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9758
2022

May 2022

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CESifo

**Impressum:**

CESifo Working Papers
ISSN 2364-1428 (electronic version)
Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH
The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute
Poschingerstr. 5, 81679 Munich, Germany
Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de
Editor: Clemens Fuest
https://www.cesifo.org/en/wp
An electronic version of the paper may be downloaded
· from the SSRN website: www.SSRN.com
· from the RePEc website: www.RePEc.org
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CESifo *Working Paper No. 9758*

# Mobile Internet Access and the Desire to Emigrate


## Abstract

In this paper, we present theory and global evidence on how mobile internet access affects desire and plans to emigrate. Our theory predicts that mobile internet access increases desire and plans to emigrate. Our empirical analysis combines survey data on 617,402 individuals from 2,120 subnational districts in 112 countries with data on worldwide 3G mobile internet rollout from 2008 to 2018. We show that an increase in mobile internet access increases the desire and plans to emigrate. Instrumenting 3G rollout with pre-existing 2G infrastructure suggests that the effects are causal. The effect on the desire to emigrate is particularly strong in high-income countries and for above-median-income individuals in lower-middle-income countries. In line with our theory, an important mechanism appears to be that access to the mobile internet lowers the cost of acquiring information on potential destinations. In addition to this, increased internet access reduces perceived material well-being and trust in government. Using municipal-level data from Spain, we also document that 3G rollout increased actual emigration flows.


JEL-Codes: F200, L860, D830.

Keywords: migration aspirations, migration intentions, internet access.

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May 20, 2022
Views presented are those of the authors and not necessarily those of the European Bank for Reconstruction and Development (EBRD) or any other organization. All interpretations, errors and omissions are the authors' own. This project receives funding from the European Union's Horizon 2020 Research and Innovation Programme under Grant Agreement No. 101004703. We thank Ralph De Haas, Sergei Guriev, Beata Javorcik, Nikita Melnikov, Milena Nikolova, Andreas Steinmayr, Joachim Winter, Alexander Yarkin and participants at OECD Conference on Migration, Princeton Northeast Labor Symposium, the 3rd International Conference on Globalization and Development, the CEMIR Junior Economist on Migration Research Workshop 2021, the VfS Annual Conference 2021, IAAEU Workshop on Labour Economics 2022, the 2022 meeting of the European Public Choice Society and the seminars at Brunel University London, City, University of London, EBRD, Prague University of Economics and Business, the Junior Economics of Migration seminar, the PILLARS internal research seminar and University of Groningen for their valuable feedback.

# 1 Introduction

The internet and mobile phones have changed how people live, work, connect, and exchange information. The number of internet users has increased about twelve-fold since 2000 - from 410 million to nearly 4.9 billion in 2021, and is expected to continue double-digit growth.[1] A vast majority of internet users have access to mobile internet: there were more than 3.5 billion mobile internet subscribers in 2019 (GSMA, 2019).[2] In recent years, research has established that the internet has major economic and political impacts. Hjort and Poulsen (2019) show that the arrival of fast broadband internet has a positive effect on employment in Africa. Zuo (2021) shows that employment probabilities of poor households and their earnings increased after obtaining internet access in the United States. Guriev, Melnikov and Zhuravskaya (2021) establish that the rollout of 3G mobile internet increases awareness of government corruption and reduces trust in political institutions when the internet is not censored. In this paper, we study how 3G mobile internet (which enables high-speed data transmissions as well as other enhancements that set it miles away from 2G, including advanced access to multimedia, high-speed internet as well as international roaming) rollout causally affects desire and plans to emigrate.[3] Given that migration affects not only productive capacities and income levels but can also boost innovation (Docquier and Rapoport, 2012; Alesina, Harnoss and Rapoport, 2016) and shape politics in both destinations (Dahlberg, Edmark and Lundqvist, 2012; Halla, Wagner and Zweimüller, 2017; Dustmann, Vasiljeva and Damm, 2019; Edo et al., 2019; Alesina, Miano and Stantcheva, 2022) and origins (Barsbai et al., 2017; Karadja and Prawitz, 2019), changes in the overall level of international migration and in destinations can have major long-term societal consequences.

We estimate the effect of mobile internet access on desire and plans to emigrate by combining

[1] https://www.itu.int/en/ITU-D/Statistics/Documents/facts/FactsFigures2021.pdf (last accessed, 16.04.2022)

[2] More households in developing countries own a mobile phone than have access to electricity or clean water, and nearly 70 % of the poorest quintile of the population in developing countries own a mobile phone (World Bank, 2016). However, not all mobile phone owners have a smart phone with access to the internet. Using the Gallup World Polls between 2016 and 2018, out of all people owning a mobile phone in Sub-Saharan Africa (Europe), 44 % (88 %) report having access to the internet.

[3] Previous research has already established that desire and plans to emigrate are strongly linked to subsequent actual migration flows (Tjaden, Auer and Laczko, 2019).

two unique data sets: Gallup World Polls (GWP) and Collins Bartholomew's Mobile Coverage Explorer.[4] Combining these allows us to use data from 617,402 individuals living in 2,120 subnational regions in 112 countries, collected over 11 years. To derive causal effects on desire and plans to emigrate, we exploit variation in subnational district 3G mobile internet penetration over time. We control for two-way (subnational district and year) fixed effects (TWFE), linear district-level time trends, as well as various individual, district and country-level characteristics. This implies that the estimates are identified by exploiting within-district variation in 3G coverage that has been stripped of any influence of constant and linearly changing district-level characteristics.

We find that 3G coverage has a sizable impact on the desire and plans to emigrate: a 10 percentage point increase in 3G mobile coverage leads to a 0.27 percentage point increase in the desire to emigrate permanently, and a 0.09 percentage point increase in plans to emigrate permanently over the ensuing 12 months.[5] An increase in 3G coverage of 36 % of population, as experienced by the average individual living in a district in our baseline sample between 2008 and 2018, increased the share of population desiring to migrate by around one percentage point (compared to an average level of 22.0%) and the share of population planning to migrate by around 0.3 percentage point (compared to an average level of 2.8 %). Although this may appear a modest effect, it implies that in a country with 10 million adult inhabitants, a move from no 3G coverage to full coverage would increase the number of people desiring to emigrate by 270,000 and planning to emigrate by 90,000.

Although our main econometric specification controls for subnational district and year fixed effects, as well as district-level linear time trends, it does not dispel all endogeneity concerns. We deal with these concerns in five distinct and complementary ways. First, we show that districts with and without 3G internet coverage display similar pre-trends in the desire to emigrate.

[4]We use three different measures of migration aspirations and intentions: (1) whether an individual would like to move permanently to another country, if he or she had the opportunity; (2) whether an individual is planning to move permanently in the next 12 months; and (3) whether an individual is likely to move away from his or her current city or area in the next 12 months, without a restriction to the migration being permanent or to another country.

[5]These estimates arise when weighting our observations using the within-country weights as provided by Gallup. Importantly, the estimated effects are even greater if using country-level population weights. We have chosen as our baseline the more conservative Gallup weights due to a concern that a few large countries could drive the effect if using weights based on population.

Second, we use the alternative estimator by de Chaisemartin and D'Haultfœuille (2020*a*). This enables us to assess pre-trends on a larger segment of our sample (as the estimator allows for varying treatment intensity) than a traditional event study focusing around large increases in 3G coverage and addresses the inference issues under the TWFE approach. Using this alternative estimation method, we find qualitatively similar results. Third, our results are robust to controlling for alternative time-varying measures of regional economic development. This dispels concerns that the reported effect is actually driven by a spurious relation between mobile internet and regional development, as 3G rollout could plausibly be swifter in faster developing subnational regions. Fourth, following the method proposed by Oster (2019), we show that our results are unlikely to be driven by the unobserved variation that is potentially related to omitted factors. Fifth, we find qualitatively similar results when we employ an instrumental variables (IV) strategy. We use pre-existing 2G mobile network coverage in 2006 to predict 3G mobile network rollout from 2008 to 2018.

To establish robustness, we show that our results are not driven by other observable economic, social and political exposures that individuals may have simultaneously experienced during the 3G rollout. In addition, our estimates are robust across a variety of specification checks. Firstly, we show that higher level of 3G coverage is not associated with regional income levels and only weakly associated with demographic characteristics using a balancing test, net of controls, fixed effects and time trends. Secondly, we use leads as treatments to assure that future increases in 3G coverage do not predict previous changes in the desire to emigrate. Thirdly, we show that our results are robust when using different survey weights, when we exclude potentially bad controls, and when omitting district-level time trends. In addition, our results remain statistically significant when clustering standard errors on the country level and after correcting for multiple hypothesis testing. Furthermore, we show that our results are robust to several additional tests. We show that our results hold up in samples (1) without districts and countries with possibly poor-quality 3G coverage data, (2) only including countries and districts that are surveyed in all years between 2008 and 2018, (3) excluding telephone interviews and countries with any telephone interviews altogether, (4) omitting years and global regions one at a time to show that our reported result is not driven by few influential observations, and (5) without top 10 refugee-origin countries as well as districts with very high or very low average desire to emigrate. In addition, we use 2G network expansion as a placebo treatment, which also suggests that enabling texting and calling are not driving our results. To provide a discussion of the heterogeneity of the found effect, we use the Causal Forests approach.

We then explore the mechanisms behind our results. We begin by showing that the effect of 3G coverage on the desire to emigrate is strong for the individuals who were without any prior network abroad, while we find no effect for those who already have a network abroad. This suggests that the internet substitutes for missing personal networks as a means of accessing information on opportunities abroad. We also show that 3G coverage does not improve the financial situation of respondents (e.g., household income) but has a negative effect on the perception of relative financial well-being as well as satisfaction with their national governments, which potentially shape emigration desires. Finally, using municipal level data from Spain, we show that 3G expansion not only alters desire to emigrate, but also increased actual emigration of home-country nationals.

The remainder of the paper is structured as follows. Section 2 reviews related literature and expands on our contributions to it. Section 3 introduces a theoretical framework we use to derive testable predictions. Sections 4 and 5 describe our data and empirical strategy. Section 6 presents the results. Section 7 explores the mechanisms. Section 8 presents evidence on how 3G coverage affected actual emigration from Spanish municipalities. Section 9 concludes.

# 2 Related Literature and Our Contributions

Our analysis connects up to several literatures. First, there is work on the income-related correlates of migration. Borjas (1987), Grogger and Hanson (2011), and McKenzie, Gibson and Stillman (2013) show that earning potential in the destination country shapes migration behavior. However, Dustmann and Okatenko (2014) show that the relationship between the intention to move (both domestically and internationally) and proxied wealth is non-monotonic. That is, the likelihood to move increases with personal income for those individuals living in the poorest global regions (Sub-Saharan Africa and Asia), while this relationship is absent for those living in relatively richer regions (Latin America).[6]

Second, there is the literature on the determinants of migration intentions. McKenzie and Rapoport (2010), Docquier, Peri and Ruyssen (2018) and Manchin and Orazbayev (2018) show that networks abroad are a major driving force of international migration intentions. Ruyssen and Salomone (2018) used the GWP to study how intentions to migrate are affected by the perception of gender discrimination of women. Pesando et al. (2021) provide descriptive evidence using data on migration intentions from GWP and Arab Barometer, data on actual migration from the Italian Statistical Institute as well as the Sant'Anna Cara reception center in Southern Italy. Across both levels of analysis, the authors find a positive *association* between internet access (measured as a percentage of the population using the internet) and both the willingness to emigrate as well as actual migration. We contribute to this literature by providing new *causal* evidence on the impact of internet access on migration aspirations and intentions, and by identifying the underlying mechanisms at play.

Third, we build on the recent literature on the impact of mobile internet technologies on political behavior. Manacorda and Tesei (2020) use a granular data set for the entire African continent on 2G network coverage combined with geo-referenced data from multiple sources on the occurrence of protests. They find that while mobile phones are instrumental to mass mobilization, this only happens during economic downturns. In the most closely related study, Guriev, Melnikov and Zhuravskaya (2021) analyze political implications of 3G internet rollout using GWP. They find that 3G expansion increases awareness of government corruption and reduces trust in political institutions. The authors further show that the effect is present only when the internet is not censored, and it is stronger when the traditional media are censored. We complement these studies by showing how 3G internet access also affects non-political outcomes — aspirations and intentions to emigrate and by showing how internet access affects high stake individual actions — actual emigration from Spain.[7]

[6]The inverse U-shaped relation between income and migration is also documented in Clemens (2014).

[7]There are also studies that investigate the impact of the diffusion of high-speed *fixed-line broadband* internet on economic and political outcomes. Hjort and Poulsen (2019) find that the arrival of fast broadband internet has a positive effect on employment in Africa. Falck, Gold and Heblich (2014) show that increased broadband internet availability reduced voter turnout in Germany. The authors relate this finding to a crowding-out of TV consumption and increased entertainment consumption. Campante, Durante and Sobbrio (2018) find that broadband internet access had a substantial negative effect on voter turnout in parliamentary elections in Italy

Our data and empirical setting provide some unique advantages that allow us to provide new evidence on the aspirations and intentions to emigrate in three more dimensions. First, we use granular (1x1 kilometer grid level) data on 3G network coverage to calculate population-averaged coverage of *mobile* internet on the subnational region, which means that our treatment variable is much less noisy compared with the country-level *share of the population with internet access*. The mobile internet is also more relevant with regard to migration behavior — it enables access to the internet even from remote locations, it is entirely portable and provides the means to communicate with most of the world's population instantly. Second, while other papers provide descriptive evidence on the relationship between internet access and migration aspirations and intentions, we provide *causal* evidence. Third, we show that 3G penetration not only affects emigration intentions but also actual emigration.

# 3 Theoretical Framework

There are two countries, denoted by 0 and 1. We analyze the decisions of residents of country 0 on whether to invest in acquiring information on opportunities abroad and whether to migrate to country 1 if mobile. We denote by vector $\boldsymbol{x_j}$ individual j's characteristics (such as age, gender, experience and family situation) that can influence earnings, the cost of acquiring information on opportunities abroad and migration costs in the case of being mobile. Vector $\boldsymbol{x_j}$ has a constant term that is used to capture potential earnings as well as information acquisition and migration costs of a reference person and n individual-specific components, given by $\boldsymbol{x_j} = (1, x_{j,1}, ..., x_{j,n})$. In addition to individual characteristics, $\boldsymbol{x_j}$ also includes the 3G coverage in the region in which $j$ lives inside country 0, denoted by $x_{j,3G}$. We denote the vector giving after-tax returns to individual characteristics in country $k$, $k \in \{0, 1\}$, by $\boldsymbol{\beta_k}$, giving as potential disposable earnings in country $k$ $\boldsymbol{\beta_k} \cdot \boldsymbol{x_j}$. As in Grogger and Hanson (2011), we divide education into primary, secondary and tertiary, and allow both returns to education and migration costs vary according to the level of education.

