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Epidemic Exposure, Financial Technology, and the Digital Divide

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Abstract

We ask whether epidemic exposure leads to a shift in financial technology usage and who participates in this shift. We exploit a dataset 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

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Epidemic Exposure, Financial Technology, and the Digital Divide

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March 2022

Abstract

We ask whether epidemic exposure leads to a shift in financial technology usage and who participates in this shift. We exploit a dataset 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.

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1. Introduction

Epidemics are frequently cited as inducing changes in economic behavior and accelerating technological and behavioral trends. The Black 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.[2] 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 dataset to detailed background information about the same individual in Gallup World Polls 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 percent, 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.

[2] 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.

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, ATM s 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.[3]

Sensitivity analyses support these findings. The results continue to hold when we adjust for multiple outcomes (Anderson, 2008). A test following Oster (2019) 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.[4] We match 1km-by-1km time-varying data on global 3G internet coverage from Collins Bartholomew's

[3] See **Appendix Table 12**.

[4] 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 sub-national regions of a country are connected to the internet in general.

Mobile Coverage Explorer to the sub-national 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 vs 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 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 2 reviews the related literature. Sections 3 and 4 then describe our data and empirical strategy. Section 5 presents the main results, including for within-sample heterogeneity, persistence of the effects and the role of 3G infrastructure. Section 6 summarizes our additional robustness checks, after which Section 7 concludes. The appendix (available online) presents further detail on our data and additional empirical results.

2. 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. (2018) 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 smooths 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 sub-literature 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.

3. Data

Our analysis combines data from several sources. First, we use Findex to measure financial behavior in more than 140 countries and Gallup World Polls (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 dataset 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 1km-by-1km level. We aggregate these data to the sub-national locations identified for each respondent by GWP.

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 2-point scale: “Yes” (1) to “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 Gallup World Polls, 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:

(vi) 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?

(vii) 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.

(viii) 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.

(ix) 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)?

(x) 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.

Ma et al. Epidemic Database: Data on worldwide large-scale epidemics are drawn from Ma et al., who construct a country-panel dataset 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 post-millennial epidemics at one time or another according to their list.[5]

The Ma et al. dataset 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.[6] Nonetheless, we also analyze more and less severe

[5] In particular, we use 237 country-year pandemic/epidemic events since the turn of the century. See **Online Appendix B** for the detailed list.

[6] 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.

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 per cent). 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.

Global 3G/2G Coverage: Collins Bartholomew's Mobile Coverage Explorer provides information on signal coverage at a 1-by-1 kilometer grid-level around the world. To calculate the share of the population covered by 3G, we use 1-by-1 kilometer population data from the Gridded Population of the World for 2015, distributed by Center for International Earth Science Information Network.[7] 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 sub-national regions distinguished by GWP. We use this population-weighted 3G network coverage variable to capture 3G mobile internet access at the sub-regional level. We adopt the same approach when calculating 2G network coverage, which enables mobile phone use but not internet access.

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

[7] The data are publicly available at: http://www.ciesin.org/

4. 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:

$$Y_{ict} = \beta_0 + \beta_1 X_{it} + \beta_2 \text{Exposure to epidemic}_{ct} + \beta_3 C_c + \beta_4 Y_t + + \varepsilon_{ict} \qquad (1)$$

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.[8]

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 (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.[9] The year dummies control for global shocks that affect all countries simultaneously. We also include as country-level time-varying regressors

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

[9] 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).

GDP per capita and bank deposits relative to GDP; these variables capture economic and financial development across countries and over time.

In further robustness checks we add interactive country-times-income quintile, country-times-labor-market status, and country-times-education fixed effects.[10] 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.

5. 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.[11]

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.[12] In our preferred model (Column 5), exposure to an epidemic leads to 10.6 (4.5) percentage point increase in

[10] 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 & D'Hautefeuille, 2020).
[11] Sample size varies across specifications because we drop singleton observations that are perfectly collinear with our fixed effects.
[12] As previously noted, these two questions on cash withdrawals (ATM vs. 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.

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) percent, 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 percent increase in daily downloads of finance-related mobile applications between January and early December 2020.

These results are robust to including individual-level income (linear and non-linear), 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.[13] 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)

[13] 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 percent 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.

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.[14]

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**)

[14] 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 **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 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.

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 $10,000 U.S., young adults (ages 26 to 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.

Event Study Estimates and Persistence

Because Findex is only available for three cross-sections spanning seven 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 two years prior to the survey.[15] To investigate pre-existing 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.[16] Panel A shows that differences between countries exposed to an epidemic in the past (or struck by one in the future) and those that 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).[17]

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

[15] 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.

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

[17] 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.

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.[18]

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 2011-2017 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.

Role of Infrastructure

Infrastructure weaknesses may hinder digital transactions and limit epidemic-induced shifts in behavior (as suggested by studies cited in Section 2). We therefore add to our specification a

[18] 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.

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.[19]

Our 3G variable captures the population-weighted portion of 1x1 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 non-parametrically controls for supply-related factors.[20] 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.[21] 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.[22]

[19] In **Appendix Table 9**, we confirm that 2G internet access has no impact on our outcomes when it is interacted with epidemic exposure.
[20] This relieves us of the need to control for other supply-side factors such as for example the prevalence of ATMs.
[21] This would result in multicollinearity in our estimates.
[22] 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 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 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, nor of the effect emerging prior to the epidemic shock.[23]

6. Additional Analysis and Robustness Checks

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.[24]

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).

[23] Again, this means that our setting satisfies the parallel trends assumption.
[24] 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.

The relevant coefficient estimates, while somewhat smaller than before, are still significant and of the same signs.

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

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**).

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.

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:

$$\text{Exposure to Epidemic}_{ct} = \alpha + \beta_1 X_{ct} + \beta_2 \mathrm{C}_c + \beta_3 \mathrm{T}_t + \varepsilon_{\mathrm{c}} \qquad (2)$$

"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 US 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 is 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.

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.[25]

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

[25] 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- vs. late-adopters. Only in the early-adopter subregions were the effects are 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.

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.[26]

7. 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 favourable 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 Swittzerland (Kiefer, Spiller and Brandes 2021), in the United States (Haar 2021) and globally (Martin 2020). These analyses do not control for personal

[26] 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 2011-2019 subperiod covered by Findex (125 of 184 countries). Our results (available upon request) continue to hold, despite the reduced sample size.

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 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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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
Labour 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

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 one percent means that one percent 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. Source: Gallup-Findex, (2011, 2014, 2017) and Ma et al. (2020) Epidemics Database.
* significant at 10%; ** significant at 5%; *** significant at 1%.

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
Labour 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

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 one percent means that one percent of significant results will result in false positives). Source: Gallup-Findex, (2011, 2014, 2017) and Ma et al. (2020) Epidemics Database. * significant at 10%; ** significant at 5%; *** significant at 1%.

