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Epidemic Exposure, Financial Technology, and the Digital Divide

We ask whether epidemic exposure leads to a shift in financial technology usage and who participates in this shift. We exploit a data set combining Gallup World Polls and Global Findex surveys for some 250,000 individuals in 140 countries, merging them with information on the incidence of epidemics and local 3G Internet infrastructure. Epidemic exposure is associated with an increase in remote-access (online/mobile) banking and substitution from bank branch based to ATM activity. The temporary nature of the effects we identify is more consistent with a demand channel rather than that of supply with high initial fixed costs. Exploring heterogeneity using a machine learning–driven approach, we find that young, high-income earners in full-time employment have the greatest tendency to shift to online/mobile transactions in response to epidemics. Baseline effects are larger for individuals with better *ex ante* 3G signal coverage, highlighting the role of the digital divide in adaption to new technologies necessitated by adverse external shocks.

JEL codes: G20, G59, I10
Keywords: epidemics, fintech, banking

Epidemics are frequently cited as inducing changes in economic behavior and accelerating technological and behavioral trends. The Black

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Death, the mother of all epidemics, is thought to have sped the adoption of earlier capital-intensive agricultural technologies such as the heavy plow and water mill by inducing substitution of capital for more expensive labor (Senn 2003, Pelham 2017). COVID-19, a more recent example, is said to have increased remote working (Brenan 2020), online shopping (Grashuis et al. 2020), and telehealth (Richardson et al. 2020).

Here we study these issues in the context of fintech adoption and usage.[1] We ask whether past epidemics induced a shift toward remote-access financial technologies such as online banking and ATMs, and away from traditional brick-and-mortar bank branches. We combine data on epidemics worldwide with nationally representative Global Findex surveys of individual financial behavior fielded in more than 140 countries in 2011, 2014, and 2017. Matching each individual in Global Findex data set to detailed background information about the same individual in Gallup World Polls (GWP) allows us to control for socioeconomic factors at a granular level.

Holding constant individual-level economic and demographic characteristics and country and year fixed effects, we find that contemporaneous epidemic exposure increases the likelihood that individuals transact via the Internet and mobile bank accounts, make online payments using the Internet, and complete account transactions using an ATM instead of with a teller at a bank branch. Exposure to an epidemic leads to 10.6 percentage point increase in online/mobile transactions using the Internet and bank account, and a 4.5 percentage point increase in mobile transactions using bank accounts. Given that the means of these outcome variables are 8.3% and 9.4%, respectively, the effect is large—doubles and triples the initial propensity. In addition, we find that separate impacts on ATM and in-branch transactions almost exactly offset. This suggests that epidemic exposure mainly affects the form of banking activity—digital or in person—without increasing or reducing its volume or extent.

Although the limited time span covered by our data allows for only a tentative analysis of persistence, our results suggest that the impact of epidemic exposure is felt mainly in the short run rather than persistently over time. This supports the conjecture that the effects we detect are driven by demand-side factors (i.e., consumer preferences, as consumers shift back and forth between in-person and digital payments vehicles in response to changes in the risk of face-to-face contact) rather than supply-side factors (as banks invest the sunk costs of permanently increasing the supply of, inter alia, ATMs in response to the increased risk of close personal contact). Consistent with this interpretation, we fail to find any effects of epidemics on banks' provision of new technologies such as ATMs.[2]

Sensitivity analyses support these findings. The results continue to hold when we adjust for multiple outcomes (Anderson 2008). A test following Oster (2019)

1. We interchangeably use the terms "fintech adoption" and "fintech usage." As will be clear later, our measures of financial technology adoption and usage at the individual level tend to be binary and thus cannot speak to the intensive margin of fintech access (i.e., how much a technology is used by an individual). In a sense, construction of variables based solely on the extensive margin is more in line with the notion of fintech adoption rather than fintech usage.

2. See Online Appendix Table 12.

confirms that our treatment effects are unlikely to be driven by omitted factors. We document the existence of parallel trends before epidemic events, present balance tests across countries that do and do not experience epidemics, find null effects for placebo outcomes, analyze epidemic intensity, implement alternative clustering techniques for standard errors, control for country-specific time trends, drop influential treatment observations, and randomize treatment countries and/or years. None of these extensions qualitatively changes our results or interpretation.

Using the data-driven approach suggested by Athey and Imbens (2016), we then identify the key heterogeneities in our treatment effects. These are individual income, employment, and age. In other words, it is mainly young high earners in full-time employment who take up online/mobile transactions in response to epidemics. These patterns are consistent with the findings of previous research on early adopters of digital technologies (Chau and Hui 1998, Dedehayir et al 2017).

Last, but not least, we highlight the importance of the digital divide by investigating the role of local Internet infrastructure in conditioning the shift toward online banking.[3] We match 1 km-by-1 km time-varying data on global 3G Internet coverage from Collins Bartholomew's Mobile Coverage Explorer to the subnational region in which each individual surveyed by Findex-Gallup resides. We find that individuals with better *ex ante* Internet coverage are more likely to shift toward online banking in response to an epidemic. This finding still obtains when we include country-by-year fixed effects that absorb all types of country-level variation in our sample, including the incidence of epidemics. Importantly, we fail to find any consistent effect for GSM (Global System for Mobile Communication, or 2G, the older radio system used in cell-phones, that only allows phone calls and sending text messages) when this is included side by side with our 3G measure, confirming that the relevant technology is related to the Internet and not to overall mobile phone usage.

In sum, we find strong evidence of epidemic-induced changes in economic and financial behavior, of differences in the extent of such shifts by more and less economically advantaged individuals, and of a role for digital infrastructure in spreading or limiting the benefits of technological alternatives. The results thus highlight both the behavioral response to epidemics and the digital divide.

Online and mobile banking, as well as branch versus ATM activities, are informative contexts for studying the broader question of whether past epidemics encouraged the adoption and use of new financial technologies and, if so, by whom and where. Individuals in a variety of different countries and settings have available banking options that involve both in-person contact (such as banking via tellers in bank branches) and remote-access alternatives (such as banking via the Internet or mobile phone app); these alternatives have been available for some time. Analogous studies of telehealth would face the obstacle that physicians' offices in many countries and settings did not, at the time of epidemic exposure, have the capacity to provide such services

3. To be clear, the measure of digital divide we use in the paper (i.e., 3G coverage) is not particular to the banking or financial industry; it is a proxy for how well different subnational regions of a country are connected to the Internet in general.

remotely. Similarly, studies of remote schooling in the context of past epidemics would be limited by the fact that few schools and homes had available a flexible video conferencing technology, such as Zoom, much less the reliable Internet needed to operate it.

Banking is different in that the diffusion and use of ATMs and online banking have been underway since the 1990s. Individuals have been using ATMs, computers, and smartphones for banking applications for years. Thus, insofar as epidemic exposure induces changes in behavior, these are likely to be more evident in this context than others.

The paper is organized as follows. Section 1 reviews the related literature. Sections 2 and 3 then describe our data and empirical strategy. Section 4 presents the main results, including for within-sample heterogeneity, persistence of the effects, and the role of 3G infrastructure. Section 5 summarizes our additional robustness checks, after which Section 6 concludes. The online appendix presents further details on our data and additional empirical results.