Potential mobility also has a stochastic component and acquiring information about oppor-

until 2008, but this pattern has reversed since.

tunities abroad can be costly. This is inspired by Bertoli, Moraga and Guichard (2020) and Porcher (2020), who analyzed costly information acquisition, in a setting with several potential destinations. We present a simpler model with a binary choice for information acquisition as GWP has no questions on the number of destinations from which respondents have gathered information. The information costs could be related to such issues as whether one could obtain a visa as well as job and housing opportunities abroad, with cost vector $\boldsymbol{\alpha}$ that specifies how information costs depend on individual characteristics. The total cost of information acquisition is $\boldsymbol{\alpha} \cdot \boldsymbol{x_j}$. Our main variable of interest is regional internet coverage, the effect of which is denoted by $\alpha_{3G}$. As mobile internet access makes finding information easier, $\alpha_{3G} < 0$. If being internationally mobile and deciding to migrate, individual $j$ faces migration cost $c_j$, which also includes the expected post-migration cost of communicating with family and friends left behind. The migration cost depends on individual characteristics $\boldsymbol{x_j}$ with a cost vector $\boldsymbol{\gamma}$ and an unobservable individual-specific component $\epsilon_j$, capturing individual-specific taste for living abroad that is unobservable to researchers:

$$c_j = \boldsymbol{\gamma} \cdot \boldsymbol{x_j} + \epsilon_j. \tag{1}$$

Cost vector $\boldsymbol{\gamma}$ includes a component related to 3G coverage denoted by $\gamma_{3G}$, with $\gamma_{3G} < 0$ as a 3G network facilitates communication. Individual-specific component $\epsilon_j$ follows a continuous distribution with density function $\phi(.)$ and differentiable cumulative distribution function $\Phi(.)$, and obtains negative values for those with a preference for living abroad. For simplicity, we assume that those who invest in information acquisition learn with certainty whether they are mobile or not, and that the probability of being mobile is individual-specific, denoted by $\theta_j$. Individual probability to be able to migrate $\theta_j$ depends on external constraints, such as immigration rules in the destination, and on psychological and social components, such as the effect of family members. It is individually optimal to invest in information acquisition if

$$\theta_j \left( \boldsymbol{\beta_1} \cdot \boldsymbol{x_j} - \boldsymbol{\beta_0} \cdot \boldsymbol{x_j} - \boldsymbol{\gamma} \cdot \boldsymbol{x_j} - \epsilon_j \right) > \boldsymbol{\alpha} \cdot \boldsymbol{x_j}. \tag{2}$$

In equation (2), the term in parentheses on the left-hand side gives the utility gain from migration, multiplied by the probability of being able to migrate. This equals the expected benefit from acquiring information on one's mobility and migrating if being able to do so. The right-hand side gives the cost of information acquisition. It is optimal to acquire information if the expected benefit from migration multiplied by the probability of being able to migrate exceeds the cost of finding out whether one could migrate. Those with too small or even negative gains from potential migration remain rationally uninformed on their mobility status, in line with Bertoli, Moraga and Guichard (2020) and Porcher (2020). Equation (2) allows deriving the maximum individual-specific component $\hat{\epsilon}_j$ with which individual j would find it optimal to acquire information:

$$\hat{\epsilon}_j = (\boldsymbol{\beta_1} - \boldsymbol{\beta_0} - \boldsymbol{\gamma} - \boldsymbol{\alpha}/\theta_j) \cdot \boldsymbol{x_j}. \tag{3}$$

Denoting the probability of individual j investing in information acquisition by $p_j$, we have

$$p_j = \Phi\left((\boldsymbol{\beta_1} - \boldsymbol{\beta_0} - \boldsymbol{\gamma} - \boldsymbol{\alpha}/\theta_j) \cdot \boldsymbol{x_j}\right). \tag{4}$$

In the individual components of vectors $\boldsymbol{\beta_0}$ and $\boldsymbol{\beta_1}$, we use superscripts for country indices, implying that $\beta^0_{3G,0}$ is the effect of 3G coverage in the region of origin on wage level in that location, and $\beta^1_{3G,0}$ is the effect of 3G coverage in the region of origin on wage level in the other country, if any. The effect of regional 3G coverage on the probability of individual j investing in information acquisition is given by:

**Proposition 1** $\frac{\partial p_j}{\partial x_{3G}} = \left(\beta^1_{3G,0} - \beta^0_{3G,0} - \gamma_{3G} - \frac{\alpha_{3G}}{\theta_j}\right) \phi\left((\boldsymbol{\beta_1} - \boldsymbol{\beta_0} - \boldsymbol{\gamma} - \boldsymbol{\alpha}/\theta_j) \cdot \boldsymbol{x_j}\right).$

**Proof 1** *Follows by differentiating equation (4).*

The effect of 3G coverage on the probability of investing in information acquisition is the product of two terms. The first term, $\left(\beta^1_{3G,0} - \beta^0_{3G,0} - \gamma_{3G} - \frac{\alpha_{3G}}{\theta_j}\right)$, is positive if the effect of 3G coverage on wages is sufficiently low. However, if 3G coverage would sufficiently boost wages in the region of origin, then an increase in 3G coverage could reduce migration. The second term,

$\phi\left((\boldsymbol{\beta_1} - \boldsymbol{\beta_0} - \boldsymbol{\gamma} - \boldsymbol{\alpha}/\theta_j) \cdot \boldsymbol{x_j}\right)$, is a scaling factor depending on the density of the individual-specific component at the cutoff point. As long as density is not zero, the sign of the effect of 3G coverage on the probability of investing in information acquisition is determined by the first term. We assume that $\beta^1_{3G,0} = 0$, implying that 3G coverage in the region of origin has no effect on wages in the destination region. As $\alpha_{3G} < 0$ and $\gamma_{3G} < 0$, our main testable hypothesis is:

**Hypothesis 1** *An increase in 3G coverage increases subsequent desire to emigrate, at least if it does not boost local wages substantially.*

Our model predicts that only a fraction $\bar{\theta}$ of those investing in the acquisition of information can migrate, in which $\bar{\theta}$ is the average value of $\theta_j$ over all individuals who invest in information acquisition of their mobility status. Therefore, migration plans increase in the desire to migrate but at a rate lower than one, giving a second testable hypothesis:

**Hypothesis 2** *An increase in 3G coverage increases subsequent plans to emigrate, at least if it does not boost local wages substantially. The increase in plans to emigrate is smaller than the increase in the desire to emigrate.*

Both testable hypotheses are derived with the caveat that there is not a substantial direct effect of 3G coverage on local wages. In the empirical analysis, we estimate the net effect of 3G coverage and, if positive, it already implies that the effect on boosting local wages is probably not very strong. A negative effect of 3G coverage on the desire to emigrate, instead, would suggest, as a potential explanation within the model, that the 3G coverage may have boosted local wages. In section 7 we analyze whether household income is directly related to the expansion of 3G coverage.

# 4 Data and Descriptive Statistics

The main data used in this paper come from the GWP and Collins Bartholomew's Mobile Coverage Explorer. We complement these data using additional country-level and bilateral origin-destination-level indicators from a variety of sources.

## 4.1 Data

### *Gallup World Polls*

Our primary data on emigration aspirations and intentions originate from the 2008-2018 GWP. These nationally representative surveys are fielded every year and interview approximately 1,000 individuals in each country on a wide range of topics.[8] Our resulting main sample includes 617,402 respondents from 112 countries.

The survey's outcome variables of interest were identified by questions asked to Gallup respondents about their desire and plans to emigrate, as well as how likely they are to move away from the city or area where they live, whether domestically or internationally.[9] The outcomes of interest, their time span, the wording of the underlying question and possible responses are:[10]

1. Desire to Emigrate (2008 – 2018): *Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country?* **Yes/No/Don't know/Refused to answer**

2. Plans to Emigrate (2008 – 2015): *Are you planning to move permanently to another country in the next 12 months?* (asked only of those who are desiring to move to another country) **Yes/No/Don't know/Refused to answer**

3. Self-assessed Likelihood to Migrate (2008 – 2018): *In the next 12 months, are you likely*

[8] If countries have sufficient telephone network coverage, households are drawn from a phone number database or on the basis of dialling random digits. If not, face-to-face interviews are conducted, with a 'random route' methodology of selecting households. Importantly, only after finding a household and identifying all of its members aged 15 or above, a household member is selected at random and up to three attempts to interview the selected member are made. If unsuccessful, a new household is approached to prevent a selection bias within a household's hierarchy. The coverage of countries, number of respondents, language of survey and method of conducting can be found here: https://www.gallup.com/file/services/177797/World_Poll_Dataset_Details_052920.pdf

[9] The GWP contains multiple questions regarding migration intentions that do not fully overlap and, hence, we combine them when possible to not lose observations. This is especially important for (2). The relevant constructed variables and exact underlying questions are all documented in Online Appendix Table A1. Moreover, question (2) is asked only during a specific time span and conditional on the respondent having answered positively to (1). Thus, (2) is automatically assigned with a negative answer for those observations that answered negatively to (1).

[10] For all four outcomes, a positive answer is recoded to 1, a negative answer is recoded to 0, and set to missing for the two residual options.

*or unlikely to move away from the city or area where you live in?*[11]

**Likely/Unlikely/Don't know/Refused to answer**

(1) captures "wishing to move abroad", which can simply reflect a general aspiration of the respondent. In our paper, we consider this group as potential migrants who look for opportunities to emigrate but are also aware that the hurdles and frictions preventing the translation of the desire to emigrate into actual emigration could be pervasive and difficult to reduce (for detailed discussion, see Docquier, Ozden and Peri (2014)).

Questions (2) and (3) reveal more concrete intentions and arrangements that individuals may undertake before leaving. Tjaden, Auer and Laczko (2019) document that question (2) is strongly related to actual migration flows. The emphasis on a relatively short time window of 12 months makes it plausible that only individuals with serious and developed migration plans answer affirmatively (Dustmann and Okatenko, 2014). In other words, question (2) filters respondents who have the means to achieve and are taking steps towards migrating internationally (Migali and Scipioni, 2018). This pattern is also revealed in Appendix Table A2: the share of respondents who actually plan to move abroad in the next 12 months (less than 3%) is substantially lower than the share of those who reported having desire to emigrate (22%).[12]

[11] This question relates to movements both within and across international borders with no constraint imposed on the distance of the move.

[12] Additionally, GWP contains a question on *preparations to emigrate within 12 months*, which is asked when a respondent indicated plans to emigrate within 12 months. However, this question is only included in GWP between 2009 and 2015 and in less sub-national regions than the question on plans to emigrate, leaving limited variation in 3G network coverage. Furthermore, this question is on average answered positively by only 1.7% of respondents. If treatment effects are not very large compared to the share of respondents answering positively, the statistical power to uncover the effect may be low. Arnold et al. (2011) suggest to perform a simulation in order to find the power of a statistical test in settings with more complicated treatment assignment. We follow Arnold et al. (2011) and simulate the outcome variable (in this case the preparations to emigrate) to study the power of a statistical test on our estimate of the effect of 3G coverage on preparations to emigrate. Using the observed 3G coverage and covariates from the data, we simulated the data for preparations to emigrate as follows: we assume that a 1 unit increase in 3G coverage could increase the probability to prepare to emigrate by 0.3 percentage points, an increase of around 20 % compared to the baseline rate of 1.7 %.

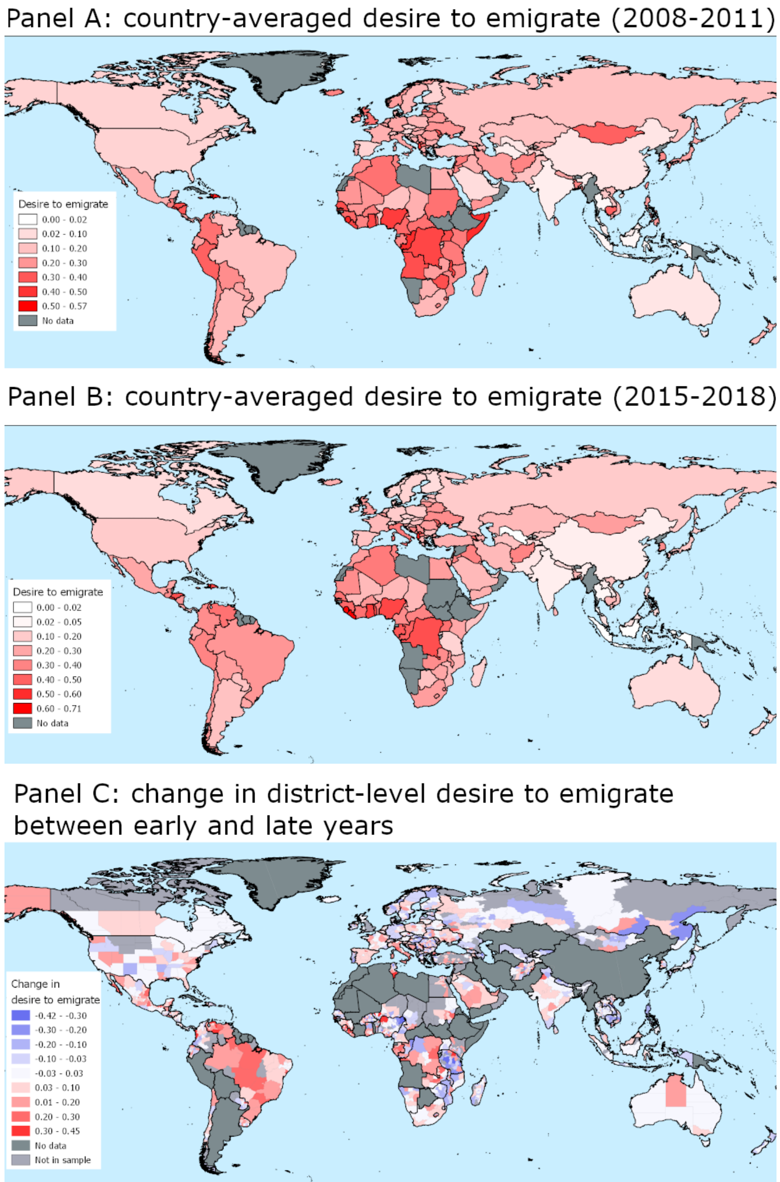
$$\Pr(Y_{idt} = 1) = 0.017 + 3G_{dt} \times 0.003 \tag{5}$$

Based on this probability, we draw a binary outcome for all observations. Thereafter, we run our baseline specification (see Equation 6) using the generated binary outcome and observed 3G coverage and covariates on the sample of observations that answered the GWP question on *preparations to emigrate within 12 months*. Using 500 realizations of the simulation described above, we reject the null hypothesis of no treatment effect at a 5 % significance level only in 9 % of the realizations. In comparison, the power using similar procedures (using

There is significant heterogeneity in emigration aspirations within and across countries. Panel A of Figure 1 shows the averaged levels of the desire to emigrate in the 2008 – 2011 period, and Panel B for the 2015 – 2018 period. Panel C of Figure 1 displays the changes in average reported desire to emigrate between early (before the median year of all observations in the subnational region) and late years (during or after the median year).

the found effect sizes reported in Table 1 as the simulated treatment effect sizes) for desire to emigrate and plans to emigrate are 99 % and 53 %, respectively. This suggests that the sample size and/or the expected effect sizes for preparations to emigrate are unfortunately too small to study the effect of mobile internet coverage, in contrast to the other outcomes with better sample coverage and higher prevalence.

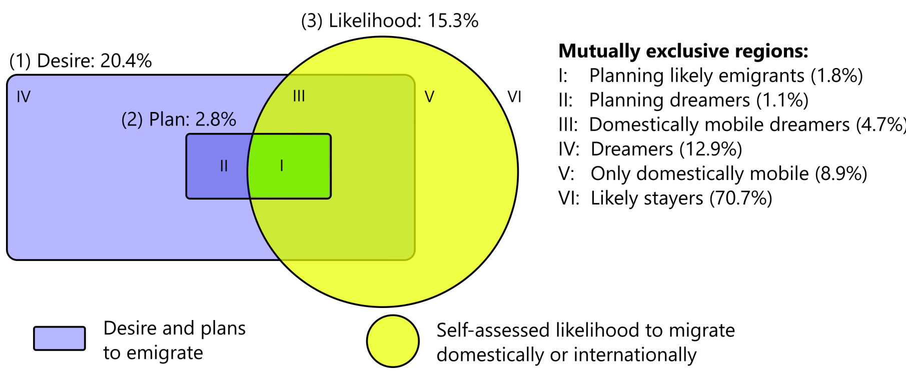
Figure 1: Desire to Emigrate around the World over Time




Note: Panel A shows the average desire to emigrate on the country-level between 2008 and 2011 and Panel B between 2015 and 2018. Because not all countries are covered each year, we take the country-level average between 2008 and 2011 for the early years, and between 2015 and 2018 for the late years. The lowest number of observations in Panel A (Panel B) is 439 (474). Panel C shows the difference between the share of respondents desiring to emigrate in the earlier time period (defined as all years before the median year for all observations in a subnational region) and the later time period (defined as all years equal to or exceeding the median year), on the subnational level, in all districts with 3G coverage data and at least 25 GWP respondents per subnational region for both the early and the late time period. All regions with less than 25 observations are shown in light gray and those without data on 3G coverage are shown in dark gray. All intervals in the legend are closed on the left and open on the right, except for the last bin.

