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JEL Codes: G20, G59, I10.

Keywords: epidemics; fintech; banking

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Epidemic Exposure, Fintech Adoption, and the Digital Divide*

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Abstract

We ask whether epidemic exposure leads to a shift in financial technology usage within and across countries and if so 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-based activity. Using a machine-learning algorithm, we show that heterogeneity in this response centers on the age, income and employment of respondents. Young, high-income earners in full-time employment have the greatest propensity to shift to online/mobile transactions in response to epidemics. These effects are larger for individuals in subnational regions 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, to take a rather more recent example, is said to have increased remote working (**Brenan, 2020**), online shopping (**Grashuis, Skevas, and Segovia, 2020**), and telehealth (**Richardson, Aissat, Williams, Fahy, et al., 2020**).

But there may be important differences across socioeconomic groups in ability to utilize such new technologies.[1] In the case of COVID, high-tech workers and workers in the professions have been better able to shift to remote work, compared to store clerks, custodians and other less well-paid individuals (**Saad and Jones, 2021**). Women have had more difficulty than men capitalizing on opportunities to work remotely, given the occupations in which they are specialized (**Coury, Huang, Kumar, Prince, Krikovich, and Yee, 2020**). Individuals older than 65, being less technologically adaptable than the young, often find it more difficult to adjust to new work modalities (**Farrell, 2020**). Small firms with limited technological capabilities have been less able to adapt their business models and stay competitive than their larger rivals, while residents of areas with limited broadband have experienced less scope for moving to remote work, remote schooling and telehealth (**Chiou and Tucker, 2020**; **Georgieva, 2020**; **Ramsetty and Adams, 2020**). COVID-19, it is said, has accelerated ongoing trends (**OECD, 2020**; **Citigroup, 2020**). If the increasing prevalence of the so-called digital divide was an ongoing trend before COVID, then the pandemic may have accelerated this one in particular.

We study these issues in the context of fintech adoption. Specifically, we ask whether past epidemics induced a shift toward new financial technologies such as online banking 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. The novelty comes from our ability to match each individual in Global Findex dataset to detailed background information about the same individual in Gallup World Polls. This allows us to control for socioeconomic factors at the most granular level possible.

Holding constant individual-level economic and demographic characteristics and country and year fixed effects, we find that contemporaneous epidemic exposure significantly increases

[1] Thus, to continue with the case of the Black Death, **Alesina, Giuliano, and Nunn (2011)** argue that the plough, which requires strength and eliminates the need for weeding, favored male relative to female labor and generated a preference for fewer children, ultimately reducing fertility.

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 a bank branch. 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 also increasing or reducing its volume or extent as illustrated later by the placebo questions that we exploit. While 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 enduringly over time.

Extensive sensitivity analysis supports these findings. Our results continue to obtain when we adjust for the fact that we consider 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 epidemic and non-epidemic countries, report null effects on placebo outcomes, analyze epidemic intensity, implement alternative clustering techniques for standard errors, control for country-specific time trends, drop influential treatment observations from the sample, and randomize treatment countries and/or years. None of these extensions qualitatively changes our interpretations.

Using the data-driven approach suggested by **Athey and Imbens (2016)**, we then go on to identify the critical dimensions in the heterogeneity of our treatment effects. These turn out to be individual income, employment and age. It is mainly the young, high-income earners in full-time employment who take up online/mobile transactions in response to epidemics, in other words. These patterns are consistent with previous research on early adopters of other digital technologies (**Chau and Hui, 1998; Dedehayir, Ortt, Riverola, and Miralles, 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. We match 1km-by-1km time-varying data on global 3G internet coverage from Collins Bartholomew's Mobile Coverage Explorer to the sub-national region in which each individual is located. 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 employ a specification with 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 (i.e., 2G) coverage when this variable is included in the estimation side by side with our 3G measure, confirming our intuition that the relevant technology for the epidemic response is related to the internet and not to the 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 IT 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 is a particularly informative context for studying the broader question of whether past epidemics induced the adoption of new 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 of a sort that may be problematic during an epidemic) and digital 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, possess 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.

The case of banking is different, in that the diffusion and adoption of online and mobile banking have been underway since the 1990s. Individuals have been using their computers and smartphones for banking applications for years. Thus, insofar as epidemic exposure induces significant and/or persistent changes in individual 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 and persistence of the effects. **Section 6** focuses on the role of infrastructure (3G coverage). **Section 7** summarizes our additional robustness checks, after which **Section 8** 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 substantial literature on the impact of digital technologies on financial behavior. For example, **D'Andrea and Limodio (2019)** analyze the staggered rollout of fiber-optic submarine cables and associated access to high-speed internet in Africa. They show that access to 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. **Mu-**

ralidharan, Niehaus, and Sukhtankar (2016) and Aker, Boumnijel, McClelland, and Tierney (2016) find that biometric smart cards and mobile money systems facilitate the efforts of governments to provide employment and pension benefits. Bachas, Gertler, Higgins, and Seira (2018) find that debit cards, by reducing the difficulty of accessing and utilizing bank services, foster financial inclusion. Callen, De Mel, McIntosh, and Woodruff (2019) show that the availability of mobile point-of-service terminals improves formal savings options, in turn alleviating extreme poverty, encouraging self-employment, and raising market 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 the 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, Morduch, Ravindran, Shonchoy, and Zaman, 2021). We contribute to this literature by showing that when social distancing becomes a necessity, access to digital financial technology helps individuals to continue their financial activities by switching from face-to-face to remote-access options.

