City Research Online

City, University of London Institutional Repository

Citation: Saka, O. ORCID: 0000-0002-1822-1309, Eichengreen, B. and Aksoy, C. G. (2021). Epidemic Exposure, Financial Technology, and the Digital Divide (21/03). London, UK: Department of Economics, City, University of London.

This is the published version of the paper.

This version of the publication may differ from the final published version.

Permanent repository link: https://openaccess.city.ac.uk/id/eprint/26917/

Link to published version: 21/03

Copyright: City Research Online aims to make research outputs of City, University of London available to a wider audience. Copyright and Moral Rights remain with the author(s) and/or copyright holders. URLs from City Research Online may be freely distributed and linked to.

Reuse: Copies of full items can be used for personal research or study, educational, or not-for-profit purposes without prior permission or charge. Provided that the authors, title and full bibliographic details are credited, a hyperlink and/or URL is given for the original metadata page and the content is not changed in any way.

City Research Online: http://openaccess.city.ac.uk/ publications@city.ac.uk

Department of Economics

Epidemic Exposure, Financial Technology, and the Digital Divide

Orkun Saka[1]
City, University of London, London School of Economics,
STICERD & Systemic Risk Centre, CESifo network

Barry Eichengreen
University of California, Berkeley, National Bureau of Economic Research
Centre for Economic Policy Research

Cevat Giray Aksoy
European Bank for Reconstruction and Development (EBRD), Kings's College London

Department of Economics
Discussion Paper Series
No. 21/03

[1] Corresponding author: address Department of Economics, City, University of London, Northampton Square, London EC1V 0HB, UK.
E-mail: o.saka@city.ac.uk

Epidemic Exposure, Financial Technology, and the Digital Divide*

Orkun Saka ⓡ Barry Eichengreen ⓡ Cevat Giray Aksoy

September, 2021

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. Heterogeneity in response centers on the age, income and employment of respondents. Young, high-income earners in full-time employment have the greatest tendency to shift to online/mobile transactions in response to epidemics. These 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 classification: G20, G59, I10.

Keywords: epidemics; fintech; banking.

*ⓡ All authors contributed equally to this manuscript and the order of author names is randomized via AEA Randomization Tool (code: AuWT141_jCPw). Saka (o.saka@city.ac.uk) is an Assistant Professor of Economics at the City, University of London, Visiting Fellow at the London School of Economics, Research Associate at the STICERD & Systemic Risk Centre and Affiliate at the CESifo network. Eichengreen (eichengr@berkeley.edu) is a Professor of Economics and Political Science at the University of California, Berkeley, Research Associate at the National Bureau of Economic Research and Research Fellow at the Centre for Economic Policy Research. Aksoy (aksoyc@ebrd.com) is a Principal Economist at the European Bank for Reconstruction and Development (EBRD), Assistant Professor of Economics at King's College London and Research Associate at IZA Institute of Labour Economics. We are grateful to seminar participants at Bank of Finland, Webinar series in Finance and Development (WEFIDEV), 2nd LTI@UniTO/Bank of Italy Conference on "Long-Term Investors' Trends: Theory and Practice", MIT-CEBRA 2021 Annual Meeting, IFABS 2021 as well as Carol Alexander, Ralph De Haas, Jonathan Fu, Zuzana Fungacova, Sean Higgins, Thomas Lambert, Xiang Li, Nicola Limodio and Enrico Sette (discussant) for their useful comments and suggestions. Leon Bost, Franco Malpassi, and Pablo Zarate provided outstanding research assistance. Views presented are those of the authors and not necessarily those of the EBRD. All interpretations, errors, and omissions are our own. Saka: https://orcid.org/0000-0002-1822-1309. Eichengreen: https://orcid.org/0000-0002-1683-4486. Aksoy: orcid.org/0000-0002-8318-6618.

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, Skevas, and Segovia, 2020**), and telehealth (**Richardson, Aissat, Williams, Fahy, et al., 2020**).

But there can be important differences across socioeconomic groups in ability to utilize such new technologies.[1] In the case of COVID, high-tech workers and individuals in the professions have been better able to shift to remote work, compared to store clerks, home health-care assistants, custodians and other less well-paid individuals (**Saad and Jones, 2021**). Women have had more difficulty capitalizing on opportunities to work remotely given the occupations in which they are specialized (**Coury, Huang, Kumar, Prince, Krikovich, and Yee, 2020**). Older individuals, being less technologically savvy, often find it harder to adjust to new work modalities (**Farrell, 2020**). Small firms with limited technological capability have been less able to adapt their business models and stay competitive, 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, accelerated ongoing trends (**OECD, 2020**; **Citigroup, 2020**). If 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 and usage.[2] Specifically, 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

[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.