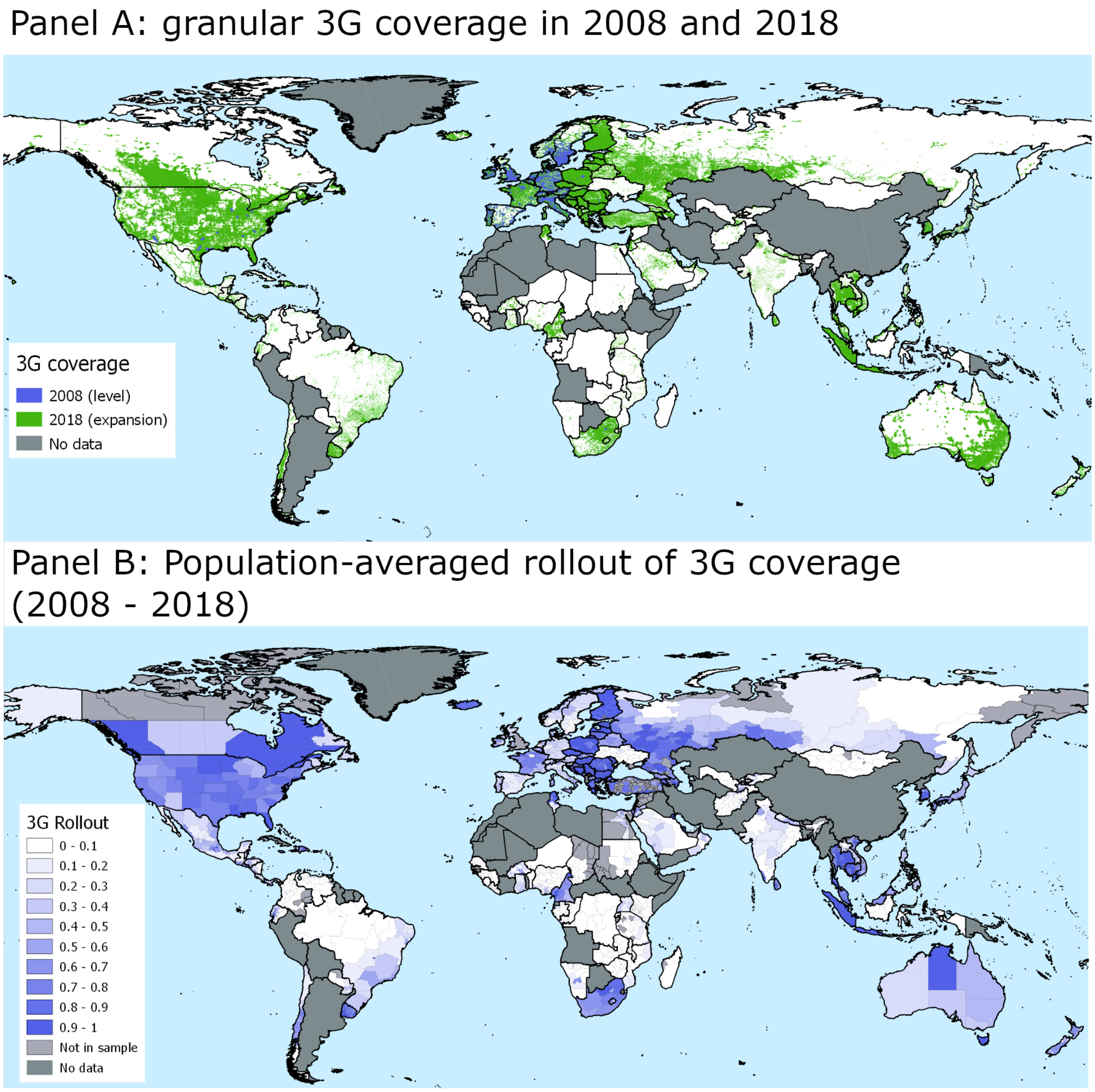
Notable patterns can be summarized as follows: (i) less than 20 % desires to emigrate in most developed countries; (ii) less than 10 % in many East Asian countries; (iii) there is substantial variation in the share of people desiring to emigrate within global regions over time — in Africa (an increase from 29 % in early (2008 – 2011) to 32 % in late (2015 – 2018) years), in Asia excluding the former USSR, Japan and South Korea (a decrease from 17 % in early to 13 % in late years), in Europe (an increase from from 19 % in early to 22 % in late years), in the former USSR (an increase from 19 % in early to 21 % in late years), in Middle- and South America (an increase from 27 % in 2010 to 32 % in late years) and in high-income non-European countries (an increase from 14 % in early to 16 % in late years); and (iv) there is substantial regional variation within-countries.

Figure 2: Venn Diagram of the Three Migration-related GWP Questions




Note: Venn diagram of the three migration-related outcomes (desire and plans to emigrate and self-assessed likelihood to migrate, including domestic migration) identifying six mutually exclusive regions. Note that the analysis is limited to the time period 2010 – 2015, as outside of this window not all underlying questions are asked in GWP. The Figure reports the unweighted proportion of respondents answering each question positively as two boxes (desire and plans to emigrate) and one circle (self-assessed likelihood to migrate). The share of respondents belonging to each of the mutually exclusive regions denoted in the Venn Diagram is reported on the right. The subsample where all four outcomes are available comprises $N = 342,328$ individuals.

Figure 3: Increase in 3G Coverage Around the World between 2008 and 2018




Note: Panel A shows the granular 3G coverage in 2008 in blue and the expansion between 2008 and 2018 in green for all data available in the Collins Mobile Coverage Explorer. Panel B shows the change in population-averaged 3G coverage between 2008 and 2018 on the subnational region in shades of blue, for those regions available in the GWP. Regions without GWP coverage within countries that are in the 3G data and at least once in the GWP data are colored light grey. Countries fully absent in either the GWP or 3G data are colored dark grey. Regions marked in different shades of blue or remaining white represent the relevant variation in subnational 3G coverage exploited in our empirical strategy. The intervals in Panel B are closed on the left and open on the right, except for the last bin.

We visually summarize our outcomes in a Venn diagram in Figure 2, which identifies six mutually-exclusive regions for migration aspirations and plans (ranging from planning likely emigrants to likely stayers). Region I is of particular interest as it combines emigration intentions with a self-assessed likelihood of moving away within 12 months. Therefore, it captures more developed plans to emigrate (I), in comparison with general plans to emigrate (I+II). Among those *planning* to emigrate (2.8 %), about two-thirds (1.8 %) report that they are also likely to move within 12 months. Moreover, region V identifies those deeming migration likely, but do not desire to emigrate. Although not a perfect measure, V predominantly captures those who intend to migrate domestically.

Respondents are also asked about which country they *desire* to move to (for question (1), from 2008 to 2018) and either whether they want to move to that country (in 2008 and 2009) or which country they *plan* to move to (for question (2), in 2010 – 2015). We use this information to construct a yearly data set on origin-destination-level rates of the desire to emigrate and plans to emigrate. We then combine these data with yearly actual flow rates from the Organisation for Economic Co-operation and Development (OECD) to examine whether our outcomes convey meaningful information (see Section 4.2 for a detailed discussion).[13]

The GWP also provides detailed information on respondents' demographic characteristics (age, gender, educational attainment, marital status, religion and urban/rural residence), labor market outcomes, household income, satisfaction with local amenities and social networks abroad. This allows us to directly control for many relevant and confounding factors of migration behavior at the individual level.

We proxy the district-year level development level by calculating the average of personal income of other people in a district (excluding the respondent) as well as by using nighttime light data (explained below). Furthermore, to control for the age structure of the country, we compute the share of respondents aged under 30 in a country for any given year using the reported age in GWP.

#### ***Collins Bartholomew's Mobile Coverage Explorer***

The information of 2G and 3G mobile network coverage around the world is obtained from Collins Bartholomew Mobile Coverage Explorer.[14] The data provide information on signal

[13]The proportion of individuals answering positively on (1) but not mentioning a destination country is less than 7 %. Similarly, less than 4 % of those answering positively to (2) do not mention a destination country. Although respondents can choose not to mention a specific destination, the vast majority does.

[14]For more information, please see: https://www.collinsbartholomew.com/mobile-coverage-maps/

coverage at 1x1 kilometer grid level, as submitted by network operators to the GSM Association. That is, we know whether or not a given 1x1 kilometer grid cell has a 2G or 3G signal (or both). However, we do not observe any information about the strength of the signal. The network coverage data is available on the yearly level, but the timing of data collection differs. Between 2011 and 2017, data is provided for the month December, whereas in 2007, 2008 and 2009, it is provided in the first quarter of the year.[15] We use the reported coverage in year $t-1$ to represent the network coverage in year $t$.[16]

To calculate the share of population that is covered by the 2G and 3G, we use 1x1 kilometer population data from the Gridded Population of the World (GPW) for 2015, which is distributed by the Center for International Earth Science Information Network.[17] We first calculate each grid point's population coverage and then aggregate this information over the subnational regions as provided in the GWP. The constructed population-weighted coverage of 3G networks is our main treatment variable.[18]

Figure 3 illustrates the increase in 3G internet coverage at the subnational region level over time.[19] In particular, Panel A of Figure 3 shows the granular 3G internet coverage in 2008 and 2018, and Panel B shows the relevant variation in population-averaged 3G coverage between 2008 and 2018 on the subnational region level. Perhaps not surprisingly, the levels of 3G internet coverage are highest in developed and densely populated countries, mostly achieving coverage levels of more than 75 % of the population in 2018. Conversely, many Latin American and Sub-Saharan African countries have coverage levels of below 25 %. Nevertheless, several

mobile-coverage-explorer/

[15]Due to the change in data provision, data between the first quarter of 2009 and December 2011 are missing. We overcome this challenge by linearly interpolating the missing information using the data from 2009 and 2011.

[16]As around 70 % of the GWP interviews are conducted in July or earlier in the year, using the network data from previous December (for the interviews in 2012 up to 2018) is more informative of the actual network coverage during the interview. In the Online Appendix, we alternatively consider the effect of lags and leads of 3G on migration-related outcomes.

[17]Since 2012, data on 4G network coverage has also been recorded in a subset of countries. As it is technically possible for an area to be covered by 4G but not by 3G, we might underestimate the share of population covered by mobile internet. We investigate this possibility and find that some urban areas in Czechia and India have 4G infrastructure without having 3G coverage. Across the whole sample in 2018, only less than 1% of the sample population is covered by 4G and not by 3G, which is not likely to bias our results.

[18]The data are not available for several populous countries such as Algeria, Angola, Argentina, Bangladesh, China, Ethiopia, Iran, Iraq, Kazakhstan, Myanmar, Morocco, Pakistan, Peru and Yemen.

[19]The data availability is somewhat limited for some countries. Data for some countries with large migration aspirations, intentions and flows in the Middle East and North Africa (MENA) region are absent from the final data set.

non-OECD countries have showed expansions in excess of 25 % over the 11-year period that we study. This offers relevant variation in 3G internet coverage on a global scale in the period studied. In the subnational districts present in our sample, 3G coverage increased from 18 % to 50 % on average.

***Additional Datasets***

- Nighttime Light Density: To control for district-level economic development, we follow Henderson, Storeygard and Weil (2012) and use nighttime light density (that is, luminosity from satellite images) data. These data come from Defense Meteorological Program Operational Line-Scan System (DMSP-OLS) and Visible Infrared Imaging Radiometer Suite (VIIRS) instruments.[20] We calculate the district-year-level nighttime light density by weighting light intensity from DMSP-OLS and VIIRS with the GPW population density in 2005. The DMSP-OLS data span until 2013. The VIIRS data are available from 2015 onwards, requiring the year 2014 to be linearly interpolated between the 2013 DMSP-OLS and the 2015 VIIRS datapoint at the district level. As the nighttime light-density data come from different sources (and thus are not directly comparable), we normalize each value to a $0 - 1$ range within each year.

- OECD: To compare bilateral rates of migration aspirations and intentions with actual migration flows, we obtain migration flow data between 2008 and 2018 (from more than 200 origin countries to 35 OECD countries) from the OECD. In particular, we use the inflows of foreign population by nationality.

- The World Bank: To control for country-level development, we obtain real gross domestic product based on purchasing power parity (GDP (PPP)) per capita per year, expressed in constant 2011 US dollars. We also use country-level population data to construct population weights, as well as the country-level data on broadband subscriptions (per 100 people).

[20] See details at these links: https://ngdc.noaa.gov/eog/dmsp/downloadV4composites.html and https://ngdc.noaa.gov/eog/viirs/download_dnb_composites.html

- Center for Systemic Peace: To control for political regime characteristics, we use the Polity2 variable from the Polity IV data set. Polity score ranges from -10 to +10, with -10 to -6 corresponding to autocracies, -5 to 5 corresponding to anocracies, and 6 to 10 to democracies.[21]

- CEPII GeoDist database: To control for dyadic origin-destination country-level factors in a gravity framework, we obtained data on shared languages and pairwise weighted distances.[22]

- United Nations Populations Division: To assess the importance of prior dyadic origin-destination country-level stocks of migrants in a gravity framework, we obtained the estimated dyadic stock of migrants from the UN Population Division in 2005.[23]

The resulting data set concerns 617,402 individuals from 2,107 subnational regions in 112 countries over 11 years of data. There are 13,073 region-by-year cells in the data, implying an average number of 47 respondents in a subnational region in a given year. Although the data is unbalanced, 83 % of all regions are present in the data for at least 5 years.

## 4.2 Evidence that Our Treatment and Outcome Variables Convey Meaningful Information

Key to the interpretation of our results is whether our treatment variable (3G) and outcome variables convey meaningful information. To provide evidence on this, we first examine the effects of 3G internet expansion on the individuals' probability of having access to the internet on the full sample.[24] Appendix Table A3 shows that a full rollout of district-level 3G coverage leads to a statistically significant 4.9 percentage point increase in the likelihood of having access to the internet — thus, the effect of full 3G rollout is about 18 % of the baseline average (in 2008, 28 % of all GWP respondents reported having access to the internet). This effect is probably

[21] For more details on the Polity IV project, see: https://www.systemicpeace.org/polityproject.html

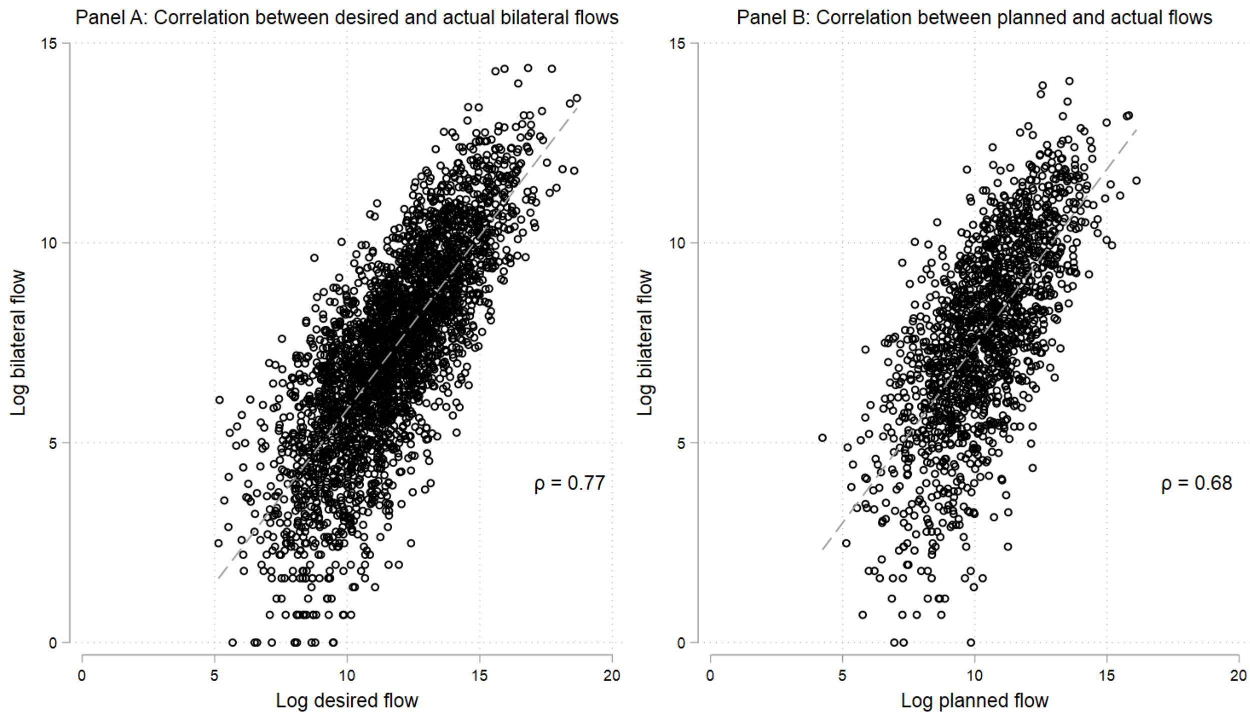
[22] For more details on GeoDist, see Mayer and Zignago (2011).

[23] For more details on the UN migrant stock data, see United Nations Population Division (2019).

[24] The 'access to the internet' variable is constructed using the following two GWP questions: *Does your home have access to the internet? (2008-2015)* and *Do you have access to the internet in any way, whether on a mobile phone, a computer, or some other device? (2016 – 2018)*

an underestimation of the effect of 3G on internet access, as prior to 2016 the question about internet access probes access *at home* only.

Figure 4: Bilateral Desire and Plans to Migrate are Strongly Related to Actual Migration




Note: Scatterplot of the log of the cumulative migration flow versus the log of the estimated population desiring (Panel A; N = 3,055) and planning (Panel B; N = 1,590) to migrate from 155 origin countries to 35 OECD countries between 2008 and 2018. The constructed flow between an origin and destination is calculated as follows: we multiply the origin-level population in 2008 by the average share of GWP respondents desiring to migrate to the particular destination country, based on all GWP responses in the origin country between 2008 and 2018. Every data point is an origin-destination pair. Dyads with no actual or desired (planned) flows are omitted in Panel A (B).

Second, we check to what extent our outcome variables are statistically significantly associated with actual migration flows. We make use of the fact that we observe individuals' most desired destination as well as the destination country they are planning to move to. We use these data to construct the number of people desiring and planning to migrate between any origin and destination country.[25] We then match our *desired* and *planned* migration-flow matrix with data on actual migration flows to OECD countries between 2008 and 2018 and

[25]Bilateral rates are constructed by weighting observations within the origin country using Gallup weights to make the data representative at the country level.

calculate the log of actual, desired, and planned flows. We omit all dyads with zero actual (166 dyads), desired (1,353) or planned (2,825) flows.[26] The results presented in Figure 4 confirm that our outcome variables are strongly associated with the official gross migrant flow data. The correlation on the origin-destination level between the log of actual migrant flows and the log of the number of respondents desiring to migrate from a specific origin to a specific destination is 0.77. The raw correlation between yearly migration flow rates and the number of respondents planning to migrate from a specific origin to a specific destination is 0.68. The latter correlation is mechanically weaker because of the lower number of Gallup respondents planning to migrate to a specific destination.[27] Thus, taken as a whole, we find that our outcomes are strongly positively related to actual migrant flows and, hence, very likely to deliver meaningful information on cross-border movements of people.