1. RELATED LITERATURE

Our paper is related to several literatures. First, there is a literature on the impact of digital technologies on financial behavior. For example, D'Andrea and Limodio (2019) analyze the rollout of submarine fiber-optic cables and access to high-speed Internet in Africa, showing that high-speed Internet promoted more efficient liquidity management by banks due to enhanced access to the interbank market, resulting in more lending to the private sector and greater use of credit by firms. Muralidharan et al. (2016) and Aker et al. (2016) find that biometric smart cards and mobile money systems facilitate governmental efforts to distribute employment and pension benefits. Bachas et al. (2018) find that debit cards, by reducing the difficulty of accessing and utilizing bank services, foster financial inclusion. Callen et al. (2019) show that mobile point-of-service terminals improve savings options, in turn alleviating extreme poverty, encouraging self-employment, and raising wages. Jack and Suri (2014) similarly find that access to mobile money enhances risk-sharing and smoothens consumption, in their context by improving access to remittances. Digital payments that connect individuals with banks, employees, and suppliers encourage entrepreneurship (Klapper 2017), while ability to conduct financial transactions by mobile phone reduces urban–rural inequality by facilitating money transfer between urban and rural members of extended families (Lee et al. 2021). We contribute to this literature by showing that when social distancing is a necessity, access to digital financial technology helps individuals to continue financial activities by switching from in-person to remote-access options.

A subliterature focuses on differential adoption of online, mobile, and e-banking. Some studies examine the role of social influences, such as the practices of friends and family (Al-Somali et al. 2009, Baptista and Oliviera 2015, Tarhini et al. 2016).

Chen et al. (2021) document a pervasive male–female gap in fintech adoption, pointing to social norms, as well as possible differences in preferences and gender-based discrimination, as potential explanations for slower adoption by women. Other studies focus on trust, defined as the belief that others will not behave opportunistically in the digital sphere (Gu et al. 2009). Finally, studies such as Breza et al. (2020) and Klapper (2020) find that information about the utility and security of online and mobile banking, obtained via first-hand experience or independent sources, is conducive to wider utilization. Our paper adds to this literature by showing how national health emergencies shape usage of such technologies, and by documenting the existence of digital divides between economic and demographic subgroups, defined by age, income, and employment.

A number of recent papers study take-up and effects of financial technologies in the context of COVID-19. Kwan et al. (2020) examine the relationship between banks' IT capacity and ability to serve customers during the recent pandemic; using U.S. data, they show that banks with better IT capabilities saw larger reductions in physical branch visits and larger increases in website traffic, consistent with a shift to digital banking. In addition, they find that banks possessing more advanced IT originated more small business Paycheck Protection Program (PPP) loans. Core and De Marco (2021) examine small business lending in Italy during COVID-19 and similarly find that banks with more sophisticated IT were better able to distribute government-guaranteed loans. Erel and Liebersohn (2020), again in the context of PPP lending, find that borrowers obtained these loans primarily from banks in zip codes with more bank branches, higher incomes, and smaller minority shares of the population, but from fintechs in places with fewer banks, lower incomes, and more minorities. Comparing zip codes with more and fewer bank branches, they find limited substitution from fintech to bank borrowing, as if fintech presence leads mainly to an increase in the overall supply of financial services (greater financial inclusion), not just reallocation from banks to fintechs. Fu and Mishra (2020) show that the COVID-19 virus and government-ordered lockdowns increased downloads of banking-related apps. We extend these findings to past epidemics and a larger set of countries, as well as providing evidence not just for the adoption of new technologies but also for the abandonment of old ones (i.e., reduced bank branch usage relative to ATMs). Our setting also allows us to consider possible long-term impacts of epidemics, as opposed to focusing only on contemporaneous effects.

Finally, there is the literature on the digital divide. World Bank (2016) emphasizes that the benefits of new digital technologies are unevenly distributed owing to lack of high-speed Internet in developing countries and regions. Chiou and Tucker (2020) show that the availability of high-speed Internet significantly affected the ability of individuals to self-isolate during the COVID-19 pandemic. UNCTAD (2020) documents that lack of Internet access limits scope for shifting to remote schooling in developing countries; McKenzie (2021) finds similar patterns for underserved areas in the United States. We contribute to this literature by showing that lack of 3G coverage slowed the adoption of online and mobile financial technologies in past epidemic outbreaks.

2. DATA

Our analysis combines data from several sources. First, we use Findex to measure financial behavior in more than 140 countries and GWP for data on household characteristics, income, and financial situation. We merge Findex with GWP using individual identifiers, giving us household-level data on financial technology adoption and its correlates. We then use the epidemic data set of Ma et al. (2020) to determine whether a country experienced an epidemic in a given year. We complement these data with information on country-level time-varying indicators (such as the level of economic and financial development, as proxied by GDP per capita and bank deposits over GDP) taken from the World Bank Global Financial Development Database. Finally, using Collins Bartholomew's Mobile Coverage Explorer we add global 3G Internet access, which we observe at the 1 km-by-1 km level. We aggregate these data to the subnational locations identified for each respondent by GWP.

2.1 Findex

Findex is a nationally representative survey fielded in some 140 countries in 2011, 2014, and 2017 (Demirguc-Kunt and Klapper 2013a, b). This provides information on saving, borrowing, payments, and use of financial technology, including mobile phones and/or Internet usage to conduct financial transactions. These data are collected in partnership with Gallup through nationally representative surveys of more than 150,000 adults in each wave. We focus on individuals aged 18 and older to ensure that those in our sample are eligible for a bank account.

The outcome variables of interest come from questions asked of all Findex respondents regarding their use of fintech and other regular financial services:

(i) Online/mobile transactions using the Internet and bank account: In the PAST 12 MONTHS, have you made a transaction online using the Internet as well as with money FROM YOUR ACCOUNT at a bank or another type of formal financial institution using a MOBILE PHONE? This can include using a MOBILE PHONE to make payments, buy things, or to send or receive money.

(ii) Mobile transaction using bank account: In the PAST 12 MONTHS, have you ever made a transaction with money FROM YOUR ACCOUNT at a bank or another type of formal financial institution using a MOBILE PHONE? This can include using a MOBILE PHONE to make payments, buy things, or to send or receive money.

(iii) Online payments (such as bills) using the Internet: In the PAST 12 MONTHS, have you, personally, made payments on bills or bought things online using the Internet?

(iv) Withdrawals using ATM: When you need to get cash (paper or coins) from your account(s), do you usually get it at an ATM?

(v) Withdrawals using a bank branch: When you need to get cash (paper or coins) from your account(s), do you usually get it over the counter in a branch of your bank or financial institution?

Responses were coded on a two-point scale: "Yes" (1) and "No" (2). Note that the last two questions above (related to ATM and branch withdrawals) come from a single question with various alternatives; thus responses to these questions are mutually exclusive.

Linking Findex to GWP, we obtain information on respondents' demographic characteristics (age, gender, educational attainment, marital status, religion, and urban/rural residence), income, labor market status, and within-country income deciles.

We also examine responses to five parallel questions as placebo outcomes:

(i) Account ownership: an account can be used to save money, to make or receive payments, or to receive wages or financial help. Do you, either by yourself or together with someone else, currently have an account at a bank or another type of formal financial institution?
(ii) Deposit money into a personal account in a typical month: In a typical MONTH, is any money DEPOSITED into your personal account(s): this includes cash or electronic deposits, or any time money is put into your account(s) by yourself or others.
(iii) Withdraw money out of a personal account in a typical month: In a typical MONTH, is any money WITHDRAWN from your personal account(s): this includes cash withdrawals in person or using your (insert local terminology for ATM/debit card), electronic payments or purchases, checks, or any other time money is removed from your account(s) by yourself or another person or institution.
(iv) Debit card ownership: a/an (local terminology for ATM/debit card) is a card connected to an account at a financial institution that allows you to withdraw money, and the money is taken out of THAT ACCOUNT right away. Do you, personally, have a/an (local terminology for ATM/debit card)?
(v) Credit card ownership: a credit card is a card that allows you to BORROW money in order to make payments or buy things, and you can pay the balance off later. Do you, personally, have a credit card?

