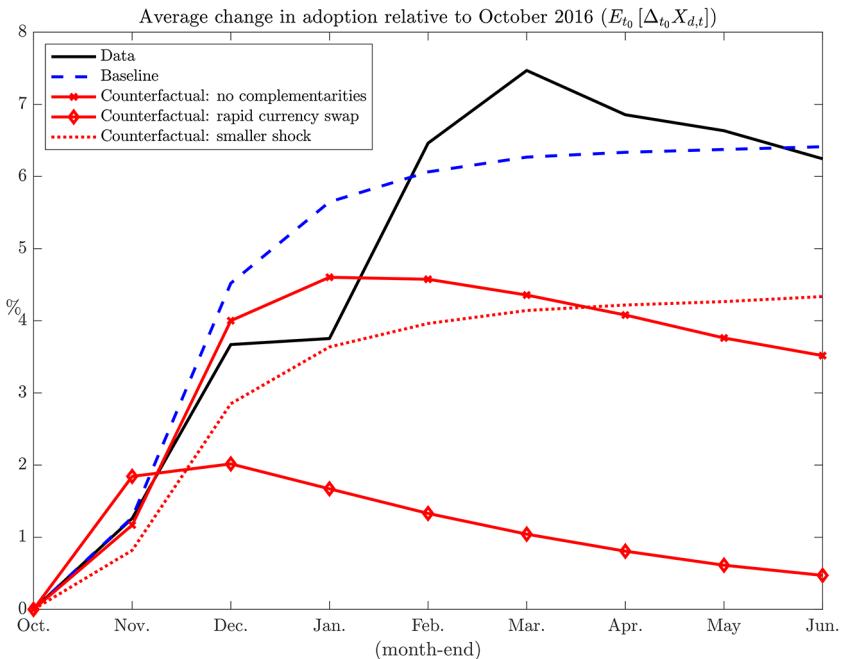
Fig. 8.—Counterfactual paths of average adoption rates across districts. Solid line reports the empirical change in average adoption rates across districts. Other lines report average changes in adoption rates constructed using $S = 100$ simulations from the model, each of a dataset of the same size as the actual data. Dashed line is the change in adoption rate obtained from the model evaluated at the point estimates reported in table 4. Solid line with crosses is the average change in adoption rate in the absence of complementarities, assuming that the switching frontier (which is flat without externalities) has the same level as the switching frontier with externalities when adoption is zero. Solid line with diamonds is the change in adoption rate when $\theta = 1.7$, corresponding to a 40% decay time of 30 days. Dotted line is the change in adoption rate when the shock has half the initial size as estimated in table 4.

approximately 45%) lower than observed. Thus, the model attributes an important share of the response of adoption rates to complementarities.

Figure H.17 repeats the same exercise under alternative assumptions about the degree of shock persistence. While cash availability had returned to normal by February 2017, it is possible that the public's perception of the benefits of cash changed more durably (even though the evidence presented in sec. IV.D and app. sec. A.1 suggests that this is unlikely to have been the case). The results of figure H.17 shed light on the extent to which such a change would affect our estimates of the contribution of complementarities to the adoption response. A higher shock persistence (on the horizontal axis) proxies for a more durable change

in the perceived benefits of cash.[75] We allow persistence to vary between our baseline value of 90 days (which, as argued in sec. II, is in line with the persistence of the actual cash shortage) and 240 days (the horizon of our sample).[76] The red line reports results when C is fixed to the value reported in table 4, while the blue line reestimates C for each degree of persistence. Naturally, with a higher degree of persistence, the strength of complementarities required to account for the long-run response of adoption declines. Nevertheless, even for a shock persistence that is three times larger than our baseline, complementarities still account for approximately 25% of the adoption response 8 months out. The intuition for this finding is that the model requires positive externalities in order to account for the data even when fundamental shocks are persistent, because without positive externalities, the model does not generate any state dependence in the adoption response. Thus, even under the alternative assumption of a more persistent shift in perceived flows benefits of cash, complementarities continue to play a positive and economically nontrivial role.

2. What If the Cash Swap Had Been Completed More Quickly?

Figure 8 also reports counterfactual adoption paths that speak to the role of the size and persistence of the shock. We first construct adoption paths under the assumption that a 90% decay rate of the shock is 1 month instead of 3 months; this captures an alternative world in which the cash swap would have been executed as rapidly as initially intended. Under this scenario, adoption would have risen only by approximately 1 percentage point, and the increase in the dispersion of adoption would have been negligible. Figure 8 also indicates that if the shock had been smaller in magnitude—which could capture a situation in which only one denomination would have been replaced, for instance—the long-run response would have been smaller. With a shock half as large, the average adoption rate rises only by approximately 4.5 percentage points versus 6.5 percentage points in the baseline case. The model thus suggests that the persistence and size of the cash crunch might have had substantial—though unintended—positive effects on adoption overall.