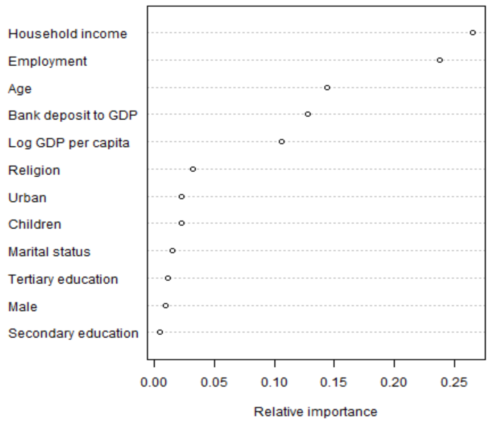
Table 3: The Impact of an Epidemic Year on Financial Technology Adoption and Access – the Role 3G Internet Access

	(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)	
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
Labour 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*Labour 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

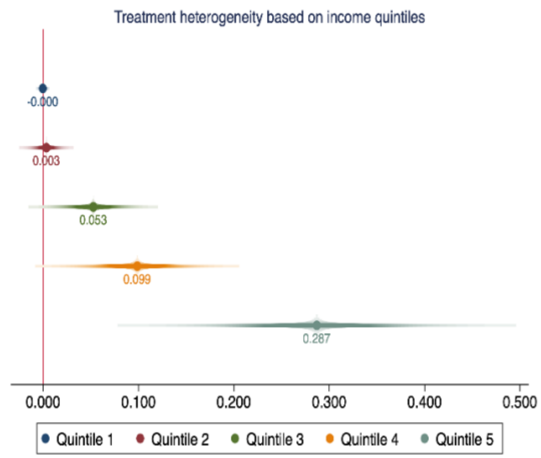
Notes: Results use the Findex-Gallup sampling weights and robust standard errors are clustered at the country level. We check whether our inference is robust to corrections that account for testing of multiple hypotheses by adjusting the p-values using the "sharpened q-value approach" and report them in brackets (in terms of interpretation, for example, a q-value of one percent means that one percent 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. Source: Gallup-Findex, (2011, 2014, 2017), Ma et al. (2020) Epidemics Database and Collins Bartholomew's Mobile Coverage Explorer. * significant at 10%; ** significant at 5%; *** significant at 1%.

Figure 1: Heterogeneity Analysis using Causal Forest

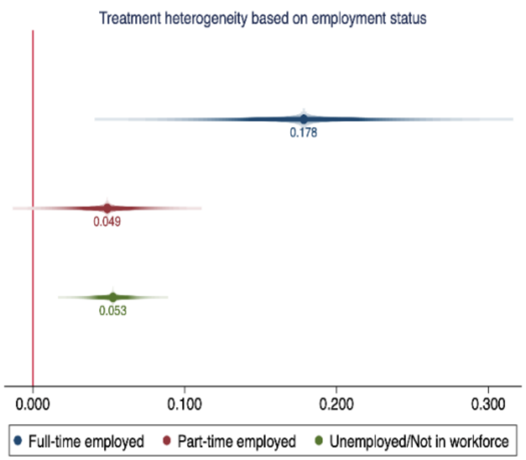
1.A: Variable importance



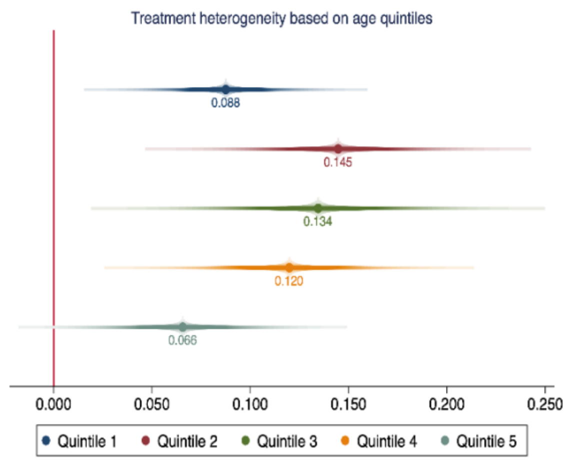
1.B: Treatment heterogeneity by household income



1.C: Treatment heterogeneity by employment status



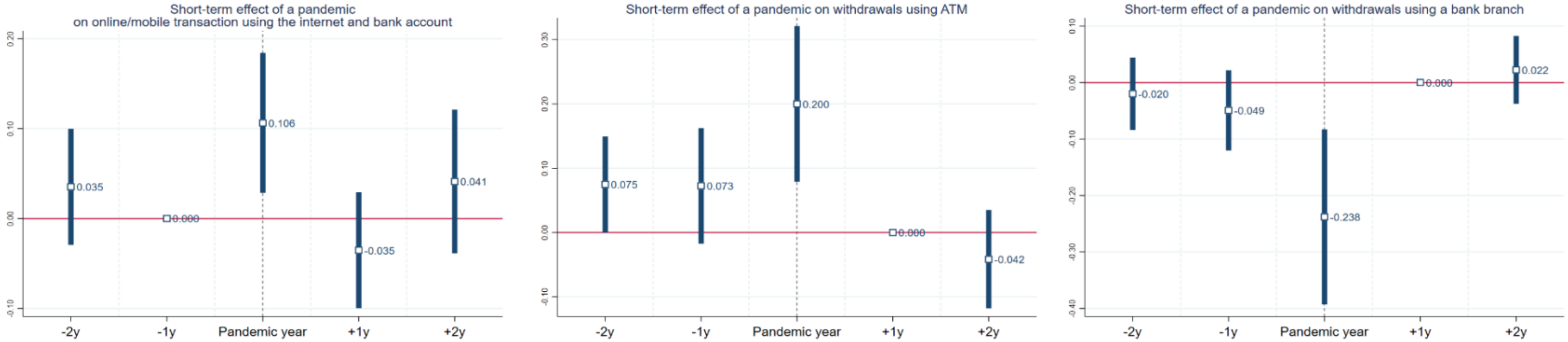
1.D: Treatment heterogeneity by age



Note: Figure A illustrates the variable importance for "Exposure to epidemic" in a causal forest framework (N=20,000 trees), which provides insights into the nature of the relationship between our treatment effect and other covariates. Figures B, C and D provide treatment heterogeneity estimates based on the top 3 covariates determined by the causal forest model in Panel A. For household income, Quintile 1: 0.17-781, Quintile 2: 782-1996, Quintile 3: 1996-4536, Quintile 4: 4536-10201, Quintile 5: 10202-72900000 (all in dollars per year). For age, Quintile 1: 18-25, Quintile 2: 26-34, Quintile 3: 35-45, Quintile 4: 46-59, Quintile 5: 60-99. Outcome is "online/mobile transaction using the internet and bank account". The specification in Column 5 of Table 1. Results are weighted, standard errors are clustered (country-level) and confidence intervals are plotted at 99% level.

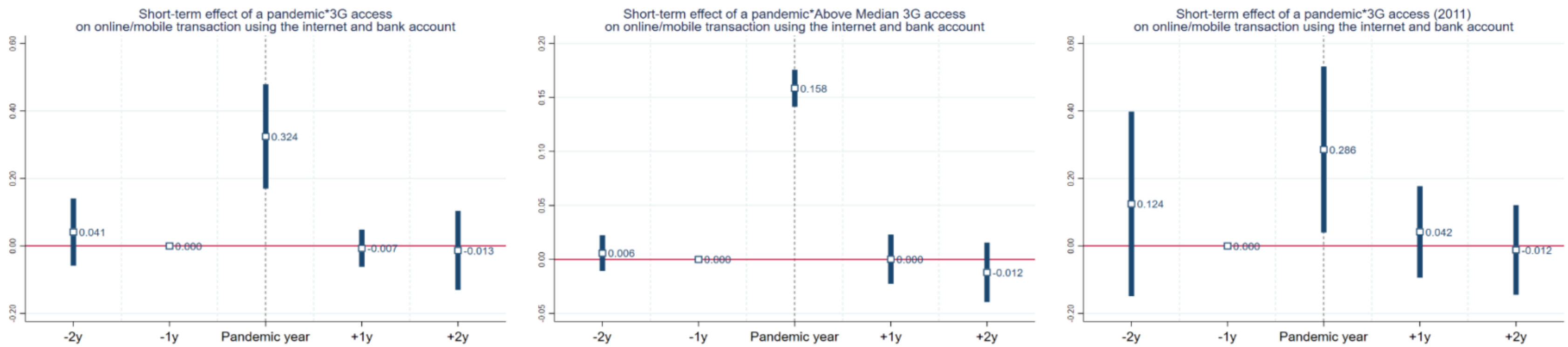
Figure 2: Event Study Estimates

Panel A: The Impact of an Epidemic on Financial Technology Adoption



Notes: Outcomes are “online/mobile transaction using the internet and bank account”, “withdrawals using ATM”, and “withdrawals using a bank branch”. Event study estimates are based on the specification in Column 5 of Table 1. In particular, we repeat the exercise for individuals in countries exposed to an epidemic in the year immediately preceding the survey, and again two years preceding the survey. Results are weighted, standard errors are clustered (country level) and confidence intervals are plotted at 99% level.