These last responses help us to determine whether what we are capturing is the impact of epidemic exposure on financial technology specifically, as distinct from its impact on financial services–related outcomes generally.

2.2 Ma et al. Epidemic Database

Data on worldwide large-scale epidemics are drawn from Ma et al., who construct a country-panel data set from the turn of the century. The authors focus on the five epidemic/pandemic waves originally identified by Jamison et al. (2017): SARS in 2003, H1N1 in 2009, MERS in 2012, Ebola in 2014, and Zika in 2016. They date epidemic

events in each country using announcement dates from the World Health Organisation. Almost all countries in the world were affected by postmillennial epidemics at one time or another according to their list.[4]

The Ma et al. data set does not contain country-specific intensity measures and therefore must be used in dichotomous form. This binary measure is consistent with the assumption of exogeneity of our treatment, since occurrence of an epidemic (as opposed to its intensity) is likely to be uncorrelated with country characteristics.[5] Nonetheless, we also analyze more and less severe epidemics separately by constructing dummy variables based on the above/below median infection cases (or deaths) per capita across all epidemics during our sample period for which we manually collect the information from Emergency Events (EM-DAT 2021) database of the Centre for Research on the Epidemiology of Disasters and supplementary sources (including Pan American Health Organization, PLISA Health Information Platform for the Americas, the Centers for Disease Control and Prevention (USA), European Centre for Disease Prevention and Control, and World Health Organization). We merge these data with the Findex-Gallup database.

Averaged across available years, H1N1 and Ebola were the top two diseases causing epidemic mortality worldwide as measured by absolute number of deaths. According to our calculations, the mortality rate was the highest for Ebola and MERS (about 40%). Many of these epidemics and pandemics affected multiple countries. In particular, 201 countries had at least one H1N1 case, followed by 50 countries for Zika, 29 countries for SARS, 26 countries for MERS, and 10 countries for Ebola.

2.3 Global 3G/2G Coverage

Collins Bartholomew's Mobile Coverage Explorer provides information on signal coverage at a 1-by-1 km grid level around the world. To calculate the share of the population covered by 3G, we use 1-by-1 km population data from the Gridded Population of the World for 2015, distributed by Center for International Earth Science Information Network.[6] We then calculate the share of a district's territory covered by 3G networks in a given year, weighted by population density at each point on the map. We first calculate each grid's population coverage and then aggregate this information over the subnational regions distinguished by GWP. We use this population-weighted 3G network coverage variable to capture 3G mobile Internet access at the subregional level. We adopt the same approach when calculating 2G network coverage, which enables mobile phone use but not Internet access.

4. In particular, we use 237 country–year pandemic/epidemic events since the turn of the century. See Online Appendix B for the detailed list.

5. In other words, countries may be hit randomly by an epidemic, as the result of exposure to an infected international traveler for instance, but how widely the infection spreads will depend on the strength of its health system, its economic resources, and other country characteristics.

6. The data are publicly available at: http://www.ciesin.org/

Online Appendix Table 1 reports descriptive statistics for the outcome and placebo variables, epidemic occurrence, and 3G Internet coverage.

3. EMPIRICAL STRATEGY

To assess the causal effect of past epidemic exposure on an individual's usage of digital and traditional financial services, we estimate a linear probability model with a difference-in-differences specification:

$$\mathrm{Y}_{ict} = \beta_0 + \beta_1 \mathrm{X}_{it} + \beta_2 \text{Exposure to epidemic}_{ct} + \beta_3 \mathrm{C}_c + \beta_4 \mathrm{Y}_t + + \varepsilon_{ict}, , \quad (1)$$

where Y_{ict} is a dummy variable indicating whether or not respondent i in country c in year t uses digital or traditional financial services. "Exposure to epidemic" is an indicator variable capturing whether a country experienced an epidemic in a year. The coefficient of interest is β_2. As noted, our identification assumption is that occurrence of an epidemic (as opposed to its intensity) is uncorrelated with country-level characteristics and hence that our treatment variable is plausibly exogenous.[7]

To control for the effects of demographic and labor market structure, we include the following in the X_i vector of individual characteristics: individual income (in level and squared), and indicator variables for living in an urban area, having a child (any child under 15), gender (male), employment status (full-time employed, part-time employed, unemployed), religion (atheist, orthodox, Protestant, Catholic, Muslim), educational attainment (tertiary education, secondary education), and within-country—year income decile.

To account for unobservable characteristics, we include fixed effects at the levels of country ($\mathrm{C_c}$) and year (Y_t). The country dummies control for all variation in the outcome variable due to factors that vary only cross-nationally. These also strengthen our identification argument, ensuring that we control for the selection of certain countries into epidemic episodes as long as the timing of the epidemic can be considered exogenous.[8] The year dummies control for global shocks that affect all countries simultaneously. We also include as country-level time-varying regressors GDP per capita and bank deposits relative to GDP; these variables capture economic and financial development across countries and over time.

7. In Appendix Tables 7 and 8, we show that the occurrence of epidemics is indeed uncorrelated with country characteristics.

8. For instance, an African country may generally be more likely to experience epidemics compared to a European country. In a fixed-effect setting, our identification strategy is likely to hold as long as one could think of the (within-country) timing of an epidemic as unpredictable (i.e., exogenous).

In further robustness checks we add interactive country-times-income quintile, country-times-labor-market status, and country-times-education fixed effects.[9] These interaction terms allow us to compare the treatment and control groups within those specific categorical bins. We cluster standard errors by country, and use sampling weights provided by Findex-Gallup to make the data representative at the country level.

4. MAIN RESULTS

The five rows of Table 1 show results for five outcome variables: whether an individual (i) engages in online transactions using both the Internet and his or her bank account, including by mobile phone; (ii) engages in mobile transactions using a bank account; (iii) makes online payments using the Internet; (iv) makes withdrawals using an ATM; and (v) makes withdrawals over the counter at a bank branch. The five columns, moving left to right, report regressions with increasingly comprehensive sets of controls.[10]

Exposure to an epidemic in the current year significantly increases the likelihood that a respondent will have engaged in online transactions. This result obtains for multiple remote-access banking transactions. In particular, epidemic exposure in the current year increases the likelihood that an individual will have made a withdrawal using an ATM while reducing the likelihood of doing so at a bank branch (in person over the counter). These last two coefficients are opposite in sign and roughly equal in magnitude, suggesting that there is near-perfect substitution between ATM-based transactions and those undertaken in person at bank branches.[11] In our preferred model (column 5), exposure to an epidemic leads to 10.6 (4.5) percentage point increase in online/mobile transactions using the Internet and bank accounts (mobile transactions using bank accounts). Given that the means of these outcome variables are 8.3% (9.4%), the effect is sizable. It represents between a doubling and tripling of the initial propensity (in the cases of Internet and mobile transactions, respectively). This compares with the results in Fu and Mishra (2022), who using very different data and a very different approach estimate a 21% to 26% increase in daily downloads of finance-related mobile applications between January and early December 2020.

9. Our results (available upon request) are also qualitatively similar when we use an alternative difference-in-differences method that is robust to treatment heterogeneity (as suggested in De Chaisemartin and D'Hautefeuille 2020).

10. Sample size varies across specifications because we drop singleton observations that are perfectly collinear with our fixed effects.

11. As previously noted, these two questions on cash withdrawals (ATM versus bank branch) are originally asked in a mutually exclusive manner (alongside a few other options) in the Findex questionnaire. This is in line with our interpretation of the related results as a "substitution" from one technology to another.