Overall, these results suggest that both our treatment and outcomes capture relevant variations in internet access and migration.

# 5 Empirical Strategy

In this section we describe the three complementary estimation strategies that we use to study the effect of 3G coverage on the desire and plans to emigrate. Our main empirical specification is a Two-Way Fixed Effects (TWFE) regression with a continuous treatment. We complement the TWFE approach by a new estimator by de Chaisemartin and D'Haultfœuille (2020*a*) that is robust to heterogeneous and dynamic treatment effects, but does not allow to exploit all available variation in mobile network coverage. Finally, we consider an instrumental variable strategy to dispel concerns about endogeneity of the rollout of mobile internet, using pre-existing 2G mobile networks as to predict subsequent mobile internet rollout.

[26]For planning to migrate, we only use the time period 2010 – 2015. For 2008 and 2009, the destination country for *planning* to migrate was not allowed to be different from the previously indicated destination country for *desiring* to migrate. As ideal and realistic destination countries may differ, we omit the data from 2008 and 2009.

[27]For a more detailed discussion about the relation between migration aspirations, intentions and realized migration, we refer the reader to Tjaden, Auer and Laczko (2019). In addition to a cross-sectional analysis, they use the GWP and OECD data to show that also temporal variation in migration intentions is predictive of subsequent bilateral migration.

## 5.1 Main Estimation Model

We estimate the effect of mobile internet access on individuals' migration aspirations and intentions using a difference-in-differences methodology. Our models take the following form:

$$Outcome_{idt} = \beta 3G_{dt} + \alpha^{'}\mathbf{X_{idt}} + \phi_d + \theta_t + \gamma_d \cdot t + \epsilon_{idt} \tag{6}$$

where $i$ indexes the individual, $d$ the subnational district, and $t$ the year.

We use outcomes (1), (2) and (3) from Section 4: (1) whether an individual would like to move permanently to another country; (2) whether an individual is planning to move abroad permanently in the next 12 months; and (3) whether an individual is likely to move away from the city or area in which he or she lives in during the next 12 months. Responses to all three questions are coded as dummy variables, with 1 representing a positive answer and 0 representing a negative answer. Additionally, we use constructed outcomes displayed graphically in Figure 2 corresponding to the union of I and III, the intersection between II and III, and the set difference of III and I. We estimate linear probability models for ease of interpretation.

To measure 3G internet coverage, our treatment variable, we follow Guriev, Melnikov and Zhuravskaya (2021) and calculate the share of the district's territory covered by 3G networks in a given year, weighted by population density at each 1x1 kilometer grid-level.[28] This captures an intention-to-treat effect of mobile internet, which includes the direct effect of individuals adopting mobile internet as well as spillover effects.

The vector of controls, $X_{idt}$, include:

- individual-level demographic characteristics (age and age-squared, a male dummy, an urban dummy, as well as dummy variables for marital status, presence of children in the household, educational attainment and not born in the country of interview);
- log of per capita income of the household;
- satisfaction with life and local amenities; and

[28] As, for the years 2011 to 2018, coverage data is updated until December, we use the known coverage in December $t-1$ to represent the 3G coverage in year $t$. For further discussion about the 3G data and its timing, see Section 4.

- district-year level average income and country-year level share of respondents under 30, political regime as measured by polity2 and log of GDP per capita.

Of course, one might worry that some of the control variables (such as household income or satisfaction with local amenities) are themselves affected by 3G-related economic shocks. In Table 1, we dispel concerns about "bad controls" (Angrist and Pischke, 2008) by adding these characteristics gradually. Doing so barely changes the point estimate for our variables of interest. Nevertheless, we keep these controls in our main specification to alleviate concerns related to omitted variable bias.[29]

In all models, we include year dummies, $\theta_t$, (to capture the impact of global shocks that affect all countries simultaneously), district dummies, $\phi_d$, (to control for time-invariant variation in the outcome variables caused by factors that vary across districts) and district-specific linear time trends, $\gamma_d \cdot t$, (to remove distinctive trends in outcome variables in various districts that might otherwise bias our estimates if they accidentally coincided with 3G internet-related changes). In the fully saturated models, the estimates are identified by exploiting within-district variation that has been stripped of any influence of constant and linearly changing district-level characteristics.

We two-way cluster standard errors by country-year and subnational district and use sampling weights provided by Gallup to make the data representative at the country level. For all outcomes related to "plans to migrate", we restrict our sample to those who are adults or become adults within one year ($\geq$17 years) as minors usually do not have the ability and/or legal right to plan migration within 12 months.

***Threats to Identification***

One can imagine several potential threats to identification. We address these as follows:

1. To alleviate concerns that the parallel trends assumption may not hold around an increase in 3G coverage, we check whether districts display similar pre-trends in terms of outcomes. We compare the trend between districts that (i) are about to get treated with 3G coverage

[29] We omit smaller subgroups of the included controls in Appendix Table A4 to show that separate omission of being able to count on friends, satisfaction with local amenities and life satisfaction does not alter the results.

and (ii) are not yet or will not be treated. We provide evidence by two event studies: one around any first increase in 3G coverage and one around large increases (at least 50 percentage points in one year) in 3G coverage.[30] The results indicate parallel trends prior to 3G adoption.[31] In addition, we also show that leads (that is, future levels of 3G coverage) do not affect current migration aspirations.

2. We also include district-specific linear time trends, which remove variation in within-district movements in migration intentions and desires due to factors that are district-specific and that evolve linearly over time. In the fully saturated models, the identification comes from 3G expansions that entail deviations from pre-existing district-specific trends (see Besley and Burgess (2004) for a similar application). As suggested by Angrist and Pischke (2008), after including a parametric trend, the identification hinges on there being a marked change in the outcome on the year of the treatment. Following Autor (2003), we also conduct an F-test of the hypothesis that the country-specific trends are jointly zero. This hypothesis is strongly rejected by the data (the p-value for this test of joint significance is 0.00). We, therefore, keep linear trends in our specifications.[32]

3. Several other factors could potentially affect 3G internet access and migration aspirations simultaneously, net of a linear local time trend. We, therefore, control for a wide range of observable factors (such as the economic development level of districts) as listed above as well as fixed effects to address potential omitted variables concerns.

4. Although we fully saturate our specifications with fixed effects and linear trends, there could still be other omitted variables that are correlated with 3G internet access. To address this concern, we use the methodology developed by Oster (2019). The results

[30] In the second, we focus on the subnational districts with a large increase in 3G coverage, although these constitute only around 25 % of the sample. The vast majority of the remaining 75 % of districts shows a more gradual increase in 3G coverage, where testing pre-trends is more challenging. The event study around such large increases is nevertheless complementary to the first. It allows us to study whether districts obtaining large hikes in 3G coverage are on different trends in migration aspirations than those who do not yet, which is a more extreme case than any first increase in 3G coverage.

[31] We present the first event study in section 6.2, where we do not reject the null hypothesis of joint insignificance of 4 years of pre-trends ($p = 0.63$). Similarly, in Online Appendix A.2 we also do not reject this null hypothesis ($p = 0.22$).

[32] In Appendix Table A15, we also show that our results are robust to *not* including district-specific trends.

suggest that our findings are unlikely to be driven by omitted variables bias.

5. Another concern is that also the expansion of 2G infrastructure can affect individuals' migration behavior. As 2G technology only allows for calling, texting and a very limited internet connectivity, it is distinct from 3G technologies. However, as 2G and 3G networks rely on similar technologies and infrastructure, expansion of both types of networks may coincide. To ensure that our results are not driven by simple communication technologies, we show that 2G network coverage has no impact on our outcomes.

6. We also conduct multiple hypothesis testing by employing a randomization inference technique recently suggested by Young (2019). In particular, this adjusts for the fact that we are testing multiple hypotheses simultaneously and controls for the tendency for false positives. The method builds on repeatedly randomizing the treatment variable in each estimation and comparing the pool of randomized estimates to the estimates derived via the true treatment variable. The results presented in Figure A11 in Online Appendix A show that our findings remain robust both for the individual coefficients and the joint tests of treatment significance.

All of these and additional identification-related issues are addressed in more detail in the Results and in Appendix A.2.

## 5.2 An Alternative to Two-Way Fixed Effects Estimators

TWFE models are suitable for estimating average treatment effects on the treated (ATT) in the case of homogeneous and non-dynamic treatment effects. By decomposing the TWFE estimator under various assumptions, however, a recent literature has shown that the TWFE estimator is problematic in the presence of heterogeneous[33] and dynamic[34] treatment effects (Goodman-Bacon, 2021; Borusyak, Jaravel and Spiess, 2021; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfoeuille, 2020*b*; Sun and Abraham, 2021).

To enable the estimation of treatment effects on the treated in the presence of heterogeneous and dynamic treatment effects, one needs to carefully select treatment and control groups. The estimators of Callaway and Sant'Anna (2021) and de Chaisemartin and D'Haultfoeuille (2020*b*) use both never treated and not yet treated units to assess the contemporaneous and dynamic treatment effect.[35] de Chaisemartin and D'Haultfœuille (2020*a*) implement an alternative estimator that identifies an ATT by calculating treatment effects using appropriate control groups. Their estimator is more suitable for our purpose than the estimators proposed by Callaway and Sant'Anna (2021), Borusyak, Jaravel and Spiess (2021) and Sun and Abraham (2021), as it allows for non-binary treatments.

We discuss the implementation of the de Chaisemartin-D'Haultfœuille estimator, which is based on pairwise difference in differences, in Online Appendix A.3. Importantly, the estimator calculates $DiD_l$, the treatment effect after obtaining treatment for the first time $l$ periods ago, using a weighted average of the elementary building blocks $DiD^{ini}_{g,l}$ (where $g$ refers to the unit receiving treatment – in our case the subnational region). This is the covariate-adjusted difference between treated and appropriate control units in the differences in outcome over $l$ periods for treated units that obtained first treatment at time $F_g$ and where treated and control units have initial treatment $ini$. In other words, treated units are only compared to control units with the same initial treatment $ini$. In a similar fashion, we calculate the pre-treatment

[33]In the case of heterogeneous treatment effects, the problem arises because the estimated $\hat{\beta}_{TWFE}$ is a weighted average of group time-level average treatment effects, where the weights are unequal over groups and time, and may be negative. In a general design, weights are more likely to be negative for periods in which many groups are treated and to groups treated for many periods (de Chaisemartin and D'Haultfoeuille, 2020*b*). In a staggered adoption design (a setting where units can move into, but not out, of a binary treatment with heterogeneous timing between groups), this implies that weights on later time periods are more probable to be negative (Borusyak, Jaravel and Spiess, 2021).

[34]When considering a setting with two time periods and one treatment (treatment status changes by one unit) and one control group (treatment status is unchanged), the possibility of dynamic effects requires one to account for the prior path of treatment and control group. Intuitively, a TWFE regression does not control for the complete past treatment history, and is thus not robust to dynamic effects. Similarly, Sun and Abraham (2021) show that the pre- and post-event effect estimates in the canonical event study setting may mix, leading to incorrect estimates of pre-event trends, as well as the instantaneous and dynamic effect of treatment.

[35]A unit that received treatment previously may carry dynamic treatment effects and may thus be unsuitable as a control unit.

difference in differences $DiD_l^{pl}$, which allows us to assess pre-trends between the same treatment and control units. We have to make the following two approximations to be able to calculate $DiD_l$ and $DiD_l^{pl}$ for a sizable part of our sample:

- **Define a threshold $\Delta 3G$, below which treatment between two consecutive years is stable.** As many districts show small increases over time, at the end of the sample period in 2018, most districts saw some increase in 3G coverage. Thus, to have sufficient number of control units for calculation of all $DiD_{g,l}^{ini}$, we need to consider units that have received minimal treatments as untreated.

- **Divide the sample into initial treatment groups $ini$.** The initial treatment is the level of 3G coverage in 2008. However, 3G coverage is continuous, which means that, apart from the regions not yet treated in 2008, all other regions have a unique level of initial treatment. To be able to match treatment and control units to calculate $DiD_{g,l}^{ini>0}$, we bin initial treatments in groups $ini = 0$ and $ini > 0$. As the estimator of de Chaisemartin and D'Haultfœuille (2020*a*) performs covariate adjustment within initial treatment bins, we need the number of observations per bin to be sufficiently large. Therefore, we can not allow for splitting up the $ini > 0$ bin in multiple bins.[36]

The estimator computes treatment effects in the outcome $Y$ for all $l$ periods after obtaining first treatment ($DiD_l$). In a similar fashion, we can calculate $DiD_l$ for the treatment variable itself, which simply tells us how much the treatment increased $l$ periods after a district received treatment for the first time. Using the $DiD_l$ for both the treatment as well as for the outcome variable, we can calculate an average effect size of a unit treatment, $\hat{\delta}^L$. In the absence of treatment heterogeneity, dynamic effects and the approximations discussed above, this corresponds to the point estimate $\beta$ of the TWFE estimator.

However, there is a trade-off as TWFE has an advantage of using all information available in a continuous treatment while the estimator by de Chaisemartin and D'Haultfoeuille (2020*b*)

[36] Covariate adjustment in the estimator of de Chaisemartin and D'Haultfœuille (2020*a*) is performed within an initial treatment bin, by performing a regression on the sample of groups where the treatment did not increase (by more than the threshold) yet. Therefore, we need to have at least as many (not yet treated) group-time periods as we have covariates and time periods (as we include time fixed effects). For more details, see Online Appendix A.3.

focuses on changes in outcome around first increases in 3G coverage. Furthermore, to find suitable control groups, one needs to define a threshold of stable treatments, which disregards some of the information available in our treatment. Therefore, we consider TWFE as our main specification and use the de Chaisemartin and D'Haultfœuille estimator as a complementary approach.

### 5.3 Instrumental Variable Strategy

***Pre-existing 2G Infrastructure and Faster 3G Rollout***

Many factors contribute to variation in the construction of 2G cell towers prior to our period of study. As the cell tower infrastructure can be shared by a 2G and a 3G Base Transceiver Station (BTS), expansion of 3G networks is more costly when 2G network infrastructure is absent. We use the variation in 2G networks prior to 2006 as a measure of pre-existing infrastructure. The larger the coverage of 2G networks in 2006, the stronger the predicted 3G expansion is in our period of study. We measure pre-period infrastructure in 2006, as 2G networks in earlier years are absent in poorer regions in our sample. We construct our instrument by interacting the 2G network in 2006 with a linear time trend. A drawback of this approach is that we can not include district-level time trends. Instead of these trends, we include interactions of a large set of district-level geographic and demographic variables interacted with a linear time trend. Importantly, as initial 3G coverage strongly predicts subsequent coverage, we include interactions between a linear time trend and (i) the level of 3G coverage in the district in 2008, (ii) a dummy variable for zero 3G coverage in the district in 2008, and (iii) a dummy variable for zero 3G coverage in the country in 2008. This prevents our instrument from capturing the effect of initial 3G coverage in the subsequent expansion of 3G networks.

## 6 Results

In this section, we present four sets of results. First, we present our baseline results on the effects of 3G rollout on migration aspirations and intentions using the OLS estimator with two-way fixed effects. Thereafter, we focus on the desire to emigrate and we present results for the de Chaisemartin-D'Haultfœuille estimator for non-binary treatment, and one IV strategy. Ultimately, we conduct a heterogeneity analysis using the recently developed causal forest procedure.

## 6.1 Main Results

Table 1 reports estimates of Equation 6 for the three main outcomes. The dependent variables are binary variables indicating that the respondent "if he/she would have the opportunity, would like to move permanently to another country" (first panel), that the respondent "is planning to move permanently to another country in the next 12 months" (second panel), and that the respondent "is likely to move away from their current city or area in the next 12 months" (third panel). In parentheses we denote which mutually exclusive groups the outcome variables pertain to, as identified in Figure 2. Column 1 reports estimates with district and year fixed effects and district-specific time trends. Column 2 adds the demographic characteristics, Column 3 adds controls related to life satisfaction and logarithm of individual income and district-level income (to control for regional development), Column 4 adds country-level controls, Column 5 fully saturates the specification with country-by-income tercile and country-by-educational-attainment fixed effects to control non-parametrically for all potentially omitted variables that can vary across countries and income terciles, and countries and educational attainment levels.

All Columns show a positive, statistically significant relationship between 3G mobile internet expansion and desire and plans to emigrate. The most conservative estimates in Column 5 restrict all variation to within-country income tercile and within-country educational attainment. These estimates are similar in magnitude to the first 4 Columns.

In our preferred model (Column 4), we find that a 10 percentage point increase in 3G coverage leads to 0.27 percentage point increase in the desire to emigrate, a 0.09 percentage point increase in emigration plans in the next 12 months, and a 0.27 percentage point increase in likelihood to migrate (domestically or internationally) in the next 12 months. Given that the mean levels of these outcome variables are 22.0, 2.8 and 17.1 %, the effects are sizable.