In this context, 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, Gholami, and Clegg, 2009; Baptista and Oliveira, 2015; Tarhini, El-Masri, Ali, and Serrano, 2016). Chen, Doerr, Frost, Gambacorta, and Shin (2021) identify 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 individuals' levels of trust, defined as the belief that others will not behave opportunistically in the digital sphere (Gu, Lee, and Suh, 2009). Finally, studies such as Breza, Kanz, and Klapper (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 certain economic and demographic subgroups, defined by age, income and employment.

A number of recent papers study the take-up and effects of financial technologies in the context of COVID-19. Kwan, Lin, Pursiainen, and Tai (2021) examine the relationship between banks' IT capacity and their 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 non-traditional 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), as distinct from 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).

Finally, there is the literature on the digital divide. WorldBank (2016) emphasizes that the benefits of new digital technologies are unevenly distributed owing to lack of high-speed internet in developing regions and countries. In the context of COVID-19, Chiou and Tucker (2020) show that the availability of high-speed internet significantly affected the ability of individuals to self-isolate during the 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 response to epidemic outbreaks between 2011 and 2017.

3. Data

Our analysis combines data from several sources. First, we use Findex to measure financial behavior in more than 140 countries. Second, Gallup World Polls (GWP) provide 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 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, we add global 3G internet access, which we observe at the micro granular geographical level. We aggregate these to the sub-national locations identified for each respondent by GWP.

3.1. Findex

Findex is a nationally representative survey fielded in some 140 countries in 2011, 2014, and 2017 (**Demirguc-Kunt and Klapper, 2012, 2013**). This is the most comprehensive data set available on how adults save, borrow, make payments and use financial technology, including mobile phones and the internet, 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 to own 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:

1. Online/Mobile transaction 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.
2. 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.
3. 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?
4. Withdrawals using ATM: When you need to get cash (paper or coins) from your account(s), do you usually get it at an ATM?
5. 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). It is important to underline the fact 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 (outcomes 6 to 10):

6. 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?
7. 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.
8. 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.
9. 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)?
10. 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 more generally.

3.2. Ma et al. Epidemic Database

Data on worldwide large-scale epidemic occurrence are drawn from Ma et al., who construct a country-panel dataset starting at the turn of the century. The authors identify and date pandemic/epidemic events using announcement dates from the World Health Organisation. According to their list, almost all countries in the world were affected by post-millennial epidemics at one time or another.[2]

The Ma et al. dataset does not contain country-specific intensity measures and thus can only be used in dichotomous form. However, the binary nature of this measure is consistent with the assumption of the exogeneity of our treatment, since occurrence of an epidemic (as opposed to its intensity) is likely to be uncorrelated with country characteristics. Nonetheless, we also analyze more and less intense epidemics separately by constructing a pair of dummy variables based on the median cases (or deaths) per capita across all epidemics during our sample period. We merge these data with the Findex-Gallup database.

3.3. Global 3G Coverage

Data on 3G mobile internet coverage are from Collins Bartholomew's Mobile Coverage Explorer, which 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 the 3G, we use 1-by-1 kilometer population data from the Gridded Population of the World for 2015, distributed by the Center for International Earth Science Information Network. To measure 3G internet access, we calculate the share of the 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 as provided in the GWP. We use this population-weighted 3G network coverage variable to capture 3G mobile internet access at the sub-regional level.

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

4. Empirical Strategy

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

$$Y_{i,c,t} = \beta_1 ExposureToEpidemic_{c,t} + \beta_2 X_{i,c,t} + \beta_3 C_c + \beta_4 T_t + \varepsilon_{i,c,t} \tag{1}$$

where $Y_{i,c,t}$ 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 during our sample period.

[2]The authors enumerate 290 country-year pandemic/epidemic observations since the turn of the century. See **Online Appendix B** for the detailed list.

The coefficient of interest is β_1. 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.[3]

To control for the effects of demographic and labor market structure, we include the following in the $X_{i,c,t}$ 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. We also include as country-level time-varying regressors GDP per capita and bank deposits relative to GDP; these variables capture economic and financial development across countries and over time.

To account for unobservable characteristics, we include fixed effects at the levels of country (C_c) and year (T_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. The year dummies control for global shocks that affect all countries simultaneously.

In further robustness checks we include interactive country-times-income decile, country-times-labor-market status, and country-times-education fixed effects. 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 using a bank branch. The five columns, moving left to right, report regressions including an increasingly comprehensive set of controls.[4]

Exposure to an epidemic in the current year significantly increases the likelihood that a

[3] In **Appendix Table 5**, we show that the occurrence of epidemics is indeed uncorrelated with country-level characteristics.