Panel B: The Impact of an Epidemic*3G Internet Coverage on Financial Technology Adoption



Notes: Outcome is “online/mobile transaction using the internet and bank account”. Event study estimates are based on the specification in Column 6 of Table 3. In particular, we repeat the exercise for individuals in sub-regions with 3G internet coverage (continues measure, above median 3G coverage, and time-invariant 3G coverage (as of 2011) to minimise potential endogeneity concerns) exposed to an epidemic in the year immediately preceding the survey, and again two years preceding the survey. Results are weighted, standard errors are clustered (country level) and confidence intervals are plotted at 99% level.

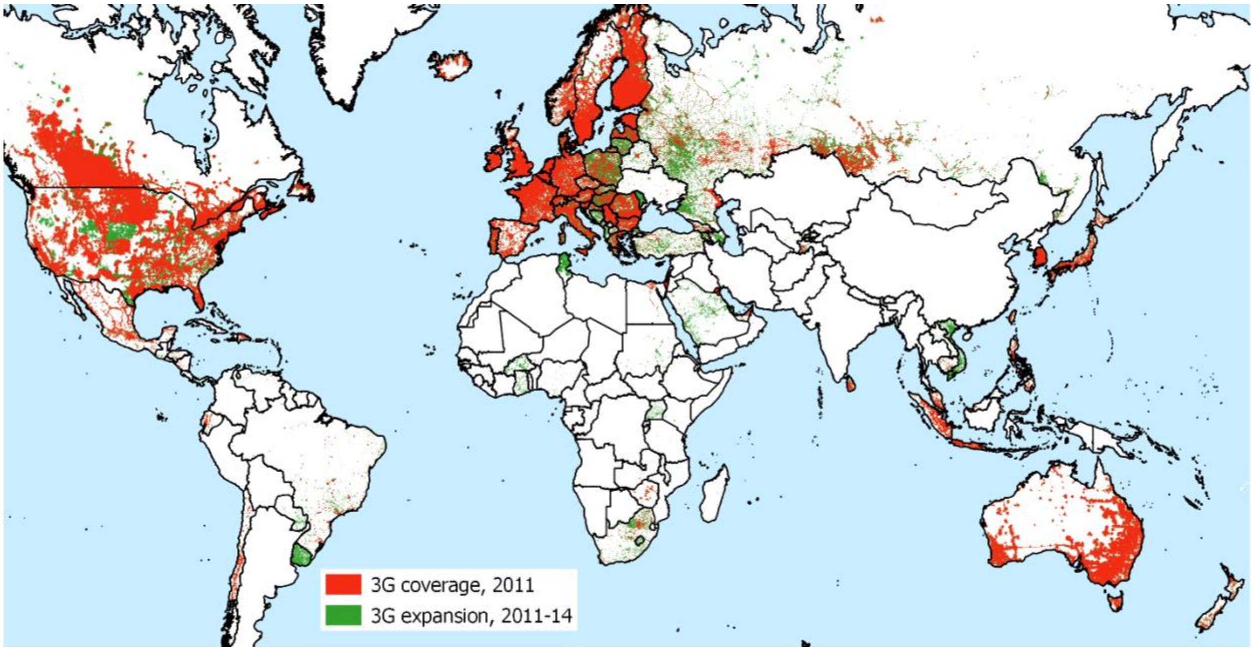
Online Appendix for

Epidemic Exposure, Financial Technology and the Digital Divide

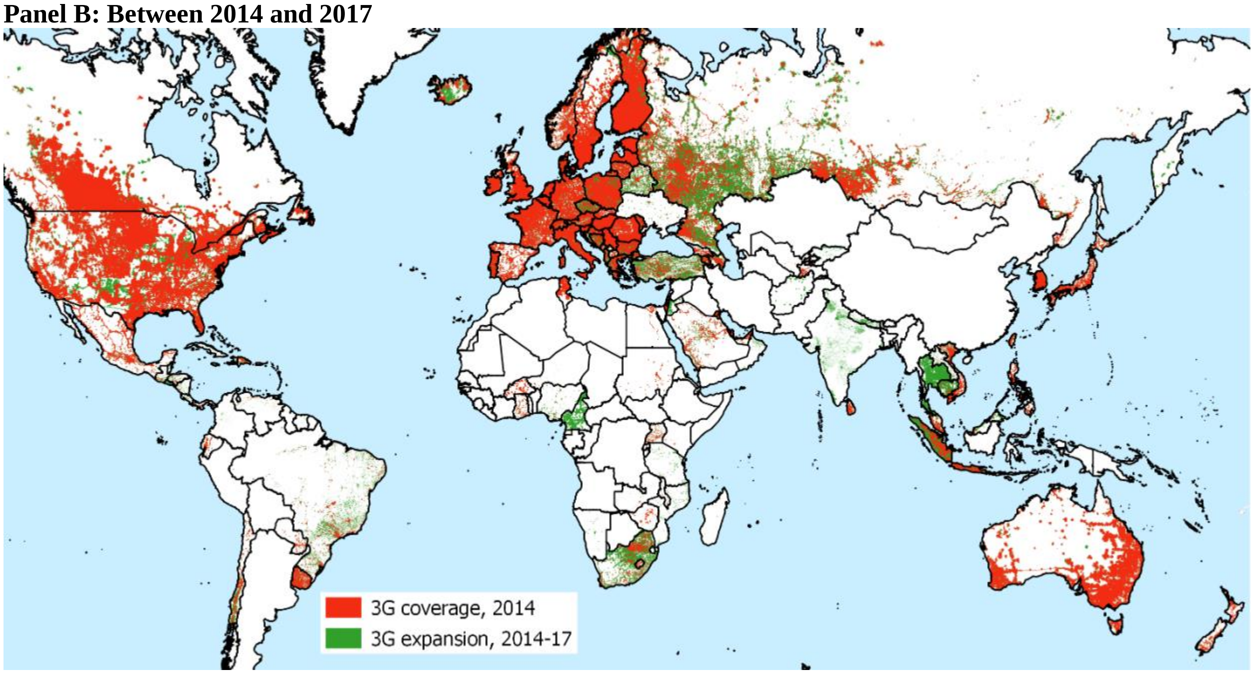
Orkun Saka, Barry Eichengreen, Cevat Giray Aksoy

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

Panel A: Between 2011 and 2014



Panel B: Between 2014 and 2017



Note: Figures illustrate the 3G mobile internet signal coverage at a 1-by-1 kilometer grid level. Source: Collins Bartholomew's Mobile Coverage Explorer.