[2] We interchangeably use the terms "fintech adoption" and "fintech usage" throughout the paper. As will be clear later, our measures of financial technology adoption and usage at the individual level are binary variables 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.

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. 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 as illustrated later by the placebo questions that we exploit. The limited time span covered by our data allows for only a tentative analysis of persistence, but our results suggest that the impact of epidemic exposure is felt mainly in the short run rather than persistently over time.

Sensitivity analyses support these findings. The results continue to obtain 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 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 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 documented as individual income, employment and age. It is mainly young, high earners in full-time employment who take up online/mobile transactions in response to epidemics, in other words. These patterns are consistent with other research on early adopters of similar 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 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 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 (Global System for Mobile Communication, or

2G, the older radio system used in cellphones, which 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 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.

Due to data availability, the time dimension in our analysis is restricted to the years of 2011, 2014 and 2017. This prevents us from tracing the effects of a specific epidemic event over consecutive periods. Subject to this caveat, we compare outcomes across countries struck and not struck by epidemic events in the past or similary due to be struck in the future. None of these comparions generate significant differences. This finding is consistent with the parallel-trends assumption necessary for a causal interpretation of our difference-in-differences results. It also implies a contemprenous response to an epidemic event but little if any persistence.

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. Moreover, the fact that in-person and remote banking activities already existed side-by-side makes it more likely that the access to new technologies during epidemics operates through changing individuals' variable cost-benefit calculations, and less likely that individuals have to sink a fixed cost (learning for the first time how to use an ATM or a smartphone-enabled banking app) in order to suddenly avoid in-person contact. Similarly, that individuals using remote-access alternatives are already familiar with in-person banking plausibly makes it more likely that the behaviour shift may be reversed in the future (that the epidemic-induced change is not persistent), which is what we observe in the data.

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 associated access to high-speed internet in Africa, showing 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. **Muralidharan, Niehaus, and Sukhtankar (2016)** and **Aker, Boumnijel, McClelland, and Tierney (2016)** find that biometric smart cards and mobile money systems facilitate governmental efforts to distribute 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 mobile point-of-service terminals improves 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, 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 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, 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 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 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 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. **WorldBank (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, Rogers, and Zhou (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.

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 provides information on saving, borrowing, payments and use of financial technology, including mobile phones and/or the 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:

1. 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.
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). 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), exact income level, labor market status, and within-country income deciles as a proxy for social status.

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

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 generally.

3.2. 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 pandemic waves originally identified by Jamison, Gelband, and Horton (2017): SARS in 2003, H1N1 in 2009, MERS in 2012, Ebola in 2014 and Zika in 2016. They date epidemic events in each country by 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.[3]

The Ma et al. dataset does not contain country-specific intensity measures and must therefore be used in dichotomous form. This binary measure is consistent with the assump-

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

tion of the exogeneity of our treatment, since occurrence of an epidemic (as opposed to its intensity) is likely to be uncorrelated with country characteristics.[4] Nonetheless, we also analyze more and less severe epidemics separately by constructing dummy variables based on the above/below median infection cases (or deaths) per capita across all epidemics during our sample period for which we manually collect the information from Emergency Events (EM-DAT, 2021) database of the Centre for Research on the Epidemiology of Disasters and supplementary sources. We merge these data with the Findex-Gallup database.

3.3. 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 the Center for International Earth Science Information Network.[5] 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.

Appendix Table 1 reports 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 usage 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

[4] 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.

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

capturing whether a country experienced an epidemic in a year during our sample period. 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.[6]

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.

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.[7] The year dummies control for global shocks that affect all countries simultaneously. We also include as country-level time-varying regressors GDP per capita and bank deposits relative to GDP; these variables capture economic and financial development across countries and over time.

In further robustness checks we add 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 at a bank branch. The five columns, moving left to

[6] In **Appendix Tables 5 and 6**, we show that the occurrence of epidemics is indeed uncorrelated with country-level characteristics.

[7] 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).

right, report regressions with increasingly comprehensive sets of controls.[8]

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.[9] 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.[10] 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, and because this could generate false positives

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

[9] 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.