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

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 the likelihood of using ATMs vs. branches.[5] In our preferred model (Column 5), exposure to an epidemic leads to 10.6 (4.5) percentage point increase in online/mobile transactions using the internet and bank account (mobile transaction using bank account). Given that the means of these outcome variables are 8.3 (9.4) percent, the effect is sizeable.

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 (2019)** to investigate the importance of unobservables.[6] 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, that would be needed in order 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 we include 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, and because this could generate false positives purely by chance, we follow **Anderson (2008)** in computing false discovery rates (FDRs). The FDR calculates the expected proportion of rejections that are type I errors and gener-

[5] 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.

[6] Estimation bounds on the treatment effect range between the coefficient from the main specification and the coefficient estimated under the assumption that observables are as important as unobservables 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.

ates an adjusted p-value (i.e., sharpened q-value) for each corresponding estimate. As seen beneath each estimate (in brackets) in **Table 1**, findings do not change when we employ this method; in fact the statistical significance of the estimates based on these adjusted p-values is usually higher than those indicated by standard p-values.

Finally, we investigate whether overall financial inclusion and levels of banking activity differ in countries experiencing an epidemic, since such geographical heterogeneity could drive differences in choice of banking technologies in our sample. When testing for an impact on financial behavior where face-to-face and electronic transactions are not alternatives, we should not observe a shift in behavior in response to epidemics. Thus, this can thus be seen as a placebo test confirming that the effects in our setting arise only when a priori this is supposed to be the case.

The additional dependent variables here are whether the individual (i) owns an account, (ii) deposited money into a personal account in a typical month (including online), (iii) withdrew money from a personal account in a typical month (including 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 has no impact on financial inclusion and activity, but only on the form – electronic or in-person – that such activity takes.

5.1. Heterogeneity

To identify heterogeneous treatment effects (variation in the direction and magnitude of effects across individuals within the population), 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 very little predictive power, it is assigned a negative importance score, which is essentially 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 are the important dimensions of treatment heterogeneity. Therefore, we re-estimate our main specification (Column 5 in **Table 1**) when 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 richer 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 (**Friedberg, 2003; Schleife, 2006**), while less-well-off individuals often have less exposure or access to such technology.

5.2. Event Study Estimates and Persistence

Given that Findex is available for only three cross-sections spanning seven years, any investigation of persistence is necessarily tentative. As a start, we repeated the analysis for individuals in countries exposed to an epidemic in the year immediately preceding the survey, and again two years preceding the survey.[7] To investigate pre-existing trends in the outcomes of interest, we also tested for changes in behavior in years prior to the exposure.

Panel A of **Figure 2** 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). It does not appear from this analysis that the change in behavior persists beyond the epidemic year.

6. Role of Infrastructure

Infrastructure weaknesses may hinder digital transactions and limit any epidemic-induced shift in behavior (see the studies cited in Section 2). We therefore add to our specification a measure of within-country subregional 3G coverage, 3G being the relevant threshold, since 2G allows only for mobile phone calls and text messages but not internet browsing.[8]

This 3G variable represents the portion of the 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 the first-order effect of mobile internet coverage. **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.

[7]We are careful not to overinterpret this result, since this past epidemic may not necessarily be the same as the one captured by our contemporaneous event 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 heterogenous across different epidemic events in our sample, this type of analysis should be interpreted with caution.

[8]In **Appendix Table 6**, we confirm that 2G internet access has no impact on our outcomes when it is interacted with epidemic exposure.

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 the same or similar online banking services throughout a country, this non-parametrically controls for supply-related factors. It focuses instead on within-country variation in online banking that is more likely to be driven by demand-related shocks. This also ensures that our estimates are not driven by any other country-specific time-varying unobservables.

A further concern is that epidemics may induce changes in 3G coverage in a region, for example via signal failures if the maintenance of local services is adversely affected by the public health emergency.[9] To make sure that subregional 3G coverage is not affected by epidemics, we follow two strategies. 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.

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; it is statistically significant only when we exclude individual controls. But the effect interacted with epidemic exposure is large and statistically significant at conventional confidence levels. Again, these results do not change if we use the Oster test for potential omitted variable bias or adjust the p-values for the presence of multiple models. According to the most conservative regression, where we observe both the baseline and interacted coefficients (Column 5 in the middle panel), the impact of epidemic exposure on the propensity to make transactions 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 mediating effect of 3G infrastructure persisting beyond the period of epidemic exposure, nor of the effect emerging prior to the epidemic shock.[10]

[9] This would result in multicollinearity in our estimates.

[10] Again, this means that our data satisfy the parallel trends assumption.

7. Additional Analysis and Robustness Checks

Additional analyses, reported in the **Online Appendix**, document the robustness of our findings. These include: (i) distinguishing treatment effects of high- and low-intensity epidemics; (ii) clustering standard errors at the level of different global regions; and (iii) controlling for country-specific linear time trends; (iv) conducting falsification analyses; (v) conducting balance tests to show that occurrence of epidemics is uncorrelated with the country characteristics; (vi) ruling out influential treatments and observations.