Table 1: The Effects of 3G Internet Expansion on Desire and Plans to Emigrate

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Outcome: | Desire to emigrate (I-IV) | | | | |
| 3G | 0.027** | 0.026** | 0.028*** | 0.027** | 0.026** |
| | (0.012) | (0.012) | (0.011) | (0.011) | (0.011) |
| Observations | 617,402 | 617,402 | 617,402 | 617,402 | 617,402 |
| $R^2$ | 0.12 | 0.16 | 0.19 | 0.19 | 0.19 |
| Average dependent variable | 0.220 | 0.220 | 0.220 | 0.220 | 0.220 |
| First year | 2008 | 2008 | 2008 | 2008 | 2008 |
| Last year | 2018 | 2018 | 2018 | 2018 | 2018 |
| Outcome: | Plans to emigrate in the next 12 months (I+II) | | | | |
| 3G | 0.008** | 0.009** | 0.009** | 0.009** | 0.008* |
| | (0.004) | (0.004) | (0.004) | (0.004) | (0.004) |
| Observations | 379,703 | 379,703 | 379,703 | 379,703 | 379,703 |
| $R^2$ | 0.06 | 0.07 | 0.07 | 0.07 | 0.08 |
| Average dependent variable | 0.028 | 0.028 | 0.028 | 0.028 | 0.028 |
| First year | 2008 | 2008 | 2008 | 2008 | 2008 |
| Last year | 2015 | 2015 | 2015 | 2015 | 2015 |
| Outcome: | Likely to migrate in the next 12 months (I+III+V) | | | | |
| 3G | 0.027** | 0.026** | 0.027*** | 0.027*** | 0.027*** |
| | (0.010) | (0.010) | (0.010) | (0.010) | (0.010) |
| Observations | 547,758 | 547,758 | 547,758 | 547,758 | 547,758 |
| $R^2$ | 0.10 | 0.13 | 0.16 | 0.16 | 0.16 |
| Average dependent variable | 0.171 | 0.171 | 0.171 | 0.171 | 0.171 |
| First year | 2008 | 2008 | 2008 | 2008 | 2008 |
| Last year | 2018 | 2018 | 2018 | 2018 | 2018 |
| District and year fixed effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| District-year trends | ✓ | ✓ | ✓ | ✓ | ✓ |
| Demographic controls | | ✓ | ✓ | ✓ | ✓ |
| Life satisfaction-related controls | | | ✓ | ✓ | ✓ |
| Income controls | | | ✓ | ✓ | ✓ |
| Country-level controls | | | | ✓ | ✓ |
| Country×income quintile fixed effects | | | | | ✓ |
| Country×education fixed effects | | | | | ✓ |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. This table reports the results of Equation 6 using the three major migration-related questions in GWP. The demographic controls include: male dummy, age, age squared, dummy variables for marital status (single, married), the presence of children in the household, living in an urban area, educational attainment (secondary education, tertiary education) and a dummy for whether the respondent is born in the country. Life satisfaction-related controls include: satisfaction with housing, healthcare, education, roads, transportation, city, life and whether the respondent can count on family or friends, whether the respondent believes they will be financially better off in five years, whether the respondent has sufficient means for food and shelter, and whether the respondent had something stolen in the past year. Income controls include the log of household income per person on the individual level and the log of the average of household income per person on the subnational region year-level. Country-level controls include: the log of real GDP per capita, polity2 score and the share of respondents aged under 30. The standard errors are clustered two-way on the country-year and district level.

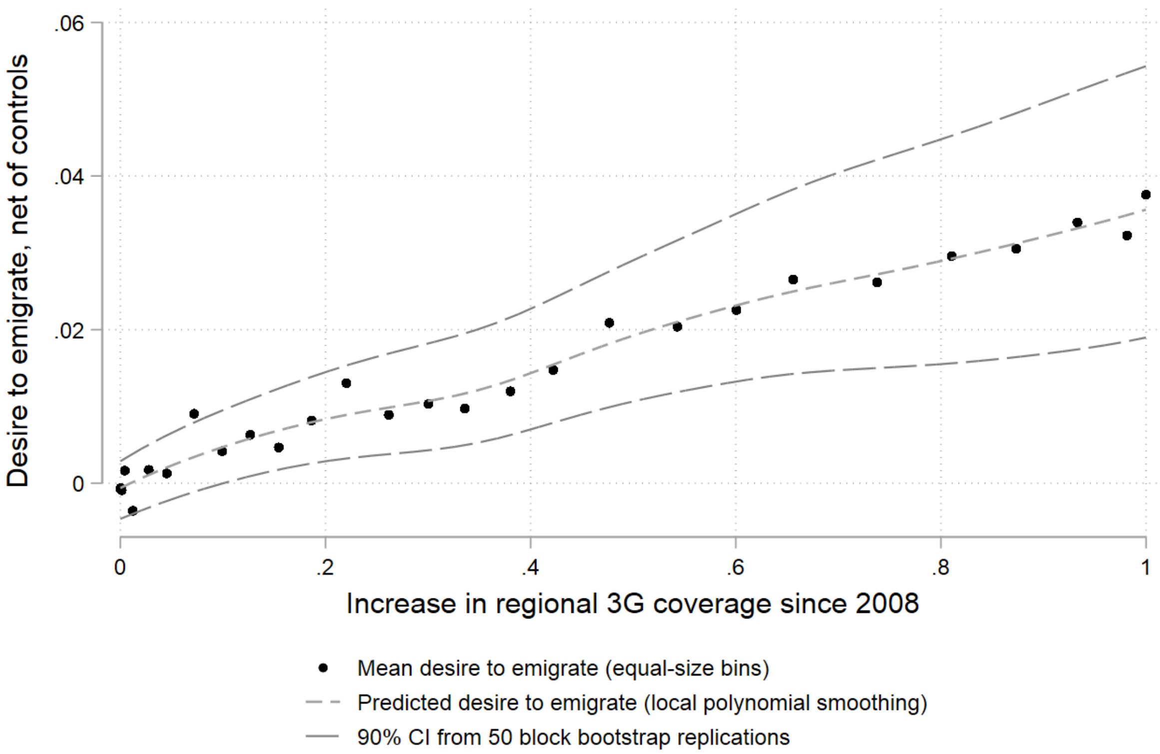
Importantly, 3G internet expansion not only has an impact on *desire* but also shapes *actual plans* to emigrate. Our estimates suggest that an increase in 3G coverage of 36 % (corresponding to the average increase in 3G coverage between 2008 and 2018 across the 2,120 subnational regions in the sample) leads to an increase in the desire to emigrate permanently by 0.21 to 1.82 percentage points (95 % confidence interval). Correspondingly, such an increase in 3G coverage leads to an increase in the share of population planning to emigrate permanently between 0.00 and 0.64 percentage points.

To corroborate that the results are driven by an increase in the number of individuals covered by 3G networks, we show the non-parametric effect of the 3G rollout on the desire to emigrate, net of all baseline controls and fixed effects, in Figure 5. The figure suggests that the effect is relatively homogeneous in the intensive margin of 3G coverage. This is in line with the way the 3G coverage variable is constructed, as it represents the share of population covered by 3G.

We also follow the method proposed by (Oster, 2019) to investigate whether our results could be driven by unobservable factors. Appendix Figure A3 shows the Oster's $\delta$, which 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 are reassuring: given the controls we have in our models, it seems unlikely that unobserved factors are 6 times more important than the observables included in our preferred specification, which makes it highly unlikely that our results can be explained by omitted variables bias.[37]

[37] The rule of thumb to be able to argue that unobservables are unlikely to fully explain the treatment effect is for Oster's $\delta$ to be over the value of one (Oster, 2019).

Figure 5: The Non-parametric Effect of 3G Rollout on the Desire to Emigrate




Notes: the Figure shows the desire to emigrate net of all controls by 40 bins of equal sample size, ordered by the relative increase in 3G coverage since 2008. 14 out of 40 bins are concentrated at a 3G coverage of 0, as these observations do not have coverage yet. The grey dashed line displays a local polynomial smoothing through the 40 points, and the outer dashed lines show a continuous 90 % block bootstrapped confidence interval using 50 bootstrap replications.

Table 2 reports estimates for three additional dependent variables.The dependent variables are a dummy indicating that the respondent "has any desire to emigrate or deems it likely to migrate in the upcoming 12 months" (first panel), that the respondent "likely plans to emigrate in the next 12 months" (second panel); and that the respondent "is only likely to migrate domestically in the next 12 months" (third panel).

Taken together, Tables 1 and 2 and Figure 5 show that 3G internet coverage has a positive, sizable and statistically significant effect on the desire and plans to emigrate, but no statistically significant effect on the perceived likelihood of domestic migration. This finding suggests that 3G expansion shapes emigration intentions and plans rather than domestic migration. This is intuitive as, even in the absence of internet connectivity, people are likely to be already well-informed about opportunities in their own country as opposed to opportunities in other countries.

## 6.2 de Chaisemartin and D'Haultfœuille Estimator and Testing for Pre-trends

In this section, we examine the validity of the pre-trends assumption and the properties of our TWFE regressions as the impact of 3G expansion is likely to vary across districts and over time. In particular, weight decompositions of group time-level treatment effects suggest that our results in Table 1 are susceptible to treatment effect heterogeneity.[38] To investigate whether our results are driven by this potential bias, we use a novel estimator by de Chaisemartin and D'Haultfœuille (2020*a*), which is valid even if the treatment effect is heterogeneous.

We proceed as follows: (i) to have sufficient observations in every initial treatment group and sufficient observations to include all baseline covariates,[39] we assign subnational regions with non-zero initial treatment in 2008 (that is, $ini > 0$) to a single bin; (ii) we omit districts where treatment is not monotonically increasing. As many districts show minor decreases in coverage, we only omit districts where treatment decreases more than 3 percentage points of population between any two subsequent years between 2008 and 2018;[40] and (iii) to have sufficient untreated and not-yet-treated observations in later time periods, we set the threshold for a first switch into treatment, $\Delta_{3G}$, to 3 percentage points of population. We opted for a threshold of 3 percentage points as the largest proportion of minor increases is concentrated below 3 percentage points; results are qualitatively similar if using 2 or 5 percentage points as the threshold. A drawback of a higher threshold $\Delta_{3G}$ is that the never treated and not-yet-treated groups include districts that experienced small increases in treatment.

[38] de Chaisemartin and D'Haultfoeuille (2020*b*) developed a procedure (TWOWAYFEWEIGHTS) to calculate how many of the weights on the group time-level treatment effects are negative and what the sum of negative weights is (where all weights sum to unity). Using TWOWAYFEWEIGHTS while allowing for heterogeneous treatment effects, we find that the sum of negative weights for the TWFE regressions featured in Column 4 of the three panels from top to bottom in Table 1 are -0.77,-0.44 and -0.78 (the total sum of weights is +1 by construction). As a substantial portion of the weights is negative, this suggests that our baseline results could be biased.

[39] As covariate adjustment is performed within every initial treatment group, increasing the number of initial treatment groups reduces the number of available observations for covariate adjustment. For a more detailed description of the covariate adjustment procedure, see Online Appendix A.3.

[40] This happens in 234 out of 2,120 subnational regions.

Table 2: The Effects of 3G Internet Expansion on Alternative Outcome Variables

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Outcome: | Any desire or plans to migrate (I-V) | | | | |
| 3G | 0.041*** | 0.040*** | 0.042*** | 0.041*** | 0.040*** |
| | (0.014) | (0.014) | (0.013) | (0.013) | (0.013) |
| Observations | 489,182 | 489,182 | 489,182 | 489,182 | 489,182 |
| $R^2$ | 0.12 | 0.18 | 0.21 | 0.21 | 0.22 |
| Average dependent variable | 0.311 | 0.311 | 0.311 | 0.311 | 0.311 |
| First year | 2008 | 2008 | 2008 | 2008 | 2008 |
| Last year | 2018 | 2018 | 2018 | 2018 | 2018 |
| Outcome: | Planning likely emigrant within 12 months (I) | | | | |
| 3G | 0.010*** | 0.011*** | 0.011*** | 0.011*** | 0.011*** |
| | (0.003) | (0.003) | (0.003) | (0.004) | (0.003) |
| Observations | 342,328 | 342,328 | 342,328 | 342,328 | 342,328 |
| $R^2$ | 0.05 | 0.06 | 0.06 | 0.06 | 0.07 |
| Average dependent variable | 0.018 | 0.018 | 0.018 | 0.018 | 0.018 |
| First year | 2008 | 2008 | 2008 | 2008 | 2008 |
| Last year | 2015 | 2015 | 2015 | 2015 | 2015 |
| Outcome: | Likely internal migrant within 12 months (V) | | | | |
| 3G | 0.009 | 0.009 | 0.009 | 0.008 | 0.008 |
| | (0.008) | (0.008) | (0.008) | (0.008) | (0.008) |
| Observations | 489,182 | 489,182 | 489,182 | 489,182 | 489,182 |
| $R^2$ | 0.04 | 0.05 | 0.06 | 0.06 | 0.06 |
| Average dependent variable | 0.092 | 0.092 | 0.092 | 0.092 | 0.092 |
| First year | 2008 | 2008 | 2008 | 2008 | 2008 |
| Last year | 2018 | 2018 | 2018 | 2018 | 2018 |
| District and year fixed effects | ✓ | ✓ | ✓ | ✓ | ✓ |
| District-year trends | ✓ | ✓ | ✓ | ✓ | ✓ |
| Demographic controls | | ✓ | ✓ | ✓ | ✓ |
| Life satisfaction-related controls | | | ✓ | ✓ | ✓ |
| Income controls | | | ✓ | ✓ | ✓ |
| Country-level controls | | | | ✓ | ✓ |
| Country×income quintile fixed effects | | | | | ✓ |
| Country×education fixed effects | | | | | ✓ |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. See notes to Table 1. The first measure is constructed using the union of positive answers on migration question (1) and (3). The second measure is the likely plans to migrate (positive to question (2) and (3), as defined in Figure 2. The third measure comprises those answering positively to (3) but negatively to (1) The standard errors are clustered two-way on the country-year and district level.

In Figure 6, we show the instantaneous and four dynamic estimators (referring to one, two, three or four years after the expansion), $DiD_l^Y$ (where $l \geq 0$), and four placebo estimators (referring to one, two, three or four years before the expansion), $DiD_l^Y$(where $l < 0$), for our treatment variable $3G$ in Panel A and outcomes of interest $Y$ in Panels B, C and D.[41] The confidence interval of the placebo estimators should enclose 0 to support the parallel trends assumption.[42] Notably, the results reported in all panels of Figure 6 provide *no evidence* of pre-trends.

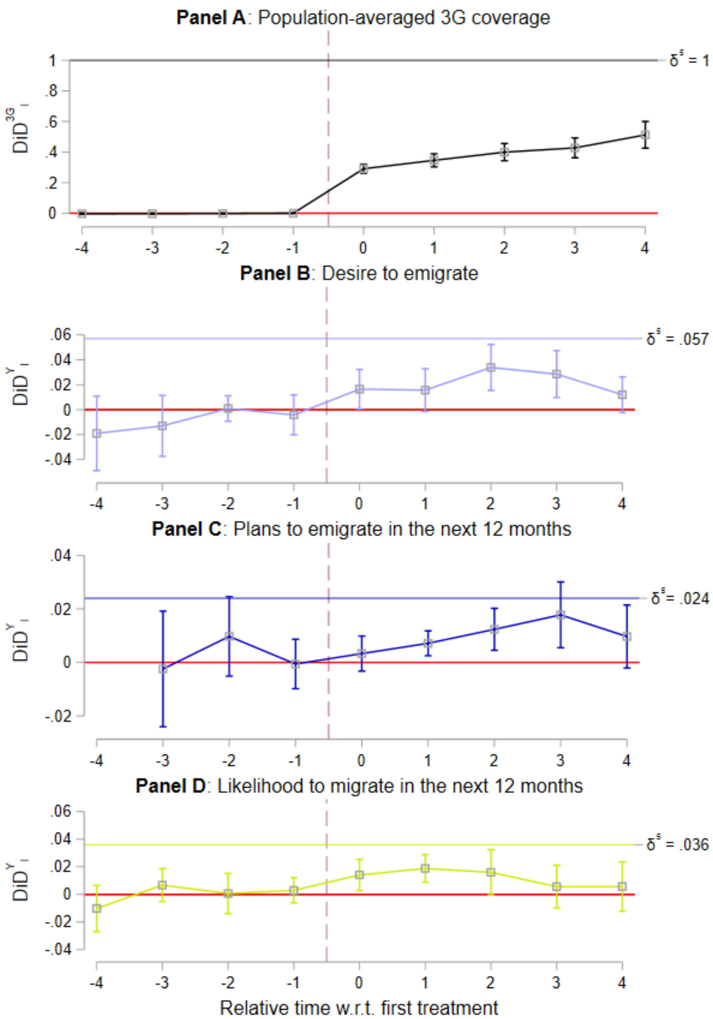
When it comes to evolution of post-treatment effects, in Panel A, we find that 3G coverage increases steadily over time after the initial jump. In Panel B, we observe that the desire to emigrate increases immediately after an initial increase in 3G coverage and then remains stable. The instantaneous effect of a one unit increase of 3G coverage is 0.055 (p = 0.04), which exceeds our TWFE estimate. The average effect of all observations following a first increase in 3G expansion $\delta^5$ is 0.057 (p < 0.001), which also exceeds our TWFE estimate. Two reasons can be underlying the difference between the TWFE and the de Chaisemartin-D'Haultfœuille estimator: (i) TWFE estimators are biased when treatment effects are heterogeneous and (ii) the average effects are calculated over the first 5 periods after receiving treatment. As for the first point, the TWFE estimator is more likely to assign a negative weight to periods where a large fraction of groups are treated, and to groups treated for many periods (de Chaisemartin and D'Haultfoeuille, 2020*b*). This may lead to a downward bias in the reported results if treatment effects are larger in the final years of the sample and if units treated for most periods in the sample (e.g. high-income countries) have higher treatment effects. As for the second point, we report the de Chaisemartin-D'Haultfœuille estimator for the first five periods after receiving treatment, but not the later time periods (as only few control groups are left in later time periods). As marginal treatment effects may be decreasing (for example, 3G expansions many periods after first expansion of 3G coverage in a district may draw few new mobile internet users), the de Chaisemartin-D'Haultfœuille estimator could capture the time period

[41] We use the DID_MULTIPLEGT command in STATA 16. As two-way clustering of standard errors is not possible in this command, we cluster standard errors at the country level. Note that, in Table A12, we find that clustering at the country level gives somewhat smaller standard errors than our baseline estimates.