TABLE 1

THE IMPACT OF AN EPIDEMIC YEAR ON FINANCIAL TECHNOLOGY ADOPTION

	(1)	(2)	(3)	(4)	(5)
Outcome ➔ Online/mobile transaction using the Internet and bank account					
Exposure to Epidemic	0.085***	0.084***	0.085***	0.109***	0.106***
	(0.018) [0.001]	(0.019) [0.001]	(0.019) [0.001]	(0.030) [0.002]	(0.030) [0.002]
Oster's δ for omitted variable bias	–	–	–	–	21.74
Observations	157,093	157,093	157,093	157,093	157,093
Outcome ➔ Mobile transaction using bank account					
Exposure to Epidemic	0.049**	0.047**	0.038**	0.044**	0.045***
	(0.019) [0.007]	(0.020) [0.009]	(0.016) [0.009]	(0.017) [0.007]	(0.015) [0.004]
Oster's δ for omitted variable bias	–	–	–	–	41.56
Observations	230,327	230,327	230,327	230,327	230,326
Outcome ➔ Online payments (such as bills) using the Internet					
Exposure to epidemic	0.033*	0.035	0.036*	0.055*	0.049
	(0.020) [0.025]	(0.021) [0.025]	(0.020) [0.025]	(0.032) [0.025]	(0.030) [0.025]
Oster's δ for omitted variable bias	–	–	–	–	13.57
Observations	164,465	164,465	164,465	164,465	164,465
Outcome ➔ Withdrawals using ATM					
Exposure to Epidemic	0.201***	0.193***	0.189***	0.178***	0.200***
	(0.038) [0.001]	(0.046) [0.001]	(0.061) [0.004]	(0.056) [0.003]	(0.046) [0.001]
Oster's δ for omitted variable bias	–	–	–	–	43.38
Observations	83,322	83,321	83,321	83,321	83,309
Outcome ➔ Withdrawals using a bank branch					
Exposure to Epidemic	−0.228***	−0.220***	−0.217***	−0.209***	−0.238***
	(0.056) [0.001]	(0.064) [0.002]	(0.074) [0.004]	(0.071) [0.004]	(0.059) [0.001]
Oster's δ for omitted variable bias	–	–	–	–	101.75
Observations	83,322	83,321	83,321	83,321	83,309
Country fixed effects	Yes	Yes	Yes	Yes	No
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Demographic characteristics	No	Yes	Yes	Yes	Yes
Education fixed effects	No	No	Yes	Yes	No
Labor market controls	No	No	Yes	Yes	No
Income decile fixed effects	No	No	Yes	Yes	No
Country-level controls	No	No	No	Yes	Yes
Country × Education fixed effects	No	No	No	No	Yes
Country × Labour mar. fixed effects	No	No	No	No	Yes
Country × Income decile fixed effects	No	No	No	No	Yes

SOURCE: Gallup-Findex, (2011, 2014, 2017) and Ma et al. (2020) Epidemics Database.

NOTES: Results use the Findex-Gallup sampling weights and robust standard errors are clustered at the country level and reported in parentheses. We check whether our inference is robust to corrections that account for testing of multiple hypotheses by adjusting the p-values using the "sharpened q-value approach" and report them in brackets (in terms of interpretation, for example, a *q*-value of 1% means that 1% of significant results will result in false positives). Oster's delta indicates the degree of selection on unobservables relative to observables that would be needed to fully explain the results by omitted variable bias. Delta values greater than 1 indicate that the results are not driven by unobservables. *Significant at 10%; **significant at 5%; ***significant at 1%.

These results are robust to including individual-level income (linear and nonlinear), demographic characteristics, labor market controls, education fixed effects, (within-country) income decile fixed effects, and year fixed effects. They are robust to including time-varying country-level controls (GDP per capita and bank deposits over GDP) and country fixed effects or, alternatively, country by education, country by labor market status, and country by income decile status fixed effects, saturating our specification so as to restrict the dependent variable to vary only within these bins.

We follow the method proposed by Oster to investigate the importance of unobservables.[12] For each panel of Table 1, the final column reports Oster's delta for our main model. This indicates the degree of selection on economic unobservables, relative to observables, needed for our results to be fully explained by omitted variable bias. The high delta values (between 10 and 52 depending on the outcome) are reassuring: given the economic controls in our models, it seems unlikely that unobserved factors are 10 to 52 times more important than the observables included in our preferred specification.

Because we analyze multiple outcomes that could generate false positives purely by chance, we follow Anderson (2008) in computing false discovery rates (FDRs), which calculate the expected proportion of rejections that are type I errors and generate an adjusted p-value (i.e., sharpened q-value) for each corresponding estimate. As seen beneath each estimate (in brackets) in Table 1, findings do not change when we employ this method; in fact the statistical significance of the estimates based on these adjusted p-values is usually higher than those indicated by standard p-values.

We also considered placebo tests—tests for changes in financial behaviors other than the choice between in-person and remote-access transactions. The additional dependent variables here are whether the individual (i) owns an account, (ii) deposited money into a personal account in a typical month (either in person or online), (iii) withdrew money from a personal account in a typical month (either in person or online), (iv) owned a debit card, and (v) owned a credit card. The results, in Table 2, are reassuring. They show insignificant effects, small coefficients and no uniform pattern of signs. An interpretation is that epidemic exposure affects the form—remote access or in person—of financial activity but not its level, and that it has no obvious impact on financial inclusion.[13]

12. Estimation bounds on the treatment effect range between the coefficient from the main specification and the coefficient estimated under the null assumption that unobservables are as important as observables for the level of *Rmax*. *Rmax* specifies the maximum *R*-squared that can be achieved if all unobservables were included in the regression. Oster (2019) uses a sample of 65 RCT papers to estimate an upper bound of the *R*-squared such that 90% of the results would be robust to omitted variables bias. This estimation strategy yields an upper bound for the *R*-squared, *Rmax*, that is 1.3 times the *R*-squared in specifications that control for observables. The rule of thumb to be able to argue that unobservables cannot fully explain the treatment effect is for Oster's delta to be greater than one.

13. Even though we cannot rule out a positive impact of 2%–3% on account and debit card ownership due to large estimated confidence intervals, coefficient sizes are sufficiently small to reject an economically meaningful increase. According to Online Appendix Table 1, such increase would correspond to around 5% of the sample mean of these two outcome variables whereas the estimated effect on online/mobile transactions corresponds to more than 100% of the sample mean. Relatedly, we also examined whether