8. Conclusion

We have documented the tendency for individuals to turn to online and mobile banking when 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 relatively 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.

These findings remind one that the COVID-19 pandemic has been felt unevenly: that the poorer portion of populations has disproportionately suffered its economic and health effects, and that 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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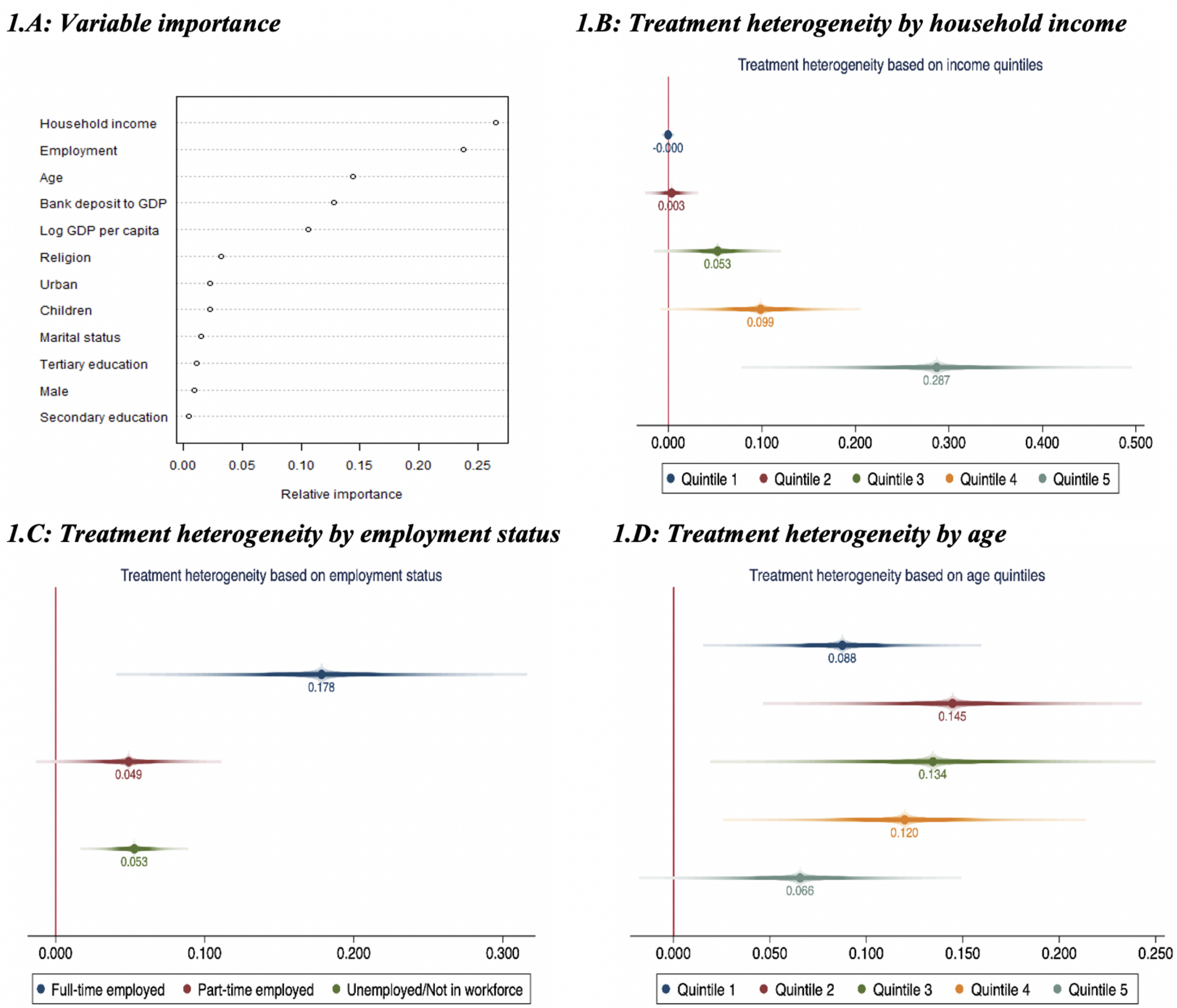
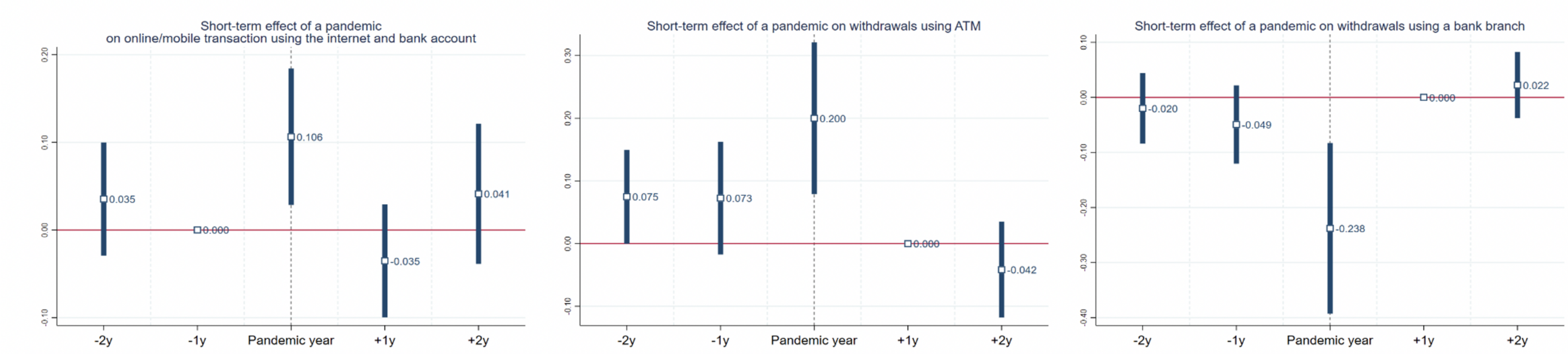