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[75] In the baseline model of sec. III, the flow benefits from cash for retailers are equal to M_t. In the two-sided model of app. sec. B.5, the flow utility to consumers is proportional to M_t. In either case, more persistent innovations to M_t are isomorphic to more persistent shifts in the flow benefits associated with cash, as perceived by either retailers or households.

[76] Recall that we normalize shock persistence so that it is expressed as the number of days expected for the shock to mean revert to within 10% of its long-run value.

3. What Sort of Intervention Maximizes Long-Run Adoption?

We next use the model to ask whether a hypothetical policy maker could have achieved higher long-run changes in adoption rates by implementing the cash swap differently.[77] In order to answer this question, we first define the cost of the cash swap as the present value of the decline in cash after the shock:

$$C(S, \theta) = \mathbb{E}_0 \left[\int_0^{+\infty} e^{-rt} (M^c - M_t) \, dt \right] = \frac{SM^c}{r + \theta}, \qquad (18)$$

where we used $t_0 = 0$ to streamline notation. We next consider the following maximization problem for the hypothetical policy maker:

$$\arg\max_{S,\theta} \quad \mathbb{E}_0[\Delta X_{d,T}] - \frac{g}{2} \text{var}[\Delta X_{d,T}] \quad \text{subject to } C(S, \theta) \le C(\hat{S}, \theta_0), \quad (19)$$

where $\hat{S}$ is the estimated value of the shock, which is reported in table 4; $\theta_0 = -\log(1 - 0.90)/(90/30)$ is the persistence of the shock used in the estimation of the model; g is a positive number; and $\Delta X_{d,T}$ is the growth of the user base in district d from $t = 0$ to $T = 8$ months.[78]

This is the problem facing the hypothetical policy maker who chooses the size and persistence of the shock to cash-based demand, aims to maximize average adoption at horizon $T = 3$ years, and possibly exhibits some aversion to dispersion in adoption rates (when $g > 0$). The aversion to dispersion could capture a preference of policy makers toward broad-based adoption. Furthermore, we assume that this policy maker is constrained in the total cost of the intervention, and we use the empirically estimated cost of the demonetization shock as the maximum cost the policy maker can incur.

Table 6 reports the numerical solution to problem (19) under different values of g. Additionally, column 1 reports the model estimates of the size and persistence of shocks and the implied long-run first and second moments of the change in adoption rates.

Results for column 1, $g = 0$, show that the constrained optimal plan—for a policy maker who does not care about long-run dispersion in adoption rates across districts—involves choosing a shock that is more persistent

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[77] We focus on how to implement the cash swap because this is one of the salient policy questions in the context of the Indian demonetization. However, one should not interpret our analysis as saying that policies such as demonetization are optimal either in any general welfare sense or, more specifically, for encouraging adoption. The problem we analyze narrowly describes a policy maker selecting the size and length of a subsidy program targeting a technology with externalities in order to maximize some objective, which need not be welfare relevant.

[78] We compute this objective using a simulated panel of districts of the same size as for our estimation.

TABLE 6
ALTERNATIVE INTERVENTIONS

	BASELINE (1)	ALTERNATIVE INTERVENTIONS			
		$g = 0$ (2)	$g = .2$ (3)	$g = .4$ (4)	$g = .6$ (5)
Shock size (percentage points)	21.5	14.2	13.1	11.5	10.7
Shock half-life (months)	.9	1.4	1.5	1.8	1.9
$\mathbb{E}_{t_0}[\Delta_{t_0} X_{d,t_0+T}]$ (percentage points)	6.4	8.8	8.8	8.2	7.8
$\mathrm{SD}_{t_0}[\Delta_{t_0} X_{d,t_0+T}]$ (percentage points)	18.6	23.6	21.9	20.5	19.9

NOTE.—Column 1 reports the estimated shock size, shock half-life, and mean and standard deviation of long-run changes in average adoption rates; we use $T = 3$ years and $s = 100$ simulations to compute these moments. Columns 2–5 report these moments under alternative scenarios. For each value of g—the aversion to dispersion in the planner's objective function—we compute the value of the shock size and persistence that maximizes the objective described in eq. (19).

but smaller than what we estimated. Thus, the model indicates that, given the total cost of the intervention implied by the model estimates, a policy maker seeking to maximize long-run adoption could have done better than the observed outcome by making the shock both more persistent and smaller. The difference with respect to the estimated shock is sizable: the shock half-life is approximately one and a half months instead of approximately 1 month in the baseline case, and the shock would have been approximately one-third smaller in size.

Because the constrained optimal shock is smaller, it also leads to more dispersion in adoption rates in the long run. The intuition for this is that with a smaller shock, a higher initial adoption rate is required for the district to enter the adoption region. The initial differences between districts are then exacerbated. As a result, long-run dispersion in the constrained optimal plan when $g = 0$ is higher than in the model estimates, as indicated in table 6.