[42] To assess whether pre-trends between treatment and control are insignificant over the 1 to $l + 1$ periods before treatment, we consider the null hypothesis that all of the placebo estimators are zero.

with the largest marginal effects. Panel C presents the results for plans to emigrate in the next 12 months. We observe that plans to emigrate increase gradually after receiving treatment. However, the instantaneous effect is not statistically significantly different from 0. The average effect of all observations following a first increase in 3G expansion $\delta^5$ is 0.024 ($p < 0.001$). Panel D shows results for the self-assessed likelihood to migrate in the next 12 months. The propensity to deem migration likely in the next 12 months increases in the first three years after first treatment, but is not statistically significant thereafter.

Figure 6: De Chaisemartin-D'Haultfouille Estimates for 3G and Migration Aspirations and Intentions




Notes: we show $DiD_l$ of the effect of first switchers in 3G coverage in Panel A on the three main outcomes in Panels B, C and D. The $p$-value for jointly insignificant pre-trends equals 0.63 in Panel B, 0.44 in Panel C and 0.52 in Panel D. $\delta^5$ denotes the estimated average effects of a 1 unit increase in 3G coverage using the instantaneous effects and the 4 dynamic effects. Per definition this is unity for panel A, as the outcome and the treatment variable are the same. The threshold for a switch into treatment is an increase of coverage of 3 % of the population. Treatment and control groups are binned in two groups, either those with initial treatment level $ini = 0$ or those with initial treatment level $ini > 0$ in 2008. Observations are weighted using the Gallup weights. After a district switches, an observation $l$ periods after the switch can no longer be part of a control group anymore and is only considered for the $l$th dynamic effect of its first switch. Note that the placebo estimators $l$ are labelled by $-l-1$ on the x-axis to indicate that these estimators are difference-in-differences before a first switch in treatment happens. As the $l$th placebo estimator require $2l+3$ years of data (see Online Appendix A.3), the $l = 3$ placebo estimator for panel C is infeasible. Standard errors are calculated using 50 bootstrap replications, clustered on the country level, 95% confidence intervals are shown.

### 6.3 Instrumental Variables Based on Pre-Existing 2G Infrastructure

We construct our instrument using the information available on pre-existing levels of 2G infrastructure prior to the period of our study (see, Campante, Durante and Sobbrio (2018) for a similar approach). The greater the coverage of 2G is, the more infrastructure exists (e.g., cell towers and cabling) that is also essential to 3G internet provision. We interact the 2G coverage in 2006, $2G_{2006,r}$, with a linear time trend. As our baseline includes regional-level linear time trends, we omit those in the IV estimations. Instead, we introduce a battery of controls related to geography and initial 3G coverage by district in 2008 interacted with a linear time trend. In addition, we control for time-varying 2G coverage ($2G_{t,r}$) to alleviate concerns about the validity of the exclusion restrictions.

Table 3 shows the 2SLS estimates. The first column is slightly different than the first two columns of Table 1, because of the inclusion of $2G_{t,r}$. The first-stage results in Column (4) present a positive and highly significant effect of initial 2G coverage. The first stage F-statistic is 41.90, suggesting a very strong relation between 2G coverage in 2006 and the expansion of 3G.

In line with our baseline results and the results of the de Chaisemartin-D'Haultfœuille estimator, the IV estimates in Column (3) also indicate that 3G expansion leads to an increase in desire to emigrate. The IV estimate (0.098) is greater in magnitude than the average effects of the de Chaisemartin-D'Haultfœuille estimates (0.057) and the TWFE estimate (0.027). This can be explained by the endogeneity of 3G network expansion. It is plausible that 3G network is expanded earlier in districts that develop more positively, and where institutional quality and other types of infrastructure are better. Operators are likely to prioritize expanding 3G network in regions where economy is expected to grow faster, but expected faster growth also reduces push factors to emigrate, meaning that our baseline estimates may be downward-biased. Additionally, the TWFE and de Chaisemartin-D'Haultfœuille results may be subject to measurement error: not all mobile phone operators in a country provide network information, not all operators provide it timely or correctly, and 3G coverage may vary within years. Although measurement error may be non-classical, it is unlikely to be correlated to the district-year-level propensity to desire emigrate.

Table 3: 2G Infrastructure IV Results

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dependent variable: | Desire to emigrate | | | 3G |
| Stage: | Baseline | Reduced | IV: second | IV: first |
| 3G | 0.027** | 0.028*** | 0.098** | |
| | (0.011) | (0.010) | (0.040) | |
| *Anderson-Rubin 95% Confidence Interval* | | | *[0.011,0.203]* | |
| $2G_{2006} \times$ year | | | | 0.040*** |
| | | | | (0.006) |
| First-stage F-statistic | | | | 41.90 |
| Observations | 617,402 | 617,402 | 617,402 | 617,402 |
| $R^2$ | 0.188 | 0.177 | 0.177 | 0.884 |
| Average dependent variable | 0.223 | 0.223 | 0.223 | 0.371 |
| District-level time trends | ✓ | | | |
| IV-related controls | | ✓ | ✓ | ✓ |
| Control for 2G | ✓ | ✓ | ✓ | ✓ |
| Baseline controls | ✓ | ✓ | ✓ | ✓ |
| District and time FEs | ✓ | ✓ | ✓ | ✓ |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. See notes to Table 1 for details of the baseline control variables (which include demographic, life satisfaction and income-related controls). 3G expansion is instrumented by the population-weighted 2G coverage in 2006 interacted by a yearly time trend for each of the three between district-level income groups. The unit of observation is the individual respondent in GWP. Column (1) shows our baseline estimate from Column (4) of Table 1, which includes district-level time trends. To include the instrument at the district *times* year level, Column (2) omits the district-level time trend but includes interactions of a linear time trend with the following district level variables: five bins of population density, area size of the subnational district, maximum altitude of the district, the share of mountains, the share covered by deserts, the initial population-weighted 3G coverage, a dummy for 3G coverage being 0 in 2008, and a dummy for 3G coverage being 0 in 2008 on the country level. The 2SLS estimation reported in Column (3) and (4) uses the same controls, reporting the second-stage result in Column (3) and the first-stage with an F-statistic of 15.02 in Column (4). The standard errors are clustered two-way in all four columns: on the country-year and district level.

## 6.4 Heterogeneity Analysis using Causal Forest

We also look beyond the average effects to understand how the causal effects vary with observable characteristics. Unlike previous literature, we don't rely on the estimation of models by explicitly choosing subgroups or the interaction effects, as both approaches suffer from the selective choice of covariates and a lack of statistical power when a high number of parameters is included in linear regression models. Instead, to identify heterogeneous treatment effects (that is, variation in the direction and magnitude of treatment effects for individuals within a population), we use the causal forests (CF) methodology, which provides a data-driven, less selective framework for heterogeneous treatment estimation (Athey et al., 2019).

This alternative statistical framework is based on an ensemble of regression trees that systematically splits the control variable space into increasingly smaller subsets. The criterion for the splits is to maximize treatment effect heterogeneity.[43] Regression trees aim to predict an outcome variable building on the mean outcome of observations with similar characteristics. Similar to bootstrapping processes, variance is calculated based on the diversity of regression trees.

We feed the causal forest algorithm the full set of control variables defined in our baseline model (i.e, Column 4 of Table 1) to estimate heterogeneous treatment effects. The model takes the following form:

$$Y_{idt} = \alpha_i(X_{it}^{'}) + \tau_i(X_{it}^{'})3G_{d,t} + u_{idt} \tag{7}$$

where $Y_{idt}$ is a dummy indicating that the respondent $i$ in subnational region $d$ and interview year $t$ “would like to move permanently to another country”, and $X_{it}^{'}$ is the full set of baseline covariates.[44] However, as we have many (2,107) subnational districts, we have many fixed effects, which may be problematic in a method based on regression trees.[45] To nevertheless incorporate the unobserved heterogeneity on the subnational region in the causal forest algorithm, we proceed in two steps. First, we run a Least Absolute Shrinkage and Selection Operator (LASSO) regression of the outcome of interest on the full set of controls and fixed effects, as suggested by Jens, Page and Reeder (2021).[46] Thereafter, we construct a single feature vector comprising the coefficients of the subnational-level fixed effects. This feature vector represents the unobserved district-level heterogeneity in the outcome of interest. Subsequently, we include this feature vector as a covariate in the CF algorithm.

[43]For a explanation of the splitting criterion, see Athey and Imbens (2016).

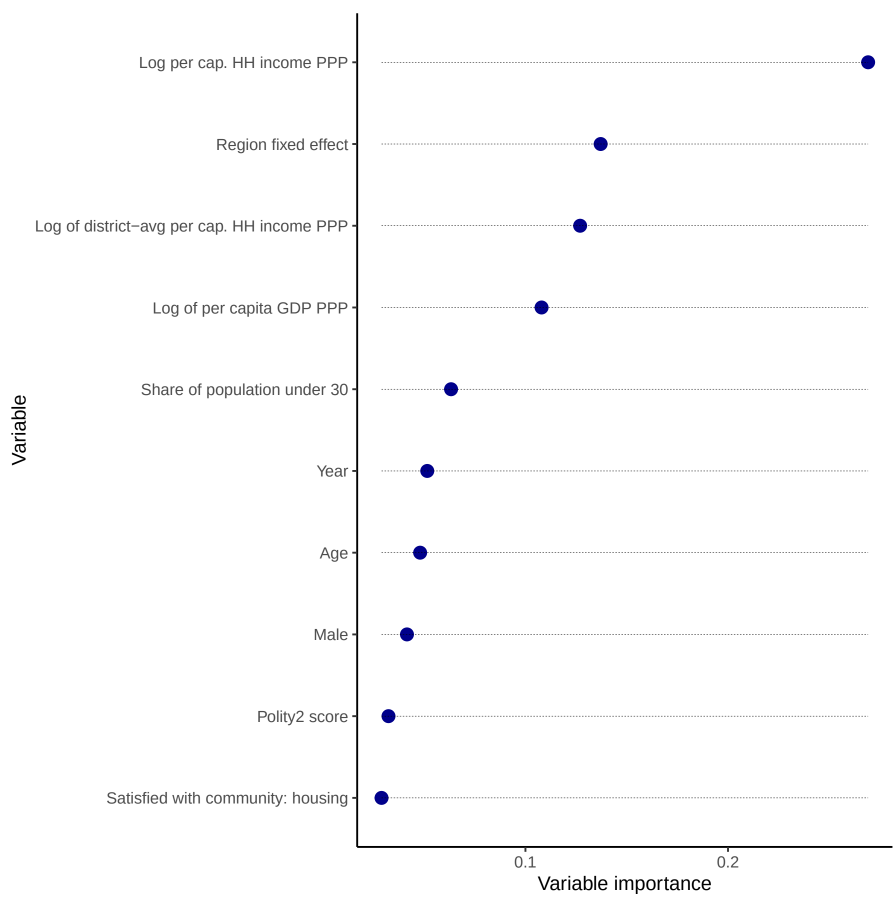
[44]To use the outcome variable with greatest coverage and simplify the analyses, we only conduct the causal forest analysis for the outcome variable ”desire to emigrate”.

[45]Including all fixed effects as binary indicators leads the algorithm to split often on these indicators early in the trees, compromising the overlap assumption and limiting the ability of the algorithm to split on other covariates that are important for treatment effect heterogeneity. This can be overcome by excluding the fixed effects altogether, but this faces the drawback that one can not account for unobserved subnational region level heterogeneity in the treatment effect.

[46]The advantage of LASSO is that it is able to select the most relevant variables in settings with near-multicollinear independent variables. In our setting, LASSO and OLS give very similar results.

The CF approach allows us to calculate Conditional Average Treatment Effects (CATE) based on the covariates of all observations. Encouragingly, the arithmetic mean of the CATE (0.0236) is very close to the treatment effect we identified in the main analysis. To assess which variables drive the treatment effect, we show the variable importance of the 10 most important covariates in Figure 7.[47] We find that all income-related covariates are important in explaining the heterogeneity, and also the subnational-level fixed effect is important.

Figure 7: Variable Importance for Treatment Effect Heterogeneity of the Causal Forest Algorithm.




Note: "Log per cap HH Income PPP" refers to the natural logarithm of total household income converted into Purchasing Power Parity terms as reported in GWP divided by the number of household members.

[47] The variable importance for a variable is calculated as a weighted (by $(1/2)^d$, where $d$ is the depth in the tree) sum of how often the trees split on that variable, which is subsequently normalized such that the sum of all variable importances for all covariates equals unity. More granular variables can be more often used to split upon, which could inflate the variable importance for the three income-related variables. If we severely limit the depth of the trees or calculate variable importance as a binary indicator (it either splits on that variable or not) for each tree, the three income-related variables remain among the five most important variables. This reassures us that the income variables are indeed the most important in explaining heterogeneity in the treatment effects.

To consider in what way personal and regional income levels affect treatment effect heterogeneity, we consider the level of the CATE in Figure 8. We find a striking pattern, which indicates strongest treatment effects for high-income individuals in high-income regions, and lowest effects for low-income individuals in low-income regions.

Table 4 shows the estimates of a doubly robust average treatment effects (DR ATE) based on regression forests for the propensity score and outcome model for below and above the median of per capita household income in each of four country income groups. These groups are based on the patterns in Figure 8. We find that treatment effects are positive and statistically significant for high-income countries and for above-median-income households in lower-middle-income countries. In most high-income countries, broadband internet was already widespread before the period we study. This suggests that mobile phone technologies play an important role, which may provide evidence of the importance of social media in obtaining relevant information. This is plausible as during our sample period, the number of Facebook users increased from 11 % in 2008 to 40 % in 2018 (weighted after country-level population: from 6 to 41 %) in the countries in sample.[48] On the other hand, the estimated effect is negative, although not statistically significant, for below-median-income individuals in low income and lower-middle-income countries. The lower-middle-income countries include large countries such as Egypt, India, Indonesia, Kenya, Nigeria, Philippines and Vietnam.[49]

What explains the finding that mobile internet access increases desire to emigrate in lower-middle-income countries only among above-median-income individuals? Our conjecture is that this reflects the joint effect of heterogeneity in labor market options abroad and differential migration costs. Those with above-median income are more likely to be secondary and tertiary educated[50], which gives them better labor market options abroad. When mobile internet access

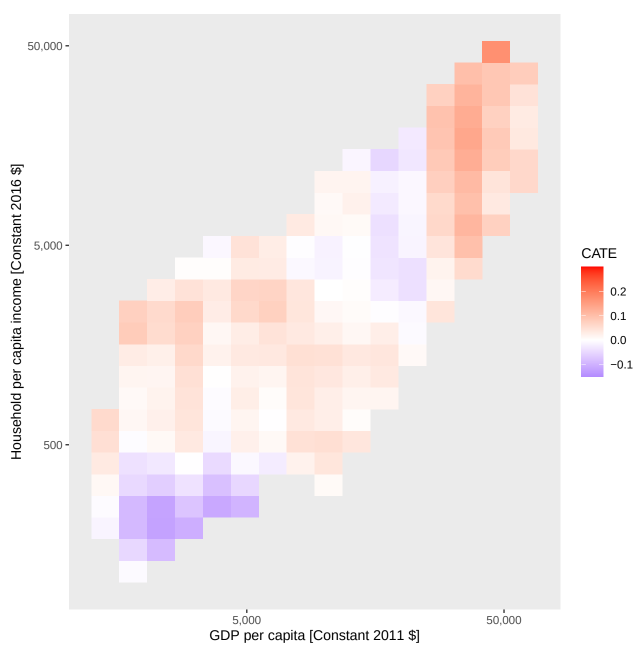
[48]The information about Facebook users is obtained from three online sources, that published the contemporaneous number of Monthly Active Users from the Facebook ads tool. These sources are: Nick Burcher (e.g. https://www.nickburcher.com/2009/07/latest-facebook-usage-statistics-by.html) (for 2008 – 2010), Social Bakers (e.g. https://web.archive.org/web/20130426082321/http://www.socialbakers.com/facebook-statistics/page-3/) (for 2011 – 2013), Internet World Stats (e.g. https://web.archive.org/web/20160304223409/http://www.internetworldstats.com/facebook.htm) (for 2014 – 2018). The latter two sources' historical archives are retrieved through the Wayback Machine (https://archive.org/web/).