Online Appendix Table 1: Sample Characteristics

Variables	(1) Mean (Standard deviation)
Main dependent variables	
Online/Mobile transaction using the internet and bank account	0.083 (0.275) – N: 157,093
Mobile transaction using bank account	0.094 (0.293) – N: 230,326
Online payments (such as bills) using the internet	0.197 (0.398) – N: 164,465
Withdrawals using ATM	0.633 (0.481) – N: 83,309
Withdrawals using a bank branch	0.309 (0.462) – N: 83,309
Placebo outcomes	
Account ownership	0.568 (0.495) – N: 254,832
Deposit money into a personal account in a typical month	0.931 (0.251) – N: 94,316
Withdraw money out of a personal account in a typical month	0.937 (0.241) – N: 94,107
Debit card ownership	0.409 (0.491) – N: 253,284
Credit card ownership	0.192 (0.394) – N: 252,624
Pandemic occurrence	0.025 (0.157)
3G coverage characteristics	
Continuous 3G coverage	0.404 (0.391)
3G coverage in 2011	0.240 (0.308)

Notes: Means (standard deviations). This table provides individual and aggregate level variables averaged across the 3 years (2011, 2014 and 2017) used in the analysis. The sample sizes for some variables are different either due to missing data or because they were not asked in every year

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

	(1)
Outcome ➔ Online/Mobile transaction using the internet and bank account	
High Exposure to Epidemic	0.119***
	(0.037) [0.002]
Low Exposure to Epidemic	0.085***
	(0.018) [0.000]
Observations	157,093
Outcome ➔ Mobile transaction using bank account	
High Exposure to Epidemic	0.039**
	(0.015) [0.013]
Low Exposure to Epidemic	0.053*
	(0.029) [0.071]
Observations	230,327
Outcome ➔ Online payments (such as bills) using the internet	
High Exposure to Epidemic	0.078**
	(0.030) [0.010]
Low Exposure to Epidemic	-0.003
	(0.009) [0.775]
Observations	164,465
Outcome ➔ Withdrawals using ATM	
High Exposure to Epidemic	0.220***
	(0.040) [0.000]
Low Exposure to Epidemic	0.086***
	(0.012) [0.000]
Observations	83,322
Outcome ➔ Withdrawals using a bank branch	
High Exposure to Epidemic	-0.262***
	(0.053) [0.000]
Low Exposure to Epidemic	-0.101***
	(0.011) [0.000]
Observations	83,322
Country fixed effects	No
Year fixed effects	Yes
Demographic characteristics	Yes
Education fixed effects	No
Labour market controls	No
Income decile fixed effects	No
Country-level controls	Yes
Country*Education fixed effects	Yes
Country*Labour mar. fixed effects	Yes
Country*Income decile fixed effects	Yes

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 one percent means that one percent of significant results will result in false positives). Source: Gallup-Findex, (2011, 2014, 2017) and Ma et al. (2020) Epidemics Database. * significant at 10%; ** significant at 5%; *** significant at 1%.

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

	(1)	(2)	(3)	(4)	(5)
Outcome ➔ Online/Mobile transaction using the internet and bank account					
Exposure to Epidemic	0.077***	0.074***	0.076***	0.078***	0.081***
	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
Observations	157,093	157,093	157,093	157,093	157,093
Outcome ➔ Mobile transaction using bank account					
Exposure to Epidemic	0.026***	0.020**	0.017**	0.017*	0.023**
	(0.008)	(0.008)	(0.008)	(0.010)	(0.010)
Observations	230,327	230,327	230,327	230,327	230,326
Outcome ➔ Online payments (such as bills) using the internet					
Exposure to Epidemic	-0.008	-0.012	-0.007	-0.006	-0.005
	(0.009)	(0.010)	(0.010)	(0.009)	(0.009)
Observations	164,465	164,465	164,465	164,465	164,465
Outcome ➔ Withdrawals using ATM					
Exposure to Epidemic	0.093***	0.082***	0.019	0.023	0.084***
	(0.012)	(0.013)	(0.015)	(0.015)	(0.012)
Observations	83,322	83,321	83,321	83,321	83,309
Outcome ➔ Withdrawals using a bank branch					
Exposure to Epidemic	-0.105***	-0.096***	-0.048***	-0.051***	-0.098***
	(0.011)	(0.011)	(0.014)	(0.013)	(0.011)
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
Labour 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

Notes: Results use the Findex-Gallup sampling weights and robust standard errors are clustered at the country level and reported in parentheses. Source: Gallup-Findex, (2011, 2014, 2017) and Ma et al. (2020) Epidemics Database. * significant at 10%; ** significant at 5%; *** significant at 1%.

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

	(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)	(0.019)	(0.019)	(0.030)	(0.030)
Observations	157,093	157,093	157,093	157,093	157,093
Outcome ➔ Mobile transaction using bank account					
Exposure to Epidemic	0.029***	0.027***	0.024**	0.032**	0.033***
	(0.009)	(0.009)	(0.009)	(0.013)	(0.012)
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)	(0.021)	(0.020)	(0.032)	(0.030)
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)	(0.046)	(0.061)	(0.056)	(0.046)
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)	(0.064)	(0.074)	(0.071)	(0.059)
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
Labour 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

Notes: Results use the Findex-Gallup sampling weights and robust standard errors are clustered at the country level and reported in parentheses. Source: Gallup-Findex, (2011, 2014, 2017) and Ma et al. (2020) Epidemics Database. * significant at 10%; ** significant at 5%; *** significant at 1%.

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

	(1)	(2)	(3)
Robustness ➔	Clustering at the Global Region-Year Level (12 regions*3 years)	Clustering at the Global Region Level (12 regions)	Adding country-specific linear time trends
Outcome ➔ Online/Mobile trans. using the internet and bank account			
Exposure to Epidemic	0.106***	0.106*	0.092***
	(0.034)	(0.049)	(0.001)
Observations	157,093	157,093	157,093
Outcome ➔ Mobile transaction using bank account			
Exposure to Epidemic	0.045	0.045	0.035**
	(0.037)	(0.030)	(0.010)
Observations	230,326	230,326	230,327
Outcome ➔ Online payments (such as bills) using the internet			
Exposure to Epidemic	0.049***	0.049*	0.026***
	(0.016)	(0.023)	(0.001)
Observations	164,465	164,465	164,465
Outcome ➔ Withdrawals using ATM			
Exposure to Epidemic	0.200***	0.200***	0.191***
	(0.017)	(0.021)	(0.007)
Observations	83,309	83,309	83,322
Outcome ➔ Withdrawals using a bank branch			
Exposure to Epidemic	-0.238***	-0.238***	-0.137***
	(0.015)	(0.019)	(0.007)
Observations	83,309	83,309	83,322
Country fixed effects	No	No	No
Year fixed effects	Yes	Yes	Yes
Demographic characteristics	Yes	Yes	Yes
Education fixed effects	No	No	No
Labour market controls	No	No	No
Income decile fixed effects	No	No	No
Country-level controls	Yes	Yes	Yes
Country*Education fixed effects	Yes	Yes	Yes
Country*Labour mar. fixed effects	Yes	Yes	Yes
Country*Income decile fixed effects	Yes	Yes	Yes

Notes: Results use the Findex-Gallup sampling weights and robust standard errors are clustered (unless otherwise stated) at the country level and reported in parentheses. Source: Gallup-Findex, (2011, 2014, 2017) and Ma et al. (2020) Epidemics Database. * significant at 10%; ** significant at 5%; *** significant at 1%.