[10] 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.

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

We also considered placebo tests – tests for changes in financial behaviors other than the choice between in-person and remote-access transactions. The additional dependent variables here are whether the individual (i) owns an account, (ii) deposited money into a personal account in a typical month (either in person or online), (iii) withdrew money from a personal account in a typical month (either in person or online), (iv) owned a debit card, and (v) owned a credit card. The results, in **Table 2**, are reassuring. They show insignificant effects, small coefficients and no uniform pattern of signs. An interpretation is that epidemic exposure affects the form – remote access or in person – of financial activity but not its level, and that it has no obvious impact on financial inclusion.[11]

5.1. Heterogeneity

To identify heterogeneous treatment effects across different types of individuals in our sample, 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.

Here household income, employment, and age are the important dimensions of treatment heterogeneity. We therefore re-estimate our main specification (Column 5 in **Table 1**) restricting the sample to each categorical domain. Results are in **Figures 1.B, 1.C** and **1.D**. The average treatment effect is driven by individuals with annual incomes above $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

[11]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.

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

Because Findex is only available for three cross-sections spanning seven years, any investigation of persistence is necessarily 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.[12] To investigate pre-existing trends in the outcomes of interest, we also tested 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.[13] 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). Nor does it appear from this analysis that the change in behavior persists beyond the epidemic year.

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

[12] We are careful not to overinterpret 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 heterogenous across different types of epidemics in our sample, this type of analysis should be interpreted with caution.

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

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.

5.3. Role of Infrastructure

Infrastructure weaknesses may hinder digital transactions and limit epidemic-induced shift in behavior (as suggested by studies cited in Section 2). We therefore add to our specification a measure of within-country subregional 3G coverage. 3G is indeed the relevant technological threshold, since 2G allows only for mobile phone calls and text messages but not internet browsing.[14]

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. **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. It focuses instead on within-country-year variation in online banking that is more likely to be driven by demand-related shocks. This also ensures that our estimates are also not driven by any 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.[15] 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

[14]In **Appendix Table 7**, we confirm that 2G internet access has no impact on our outcomes when it is interacted with epidemic exposure.

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

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 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 statistically significant at conventional confidence levels. Again, these results survive the Oster test for potential omitted variable bias and when we adjust the 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.[16]

6. Additional Analysis and Robustness Checks

6.1. Are more intense epidemics different?

We can re-estimate our model with separate binary treatment indicators for high and low intensity epidemics. We calculate the number of people (as a share of the population) infected in each epidemic event by manually collecting the relevant data from EM-DAT database and supplementary sources and use the median value as our threshold. **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.[17]

6.2. 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

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

[17] 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.

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 **Appendix Table 3**.

6.3. 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 **Appendix Table 3**)

6.4. 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 **Appendix Table 4**) 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 **Appendix Table 4**) 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.

6.5. 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 **Appendix Tables 5** and **6**. In particular, we estimate the following country-year level specification in **Appendix Table 5**:

$$ExposureToEpidemic_{c,t} = \beta_1 X_{c,t} + \beta_2 C_c + \beta_3 T_t + \varepsilon_{c,t} \tag{2}$$

"Exposure to epidemic" is an indicator variable capturing whether a country experienced an epidemic in a year (i.e., our treatment variable in **Equation 1**). $X_{c,t}$ 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 **Appendix Table 5** 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 we include in the analysis is correlated with epidemic occurrence.

Columns 1 and 2 of **Appendix Table 6** 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 **Appendix Tables 5** and **6** are consistent with the assumption that the occurrence of epidemics is plausibly exogenous to country-level characteristics.

6.6. 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. **Appendix Table 7** 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.

6.7. *Ruling out influential treatments*

We rule out the importance of influential treatments by excluding one treatment country at a time.[18] This means we turn off the treatment for a specific country where it is assumed not to have been exposed to an epidemic at all. **Appendix Table 8** 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 **Appendix Table 9** but drop one country at a time (with all its observations in our sample) in each estimation for 10 consecutive trials. Again we find that the estimates are not driven by a single country.

7. 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 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.

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.

[18] For this purpose, we focus only on those countries that drive our contemporaneous treatment variable.

References

Aker, J. C., Boumnijel, R., McClelland, A., Tierney, N., 2016. Payment mechanisms and antipoverty programs: Evidence from a mobile money cash transfer experiment in Niger. Economic Development and Cultural Change 65, 1–37.