[49]For the full classification of countries in the four groups, see Appendix Table A18.

[50]In lower-middle-income countries, 57 % (46 %) of above-median (below-median) income individuals have completed secondary education and 19 % (5 %) have completed tertiary education.

reduces the costs of information acquisition, a larger fraction of them find exploring opportunities abroad appealing. The much weaker but negative effects in the below-median-income group could reflect two mechanisms. Firstly, it could be that emigration is not a realistic strategy for most people in this group, due to lack of marketable skills in potential destinations abroad, as well as greater difficulties to obtain a visa as a low-skilled and low-income potential migrant. In this group, reduction of information costs does not change these arguments against emigration, which tends to push for a zero effect. Secondly, low-income individuals with little previous access to reliable information on options abroad may become more acutely aware on the difficulties and costs they would face if trying to emigrate. In the group that had previously unrealistic expectations on opportunities abroad, spread of information would tend to have a negative effect. Similar mechanisms are likely to be prevalent in low-income countries, in which greater difficulties to emigrate could explain why the estimated effects are close to zero.

Figure 8: Heatmap of Conditional Average Treatment Effect (CATE) of 3G Coverage on the Desire to Emigrate




Notes: Shown are the CATE for 16 bins of GDP per capita and 25 bins for household per capita income. Only cells with at least 1,000 observations are displayed. The lowest CATE of a bin is -0.11 and the highest value is +0.22.

Table 4: Estimates of Heterogeneous Effects over Country Income Group and Household Per Capita Income Levels

| | Median of per capita household income within country income group | |
|---|---|---|
| Country income group | Below median | Above median |
| Low-income | -0.020 | 0.030 |
| | (0.061) | (0.048) |
| Lower-middle-income | -0.022 | 0.124*** |
| | (0.055) | (0.046) |
| Upper-middle-income | -0.016 | 0.009 |
| | (0.039) | (0.047) |
| High-income | 0.074* | 0.090** |
| | (0.040) | (0.038) |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Reported are the Doubly Robust Average Treatment Effects (DR ATE) for observations below and above the median of per capita household income for 4 groups of country-level GDP per capita. To compute the DR ATE, we use overlap weights as propensity scores for parts of the sample approach 0 and 1. For an explanation of such an estimator, see Li, Morgan and Zaslavsky (2018). The countries in each respective country income group can be seen in Table A18, and are based on GDP (PPP) per capita in 2008. Standard errors in parentheses.

# 7 Mechanisms

In this section, we discuss potential mechanisms that can explain the relationship between mobile internet access and the desire to emigrate. First, we evaluate the role of the costs of acquiring information. We assess this by considering whether the effect is driven by those who do not have close personal networks abroad and how potential destinations change. Second, we consider whether mobile internet coverage affects perceptions of material well-being, trust in institutions and variables such as life satisfaction, optimism or sense of purpose in life.

## 7.1 Reduced Costs of Information and Networks Abroad

***Does internet access substitute for personal networks abroad?***

To assess whether internet access decreases information costs, we consider whether the effect size is larger for individuals without first-hand access to information. As the GWP asked respondents whether they had someone abroad to rely on between 2008 and 2015, we can consider the differential effect on the group that has someone to rely on and the group that does not. These close prior networks have been shown to explain a substantial part of the variation in the desire to migrate (Manchin and Orazbayev, 2018).

Table 5 shows that the effect of 3G on the desire to emigrate is strong for the individuals without any close personal network abroad, and insignificant for the group with such a network abroad. This is particularly striking when considering the lower baseline level of desire to emigrate for those without close personal network abroad. Only 16 % of those desire to emigrate, whereas this is close to 30 % for those with close personal networks abroad. The relative effect size of a full rollout of 3G coverage is 27 % for those without close prior networks abroad (Column 2), whereas this is only 14 % for the full sample (Column 1). This suggests that internet access is likely to affect desire to emigrate primarily through the cost of information acquisition, substituting for personal networks. If internet access would affect desire to emigrate primarily by reducing migration costs or communication costs with those left in the home country after migration, then its effects should not depend on close personal networks abroad. Of course, absence of evidence is no evidence of absence: even though the estimated effect of 3G coverage is statistically insignificant among those with close personal network abroad, the point estimate is still positive, and we cannot rule out that internet access would also have an effect through migration costs.

Table 5: Baseline Results of 3G Internet Expansion for 2008 – 2015 for Those With and Without Close Personal Network Abroad

| **Those with people to rely on abroad:** | (1) All respondents | (2) No | (3) Yes |
|---|---|---|---|
| 3G | 0.030** | 0.044*** | 0.016 |
| | (0.015) | (0.016) | (0.025) |
| Demographic controls | ✓ | ✓ | ✓ |
| Amenities, satisfaction, and income controls | ✓ | ✓ | ✓ |
| Country-level controls | ✓ | ✓ | ✓ |
| Observations | 388,368 | 252,172 | 136,130 |
| $R^2$ | 0.19 | 0.18 | 0.21 |
| Average dependent variable | 0.209 | 0.161 | 0.298 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by district and country-year, in parentheses. The specification of Columns 1 – 3 is identical to that of Table 1 for the subsample of (1) all respondents that answered the question whether one has someone to rely abroad (asked between 2008 and 2015), (2) only those that have no one to rely on abroad, and (3) only those that have someone to rely on abroad.

***Does internet access change preferred destinations?***

Because of prior networks, reductions in the cost of relevant information are not equally shared across all origin-destination dyads. Origin-destination pairs with strong prior ties are less likely to be affected, as previous migrants from such regions can provide first-hand information to prospective migrants. Therefore, differential changes in migration and information costs could divert migration flows from destination countries with strong prior networks to those without.

Using the reported desired destination in Gallup, we calculate the number of people desiring to migrate from origin country $o$ to destination country $d$, as displayed in Figure 4. Table 6 reports gravity model estimates for the effect of origin country 3G coverage on constructed desired migration flows from 2008 to 2018, where the unit of observation is the origin-destination-year. Column 1 reports the effect of 3G access on the number of people desiring to migrate (from a specific origin to a specific destination). Moving from no to full 3G coverage increases desired

Table 6: Gravity Model of Country-Level Desired Bilateral Migration and the Effect of 3G and Pre-existing Migrant Networks

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Desired bilateral emigration | | | | | |
| $3G_{ot}$ | 0.291*** | 0.654*** | 0.065 | | | |
| | (0.059) | (0.116) | (0.113) | | | |
| $3G_{ot} \times \ln(\text{Stock}_{od,2005}+1)$ | | -0.173*** | | -0.262*** | | -0.301*** |
| *(Standardized)* | | (0.061) | | (0.059) | | (0.072) |
| $3G_{ot} \times \ln(\text{GDPpc}_{dt})$ | | | 0.205** | | 0.010 | 0.218* |
| *(Standardized)* | | | (0.104) | | (0.102) | (0.117) |
| $3G_{ot} \times \ln(\text{Distance}_{od})$ | | | | | | -0.004 |
| *(Standardized)* | | | | | | (0.053) |
| $3G_{ot} \times$ Polity $IV_{dt}$ | | | | | | -0.006 |
| | | | | | | (0.013) |
| $3G_{ot} \times$ Common $\text{language}_{od}$ | | | | | | 0.035 |
| | | | | | | (0.123) |
| Observations | 64,977 | 64,977 | 64,977 | 64,977 | 64,977 | 64,977 |
| Origin-year-level controls | ✓ | ✓ | ✓ | - | - | - |
| Origin-destination FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Destination-year FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Origin-year FE | | | | ✓ | ✓ | ✓ |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at origin-destination level. The dependent variable is the estimated (based on GWP responses) number of people desiring to migrate from a specific origin to a specific destination in a given year. $3G_{ot}$ is the population-averaged 3G coverage in country $o$ in year $t$. $\ln(\text{Stock}_{od,2005}+1)$ is the log of the stock of migrants (plus one) in origin country $o$ in destination $d$ in 2005, $\ln(\text{GDPpc}_{dt})$ is the real GDP (PPP) per capita in destination $d$ at time $t$, the Polity $IV_{dt}$ integer score ranging from -10 (strongly autocratic) to +10 (strongly democratic), Common $\text{language}_{od}$ is a binary indicator for whether the origin and destination countries share a language that is spoken by at least 9% of population in both countries, and $\ln(\text{Distance}_{od})$ denotes the natural log of the population-weighted distances between the origin and destination (the last three are obtained from the CEPII GeoDist database, for details see Mayer and Zignago (2011)). $\ln(\text{Stock}_{od,2005}+1)$, $\ln(\text{GDPpc}_{dt})$ and $\ln(\text{Distance}_{od})$ are standardized such that the means are 0 and standard deviations 1. The regressions in all six columns control for $\ln(\text{Stock}_{od,2005}+1)$, $\ln(\text{GDPpc}_{dt})$, $\ln(\text{Distance}_{od})$, Polity $IV_{dt}$ and Common $\text{language}_{od}$. The additional origin-year-level controls include the unemployment rate, the total population and the polity score in the origin country. We estimate the models in Columns 1 – 6 using the Pseudo-Poisson Maximum Likelihood (PPML) estimator (see Silva and Tenreyro (2006)).

bilateral migration flows by 29%, on average. In Column 2, we include an interaction between the log of the stock of migrants from origin $o$ in destination $d$ in 2005. We find that the effect of 3G on desired emigration is reduced for those dyads with a large prior stock of migrants. A one standard deviation larger log of dyadic migrant stock is associated with a 17% smaller effect of 3G on bilateral preferred flows. As an example, moving from the dyad Armenia-Spain (there were 7,200 Armenian migrants in Spain in 2005) to the dyad Argentina-Spain (there were 244,000 Armenian migrants in Spain in 2005) corresponds to a one standard deviation difference in the log of prior stock of migrants.[51] In Column 3, we use a similar specification now interacting PPP GDP per capita on the country-year level with 3G coverage in the origin. We find that preferred flows are more sensitive to destination-country PPP GDP per capita when being covered by 3G networks, although it is just statistically significant at a 5 % level. In Column 4 and 5, we include origin-year fixed effects to control for unobserved time-varying country-level factors. These show similar results for the interaction with prior stock of migrants, but insignificant results for the interaction with GDP per capita. As prior stocks of migrants may be correlated to other factors affecting migration aspirations, we include interactions with the Polity IV indicator (on the destination-year level), a dummy for sharing a common language (on the origin-destination level) and the log of weighted distance between the origin-destination pair (on the origin-destination level) in Column 6. We find that the interaction effect of the prior stock of migrants remains comparable and highly significant, whereas the interaction effect of GDP per capita is just insignificant at a 5 % level. Column 6 indicates that a one standard deviation larger log of dyadic migrant stock is associated with a 30 % smaller effect of 3G on bilateral preferred flows after controlling for other factors. Altogether, this suggests that internet access not only affects the extent to which people want to migrate, but also the destination to which Gallup respondents desire to migrate to. As destinations change towards countries with lower stocks of migrants, the reduction of costs associated with finding information about prospective destinations and actual migration likely mediate this effect. Furthermore, the pos-

[51] As many dyads have small stocks of migrants in 2005, small absolute increases in preferred bilateral migration rates (based on only a few Gallup respondents) of low-stock dyads may inflate the percentage increase of these dyads a lot. To alleviate concerns that this drives the results found, we omit dyads with less than 1,000 migrants in 2005 in an additional sensitivity analysis (results available upon request). The estimate of $3\mathrm{G}_{\mathrm{ot}} \times \ln(\mathrm{Stock}_{\mathrm{od,2005}}+1)$ remains similar and statistically significant.

itive effects for the interaction of 3G and destination-level GDP per capita in Columns 3 and 6 suggest that preferred flows redirect to more prosperous destinations, which is consistent with an information-channel supplying information about earnings opportunities abroad.

If actual migration patterns change in line with desire to emigrate, immigrants' birthplace diversity increases in receiving countries, which may boost innovation (Alesina, Harnoss and Rapoport, 2016). Furthermore, increased migration could influence the politics of receiving societies. For example, there is convincing evidence that ethnic diversity reduces support for income redistribution (Dahlberg, Edmark and Lundqvist, 2012; Alesina, Miano and Stantcheva, 2022). Both sending and receiving countries could benefit from additional networks boosting international trade (Gould, 1994; Parsons and Vézina, 2018). Sending countries could also benefit from additional knowledge flows (Kerr, 2008; Fackler, Giesing and Laurentsyeva, 2020).

## 7.2 Well-being and Satisfaction with Institutions

To further explore possible mechanisms, we consider the direct effect of 3G rollout on outcomes that may affect migration behavior.[52] We use various indices as constructed by Gallup, supplemented with reported log household income, a constructed aggregate index of material prospects, the first principal component of trust in the government as constructed by Guriev, Melnikov and Zhuravskaya (2021), and information on banking and remittances.

***Does mobile internet access affect perceived material well-being?***

The first mechanism is related to perceived material well-being. In particular, we test whether respondents' perceived economic and financial conditions change after obtaining mobile internet access. To do so, we consider four outcome variables in Panel A of Table 7. The outcomes across the columns in the top panel are as follows: "(log) household income" (Column 1); "material prospects index" (Column 2); "job climate index" (Column 3); and "financial well-being index" (Column 4).

In Column 1, we find no statistically significant relationship between our treatment variable

[52]Apart from log household income, the outcomes presented in this section all strongly correlate to the desire to emigrate, before 3G coverage arrives. Appendix Table A19 shows the results of similar regressions of the desire to emigrate on these outcomes.

Table 7: The Effect of 3G on Material Well-being and Satisfaction with Life and Institutions

| **Panel A: Material well-being** | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Dependent variable: | Log of household income (PPP) per capita | Material prospects first principal component | Job climate index | Financial well-being index |
| 3G | -0.026 | -0.030** | -0.036** | -0.114* |
| | (0.035) | (0.014) | (0.018) | (0.067) |
| Observations | 617,402 | 569,708 | 614,435 | 172,653 |
| $R^2$ | 0.71 | 0.24 | 0.19 | 0.23 |

| **Panel B: Life satisfaction and optimism** | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Dependent variable: | Optimism index | Daily experience index | Life evaluation index | Life purpose index |
| 3G | -0.018 | -0.005 | 0.030 | -0.046 |
| | (0.014) | (0.007) | (0.021) | (0.073) |
| Observations | 617,220 | 615,880 | 580,644 | 172,467 |
| $R^2$ | 0.22 | 0.12 | 0.21 | 0.20 |

| **Panel C: Institutional satisfaction** | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Dependent variable: | Law and order index | Corruption index | Community basics index | Trust in government first principal component |
| 3G | 0.015 | -0.017 | 0.010 | -0.037** |
| | (0.009) | (0.014) | (0.010) | (0.015) |
| Observations | 616,783 | 588,979 | 617,402 | 486,283 |
| $R^2$ | 0.19 | 0.22 | 0.25 | 0.23 |

| **Panel D: Mobile banking and remittances** | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Dependent variable: | Owns a bank account | Used cellphone to receive cash in last 12 months | Received money or goods from friend/ family from same country | Received money or goods from friend/ family from another country |
| 3G | -0.020 | 0.003 | -0.009 | 0.004 |
| | (0.038) | (0.026) | (0.015) | (0.008) |
| Observations | 169,581 | 161,081 | 566,956 | 566,956 |
| $R^2$ | 0.40 | 0.21 | 0.12 | 0.10 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by district and country-year, in parentheses. The specification used in Columns 1 – 4 of Panels A – D is similar to that of Table 1. We only exclude the control variables related to local amenities as some of these amenities are used in the construction of the GWP indices. The number of observations is varying by item because of imperfect variable coverage over time and subnational regions. Except for column 1 of Panel A and panel B (2013-2015) and Column 1 and 2 of Panel D (2011, 2014 and 2017), all outcomes are covered between 2008 and 2018. In Columns 1 and 2 of Panel D we omit the district-level time trends, as we only have 3 time periods available (2011, 2014 and 2017). All dependent variables in Panels A-C are GWP indices, except for "(log) household income" (which is the reported log of per capita household income), "material prospects" (a first principal component of the following questions (weights in parentheses): living comfortably on present income (0.69), now is a good time to find a job (0.34), and not having enough money to afford food (-0.65)), and "trust in goverment" (a first principal component of four questions related to trust in the government, as constructed by Guriev, Melnikov and Zhuravskaya (2021).). For all items in Panel A – C a higher value of the dependent variable implies a higher value of the item. For example, a higher value of "Material prospects first principal component" implies a better subjective evaluation of material well-being and a higher value of "Corruption index" implies a larger perception of corruption. For construction of the GWP indices, see https://www.oecd.org/sdd/43017172.pdf (Last accessed on 08-12-2021).

and per capita household income (an objective measure of material well-being).[53] This also addresses the caveat in Hypotheses 1 and 2 that the results hold if the mobile internet access does not boost local wages substantially. Not only do we find no substantial boost, but the point estimate on the effect on log per capita household income is negative, although statistically insignificant. The results reported in Columns 2 to 4 indicate that access to the mobile internet leads to a fall in the material prospects index and job climate index (measures the attitudes about a community's efforts to provide economic opportunities). We also find that mobile internet access has a negative effect on the financial well-being index (measures respondents' subjective personal economic situations and the economic situation of the community in which they live).