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

	(1)	(2)
Placebo treatment ➔	Randomising epidemics across the same-continent countries but with the original epidemic year	Randomising epidemics across the same-continent countries and across years
Outcome ➔ Online/Mobile trans. using the internet and bank account		
Placebo treatment	-0.019	-0.073
	(0.072)	(0.073)
Observations	157,093	157,093
Outcome ➔ Mobile transaction using bank account		
Placebo treatment	0.010	-0.022
	(0.048)	(0.044)
Observations	230,326	230,326
Outcome ➔ Online payments (such as bills) using the internet		
Placebo treatment	0.001	-0.013
	(0.023)	(0.023)
Observations	164,465	164,465
Outcome ➔ Withdrawals using ATM		
Placebo treatment	0.002	-0.034
	(0.025)	(0.027)
Observations	83,309	83,309
Outcome ➔ Withdrawals using a bank branch		
Placebo treatment	-0.020	0.014
	(0.017)	(0.018)
Observations	83,309	83,309
Country fixed effects	No	No
Year fixed effects	Yes	Yes
Demographic characteristics	Yes	Yes
Education fixed effects	No	No
Labour market controls	No	No
Income decile fixed effects	No	No
Country-level controls	Yes	Yes
Country*Education fixed effects	Yes	Yes
Country*Labour mar. fixed effects	Yes	Yes
Country*Income decile fixed effects	Yes	Yes

Notes: Results use the Findex-Gallup sampling weights and robust standard errors are clustered (unless otherwise stated) at the country level and reported in parentheses. Source: Gallup-Findex, (2011, 2014, 2017) and Ma et al. (2020) Epidemics Database. * significant at 10%; ** significant at 5%; *** significant at 1%.

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

	(1)	(2)	(3)	(4)
Sample period ➔	2000-2017	2000-2017	2011, 2014, 2017	2011, 2014, 2017
Outcome ➔	Epidemic occurrence	Epidemic occurrence	Epidemic occurrence	Epidemic occurrence
GDP per capita (constant 2010 USD) (log)	-0.043	-0.008	0.029	0.026
	(0.027)	(0.023)	(0.106)	(0.120)
Urban population as a share of total pop. (log)	-0.019	0.088	0.090	0.110
	(0.078)	(0.055)	(0.279)	(0.269)
Account at a formal financial inst. (% age 15+) (log)	0.009	0.018	0.014	0.024
	(0.017)	(0.018)	(0.029)	(0.029)
ATMs per 100,000 adults (log)	-0.002	-0.001	0.005	-0.002
	(0.004)	(0.004)	(0.021)	(0.022)
Financial system deposits to GDP (%) (log)	-0.035	-0.025	0.112	0.099
	(0.022)	(0.018)	(0.103)	(0.095)
Deposit money banks' assets to GDP (%) (log)	0.013	0.022	-0.054	-0.046
	(0.015)	(0.014)	(0.061)	(0.056)
Bank net interest margin (%) (log)	0.014	0.006	-0.017	-0.004
	(0.010)	(0.008)	(0.029)	(0.031)
Bank overhead costs to total assets (%) (log)	-0.016	-0.010	-0.002	-0.000
	(0.011)	(0.008)	(0.019)	(0.019)
Bank Z-score	-0.000	-0.000	0.000	-0.000
	(0.001)	(0.000)	(0.001)	(0.002)
Lerner index	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	2,610	2,610	435	435
Country fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Continent by year fixed effects	No	Yes	No	Yes
Country income group fixed effects	No	Yes	No	Yes

Source: World Bank and Ma et al. (2020) Epidemics Database. Notes: Robust standard errors reported in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Columns 1 and 2 present results from a country-year level analysis between 2000 and 2017 (18 years*145 countries=2,610 country-year observations). Columns 3 and 4 present results from a country-year level analysis for Findex years 2011, 2014 and 2017 (3 years*145 countries=435 country-year observations). Income group refers to the World Banks's income classification, which assigns the world's economies to four income groups—low, lower-middle, upper-middle, and high-income countries. To obtain a balance sample, missing observations in some countries were imputed using own country sample averages. Account at a formal financial inst. (% age 15+) and ATMs per 100,000 adults capture financial access, Financial system deposits to GDP (%), Private credit by deposit money banks to GDP (%), and Deposit money banks' assets to GDP (%) capture financial depth, Bank net interest margin (%) and Bank overhead costs to total assets (%) capture financial efficiency, Bank Z-score captures the probability of default of a country's commercial banking system, Lerner index captures market power in the banking market. It compares output pricing and marginal costs (that is, markup). An increase in the Lerner index indicates a deterioration of the competitive conduct of financial intermediaries.

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

	(1)	(2)	(3)	(4)
Specification ➔	Changes in country-level characteristics (2000-2017)	Changes in country-level characteristics (2000-2017)	Baseline Check (for 2011-2017 period)	Baseline Check (for 2011-2017 period)
Outcome ➔	Epidemic occurrence	Epidemic occurrence	Epidemic occurrence	Epidemic occurrence
Δ in GDP per capita (constant 2010 USD) (log)	0.004	0.061	0.014	0.001
	(0.064)	(0.070)	(0.024)	(0.079)
Δ in Urban population as a share of total pop. (log)	0.341	0.132	0.045	0.053
	(0.263)	(0.165)	(0.032)	(0.045)
Δ in Account at a formal financial inst. (% age 15+) (log)	0.025	-0.005	-0.000	-0.001
	(0.059)	(0.054)	(0.008)	(0.023)
Δ in ATMs per 100,000 adults (log)	0.035	0.015	0.002	-0.002
	(0.043)	(0.041)	(0.021)	(0.009)
Δ in Financial system deposits to GDP (%) (log)	0.051	0.009	-0.032	-0.075
	(0.043)	(0.034)	(0.022)	(0.045)
Δ in Deposit money banks' assets to GDP (%) (log)	-0.016	0.002	0.028	0.032
	(0.025)	(0.026)	(0.033)	(0.020)
Δ in Bank net interest margin (%) (log)	0.010	-0.024	0.061	0.045
	(0.029)	(0.029)	(0.060)	(0.059)
Δ in Bank overhead costs to total assets (%) (log)	0.019	0.030	0.001	-0.069
	(0.029)	(0.030)	(0.013)	(0.059)
Δ in Bank Z-score	0.001	0.002	0.000	-0.003
	(0.002)	(0.001)	(0.000)	(0.003)
Δ in Lerner index	0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	2,610	2,610	1,015	1,015
Country fixed effects	Yes	Yes	Yes	Yes
Country income group fixed effects	No	Yes	No	Yes

Source: World Bank and Ma et al. (2020) Epidemics Database. Notes: Robust standard errors reported in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Columns 1 and 2 present results from a country-year level analysis between 2000 and 2017 (18 years*145 countries=2,610 country-year observations). All explanatory variables in Columns 1 and 2 are based on changes between 2000 and 2017 (not in levels). Columns 3 and 4 present results from a country-year level analysis between 2011 and 2017 (7 years*145 countries=1,015 country-year observations), in which all explanatory variables are measured in 2011. Income group refers to the World Banks's income classification, which assigns the world's economies to four income groups—low, lower-middle, upper-middle, and high-income countries. To obtain a balance sample, missing observations in some countries were imputed using own country sample averages. Account at a formal financial inst. (% age 15+) and ATMs per 100,000 adults capture financial access, Financial system deposits to GDP (%), Private credit by deposit money banks to GDP (%), and Deposit money banks' assets to GDP (%) capture financial depth, Bank net interest margin (%) and Bank overhead costs to total assets (%) capture financial efficiency, Bank Z-score captures the probability of default of a country's commercial banking system, Lerner index captures market power in the banking market. It compares output pricing and marginal costs (that is, markup). An increase in the Lerner index indicates a deterioration of the competitive conduct of financial intermediaries.