TABLE 2

THE IMPACT OF AN EPIDEMIC YEAR ON FINANCIAL TECHNOLOGY ADOPTION—PLACEBO OUTCOMES

	(1)	(2)	(3)	(4)	(5)
Outcome ➔ Account ownership					
Exposure to Epidemic	0.037	0.032	0.026	0.022	0.029
	(0.031) [1.000]	(0.034) [1.000]	(0.035) [1.000]	(0.034) [1.000]	(0.033) [1.000]
Observations	254,832	254,832	254,832	254,832	254,832
Outcome ➔ Deposit money into a personal account in a typical month					
Exposure to Epidemic	−0.013	−0.012	−0.012	−0.010	-0.007
	(0.021) [1.000]	(0.021) [1.000]	(0.021) [1.000]	(0.021) [1.000]	(0.021) [1.000]
Observations	94,340	94,338	94,338	94,338	94,316
Outcome ➔ Withdraw money out of a personal account in a typical month					
Exposure to Epidemic	−0.002	−0.001	0.000	0.000	0.003
	(0.008) [1.000]	(0.008) [1.000]	(0.007) [1.000]	(0.009) [1.000]	(0.010) [1.000]
Observations	94,128	94,126	94,126	94,126	94,107
Outcome ➔ Debit card ownership					
Exposure to Epidemic	0.032	0.028	0.023	0.025	0.026
	(0.035) [1.000]	(0.038) [1.000]	(0.037) [1.000]	(0.037) [1.000]	(0.033) [1.000]
Observations	253,284	253,284	253,284	253,284	253,284
Outcome ➔ Credit card ownership					
Exposure to Epidemic	0.001	−0.001	−0.002	−0.003	−0.006
	(0.014) [1.000]	(0.016) [1.000]	(0.014) [1.000]	(0.013) [1.000]	(0.014) [1.000]
Observations	252,624	252,624	252,624	252,624	252,624
Country fixed effects	Yes	Yes	Yes	Yes	No
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Demographic characteristics	No	Yes	Yes	Yes	Yes
Education fixed effects	No	No	Yes	Yes	No
Labor market controls	No	No	Yes	Yes	No
Income decile fixed effects	No	No	Yes	Yes	No
Country-level controls	No	No	No	Yes	Yes
Country × Education fixed effects	No	No	No	No	Yes
Country × Labor mar. fixed effects	No	No	No	No	Yes
Country × Income decile fixed effects	No	No	No	No	Yes

SOURCE: Gallup-Findex, (2011, 2014, 2017) and Ma et al. (2020) Epidemics Database.

NOTES: Results use the Findex-Gallup sampling weights and robust standard errors are clustered at the country level and reported in parentheses. We check whether our inference is robust to corrections that account for testing of multiple hypotheses by adjusting the *p*-values using the "sharpened q-value approach" and report them in brackets (in terms of interpretation, for example, a q-value of 1% means that 1% of significant results will result in false positives). *Significant at 10%; **significant at 5%; ***significant at 1%.

4.1 Heterogeneity

To identify heterogeneous treatment effects across individuals, we use a Causal Forest methodology (Athey and Imbens 2016). We build regression trees that split the control variable space into increasingly smaller subsets. Regression trees aim to predict an outcome variable by building on the mean outcome of observations with similar characteristics. When a variable has little predictive power, it is assigned a negative importance score, which is equivalent to low importance for treatment heterogeneity. Causal Forest estimation combines such regression trees to identify treatment effects, where each tree is defined by different orders and subsets of covariates. Figure 1(A) presents the result based on 20,000 regression trees, where we set the threshold as 0.15 and above.

Household income, employment, and age turn out to be the important dimensions of treatment heterogeneity. We therefore re-estimate our main specification (column 5 in Table 1) restricting the sample to each categorical domain. Results are in Figures 1(B), 1(C), and 1(D). The average treatment effect is driven by individuals with annual incomes above US$10,000, young adults (age 26—34), and those in full-time employment at the time of the epidemic. It makes sense that better off, more economically secure, and younger individuals should be more inclined to switch to new financial technologies. Technology adoption in general declines with age (Frieberg 2003, Scheife 2006), while less well-off individuals often have less exposure or access to such technology.

4.2 Event Study Estimates and Persistence

Because Findex is only available for three cross-sections spanning 7 years, any investigation of persistence is tentative. As a start, we employ the specification in equation (1) but redefine the treatment variable to indicate individuals in countries exposed to an epidemic in the year immediately preceding the survey, and in a separate estimation as indicating individuals exposed to an epidemic 2 years prior to the survey.[14] To investigate preexisting trends, we define similar variables for changes in behavior in years prior to the exposure.

Figure 2 reports the coefficients for these treatment variables generated via separate regressions on the same sample of individuals.[15] Panel A shows that differences

epidemic exposure had a negative impact on respondents' confidence in banks. There is some sign of a negative response in this analysis (available upon request), although this effect is imprecisely estimated.

14. We want to avoid overinterpreting this result, since past epidemics may not necessarily represent the same events as the ones captured by our contemporaneous treatment dummy. Therefore, failing to find an effect in this setting does not automatically translate to a short-term impact for the epidemic episodes that we capture with our contemporaneous epidemic variable. To the extent that treatment effects might be heterogeneous across different types of epidemics in our sample, this type of analysis should be interpreted with caution.

15. Some coefficients in Figure 2 cannot be estimated due to lack of variation in the corresponding treatment variable and are thus denoted as zeros.

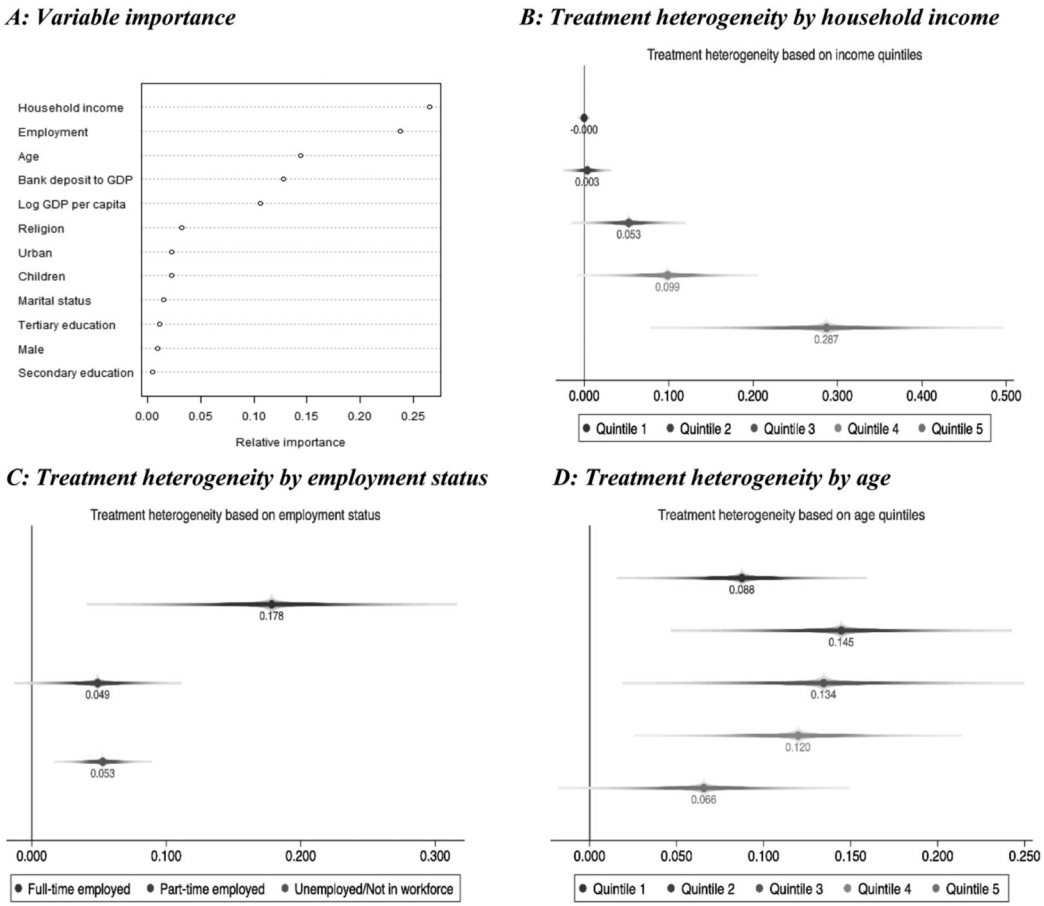

Fig 1. Heterogeneity Analysis Using Causal Forest.