Al-Somali, S. A., Gholami, R., Clegg, B., 2009. An investigation into the acceptance of online banking in Saudi Arabia. Technovation 29, 130–141.

Alesina, A., Giuliano, P., Nunn, N., 2011. Fertility and the plough. American Economic Review 101, 499–503.

Anderson, M. L., 2008. Multiple inference and gender differences in the effects of early intervention: A reevaluation of the abecedarian, perry preschool, and early training projects. Journal of the American statistical Association 103, 1481–1495.

Athey, S., Imbens, G., 2016. Recursive partitioning for heterogeneous causal effects. Proceedings of the National Academy of Sciences 113, 7353–7360.

Bachas, P., Gertler, P., Higgins, S., Seira, E., 2018. Digital financial services go a long way: Transaction costs and financial inclusion. In: *AEA Papers and Proceedings*, vol. 108, pp. 444–48.

Baptista, G., Oliveira, T., 2015. Understanding mobile banking: The unified theory of acceptance and use of technology combined with cultural moderators. Computers in Human Behavior 50, 418–430.

Brenan, M., 2020. Covid-19 and remote work: An update. Gallup (13 October 2020) https://news.gallup.com/poll/321800/covid-remote-work-update.aspx.

Breza, E., Kanz, M., Klapper, L. F., 2020. Learning to navigate a new financial technology: Evidence from payroll accounts. NBER Working Paper No.28249 .

Callen, M., De Mel, S., McIntosh, C., Woodruff, C., 2019. What are the headwaters of formal savings? Experimental evidence from Sri Lanka. The Review of Economic Studies 86, 2491–2529.

Chau, P. Y., Hui, K. L., 1998. Identifying early adopters of new it products: A case of Windows 95. Information & Management 33, 225–230.

Chen, S., Doerr, S., Frost, J., Gambacorta, L., Shin, H. S., 2021. The fintech gender gap. CEPR Discussion Paper No.16270 .

Chiou, L., Tucker, C., 2020. Social distancing, internet access and inequality. NBER Working Paper No.26982 .

Citigroup, 2020. Will covid-19 lead to accelerating trends? Citi GPS (6 April 2020) https://www.citivelocity.com/citigps/will-covid-19-lead-to-accelerating-trends/.

Core, F., De Marco, F., 2021. Public guarantees for small businesses in Italy during covid-19. CEPR Discussion Paper No.15799 .

Coury, S., Huang, J., Kumar, A., Prince, S., Krikovich, A., Yee, L., 2020. Women in the workplace. McKinsey/Leanin.org (September 2020) https://www.mckinsey.com/featured-insights/diversity-and-inclusion/women-in-the-workplace.

D'Andrea, A., Limodio, N., 2019. High-speed internet, financial technology and banking in Africa. BAFFI CAREFIN Centre Research Paper .

Dedehayir, O., Ortt, R. J., Riverola, C., Miralles, F., 2017. Innovators and early adopters in the diffusion of innovations: A literature review. International Journal of Innovation Management 21, 1–27.

Demirgüç-Kunt, A., Klapper, L., 2013. Measuring financial inclusion: Explaining variation in use of financial services across and within countries. Brookings Papers on Economic Activity 2013, 279–340.

Demirgüç-Kunt, A., Klapper, L. F., 2012. Measuring financial inclusion: The global findex database. World Bank Policy Research Working Paper .

EM-DAT, 2021. The international disaster database. Tech. rep., Centre for Research on the Epidemiology of Disasters, Brussels: Centre for Research on the Epidemiology of Disasters, https://www.emdat.be/database.

Erel, I., Liebersohn, J., 2020. Does fintech substitute for banks? Evidence from the paycheck protection program. NBER Working Paper No.27659 .

Farrell, C., 2020. How the coronavirus punishes many older workers. PBS (7 May) https://www.pbs.org/wnet/chasing-the-dream/stories/how-coronavirus-punishes-older-workers/.

Friedberg, L., 2003. The impact of technological change on older workers: Evidence from data on computer use. ILR Review 56, 511–529.

Fu, J., Mishra, M., 2020. Fintech in the time of covid-19: Trust and technological adoption during crises. Swiss Finance Institute Research Paper .

Georgieva, K., 2020. The global economic reset – promoting a more inclusive recovery. IMF Blog (11 June) https://blogs.imf.org/2020/06/11/the-global-economic-reset-promoting-a-more-inclusive-recovery/.