Fig. 1. **Heterogeneity Analysis using Causal Forest.** Figure A illustrates the variable importance for "Exposure to epidemic" in a causal forest framework (N=20,000 trees), which provides insights into the nature of the relationship between our treatment effect and other covariates. Figures B, C and 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.

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



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

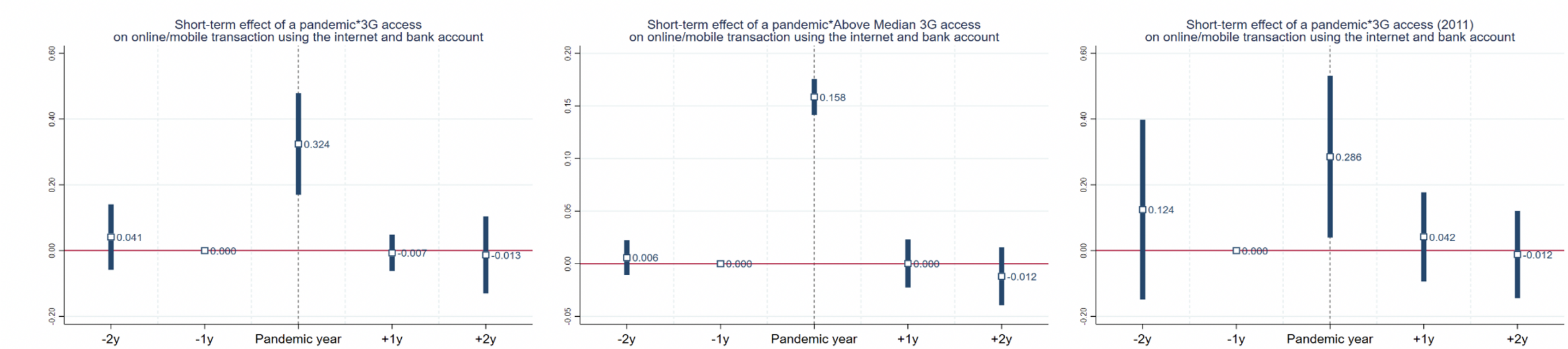


Fig. 2. **Event Study Estimates.** *Panel A:* Outcomes are "online/mobile transaction using the internet and bank account", "withdrawals using ATM", and "withdrawals using a bank branch". Event study estimates are based on the specification in Column 5 of **Table 1**. In particular, we repeat the exercise 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:* 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 (separately for continuous measure, above median 3G coverage, and time-invariant 3G coverage -as of year 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.

	(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

Table 1: **The Impact of an Epidemic Year on Financial Technology Adoption.** * significant at 10%; ** significant at 5%; *** significant at 1%. 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.

	(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

Table 2: **The Impact of an Epidemic Year on Financial Technology Adoption – Placebo Outcomes.** * significant at 10%; ** significant at 5%; *** significant at 1%. 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.

	(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

Table 3: **The Impact of an Epidemic Year on Financial Technology Adoption – Role of 3G Internet Infrastructure.** * significant at 10%; ** significant at 5%; *** significant at 1%. 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.

Online Appendix for

Epidemic Exposure, Fintech Adoption and the Digital Divide

Orkun Saka, Barry Eichengreen, Cevat Giray Aksoy

Online Appendix A

Robustness checks

In this section we report further analyses establishing the robustness of our findings. We start by summarizing the characteristics of our sample in **Appendix Table 1**.

Are more intense epidemics different?

In **Appendix Table 2**, we re-estimate our baseline model where we use indicators for the high intensity epidemics (above within-sample-median deaths per capita) and low intensity epidemics (below within-sample-median deaths per capita) in the same estimation. The effects we identify are larger for high intensity epidemics. We repeat the analysis by using cases per capita as a measure of epidemic intensity and find qualitatively identical results (available upon request).

Robustness to alternative levels of clustering

In our main specification, we cluster the standard errors at the country level. We establish robustness of our results using alternative assumptions about the variance-covariance matrix: the results are robust to clustering at global region-year level (assuming that residuals co-move within these units) and clustering only at global region level (see Columns 1 and 2 of **Appendix Table 3**).