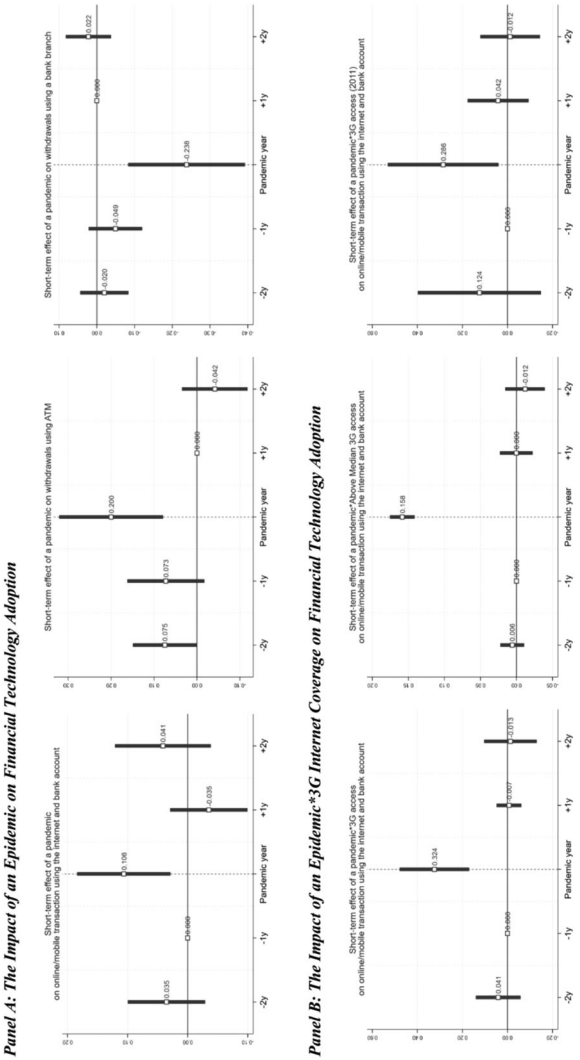

Fig 2. Event Study Estimates.

between countries exposed to an epidemic in the past (or struck by one in the future) and those who were not so affected are small and statistically insignificant. These event-study graphs are consistent with the idea that the epidemic shock was exogenous with respect to banking activity (i.e., that our estimates satisfy the parallel trends assumption).[16]

It does not appear from this analysis that the change in behavior persists beyond the epidemic year. This is consistent with a model of low switching costs in which individuals are continually optimizing which technology to use (e.g., cash or digital payment), such that they switch to digital when arrival of an epidemic increases the riskiness of face-to-face exposure associated with cash payments, but they switch back to cash once spread of the epidemic has been suppressed. One can of course imagine a different model in which individuals must incur a significant fixed cost when adopting digital payments. In this case, having sunk that cost in response to epidemic exposure, they will continue using digital payments after the epidemic has been suppressed. Our preliminary analysis of persistence is more consistent with the first model than the second.[17]

These results can be interpreted in terms of a model of high fixed costs of learning about electronic banking and low variable costs, once those fixed costs have been sunk, of switching between in-person and electronic modalities. Intuitively, an individual already familiar with banking both via a teller and using a smartphone, having earlier sunk the costs of learning about the latter, can easily switch to banking entirely with his/her smartphone in response to an epidemic outbreak, but equally well shift back to doing some or all of his/her transactions with a teller, as is convenient, once the outbreak is over. In contrast, an individual who does all his transactions with a teller at a bank branch and possesses no smartphone (or no familiarity with the relevant banking app) may choose to invest in the latter and shift to banking electronically in response to the shock of a major epidemic outbreak and then, having sunk those costs, continue to bank electronically to a greater extent than before once the epidemic event is over. The lack of persistent effects in our data thus suggests that many individuals in our sample had already familiarized themselves with ATMs and online and/or cellphone-enabled banking in the 2011—17 period covered by our data. That switching from in-person to remote-access banking occurs disproportionately among relatively young (as well as affluent and fully employed) individuals who are presumably already familiar with both modalities is further consistent with this observation.

16. This evidence supports our approach of considering a country as treated only during the year of treatment, as opposed to also earlier and later years.

17. From a supply versus demand perspective, these results are consistent with a demand-driven story, where consumers switch their demand for payments services from cash to Fintech and then back to cash. Were patterns driven by supply-side factors (that prior to the epidemic banks did not make digital payments services available to their customers, but that they increase their supply in response to epidemic-related risks), then we would be less likely to see consumers switch back subsequently, this supply-side constraint having been relaxed.

4.3 Role of Infrastructure

Infrastructure weaknesses may hinder digital transactions and limit epidemic-induced shifts in behavior (as suggested by studies cited in Section 1). We therefore add to our specification a measure of within-country subregional 3G coverage. 3G is indeed the relevant technological threshold since 2G allows only for mobile phone calls and text messages but not Internet browsing.[18]

Our 3G variable captures the population-weighted portion of 1×1 km squares with a 3G connection in each subregion distinguished by Gallup. We interact it with our measure of epidemic exposure and also include it separately to control for any first-order effect of mobile Internet coverage. Online Appendix Figure 1 provides a visual summary of 3G mobile Internet expansion around the world between 2011 and 2017. There is substantial variation within and between countries in 3G coverage and how it changes over time.

We initially treat 3G availability as exogenous, since the technology was licensed and deployed to facilitate calls, texts, and Internet browsing and not because of online banking availability. Nonetheless, to address the concern that causality may run from banking provision to 3G coverage, we include additional dummies for each country–year pair. Since banks usually provide very similar online banking services throughout a country, this nonparametrically controls for supply-related factors.[19] It focuses instead on within-country–year variation in online banking that is more likely to be driven by demand shocks. This ensures that our estimates are also not driven by other country-specific time-varying unobservables.

A further concern is that epidemics may lead to changes in 3G coverage, for example, via signal failures if the maintenance of local services is adversely affected by the public health emergency.[20] We follow two strategies to limit the danger that subregional 3G coverage is affected by epidemics. First, we minimize the variation in 3G coverage by specifying it in binary form, where above-median values take the value of 1 and 0 otherwise. So long as a region does not experience a very large change in coverage in response to an epidemic—so long as it does not jump from one category to another—this will minimize endogeneity. Second, we eliminate time variation in the 3G variable by only using the initial (2011) values for each subregion.[21]

Table 3 shows the result for online transactions using the Internet and the individual's bank account, including by mobile phone. 3G coverage itself has little effect: its coefficient is small, and statistically significant only when we exclude individual

18. In Online Appendix Table 9, we confirm that 2G Internet access has no impact on our outcomes when it is interacted with epidemic exposure.

19. This relieves us of the need to control for other supply-side factors such as for example the prevalence of ATMs.

20. This would result in multicollinearity in our estimates.

21. We also tested for the possibility that epidemic exposure would lead to a change in the availability of ATMs. Online Appendix Table 12 show that there is no evidence of such an effect.