Grashuis, J., Skevas, T., Segovia, M. S., 2020. Grocery shopping preferences during the covid-19 pandemic. Sustainability 12, 5369.

Gu, J.-C., Lee, S.-C., Suh, Y.-H., 2009. Determinants of behavioral intention to mobile banking. Expert Systems with Applications 36, 11605–11616.

Jack, W., Suri, T., 2014. Risk sharing and transactions costs: Evidence from Kenya's mobile money revolution. American Economic Review 104, 183–223.

Jamison, D., Gelband, H., Horton, S., 2017. Disease control priorities: Improving health and reducing poverty. 3rd edition. Tech. rep., Washington (DC): The International Bank for Reconstruction and Development / The World Bank.

Klapper, L., 2017. How digital payments can benefit entrepreneurs. IZA World of Labor 396.

Klapper, L., 2020. Covid-19 shows why we must build trust in digital financial services. World Economic Forum COVID Action Platform (17 December 2020) https://www.weforum.org/agenda/2020/12/covid-19-trust-in-digital-financial-services/.

Kwan, A., Lin, C., Pursiainen, V., Tai, M., 2021. Stress testing banks' digital capabilities: Evidence from the covid-19 pandemic. Tech. rep., Working Paper, University of Hong Kong and University of St. Gallen.

Lee, J. N., Morduch, J., Ravindran, S., Shonchoy, A., Zaman, H., 2021. Poverty and migration in the digital age: Experimental evidence on mobile banking in Bangladesh. American Economic Journal: Applied Economics 13, 38–71.

Ma, C., Rogers, J. H., Zhou, S., 2020. Global economic and financial effects of 21st century pandemics and epidemics. Covid Economics 5, 56–78.

McKenzie, L., 2021. Bridging the digital divide: Lessons from covid-19. Washington, DC: Inside Higher Ed .

Muralidharan, K., Niehaus, P., Sukhtankar, S., 2016. Building state capacity: Evidence from biometric smartcards in India. American Economic Review 106, 2895–2929.

OECD, 2020. Job creation and local economic development. Paris: Organisation for Economic Cooperation and Development (OECD) .

Oster, E., 2019. Unobservable selection and coefficient stability: Theory and evidence. Journal of Business & Economic Statistics 37, 187–204.

Pelham, B., 2017. Medieval ingenuity in fourteenth century english milling in middlesex, norfolk, and northumberland counties. unpublished thesis, University of Central Florida .

Ramsetty, A., Adams, C., 2020. Impact of the digital divide in the age of covid-19. Journal of the American Medical Informatics Association 27, 1147–1148.

Richardson, E., Aissat, D., Williams, G. A., Fahy, N., et al., 2020. Keeping what works: Remote consultations during the covid-19 pandemic. Eurohealth 26, 73–76.

Saad, L., Jones, J., 2021. Seven in 10 U.S. white-collar workers still working remotely. Gallup (17 May 2021) https://news.gallup.com/poll/348743/seven-u.s.-white-collar-workers-still-working-remotely.aspx.

Schleife, K., 2006. Computer use and the employment status of older workers. Review of Labour Economics and Industrial Relations 20.

Senn, M. A., 2003. English life and law in the time of the Black Death. Real Property Probate & Trust Journal 38, 507.

Tarhini, A., El-Masri, M., Ali, M., Serrano, A., 2016. Extending the utaut model to understand the customers' acceptance and use of internet banking in lebanon: A structural equation modeling approach. Information Technology & People .

UNCTAD, 2020. The covid-19 crisis: Accentuating the need to bridge digital divides. New York: UNCTAD .

WorldBank, 2016. World development report 2016: Digital dividends. Washington, D.C.: World Bank .