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

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome ➔ Online/Mobile transaction using the internet and bank account						
Exposure to Epidemic*3G	0.283***	0.296***	0.294***	0.322***	0.343***	0.341***
	(0.050)	(0.055)	(0.057)	(0.044)	(0.045)	(0.053)
3G	0.050**	0.035	0.023	0.023	0.022	0.001
	(0.024)	(0.025)	(0.025)	(0.023)	(0.022)	(0.011)
Exposure to Epidemic*2G	0.013	0.006	-0.002	-0.021	-0.026	-0.038**
	(0.024)	(0.023)	(0.021)	(0.026)	(0.025)	(0.018)
2G	-0.020	-0.021	-0.017	-0.014	-0.012	0.011
	(0.017)	(0.018)	(0.017)	(0.020)	(0.020)	(0.014)
Exposure to Epidemic	0.079**	0.082**	0.089***	0.160**	0.162***	--
	(0.031)	(0.032)	(0.032)	(0.061)	(0.060)	
Observations	127,184	127,184	127,184	127,184	127,184	127,184
Exp. to Epidemic*Above median 3G	0.288***	0.226***	0.239***	0.237***	0.164***	0.162***
	(0.014)	(0.014)	(0.014)	(0.013)	(0.012)	(0.006)
Above median 3G	0.003	-0.002	-0.006	-0.004	-0.006	-0.004
	(0.014)	(0.013)	(0.013)	(0.012)	(0.012)	(0.004)
Exp. to Epidemic*Above median 2G	0.031	0.026	0.018	0.004	0.000	-0.006
	(0.039)	(0.038)	(0.037)	(0.038)	(0.036)	(0.032)
Above median 2G	-0.005	-0.009	-0.007	-0.004	-0.003	0.014
	(0.017)	(0.018)	(0.017)	(0.021)	(0.021)	(0.015)
Exposure to Epidemic	0.073*	0.074*	0.080**	0.147**	0.148**	--
	(0.040)	(0.040)	(0.040)	(0.060)	(0.059)	
Observations	127,184	127,184	127,184	127,184	127,184	127,184
Exposure to Epidemic*3G(2011)	0.234***	0.261***	0.258***	0.261***	0.283***	0.289***
	(0.087)	(0.080)	(0.089)	(0.090)	(0.094)	(0.093)
3G(2011)	0.078***	0.052***	0.029**	0.028**	0.021	0.013
	(0.015)	(0.014)	(0.013)	(0.014)	(0.013)	(0.011)
Exposure to Epidemic*2G(2011)	0.040*	0.034	0.026	0.005	-0.004	-0.014
	(0.022)	(0.022)	(0.022)	(0.024)	(0.022)	(0.022)
2G(2011)	-0.023	-0.026*	-0.021	-0.020	-0.018	0.009
	(0.015)	(0.015)	(0.015)	(0.019)	(0.019)	(0.020)
Exposure to Epidemic	0.052*	0.055*	0.061*	0.150**	0.154***	--
	(0.031)	(0.032)	(0.033)	(0.059)	(0.058)	
Observations	95,745	95,745	95,745	95,745	95,745	95,745

Notes: In terms of control variables, columns are structured as in Table 3. 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 one percent means that one percent 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. Source: Gallup-Findex, (2011, 2014, 2017), Ma et al. (2020) Epidemics Database and Collins Bartholomew's Mobile Coverage Explorer. * significant at 10%; ** significant at 5%; *** significant at 1%.

Online Appendix Table 10: Robustness to Excluding Influential Treatments

	(1) Outcome: Online/Mobile transaction using the internet and bank account	(2) Outcome: Mobile transaction using bank account	(3) Outcome: Online payments (such as bills) using the internet	(4) Outcome: Withdrawals using ATM	(5) Outcome: Withdrawals using a bank branch
Exposure to Epidemic – excl. Guinea	0.106***	0.045***	0.049	0.200***	-0.238***
	(0.030) [0.001]	(0.015) [0.003]	(0.030) [0.104]	(0.046) [0.000]	(0.059) [0.000]
Observations	157,093	230,326	164,465	83,309	83,309
Exposure to Epidemic – excl. Italy	0.106***	0.045***	0.049	0.200***	-0.238***
	(0.030) [0.001]	(0.015) [0.003]	(0.030) [0.104]	(0.046) [0.000]	(0.059) [0.000]
Observations	157,093	230,326	164,465	83,309	83,309
Exposure to Epidemic – excl. Liberia	0.106***	0.045***	0.049	0.200***	-0.238***
	(0.030) [0.001]	(0.015) [0.003]	(0.030) [0.104]	(0.046) [0.000]	(0.059) [0.000]
Observations	157,093	230,326	164,465	83,309	83,309
Exposure to Epidemic – excl. Mali	0.106***	0.045***	0.049	0.200***	-0.238***
	(0.030) [0.001]	(0.015) [0.003]	(0.030) [0.104]	(0.046) [0.000]	(0.059) [0.000]
Observations	157,093	230,326	164,465	83,309	83,309
Exposure to Epidemic – excl. Nigeria	0.113***	0.044**	0.020	0.082***	-0.084***
	(0.037) [0.003]	(0.019) [0.018]	(0.020) [0.332]	(0.012) [0.000]	(0.014) [0.000]
Observations	157,093	230,326	164,465	83,309	83,309
Exposure to Epidemic – excl. Senegal	0.106***	0.045***	0.049	0.220***	-0.238***
	(0.030) [0.001]	(0.015) [0.003]	(0.030) [0.104]	(0.041) [0.000]	(0.059) [0.000]
Observations	157,093	230,326	164,465	83,309	83,309
Exposure to Epidemic – excl. Sierra L.	0.106***	0.045***	0.049	0.200***	-0.238***
	(0.030) [0.001]	(0.015) [0.003]	(0.030) [0.104]	(0.046) [0.000]	(0.059) [0.000]
Observations	157,093	230,326	164,465	83,309	83,309
Exposure to Epidemic – excl. Spain	0.106***	0.045***	0.049	0.200***	-0.238***
	(0.030) [0.001]	(0.015) [0.003]	(0.030) [0.104]	(0.046) [0.000]	(0.059) [0.000]
Observations	157,093	230,326	164,465	83,309	83,309
Exposure to Epidemic – excl. UK	0.106***	0.045***	0.049	0.200***	-0.238***
	(0.030) [0.001]	(0.015) [0.003]	(0.030) [0.104]	(0.046) [0.000]	(0.059) [0.000]
Observations	157,093	230,326	164,465	83,309	83,309
Exposure to Epidemic – excl. USA	0.106***	0.045***	0.049	0.200***	-0.238***
	(0.030) [0.001]	(0.015) [0.003]	(0.030) [0.104]	(0.046) [0.000]	(0.059) [0.000]
Observations	157,093	230,326	164,465	83,309	83,309
Country fixed effects	No	No	No	No	No
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Demographic characteristics	Yes	Yes	Yes	Yes	Yes
Education fixed effects	No	No	No	No	No
Labour market controls	No	No	No	No	No
Income decile fixed effects	No	No	No	No	No
Country-level controls	Yes	Yes	Yes	Yes	Yes
Country*Education fixed effects	Yes	Yes	Yes	Yes	Yes
Country*Labour mar. fixed effects	Yes	Yes	Yes	Yes	Yes
Country*Income decile fixed effects	Yes	Yes	Yes	Yes	Yes

Notes: Results use the Findex-Gallup sampling weights and robust standard errors are clustered at the country level and reported in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