TABLE 3

THE IMPACT OF AN EPIDEMIC YEAR ON FINANCIAL TECHNOLOGY ADOPTION AND ACCESS—THE ROLE 3G INTERNET ACCESS

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome ➔ Online/mobile transaction using the Internet and bank account						
Exposure to epidemic × 3G	0.286***	0.296***	0.290***	0.311***	0.330***	0.324***
	(0.053) [0.001]	(0.058) [0.001]	(0.061) [0.001]	(0.048) [0.001]	(0.049) [0.001]	(0.059) [0.001]
3G	0.044**	0.029	0.018	0.019	0.019	0.006
	(0.022)	(0.023)	(0.023)	(0.022)	(0.022)	(0.013)
Exposure to Epidemic	0.092***	0.090***	0.091***	0.147***	0.145***	–
	(0.021)	(0.021)	(0.022)	(0.048)	(0.046)	
Oster's δ for omitted variable bias	–	–	–	–	–	116.30
Observations	127,184	127,184	127,184	127,184	127,184	127,184
Exp. to epidemic × Above median 3G	0.287***	0.227***	0.240***	0.238***	0.165***	0.158***
	(0.012) [0.001]	(0.013) [0.001]	(0.013) [0.001]	(0.013) [0.001]	(0.011) [0.001]	(0.007) [0.001]
Above median 3G	0.002	−0.003	−0.007	−0.005	-0.006	-0.001
	(0.012)	(0.012)	(0.012)	(0.011)	(0.011)	(0.004)
Exposure to epidemic	0.097***	0.094***	0.094***	0.150***	0.149***	–
	(0.021)	(0.021)	(0.021)	(0.050)	(0.048)	
Oster's δ for omitted variable bias	–	–	–	–	–	7.29
Observations	127,184	127,184	127,184	127,184	127,184	127,184
Exposure to epidemic × 3G (2011)	0.243***	0.269***	0.264***	0.264***	0.284***	0.286***
	(0.088) [0.003]	(0.080) [0.001]	(0.090) [0.002]	(0.090) [0.002]	(0.093) [0.002]	(0.093) [0.002]
3G (2011)	0.072***	0.045***	0.024**	0.023**	0.017	0.015
	(0.014)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
Exposure to epidemic	0.086***	0.084***	0.085***	0.156***	0.154***	–
	(0.023)	(0.024)	(0.024)	(0.055)	(0.054)	

(*Continued*)

TABLE 3

(CONTINUED)

	(1)	(2)	(3)	(4)	(5)	(6)
Oster's δ for omitted variable bias	–	–	–	–	–	23.61
Observations	95,745	95,745	95,745	95,745	95,745	95,745
Country fixed effects	Yes	Yes	Yes	Yes	No	No
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Demographic characteristics	No	Yes	Yes	Yes	Yes	Yes
Education fixed effects	No	No	Yes	Yes	No	No
Labor and income decile controls	No	No	Yes	Yes	No	No
Country-level controls	No	No	No	Yes	Yes	Yes
Country × Education fixed effects	No	No	No	No	Yes	Yes
Country× Labor mar. fixed effects	No	No	No	No	Yes	Yes
Country × Income decile fixed effects	No	No	No	No	Yes	Yes
Country × Year fixed effects	No	No	No	No	No	Yes

SOURCE: Gallup-Findex, (2011, 2014, 2017), Ma et al. (2020) Epidemics Database and Collins Bartholomew's Mobile Coverage Explorer.
NOTES: Results use the Findex-Gallup sampling weights and robust standard errors are clustered at the country level. We check whether our inference is robust to corrections that account for testing of multiple hypotheses by adjusting the p-values using the "sharpened q-value approach" and report them in brackets (in terms of interpretation, for example, a q-value of 1% means that 1% of significant results will result in false positives). Oster's delta indicates the degree of selection on unobservables relative to observables that would be needed to fully explain the results by omitted variable bias. Delta values greater than 1 indicate that the results are not driven by unobservables. *Significant at 10%; ** significant at 5%; ***significant at 1%.

controls. But when interacted with epidemic exposure, its effect is large and significant at conventional confidence levels. Again, these results survive the Oster test for potential omitted variable bias and when we adjust *p*-values for multiple models. According to the most conservative regression, including both the baseline and interacted coefficients (column 5, middle panel), the impact of epidemic exposure on the propensity to transact using the Internet is more than twice as large with 3G coverage. Panel B in Figure 2 shows that there is no evidence of the additional effect of 3G infrastructure persisting beyond the period of epidemic exposure, or of the effect emerging prior to the epidemic shock.[22]

5. ADDITIONAL ANALYSIS AND ROBUSTNESS CHECKS

5.1 Are More Intense Epidemics Different?

We can re-estimate our model with separate binary treatment indicators for high- and low-intensity epidemics. We calculate the number of people (as a share of the population) infected in each epidemic event by manually collecting the relevant data from EM-DAT database and supplementary sources and use the median value as our threshold. Online Appendix Table 2 shows that treatment effects tend to be larger for high-intensity epidemics, in line with the idea that individuals are more likely to switch to remote banking in response to more serious epidemic-induced health risks.[23]

5.2 Are Successive (Repeat) Epidemics Different?

Some countries experienced a succession of different epidemics in the sample period, raising the possibility of heterogeneity due to repeat instances of treatment. We therefore kept only the first epidemic event in our treatment and turned off the incidence variable for later events in the same country. In Online Appendix Table 3 we do this by taking into account the full sample spanning 2000–19, while in Online Appendix Table 4 we repeat the exercise for the period starting in 2011 (the first year covered by Findex). The relevant coefficient estimates, while somewhat smaller than before, are still significant and of the same signs.

5.3 Robustness to Alternative Levels of Clustering

We can also establish the robustness of our results under alternative assumptions about the variance–covariance matrix. In our main specification, we cluster the standard errors at the country level. Results are robust to instead clustering at global

22. Again, this means that our setting satisfies the parallel trends assumption.

23. The results are qualitatively the same when we use epidemic-induced death numbers instead of infection cases as a threshold to decide on the low-/high-intensity epidemics.

region–year level (12 units x 3 years; assuming that residuals comove within these units) and clustering only at global region level (12 units) as reported in columns 1 and 2 of Online Appendix Table 5).

5.4 Country-Specific Time Trends

Controlling for country-specific linear time trends allows us to remove distinctive trends in fintech adoption in individual countries that might otherwise bias our estimates if they accidentally coincided with other epidemic-related changes. The results remain robust (see column 3 of Online Appendix Table 5).

5.5 Falsification

We conduct two falsification exercises by creating placebo treatment variables. In the first, we keep the same epidemic year for a given epidemic event but randomly choose a different country *from the same continent* as the country where the epidemic actually took place. For instance, the Ebola pandemic in 2014 had a particularly devastating impact on African countries such as Senegal, Sierra Leone, and Liberia, raising the possibility that something else distinctive to Africa may be driving our estimates. But when we randomly assign the epidemic events to other unaffected countries (instead of the affected country) in the same continent while still keeping the same epidemic year, our estimates (column 1 of Online Appendix Table 6) are small and statistically indistinguishable from zero.

Alternatively, we randomize both the epidemic country and the year for each epidemic event. Again, the results (column 2 of Online Appendix Table 6) confirm that the potential geographical clustering of epidemic events in the same continent does not drive our results. Financial technology adoption occurs only in countries actually affected by the epidemic event, but not in countries with similar geographies that were not stricken by an epidemic.

5.6 Balance Test

Our identification assumption is that the occurrence/start of an epidemic is uncorrelated with country characteristics and hence that our treatment variable is plausibly exogenous. We provide direct evidence on this in Online Appendix Tables 7 and 8. In particular, we estimate the following country–year level specification in Online Appendix Table 5:

$$\text{Exposure to Epidemic}_{ct} = \alpha + \beta_1 \mathrm{X}_{ct} + \beta_2 \mathrm{C}_c + \beta_3 \mathrm{T}_t + \varepsilon_c. \tag{2}$$

"Exposure to epidemic" is an indicator variable capturing whether a country experienced an epidemic in a year (i.e., our treatment variable in equation (1)). X_{ct} refers to country-level covariates, which include GDP per capita (in constant 2010 U.S. dollars), urban population as a share of total population, and other variables (such as ATMs per 100,000 adults and bank net interest margin) that measure a country's

level of the financial development. We include country and year fixed effects throughout and further saturate the models with continent by year fixed effects and country income group (low, lower middle, upper middle, and high-income countries) fixed effects. We estimate standard errors robust to heteroscedasticity.