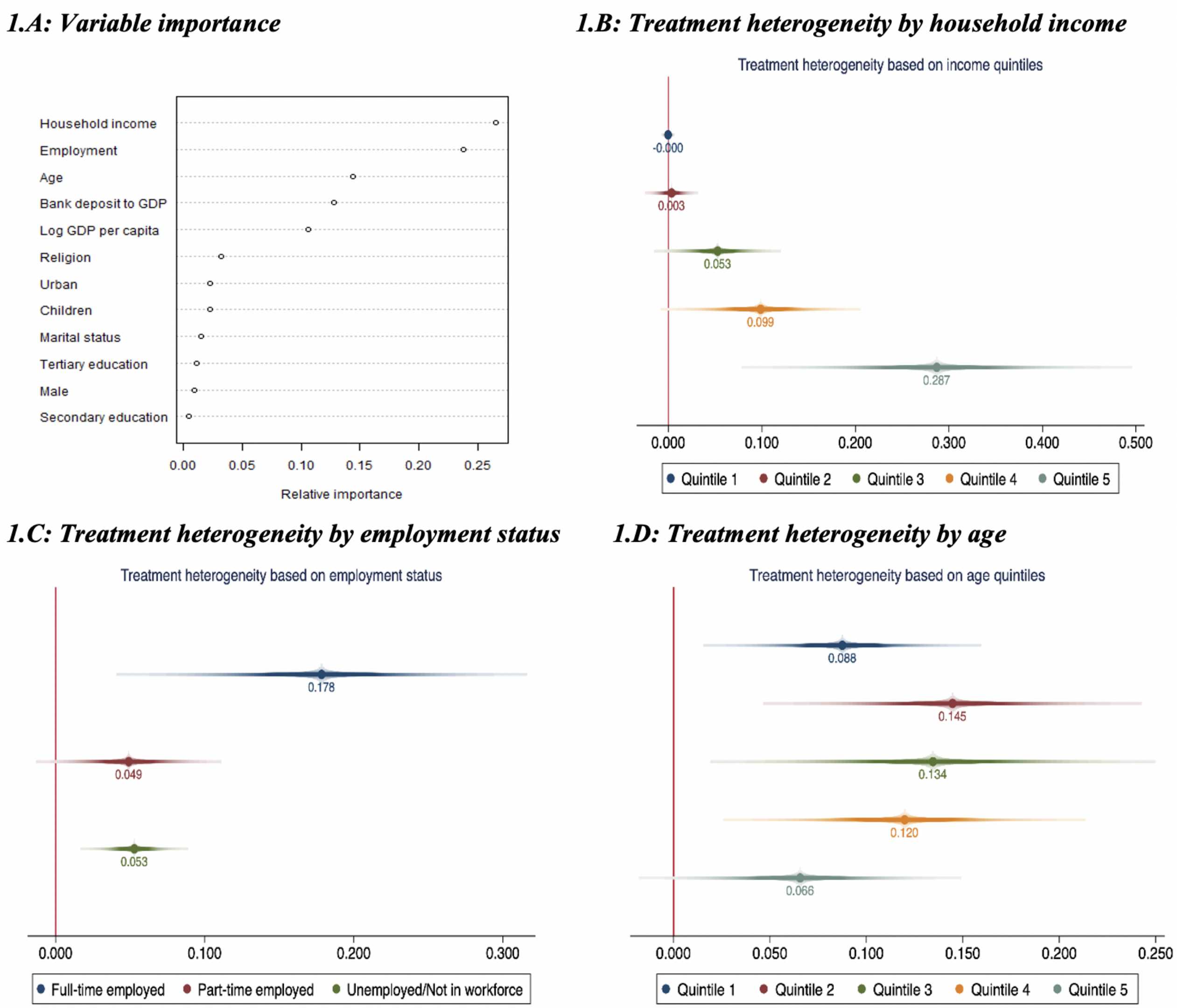
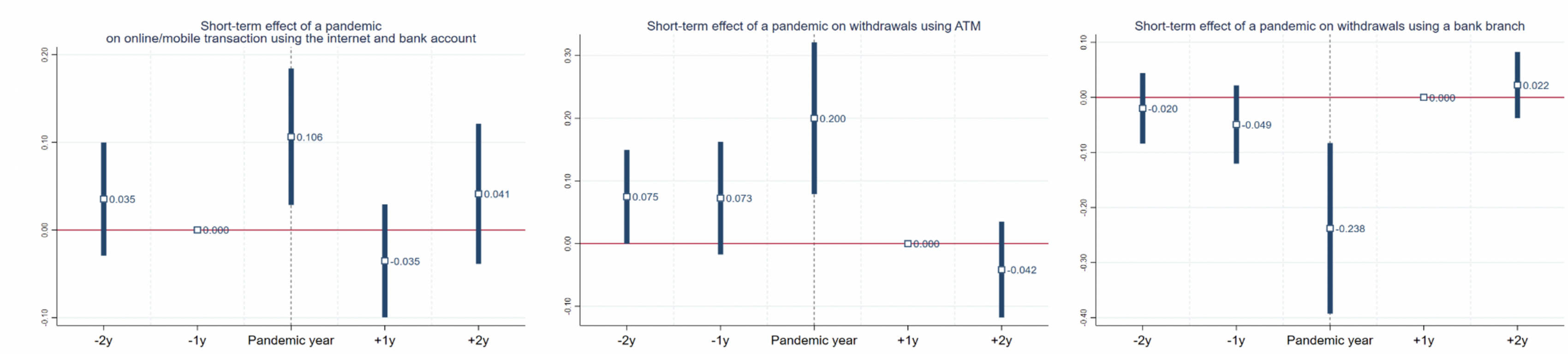