Robustness to controlling for country-specific linear time trends

We control for country-specific linear time trends, which allow us to remove distinctive trends in fintech adoption in various countries that might otherwise bias our estimates if they accidentally coincided with epidemic-related changes. Despite the short time dimension of our dataset (i.e., only three years covered), our results remain robust (see Column 3 of **Appendix Table 3**).

Falsification

We conduct two falsification exercises by creating placebo treatment variables. In the first one (see Column 1 of **Appendix Table 4**), we keep the same epidemic year for a given epidemic event but randomly choose a different country from the same continent as the original country where the epidemic actually took place. In the second one (see Column 2 of **Appendix Table 4**), we randomize both the epidemic country and the year for each epidemic event in our sample. Placebo treatment variables created via these two different strategies both result in estimates that are statistically indistinguishable from zero.

Balance Test

As discussed in Section 4, 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. To validate this argument, we provide direct evidence in **Appendix Table 5**. In particular, we use three outcome variables (epidemic occurrence, high intensity epidemics and low intensity epidemics). As country level covariates, we consider GDP (current USD), urban population as a share of total pop. as well as several other variables that measure the financial development level of countries. Odd numbered columns report the estimates without country and year fixed effects, while even even numbered columns report the estimates with country and year fixed effects.

In line with our identification assumption, almost none of the estimates is statistically significant (only 1 out of 66 coefficients are significant at the 10 percent level). Overall, the results presented in **Appendix Table 5** show that the occurrence of epidemics is exogenous to country-level economic or financial characteristics.

2G Internet Access as a Placebo Treatment

We also check whether the previous-generation mobile networks (2G), which is qualitatively different from the mobile broadband internet (3G), matter for financial technology usage. In particular, we follow the structure of **Table 3** but also include 2G coverage as a placebo treatment in **Appendix Table 6**. In contrast to the effect of 3G, the 2G networks has no consistent impact on our outcomes when it is interacted with epidemic exposure. These results suggest that 3G infrastructure, as distinct from the mobile phone or general communication technology, is the relevant one in the context of our study.

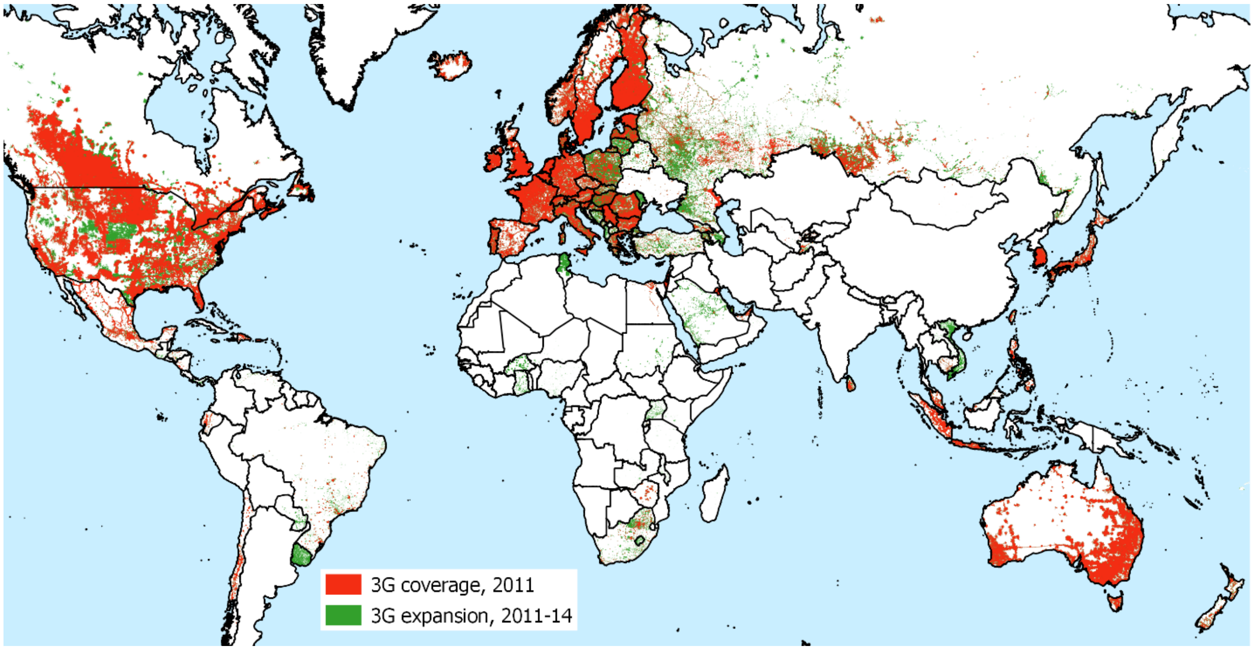
Ruling Out Influential Treatments and Observations

We rule out the importance of influential treatments by excluding one treatment country at a time. **Appendix Table 7** shows that our coefficient estimates are quite stable even as each treated country is eliminated ("turned off ") in our treatment dummy in every iteration.