Columns 1 and 2 of Online Appendix Table 7 present results from a country–year level analysis between 2000 and 2017 and Columns 3 and 4 present results from a country–year level analysis for 2011, 2014, and 2017 (i.e., Findex survey years). Reassuringly, none of the country-level covariates that we include in the analysis are correlated with epidemic occurrence.

Columns 1 and 2 of Online Appendix Table 8 further show that occurrence of an epidemic is also not correlated with *changes* in country-level characteristics (i.e., all the covariates are based on changes between 2000 and 2017). Finally, Columns 3 and 4 show that country characteristics *at baseline* are not correlated with the occurrence of an epidemic (i.e., all explanatory variables are measured in 2011).

The results presented in Online Appendix Tables 7 and 8 are consistent with the assumption that the occurrence of epidemics is plausibly exogenous to country-level characteristics.

5.7 2G Coverage as a Placebo Treatment

There may be concern that the 3G variable is endogenous and captures other subregional characteristics (economic wealth, economic growth, etc.) and not just Internet infrastructure. This would lead us to incorrectly attribute the effects reported in **Table 3** to 3G rather than the unobserved characteristic. However, similar concerns could be raised for an alternative variable capturing previous-generation mobile networks (i.e., 2G) that allow for mobile communication but not Internet use. But if such technology does not generate similar responses, it is more likely that our 3G variable captures the local Internet infrastructure rather than another unobserved characteristic.

We follow the structure of Table 3 but now also include 2G coverage as a placebo treatment. Online Appendix Table 9 illustrates that, in contrast to the effect of 3G, 2G has no consistent impact on our outcomes when it is interacted with epidemic exposure. These results suggest that 3G infrastructure and the mobile Internet it enables is the infrastructure relevant in this context and that it is unlikely to be picking up the effects of an omitted variable.[24]

24. In a related robustness check, we also computed an initial penetration measure for each subregion in each country using the first year in which the subregion received a 3G signal. Effectively, this separated the subregions into two categories: early- versus late adopters. Only in the early-adopter subregions were the effects sizable and significant even for older age groups. This is in line with the argument that the heterogeneity across different age groups in terms of technology adoption may also depend on the date of penetration for the new technology. In places where a technology is relatively older, one would expect to see older age cohorts to be more engaged with it either because they must have been younger when the technology first came about or technology must have saturated as the time passed and spread to different parts of the society.

5.8 Ruling out Influential Treatments and Observations

We rule out the importance of influential treatments by excluding one treatment country at a time. This means we turn off the treatment for a specific country where it is assumed not to have been exposed to an epidemic at all. Online Appendix Table 10 shows that our coefficient estimates are stable when one country after another is iteratively eliminated from our main treatment.

We repeat a similar analysis with Online Appendix Table 11 but drop one country at a time in each estimation for 10 consecutive trials. Again we find that the estimates are not driven by a single country.[25]

6. CONCLUSION

We have documented the tendency to turn to online and mobile banking when individuals are exposed to an epidemic. The effects do not seem to reflect a change in the volume of financial transactions, only their form. Intuitively, one should see the substitution of electronic for person-to-person transactions in an environment where personal contact becomes riskier. It is less obvious that one should observe an increase (or reduction) in the overall volume of such transactions (something that we do not observe here). The effect is greatest among young, economically well-off individuals who reside in areas with good Internet infrastructure and coverage, not surprisingly since such individuals tend to be early adopters with favorable access to new digital technologies.

An obvious question related to external validity is whether our results carry over to the COVID-19 pandemic. Because comparable data are not available for the COVID period, any answer is necessarily conjectural. But there are reasons to conjecture that our results carry over. COVID-19 was a large, global pandemic; we find if anything an even larger shift toward online and mobile banking in response to large epidemics. Unlike a number of past epidemics, COVID-19 affected high- as well as low-income economies. But our estimates control for per capita income in the countries surveyed, suggesting that differences in the per capita incomes of the countries affected by this and earlier epidemics do not weaken the applicability of our results. Finally, a variety of other studies have reported evidence of COVID-19 inducing or accelerating shifts to online and mobile banking, in Switzerland (Kiefer, Spiller, and Brandes 2021), in the United States (Haar 2021) and globally (Martin 2020). These analyses do not control for personal and country characteristics that may contribute to this shift, as we do here, but they are consistent with our conclusions.

Our finding that the shift toward digital financial technology in response to past epidemics was temporary rather than enduring sits uneasily with other work on digital

25. In addition, we dropped all countries that never experienced an epidemic in the sample period (25 of 184 countries in all), and did so a second time for the 2011–19 subperiod covered by Findex (125 of 184 countries). Our results (available upon request) continue to hold, despite the reduced sample size.

technology adoption (e.g., Higgins 2021) finding persistent effects. It may simply be that fintech is different from other digital technologies in this respect. Or it could be that past epidemics being of relatively short duration, users did not see the need to permanently alter their practices. Here COVID-19 might be an exception. As noted above, the absence of comprehensive data on epidemic duration prevents us from exploring this systematically. We would note, however, that at the time of writing the duration of the COVID-19 pandemic was not significantly longer than those of Zika (20 months) or H1N1 (15 months).

Our results similarly have implications for corporate and government responses to COVID-19. Banks are likely to focus on investments in online and mobile platforms as opposed to opening and maintaining branch offices. They will employ fewer tellers facilitating cash transactions and more agents with specialized training provide customers with guidance on specialized transactions. Regulation and public policy will have to adapt to a world where individuals do more financial transactions digitally and fewer using cash. The push to issue central bank digital currencies, so as to allow central banks to retain control and oversight of the payments system, can be seen as a response to these ongoing trends.

A final point relevant to policy flows from the observation that the COVID-19 pandemic has been felt unevenly: the poorer portion of populations has disproportionately suffered its economic and health effects, and women have been disproportionately affected economically in many countries. 3G coverage is another instance of the same phenomenon: coverage tends to arrive late in poor, rural, and remote areas and in relatively poor neighborhoods in advanced countries, offering their residents less scope for substituting digital for in-person banking. Digital technology enables individuals to maintain customary levels of banking and financial activity while limiting epidemic risks to their health, but only if the necessary infrastructure is rolled out in a manner that encompasses poorer, more remote regions.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of the article.

Online Appendix Figure 1: 3G Mobile Internet Expansion Around the World

Online Appendix Table 2: The Impact of an Epidemic Year on Financial Technology Adoption by Epidemic Intensity

Online Appendix Table 3: The Impact of an Epidemic Year on Financial Technology Adoption – Treatment includes only the first epidemic event for each country during the period 2000-2019

Online Appendix Table 4: The Impact of an Epidemic Year on Financial Technology Adoption –Treatment includes only the first epidemic event for each country during the period 2011-2019

Online Appendix Table 5: The Impact of an Epidemic Year on Financial Technology Adoption – Alternative clustering and time trends

Online Appendix Table 6: The Impact of an Epidemic Year on Financial Technology Adoption – Placebo Treatments

Online Appendix Table 7: Balance Test – Country-level characteristics

Online Appendix Table 8: Balance Test – Country-level characteristics

Online Appendix Table 9: The Impact of an Epidemic Year on Financial Technology Adoption and Access – 2G Coverage as a Placebo Treatment

Online Appendix Table 10: Robustness to Excluding Influential Treatments

Online Appendix Table 11: Robustness to Dropping One Treated Country at a Time

Online Appendix Table 12: Placebo Test – Country-level characteristics

Online Appendix B