Fig. 1. **Heterogeneity Analysis using Causal Forest.** Figure A illustrates the variable importance for "Exposure to epidemic" in a causal forest framework (N=20,000 trees), which provides insights into the nature of the relationship between our treatment effect and other covariates. Figures B, C and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

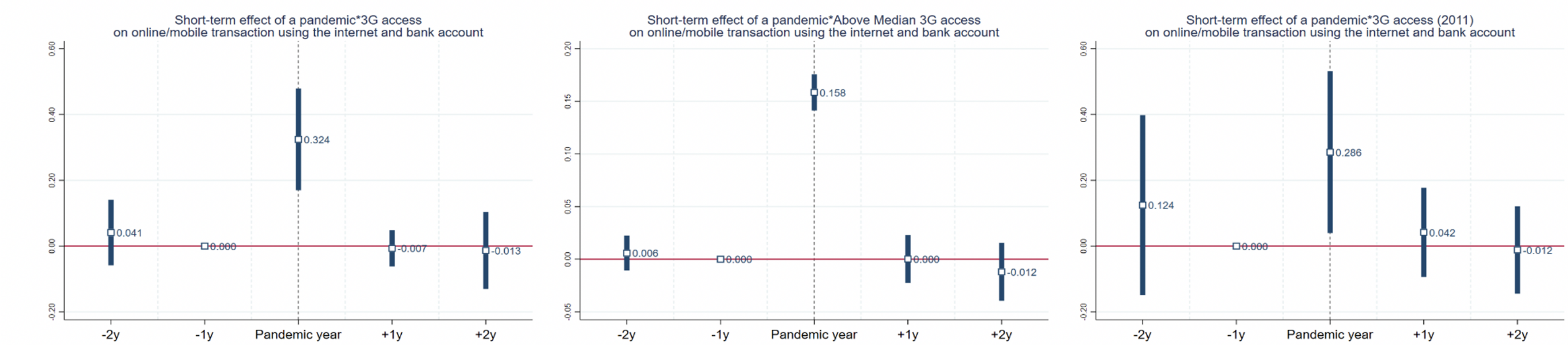


Fig. 2. **Event Study Estimates.** *Panel A:* Outcomes are "online/mobile transaction using the internet and bank account", "withdrawals using ATM", and "withdrawals using a bank branch". Event study estimates are based on the specification in Column 5 of **Table 1**. In particular, we repeat the exercise 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.