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

	(1) Outcome: Online/Mobile transaction using the internet and bank account	(2) Outcome: Mobile transaction using bank account	(3) Outcome: Online payments (such as bills) using the internet	(4) Outcome: Withdrawals using ATM	(5) Outcome: Withdrawals using a bank branch
Exposure to Epidemic – drop Guinea	0.106***	0.045***	0.049	0.200***	-0.238***
	(0.030) [0.001]	(0.015) [0.003]	(0.030) [0.104]	(0.046) [0.000]	(0.059) [0.000]
Observations	156,402	229,579	163,732	83,309	83,309
Exposure to Epidemic – drop Italy	0.106***	0.045***	0.049	0.200***	-0.238***
	(0.030) [0.001]	(0.017) [0.010]	(0.030) [0.105]	(0.046) [0.000]	(0.059) [0.000]
Observations	156,173	229,156	163,537	82,655	82,655
Exposure to Epidemic – drop Liberia	0.106***	0.045***	0.050	0.200***	-0.238***
	(0.030) [0.001]	(0.015) [0.003]	(0.043) [0.104]	(0.046) [0.000]	(0.059) [0.000]
Observations	157,093	230,326	164,465	83,309	83,309
Exposure to Epidemic – drop Mali	0.106***	0.045***	0.049	0.223***	-0.270***
	(0.030) [0.001]	(0.015) [0.003]	(0.030) [0.104]	(0.038) [0.000]	(0.045) [0.000]
Observations	157,093	230,326	164,465	83,108	83,108
Exposure to Epidemic – drop Nigeria	0.114***	0.051***	0.050	0.083***	-0.086***
	(0.037) [0.003]	(0.019) [0.009]	(0.043) [0.249]	(0.012) [0.000]	(0.014) [0.000]
Observations	155,523	227,889	162,846	82,478	82,478
Exposure to Epidemic – drop Senegal	0.088***	0.044***	0.021	0.220***	-0.262***
	(0.018) [0.001]	(0.018) [0.018]	(0.020) [0.290]	(0.040) [0.000]	(0.053) [0.000]
Observations	155,453	227,741	162,797	83,050	83,050
Exposure to Epidemic – drop Sierra L.	0.106***	0.054***	0.078**	0.220***	-0.238***
	(0.030) [0.001]	(0.019) [0.005]	(0.030) [0.010]	(0.040) [0.000]	(0.059) [0.000]
Observations	157,093	227,766	162,774	83,309	83,309
Exposure to Epidemic – drop Spain	0.106***	0.045***	0.049	0.200***	-0.238***
	(0.030) [0.001]	(0.015) [0.003]	(0.030) [0.104]	(0.046) [0.000]	(0.059) [0.000]
Observations	157,093	230,271	164,465	82,455	82,455
Exposure to Epidemic – drop UK	0.106***	0.045***	0.049	0.200***	-0.238***
	(0.030) [0.001]	(0.015) [0.003]	(0.030) [0.104]	(0.046) [0.000]	(0.059) [0.000]
Observations	156,200	229,433	163,567	83,309	83,309
Exposure to Epidemic – drop USA	0.106***	0.035***	0.049	0.200***	-0.238***
	(0.030) [0.001]	(0.010) [0.001]	(0.030) [0.104]	(0.046) [0.000]	(0.059) [0.000]
Observations	157,245	229,397	163,610	82,505	82,505
Country fixed effects	No	No	No	No	No
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Demographic characteristics	Yes	Yes	Yes	Yes	Yes
Education fixed effects	No	No	No	No	No
Labour market controls	No	No	No	No	No
Income decile fixed effects	No	No	No	No	No
Country-level controls	Yes	Yes	Yes	Yes	Yes
Country*Education fixed effects	Yes	Yes	Yes	Yes	Yes
Country*Labour mar. fixed effects	Yes	Yes	Yes	Yes	Yes
Country*Income decile fixed effects	Yes	Yes	Yes	Yes	Yes

Notes: Results use the Findex-Gallup sampling weights and robust standard errors are clustered at the country level and reported in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

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

	(1)	(2)	(3)	(4)
Sample period ➔	2001-2017	2001-2017	2011, 2014, 2017	2011, 2014, 2017
Outcome ➔	ATMs per 100,000 people	ATMs per 100,000 people	ATMs per 100,000 people	ATMs per 100,000 people
Epidemic occurrence	-7.497	-38.254	125.295	69.241
	(44.471)	(60.702)	(187.74)	(210.09)
Observations	1,845	1,845	407	407
Country fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Country-level characteristics	Yes	Yes	Yes	Yes
Continent by year fixed effects	No	Yes	No	Yes
Country income group fixed effects	No	Yes	No	Yes

Source: World Bank and Ma et al. (2020) Epidemics Database. Notes: Robust standard errors reported in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Country-level characteristics include GDP per capita (constant 2010 USD) (log), Urban population as a share of total pop. (log), account at a formal financial inst. (% age 15+), financial system deposits to GDP (%), private credit by deposit money banks to GDP (%), and deposit money banks' assets to GDP (%) to capture financial depth, bank net interest margin (%) and bank overhead costs to total assets (%) to capture financial efficiency, bank Z-score to capture the probability of default of a country's commercial banking system, Lerner index to capture market power in the banking market. It compares output pricing and marginal costs (that is, markup). An increase in the Lerner index indicates a deterioration of the competitive conduct of financial intermediaries.

Online Appendix B

Full List of Epidemics

Country	Year	Epidemic	Total no of affected people	Total no of deaths
Australia	2003	SARS	6	0
Canada	2003	SARS	251	43
China	2003	SARS	5,327	349
France	2003	SARS	7	1
Germany	2003	SARS	9	0
Hong Kong	2003	SARS	1,755	299
India	2003	SARS	3	0
Indonesia	2003	SARS	2	0
Ireland	2003	SARS	1	0
Italy	2003	SARS	4	0
Kuwait	2003	SARS	1	0
China, Macao SAR	2003	SARS	1	0
Malaysia	2003	SARS	5	2
Mongolia	2003	SARS	9	0
New Zealand	2003	SARS	1	0
Philippines	2003	SARS	14	2
Romania	2003	SARS	1	0
Russia	2003	SARS	1	0
Singapore	2003	SARS	238	33
South Africa	2003	SARS	1	1
South Korea	2003	SARS	3	0
Spain	2003	SARS	1	0
Sweden	2003	SARS	5	0
Switzerland	2003	SARS	1	0
Taiwan	2003	SARS	346	81
Thailand	2003	SARS	9	2
United Kingdom	2003	SARS	4	0
United States	2003	SARS	27	0
Vietnam	2003	SARS	63	5
Afghanistan	2009	H1N1	853	17
Albania	2009	H1N1	426	12
Algeria	2009	H1N1	916	57
Angola	2009	H1N1	37	0
Argentina	2009	H1N1	11,458	626
Armenia	2009	H1N1	119	2
Austria	2009	H1N1	964	40
Azerbaijan	2009	H1N1	14	0
Bahrain	2009	H1N1	1,325	8
Bangladesh	2009	H1N1	1,015	7
Barbados	2009	H1N1	154	3

Belarus	2009	H1N1	88	0
Belgium	2009	H1N1	76,973	19
Bhutan	2009	H1N1	91	0
Bolivia	2009	H1N1	2,310	59
Bosnia and Herz.	2009	H1N1	714	13
Botswana	2009	H1N1	31	0
Brazil	2009	H1N1	58,178	2,135
Brunei Darussalam	2009	H1N1	971	2
Bulgaria	2009	H1N1	766	40
Burundi	2009	H1N1	7	0
Cambodia	2009	H1N1	531	6
Cameroon	2009	H1N1	4	0
Canada	2009	H1N1	25,828	429
Cape Verde	2009	H1N1	118	0
Chad	2009	H1N1	1	0
Chile	2009	H1N1	12,258	156
China	2009	H1N1	120,940	800
Colombia	2009	H1N1	4,310	272
Congo Brazzaville	2009	H1N1	222	0
Congo Kinshasa	2009	H1N1	21	0
Costa Rica	2009	H1N1	1,867	67
Croatia	2009	H1N1	50,255	26
Cuba	2009	H1N1	973	69
Czech Republic	2009	H1N1	2,445	102
Djibouti	2009	H1N1	9	0
Dominican Republic	2009	H1N1	491	23
Ecuador	2009	H1N1	2,251	200
Egypt	2009	H1N1	15,812	278
El Salvador	2009	H1N1	834	33
Estonia	2009	H1N1	738	21
Ethiopia	2009	H1N1	19	0
Fiji	2009	H1N1	234	0
Finland	2009	H1N1	6,122	56
France	2009	H1N1	1,980,000	344
Gabon	2009	H1N1	4	0
Georgia	2009	H1N1	1,300	33
Germany	2009	H1N1	222,360	258
Ghana	2009	H1N1	676	3
Greece	2009	H1N1	17,977	149
Guatemala	2009	H1N1	1,170	24
Guyana	2009	H1N1	27	0
Honduras	2009	H1N1	560	18
Hungary	2009	H1N1	283	134
Iceland	2009	H1N1	676	2