However, as aversion to dispersion increases (i.e., as g increases), the constrained optimal plan progressively involves smaller and more persistent shocks. The intuition for this result is that a more persistent shock tends to reduce long-run dispersion in outcomes, because it reduces the degree of state dependence of adoption rates. The phase diagram in figure 2A2 can be used to understand this. In that diagram, a more persistent shock implies that the economy moves up more slowly; in other words, the adoption trajectory illustrated in figure 2A2 shifts down at all dates $t > 0$ as shock persistence increases. This implies that for a given initial user base and shock size, the economy is more likely to stay in the adoption region as the shock becomes more persistent. In other words, persistence weakens state dependence.[79]

[79] Note that this is consistent with theoretical discussion of sec. III.B.1. In particular, fig. 3 (which corresponds to the case of short-lived shocks, $\theta > k$) shows that the boundaries

A policy maker who cares about dispersion therefore has a motive to further increase the persistence of the intervention. Compared with $g = 0$, the constrained optimal plan with an aversion to dispersion of $g = 0.6$ is associated with a shock that is smaller (by about $1 - 10.7/14.2 = 25\%$) but more persistent (by about $1 - 10.7/14.2 = 26\%$). Thus, while the size and persistence of the shock had positive effects on long-run adoption—as discussed above—the model also suggests that if the objective of the policy had been to increase long-run adoption while minimizing the dispersion in outcomes across districts, a more persistent but smaller intervention would have been preferable. That said, long-run adoption gains under these alternative policies are relatively mild, in the order of 25%–35% of the long-run adoption increase implied by our baseline estimates.

The analysis of this section has shown that the simple model of section III can account well for key moments of the data. Counterfactuals suggest that complementarities account for 45% of the medium-run response of adoption and that a smaller but more persistent intervention may have led to a larger increase in long-run adoption rates, along with a lower long-run dispersion in adoption across districts.

VI. Conclusion

An increasing number of new technologies feature network externalities. When this is the case, the technology's ability to grow and scale is subject to coordination frictions. Are these frictions empirically relevant? Furthermore, can policy interventions help address them? We used the Indian demonetization of 2016 and its subsequent effect on the adoption of electronic wallets as a laboratory to study these questions.

We started by showing that the demonetization led to a large and persistent increase in the overall use of this technology, even though the demonetization shock itself was temporary. We argued that this large and persistent increase is consistent with a dynamic technology adoption model with externalities, and we derived some additional testable predictions unique to externalities. In particular, we showed that in this model, a temporary shock can cause a persistent increase in the adoption rate of the platform (as opposed to only its size) and that the response of adoption rates depends positively on initial adoption levels.

Using micro data on electronic payments, we then showed that these additional testable predictions are supported by the data. At the district level, we proposed a novel identification strategy based on heterogeneity in the presence of chest banks to estimate the causal impact of the cash

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of the regions for which the shock has permanent effects depend on the initial user base (X_0). On the other hand, fig. H.16 (which corresponds to the case of more persistent shocks, $\theta < k$) shows that the boundaries of these regions are independent of X_0 and depend on only the strength of complementarities.

crunch. We showed that the cash crunch caused a persistent increase in the adoption rate of electronic wallets by firms. Additionally, the adoption responses are characterized by positive state dependence at both the district and the firm level. Finally, we provided a structural estimation of our dynamic model. This estimation suggests that about 45% of the total adoption response is due to complementarities.

Our analysis also highlighted some of the challenges faced by policy makers in environments with complementarities. In those environments, large but temporary interventions can have permanent effects on adoption because they effectively act as coordinating devices that help firms overcome coordination frictions. However, because of state dependence, an intervention that is too brief can also exacerbate inequality in adoption rates. Policy makers may therefore face a trade-off between the length of the intervention and how much it will exacerbate initial difference in adoption rates. These results have implications beyond our setting, as externalities are an increasingly common feature of technologies in the new economy.[80]

Before concluding, an important point to highlight is that this paper does not aim to evaluate the net welfare effect of the demonetization. First, an assessment of the welfare impact of the increase in adoption would require a model that incorporates also the asymmetric impact of price between retailers and consumers and also accounts for the interaction between different forms of payments (e.g., Bedre-Defolie and Calvano 2013; Edelman and Wright 2015; Koulayev et al. 2016; Huynh et al. 2020). Second, consistent with Chodorow-Reich et al. (2019), our analysis of consumption data has suggested that this policy had significant economic cost for the population. As a result, any potential benefit in terms of electronic payment adoption—as well as other aspects that were affected by the policy—needs to be carefully weighted against these costs.

Our work suggests two avenues for future research. First, we highlighted some general testable predictions of dynamic adoption models with externalities that could be tested in contexts other than the adoption of payment technologies. Second, future work should study strategic changes in firms' behavior in response to the adoption of electronic payments, in particular regarding pricing and competition.

Data Availability

Data and code for replicating the tables and figures in this article can be found in Crouzet, Gupta, and Mezzanotti (2023) in the Harvard Dataverse, https://doi.org/10.7910/DVN/ZB45FV.

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[80] On top of the network-based fintech sector already discussed, complementarities in adoption can also be generated by the type of social data acquisition that is typical of many online services (Bergemann, Bonatti, and Gan 2020).

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