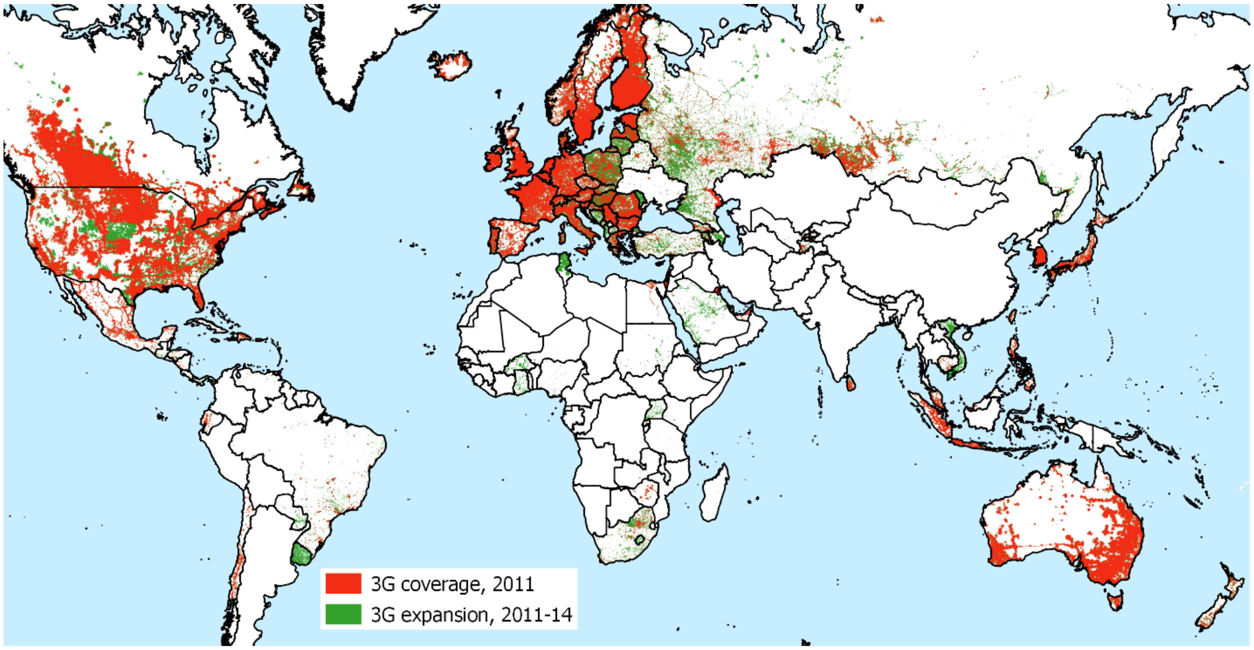
Online Appendix for

Epidemic Exposure, Financial Technology, and the Digital Divide

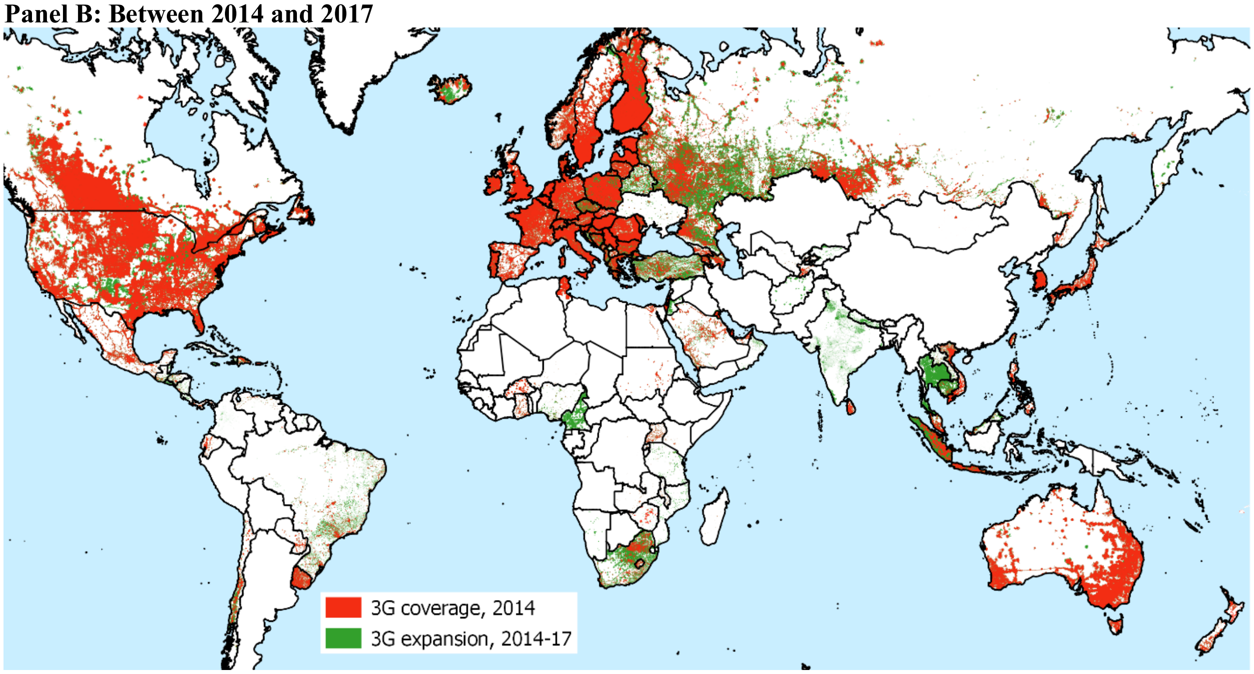
Orkun Saka, Barry Eichengreen, Cevat Giray Aksoy

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)
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.*

Appendix Table 6: 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.*

Appendix Table 7: 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 8: 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 9: 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