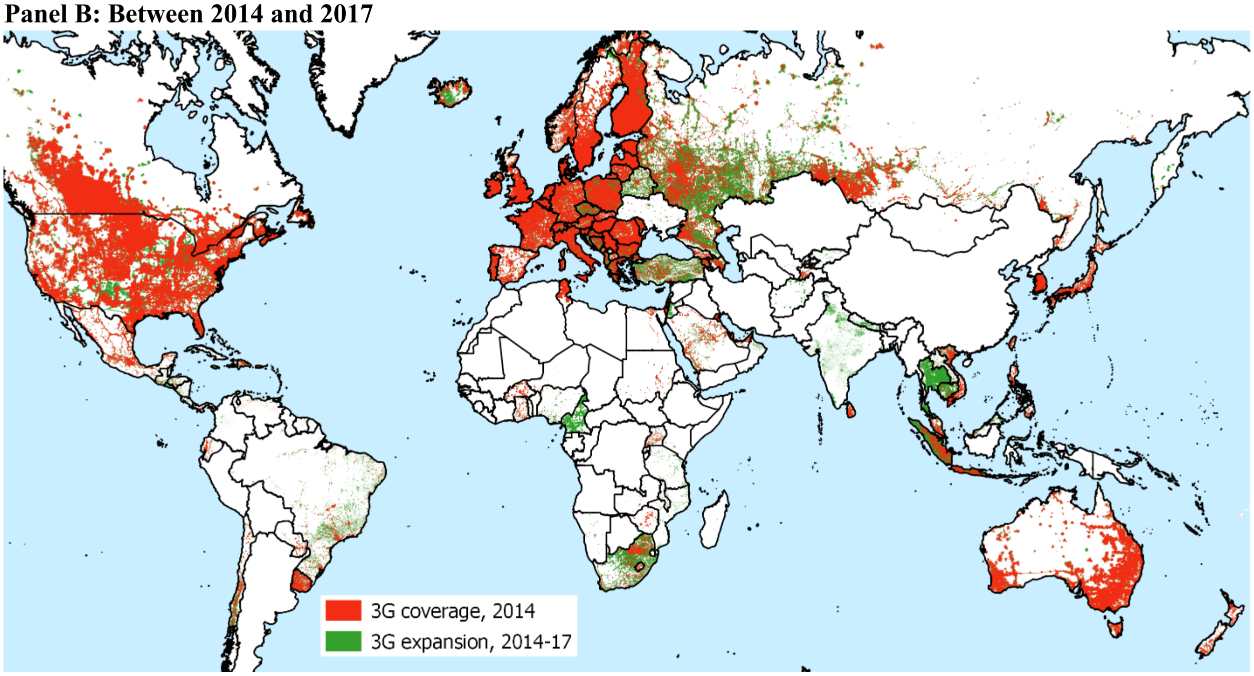
We repeat a similar analysis with **Appendix Table 8** in which we drop one treated country at a time in each estimation for 10 consecutive trials and again find that our estimates are not driven by any single country.

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.

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

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%.

Appendix Table 3: 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%.

Appendix Table 4: 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%.

Appendix Table 5: Balance Test – Country-level characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome ➔	Epidemic occurrence	Epidemic occurrence	High intensity epidemics	High intensity epidemics	Low intensity epidemics	Low intensity epidemics
GDP current USD (log)	0.004	0.041	0.000	0.060	0.003	-0.018
	(0.004)	(0.080)	(0.003)	(0.073)	(0.002)	(0.036)
Urban population as a share of total pop. (log)	-0.001	0.006	0.002	0.051	-0.003	-0.045
	(0.011)	(0.445)	(0.008)	(0.044)	(0.007)	(0.102)
Account at a formal financial inst. (% age 15+) (log)	-0.015	0.033	-0.005	0.035	-0.010	0.039
	(0.016)	(0.027)	(0.012)	(0.022)	(0.011)	(0.025)
ATMs per 100,000 adults (log)	-0.002	0.033	-0.004	0.016	-0.002	0.016
	(0.011)	(0.027)	(0.007)	(0.011)	(0.009)	(0.025)
Financial system deposits to GDP (%) (log)	-0.028	-0.068	-0.011	-0.048	-0.016	-0.020
	(0.016)	(0.131)	(0.010)	(0.129)	(0.012)	(0.031)
Private credit by deposit money banks to GDP (%) (log)	-0.006	0.068	-0.006	-0.081	-0.000	-0.013
	(0.013)	(0.137)	(0.008)	(0.140)	(0.008)	(0.025)
Deposit money banks' assets to GDP (%) (log)	0.025	-0.012	0.001	-0.037	0.023	0.025
	(0.017)	(0.047)	(0.006)	(0.047)	(0.016)	(0.028)
Bank net interest margin (%) (log)	-0.047	-0.039	-0.015	0.000	-0.031	-0.039
	(0.024)	(0.035)	(0.014)	(0.012)	(0.019)	(0.033)
Bank overhead costs to total assets (%) (log)	0.029	-0.001	-0.004	-0.018	0.024	0.016
	(0.019)	(0.035)	(0.007)	(0.029)	(0.017)	(0.022)
Bank Z-score	-0.000	0.005	0.000	0.002	0.000	0.003
	(0.000)	(0.005)	(0.000)	(0.002)	(0.000)	(0.005)
Lerner index	-0.000*	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	356	356	356	356	356	356
Country fixed effects	No	Yes	No	Yes	No	Yes
Year fixed effects	No	Yes	No	Yes	No	Yes

Source: World Bank and Ma et al. (2020) Epidemics Database. * significant at 10%; ** significant at 5%; *** significant at 1%. 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.*

Appendix Table 6: 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%.