India	2009	H1N1	33,783	2,024
Indonesia	2009	H1N1	1,098	10
Iran	2009	H1N1	3,672	147
Iraq	2009	H1N1	2,880	42
Ireland	2009	H1N1	3,189	26
Israel	2009	H1N1	4,330	94
Italy	2009	H1N1	3,046,933	244
Ivory Coast	2009	H1N1	9	0
Jamaica	2009	H1N1	191	7
Japan	2009	H1N1	11,636	198
Jordan	2009	H1N1	3,033	19
Kazakhstan	2009	H1N1	17	0
Kenya	2009	H1N1	417	0
Kuwait	2009	H1N1	8,669	30
Lao People's Dem. Rep.	2009	H1N1	242	1
Lebanon	2009	H1N1	1,838	5
Lesotho	2009	H1N1	65	0
Libya	2009	H1N1	764	1
Lithuania	2009	H1N1	68	23
Luxembourg	2009	H1N1	333	3
Macedonia, FYR	2009	H1N1	2600	26
Madagascar	2009	H1N1	877	3
Malawi	2009	H1N1	9	0
Malaysia	2009	H1N1	12,210	92
Mali	2009	H1N1	29	0
Malta	2009	H1N1	718	5
Mauritius	2009	H1N1	69	8
Mexico	2009	H1N1	70,715	1,316
Moldova	2009	H1N1	2,524	46
Mongolia	2009	H1N1	1,259	30
Montenegro	2009	H1N1	119	7
Morocco	2009	H1N1	2,890	64
Mozambique	2009	H1N1	57	2
Myanmar	2009	H1N1	137	0
Namibia	2009	H1N1	75	1
Nepal	2009	H1N1	172	3
Netherlands	2009	H1N1	1,473	62
New Zealand	2009	H1N1	3,199	50
Nicaragua	2009	H1N1	2,172	11
Nigeria	2009	H1N1	11	2
Cyprus	2009	H1N1	297	10
Norway	2009	H1N1	12,654	29
Oman	2009	H1N1	6,349	33
Pakistan	2009	H1N1	253	25

Palestine	2009	H1N1	1,676	43
Panama	2009	H1N1	813	12
Papua New Guinea	2009	H1N1	12	0
Paraguay	2009	H1N1	855	54
Peru	2009	H1N1	9,165	223
Philippines	2009	H1N1	5,212	32
Poland	2009	H1N1	2,024	181
Portugal	2009	H1N1	166,922	122
Puerto Rico	2009	H1N1	695	44
Qatar	2009	H1N1	550	10
Romania	2009	H1N1	7,006	122
Russia	2009	H1N1	25,339	604
Rwanda	2009	H1N1	482	0
Saudi Arabia	2009	H1N1	14,500	128
Serbia	2009	H1N1	695	83
Seychelles	2009	H1N1	33	0
Singapore	2009	H1N1	1,217	21
Slovak Republic	2009	H1N1	955	56
Slovenia	2009	H1N1	990	19
Solomon Islands	2009	H1N1	4	1
South Africa	2009	H1N1	12,640	93
South Korea	2009	H1N1	107,939	250
Spain	2009	H1N1	1,538	300
Sri Lanka	2009	H1N1	642	38
Sudan	2009	H1N1	145	5
Suriname	2009	H1N1	110	2
Swaziland	2009	H1N1	5	0
Switzerland	2009	H1N1	11,221	18
Syrian Arab Republic	2009	H1N1	452	152
Sao Tome and Principe	2009	H1N1	66	2
Tajikistan	2009	H1N1	16	0
Tanzania	2009	H1N1	770	1
Thailand	2009	H1N1	31,902	249
Trinidad and Tobago	2009	H1N1	211	5
Tunisia	2009	H1N1	1,200	24
Turkey	2009	H1N1	12,316	656
Uganda	2009	H1N1	263	0
Ukraine	2009	H1N1	494	213
United Arab Emirates	2009	H1N1	125	6
United Kingdom	2009	H1N1	28,456	474
United States	2009	H1N1	113,690	3,433
Uruguay	2009	H1N1	550	33
Venezuela	2009	H1N1	2,187	135
Vietnam	2009	H1N1	11,186	58

Yemen	2009	H1N1	5,038	31
Zambia	2009	H1N1	726	0
Zimbabwe	2009	H1N1	1,318	0
Austria	2012	MERS	2	1
China	2012	MERS	1	0
Egypt	2012	MERS	1	0
France	2012	MERS	2	1
Germany	2012	MERS	3	2
Greece	2012	MERS	1	1
Iran	2012	MERS	6	2
Italy	2012	MERS	1	0
Jordan	2012	MERS	19	6
Kuwait	2012	MERS	4	2
Lebanon	2012	MERS	2	0
Malaysia	2012	MERS	2	1
Netherlands	2012	MERS	2	0
Oman	2012	MERS	11	3
Philippines	2012	MERS	3	0
Qatar	2012	MERS	19	5
Saudi Arabia	2012	MERS	2,167	804
South Korea	2012	MERS	185	38
Thailand	2012	MERS	3	0
Tunisia	2012	MERS	3	1
Turkey	2012	MERS	1	1
United Arab Emirates	2012	MERS	92	13
United Kingdom	2012	MERS	5	3
United States	2012	MERS	2	0
Yemen	2012	MERS	1	1
Guinea	2014	Ebola	3,811	2,543
Italy	2014	Ebola	1	0
Liberia	2014	Ebola	10,765	4,809
Mali	2014	Ebola	8	6
Nigeria	2014	Ebola	20	8
Senegal	2014	Ebola	1	0
Sierra Leone	2014	Ebola	14,124	3,956
Spain	2014	Ebola	1	0
United Kingdom	2014	Ebola	1	0
United States	2014	Ebola	4	1
Argentina	2016	Zika	26	0
Bahamas	2016	Zika	25	0
Barbados	2016	Zika	135	0
Belize	2016	Zika	82	0
Bolivia	2016	Zika	186	0
Brazil	2016	Zika	128,793	8

Canada	2016	Zika	468	0
Chile	2016	Zika	30	0
Colombia	2016	Zika	8,017	0
Costa Rica	2016	Zika	7,533	0
Ecuador	2016	Zika	874	0
El Salvador	2016	Zika	3	0
Guatemala	2016	Zika	822	0
Guyana	2016	Zika	33	0
Haiti	2016	Zika	3,065	0
Honduras	2016	Zika	260	0
Jamaica	2016	Zika	186	0
Nicaragua	2016	Zika	608	0
Panama	2016	Zika	756	0
Paraguay	2016	Zika	6	0
Peru	2016	Zika	778	0
Puerto Rico	2016	Zika	37,478	0
Suriname	2016	Zika	622	0
Trinidad and Tobago	2016	Zika	722	0
United States	2016	Zika	224	0
Uruguay	2016	Zika	8	0
Venezuela	2016	Zika	2	0

Notes: When available the table reports the confirmed cases and deaths. Sources: EM-DAT, Ma et al. (2020), Pan American Health Organization, PLISA Health Information Platform for the Americas, the Centers for Disease Control and Prevention (USA), the European Centre for Disease Prevention and Control, and World Health Organization.

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