Appendix Table 7: 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%.

Appendix Table 8: 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 B

Full List of Epidemics from the Ma et al. (2020) Dataset

Country	Year of the epidemic
Afghanistan	2009
Albania	2009
Algeria	2009, 2012
Angola	2009
Argentina	2009, 2016
Armenia	2009
Australia	2003, 2009
Austria	2009, 2012
Azerbaijan	2009
Bahamas	2016
Bahrain	2009
Bangladesh	2009
Barbados	2009, 2016
Belarus	2009
Belgium	2009
Belize	2016
Bhutan	2009
Bolivia	2009, 2016
Bosnia and Herzegovina	2009
Botswana	2009
Brazil	2009, 2016
Brunei Darussalam	2009
Bulgaria	2009
Burundi	2009
Cambodia	2009
Cameroon	2009
Canada	2003, 2009, 2016
Cape Verde	2009
Chad	2009
Chile	2009, 2016
China	2003, 2009, 2012
Colombia	2009, 2016
Congo Brazzaville	2009
Congo Kinshasa	2009
Costa Rica	2009, 2016
Croatia	2009
Cuba	2009
Czech Republic	2009
Djibouti	2009

Dominican Republic	2009
Ecuador	2009, 2016
Egypt	2009, 2012
El Salvador	2009, 2016
Estonia	2009
Ethiopia	2009
Fiji	2009
Finland	2009
France	2003, 2009, 2012
Gabon	2009
Georgia	2009
Germany	2003, 2009, 2012
Ghana	2009
Greece	2009, 2012
Guatemala	2009, 2016
Guinea	2014
Guyana	2009, 2016
Haiti	2016
Honduras	2009, 2016
Hong Kong	2003
Hungary	2009
Iceland	2009
India	2003, 2009
Indonesia	2003, 2009
Iran	2009
Iran	2012
Iraq	2009
Ireland	2003, 2009
Israel	2009
Italy	2003, 2009, 2012, 2014
Ivory Coast	2009
Jamaica	2009, 2016
Japan	2009
Jordan	2009, 2012
Kazakhstan	2009
Kenya	2009
Kuwait	2003, 2009, 2012
Lao People's Democratic Republic	2009
Lebanon	2009, 2012
Lesotho	2009
Liberia	2014
Libya	2009
Lithuania	2009
Luxembourg	2009

China, Macao SAR	2003
Macedonia, FYR	2009
Madagascar	2009
Malawi	2009
Malaysia	2003, 2009, 2012
Mali	2009, 2014
Malta	2009
Mauritius	2009
Mexico	2009
Moldova	2009
Mongolia	2003, 2009
Montenegro	2009
Morocco	2009
Mozambique	2009
Myanmar	2009
Namibia	2009
Nepal	2009
Netherlands	2009, 2012
New Zealand	2003, 2009
Nicaragua	2009, 2016
Nigeria	2009, 2014
Cyprus (Greek)	2009
Norway	2009
Oman	2009, 2012
Pakistan	2009
Palestine	2009
Panama	2009, 2016
Papua New Guinea	2009
Paraguay	2009, 2016
Peru	2009, 2016
Philippines	2003, 2009, 2012
Poland	2009
Portugal	2009
Puerto Rico	2009, 2016
Qatar	2009, 2012
Romania	2003, 2009
Russia	2003, 2009
Rwanda	2009
Saudi Arabia	2009, 2012
Senegal	2014
Serbia	2009
Seychelles	2009
Sierra Leone	2014
Singapore	2003, 2009

Slovak Republic	2009
Slovenia	2009
Solomon Islands	2009
South Africa	2003, 2009
South Korea	2003, 2009, 2012
Spain	2003, 2009, 2014
Sri Lanka	2009
Sudan	2009
Suriname	2009, 2016
Swaziland	2009
Sweden	2003
Switzerland	2003, 2009
Syrian Arab Republic	2009
Sao Tome and Principe	2009
Taiwan	2003
Tajikistan	2009
Tanzania	2009
Thailand	2003, 2009, 2012
Trinidad and Tobago	2009, 2016
Tunisia	2009
Tunisia	2012
Turkey	2009, 2012
Uganda	2009
Ukraine	2009
United Arab Emirates	2009, 2012
United Kingdom	2003, 2009, 2012, 2014
United States	2003, 2009, 2012, 2014, 2016
Uruguay	2009, 2016
Venezuela	2009, 2016
Vietnam	2003, 2009
Yemen	2009, 2012
Zambia	2009
Zimbabwe	2009

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