Overall, these results suggest that individuals' perceived material well-being declines after mobile internet penetration, while there is no effect on their household income. Such increased dissatisfaction could be a push factor to emigrate.

***Does mobile internet access affect views about life?***

In Panel B of Table 7 we explore the impact of mobile internet access on views about life. In particular, we present evidence using four outcome variables. The outcome variables across the columns in the middle panel are as follows: "optimism index (measures respondents' positive attitudes about the future)" (Column 1); "daily experience index (a measure of respondents' experienced well-being on the day before the survey)" (Column 2); "life evaluation index (respondents' perceptions of where they stand now and in the future)" (Column 3); and "life purpose index (measures whether one likes what she does daily and is motivated to achieve one's goals)" (Column 4). We find no effect on any of these outcomes.

***Does mobile internet access affect satisfaction with institutions?***

To investigate whether a fall in satisfaction with institutions can also explain our results, we regress various outcomes on mobile internet access, the results of which are reported in Panel C of Table 7. The outcome variables across the columns in the last panel are as follows: "law and

[53] As household income is an imperfect measure of wages, we perform the following two robustness tests: (1) we find that the effect remains insignificant if we focus on single person households and (2) we find that 3G has no effect on whether the respondent is in employment or not.

order index" (Column 1); "corruption index" (Column 2); "community basics index" (Column 3); and "trust in government" (Column 4).

The results in Columns 1 – 3 are based on indices constructed by Gallup and show that there is no effect on the law and order index (gauges respondents' sense of personal security), corruption index (measures perceptions in a community about the level of corruption in business and government) and community basics index (measures everyday life in a community, including environment, housing and infrastructure). In Column 4, in line with Guriev, Melnikov and Zhuravskaya (2021), we find that 3G mobile internet has a negative effect on trust in government.

***Does mobile internet access affect access to financial services and remittances?***

To investigate whether an increase in access to financial services can explain the results found, we consider whether mobile internet access has an effect on the adoption of financial services[54] and on domestic and international remittances directly. As mobile banking has the potential to reduce the costs of remittances, this could increase the benefits to migration for those staying behind, possibly fostering emigration. We report the results in Panel D of Table 7. The outcome variables across the columns in the last panel are as follows: "Owns a bank account" (Column 1); and "Used cellphone to receive cash in the last 12 months" (Column 2); "Received money or goods from friend/ family from same country" (Column 3); "Received money or goods from friend/ family from another country" (Column 4). We find no evidence for mobile internet access having affected any of these outcomes.

***Back-of-the-envelope estimates***

Using the estimates of Tables 7 and A19, we can calculate a back-of-the-envelope estimate on how big part of the effect of mobile internet access on desire to emigrate could be driven by the proposed mechanisms. Altogether, four out of 16 proposed mechanisms in Table 7 are statistically significant. By multiplying the statistically significant coefficients in Tables 7 and A19, we get an idea on the relative strengths of the suggested mechanisms. The effect of moving from no to full 3G coverage on the desire to emigrate is 0.0030 through material

[54]For outcomes related to banking we make use of the FINDEX module, which is an add-on to Gallup conducted in 2011, 2014, and 2017, on the same individuals as in the GWP. Because we have only 3 time periods per subnational district available, we drop the district-level time trends.

prospects first principal component, 0.0026 through the job climate index, -0.0035 through the financial well-being index (a negative sign suggests a counteracting force), and 0.0046 through the trust in government. As some of these mechanism may be partly multicollinear, adding the mechanisms together is likely to give an overestimate on their joint effect. With this caveat in mind, the estimated combined effect would be 0.0067 if summing these. Along similar lines, we can estimate how much of the effect can be explained by access to information, using the estimates of Table 5. If we assume that the difference in effect size between those with and without prior networks is fully driven by a difference in available related information related to close networks, other channels are able to explain only 0.016 pp, and the channel concerning information related to close networks an additional 0.028 pp. As the additional channel is available for the 65 % of population without prior networks, this contributes 0.018 pp to the total effect size.

In summary, our results suggest that access to the mobile internet led to a decrease in perceived material well-being and trust in government, which could explain at most one third of the estimated relationship between mobile internet access and the desire to emigrate in column 4 of Table 1. They also suggest that the availability of information previously only available through close networks could explain more than half of the found effect of mobile internet access on the desire to emigrate.

# 8 Does Mobile Internet Also Affect Real Migration Behavior? The Case of Spain.

As few countries have reliable subnational emigration registries, estimating the effect of 3G coverage expansion on actual emigration on a large scale is infeasible. However, Spain has such data. The Spanish Statistical Office (INE) maintains a population registry where inflows and outflows are recorded by person based on municipal registrations, including supplementary information such as country of origin.[55] Data is published for all municipalities with more than

[55]This registry is called *Diseño de registro de la Estadística de Variaciones Residenciales (EVR)*. Data can be found here: https://www.ine.es/dyngs/INEbase/es/operacion.htm?c=Estadistica_C&cid=

10,000 inhabitants. These municipalities contain 76 % of the population of Spain (in 2008). We focus on emigration rates of individuals born in Spain, as we expect internet access to affect them most. For individuals not born in Spain emigration might mean simply returning to their country of origin and may in some cases be the end of a stay that was already initially planned to be temporary.

Using the Mobile Coverage Explorer and the GPW population density, we calculated the share of population covered by 3G in these municipalities. The first time nonzero coverage is reported to the Mobile Coverage Explorer was in December 2008.[56] As population-averaged coverage was already 80 % in all municipalities with more than 10,000 inhabitants in 2008, recorded variation in 3G coverage is limited over time and concentrated among smaller municipalities. In December 2008, the 50 province capitals of Spain already had a population-averaged reported 3G coverage of 87 %, whereas the municipalities that are not province capitals had an average coverage of 71 %. Between 2003 and 2015, migration from all municipalities in the sample increased gradually. In 2003, only 0.03 % of the population emigrated, whereas, in 2015, 0.11 % of the population emigrated.

To assess the question of whether 3G expansion has effects on actual migration from Spain of Spanish-born individuals, we estimate the following linear continuous difference in differences model:

$$m_{dt} = \beta_1 3G_{d(t-1)} + \beta_2 u_{pt} + \phi_d + \theta_t + \epsilon_{dt} \tag{8}$$

where $m_{dt}$ is the emigration rate of Spanish-born individuals from municipality $d$ in year $t$. We control for the unemployment rate $u_{pt}$ at the provincial level. Our sample contains 657 municipalities in 50 provinces, of which 29 have a population exceeding 200,000 in 2008. Our resulting sample covers the years 2010 to 2020, as prior years have no information on the first lag of 3G coverage.[57]

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[56] 3G networks were present in Spain prior to 2008. See, for example, https://www.elmundo.es/navegante/2004/10/26/empresas/1098805246.html (accessed on 21-10-2021)

[57] As in the empirical strategy in section 5, we use coverage reported in December as representative for coverage in the next year. Therefore, the first available year is 2010.

As variation in 3G coverage is larger within smaller municipalities, we report the results for small and large municipalities separately. Table 8 reports the estimation results of Equation 8, for all, the small and the large municipalities in Column 1, 2, and 3, respectively. We find that a 10 percentage point increase in 3G coverage on the municipality level increases emigration by 0.0016 percentage points for all municipalities, significant at a 5 % level. For the smaller municipalities, we find a point estimate of 0.0014 (significant at the 5 % level) and for the larger municipalities a point estimate of 0.0018 (insignificant). For the smaller municipalities, the average yearly emigration rate is about 0.09 %, implying an increase in migration of around 1.5 % due to a 10 percentage point increase in mobile internet. These results suggests that the rollout of mobile internet not only led to increases in stated aspirations and intentions, but also to more actual migration, in the case of Spain. However, Spain is among high-income countries, which experienced stronger effects of 3G coverage on the desire to emigrate then many other countries in our causal forest analysis (see Figure 8). Therefore, further studies of the effect of 3G rollout on actual migration in lower-income countries would provide valuable knowledge.

Table 8: The Effect of 3G Rollout on Emigration of Spanish-born Individuals from Spain

| | (1) | (2) | (3) |
|---|---|---|---|
| Dependent variable: | | Emigration rate (× 100) | |
| **Population in 2008:** | All | ≤200,000 | > 200,000 |
| 3G Coverage$_{t-1}$ | 0.016** | 0.014*** | 0.018 |
| | (0.007) | (0.005) | (0.028) |
| Observations | 6,570 | 6,280 | 290 |
| $R^2$ | 0.873 | 0.838 | 0.951 |
| Average emigration rate (× 100) | 0.094 | 0.093 | 0.105 |
| Municipality and year FE | ✓ | ✓ | ✓ |
| Provincial unemployment | ✓ | ✓ | ✓ |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. The dependent variable is the average international migration rate of Spanish nationals from a municipality between 2010 and 2019, multiplied by 100. The unit of observation is the municipality. We control for yearly averaged unemployment rates on the provincial level. Column 1 includes all municipalities, Column 2 includes all municipalities with a population of less than 200,000 in 2008, Column 3 includes all municipalities with a population exceeding 200,000 in 2008. Standard errors are clustered two-way: on the district and the province-year levels.

# 9 Conclusion

In this article, we show that mobile internet access increases both desire and plans to emigrate. Our analysis combines Gallup World Polls data from more than 600,000 respondents living in 2,120 sub-national regions in 112 countries, collected between 2008 and 2018, and geocoded data on worldwide 3G mobile internet rollout. The effects are sizable. The average increase in 3G coverage between 2008 and 2018 was 36 % across the 2,120 subnational regions. Our estimates suggest that such an increase goes together with a 0.21 to 1.82 percentage point increase (95 % confidence interval) in the desire to emigrate permanently. These are conservative estimates, identified by exploiting within-district variation in 3G coverage that has been stripped of any influence of constant and linearly changing district-level characteristics. The estimates imply that in a country with 10 million adult inhabitants, a move from no 3G coverage to full coverage would increase the number of people desiring to emigrate by 56,000 to 486,000 and planning to emigrate by 200 to 176,000. That the desire to emigrate increases more than the emigration plans is in line with the idea captured in our model that only a fraction of those desiring to emigrate is actually able to do so. Using an instrumental variable (pre-existing 2G infrastructure), we provide supplementary evidence that the effects are causal. Furthermore, the effects estimated using instrumental variables are considerably larger, suggesting that the estimated effects without using instruments are likely to underestimate the true effect. These effects are likely to translate into subsequent actual migration behavior, as migration aspirations and intentions are strongly correlated with actual migrant flows.

Causal forest analysis reveals substantial heterogeneity in the effects of mobile internet access. The effect on desire to emigrate is strongest in high-income countries and for above-median-income individuals in lower-middle-income countries. The estimated effect is negative, although not statistically significant, for below-median-income individuals in low income and lower-middle-income countries. This may reflect these individuals becoming more acutely aware on the difficulties and costs they would face if trying to emigrate. Using data on actual emigration from Spanish municipalities confirms that increased mobile internet access goes together with increased emigration. Our estimates suggest that switching from no mobile internet access to full 3G coverage in Spain increased annual emigration by about 15 percent compared with emigration rates that could have been expected in the absence of 3G coverage.

Our theoretical model suggests that mobile internet access is likely to increase both desire and plans to emigrate by reducing the cost of information acquisition. In line with this prediction, our analysis reveals that mobile internet access has strongest effects on respondents who do not have personal networks abroad, which can be explained by internet access substituting for personal contacts. Furthermore, we find that increased mobile internet access reduces perceived material well-being and also erodes trust in own government. Such increased dissatisfaction could be an additional channel through which mobile internet access increases desire and plans to emigrate. Finally, our results suggest that mobile internet access may not only increase overall international migration but also redirect migration flows towards less popular destinations. This could have far-reaching implications on both origin and destination countries.

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# A Online Appendix

## A.1 Additional Information on Outcome Variables

***Construction of Outcome Variables from GWP***

GWP contains several relevant questions on migration: about desires and plans to emigrate, the likelihood to migrate (either domestically or internationally), and two questions to identify potential destination countries. Appendix Table A1 provides the relevant questions as they were stated in the GWP, and provides information on how we combined the variables if any modification was needed. The leftmost column contains the numbers of the outcomes reported in the main text.

Variable (1) refers to the desire to emigrate. Variable (2) refers to emigration plans and comprises two questions that are slightly different. Individuals that did not name a country in WP3120 are not asked WP6880 and are thus flagged as not planning to emigrate. However, it is unlikely that a respondent planning to emigrate is unable to identify the intended destination country in the preceding question. A greater issue is posed by individuals planning to emigrate to a feasible destination country, instead of their preferred destination. They might have identified another country in WP3120, which they only desire to emigrate to. These individuals then would answer negatively to WP6880, as they do not plan to emigrate to the country mentioned in WP3120. Therefore, for some individuals we might underestimate their plans to emigrate when considering WP6880. However, within-country positive rates of WP10252 (which asked about plans to emigrate within 12 months) and WP6880 (which asked about plans to emigrate to the preferred destination country within 12 months) are comparable, suggesting that emigration plans usually refer to plans to emigrate to the preferred destination country, and combining the two questions is justified. By combining WP10252 and WP6880, we are able to obtain a measure of plans to emigrate between 2008 and 2015. Having a longer sample is especially important as the positive rate of variable (2) is low and thus expected effect sizes are small and because 3G coverage is interpolated in 2011, giving limited treatment variation between 2010 and 2015.

***Descriptives***

Appendix Table A2 presents an overview of the main variables, including the data source and the level of observation. Averaging across all country-years, 22 % of respondents report that they would like to move permanently to another country, while only 3 % report that they are planning to move permanently in the next 12 months. 17 % report being likely to move away from the city or area in which they live in the next 12 months. 46 % of survey respondents are men. The average age of respondents is 40, 15 % have completed tertiary education and 58 % are partnered.

## A.2 Robustness Checks

In this section we report further analyses establishing the robustness of our findings.

***Event Study Approach and Assessment of Pre-Trends***

Our preferred alternative to TWFE is the de Chaisemartin-D'Haultfœuille estimator for continuous treatments. Another alternative is event study approach, focusing on districts experiencing an increase of at least 50 % in their 3G coverage between two subsequent years. An advantage of focusing on sharp increases is that instantaneous and dynamic effects can be distinguished, as there is no significant increase in 3G coverage before or after the event. However, in our case the event study approach comes at the cost of observations. In the event study, we focus on the less than 20% of the sample that lives in a district with a sharp expansion of 3G coverage. Nonetheless, an event study approach provides a valuable complementary perspective on the effects of mobile internet access, which is why we present also results using it.

In Figure 6, we find no significant pre-trend in the desire to emigrate using the de Chaisemartin-D'Haultfœuille estimator for continuous treatments. In the estimates in Figure 6, the control group also contains units with only small year-on-year increases in 3G coverage, whereas the event study contains only (treatment and control) districts that receive a sharp increase in 3G coverage between 2008 and 2018. As (i) treated districts receiving a large treatment may be different than all units that receive a first increase in treatment and (ii) control districts never receiving a sharp increase in 3G coverage may be on a different trend in the desire to emigrate than units that do, a test on the presence of pre-trends using the de Chaisemartin-D'Haultfœuille estimator that includes also small treatments and a test using the event study approach are complementary.

In the event study, we focus on districts that experienced an increase of at least 50 % in their 3G coverage between two subsequent years, and analyze how the desire to emigrate develops with regard to this event, net of all baseline controls. As such an event design is subject to the same issues as TWFE estimators (see e.g. Sun and Abraham (2021)), we estimate the model using the de Chaisemartin-D'Haultfœuille estimator for binary treatments.

Appendix Figure A2 shows the results from the event study. We show the first three placebo, the instantaneous and the first four dynamic estimators. Earlier placebo estimators and later dynamic estimators are omitted as they are based on less than 1,000 observations each. These estimators are the same as the de Chaisemartin-D'Haultfœuille estimators used in Section 6.2. In contrast to the use case of the de Chaisemartin-D'Haultfœuille estimators in Section 6.2, here we have a binary treatment indicator (which is 1 in a subnational region in all periods after receiving an increase of at least 50 % in 3G coverage between two subsequent years, and 0 otherwise) and thus only one initial treatment group, which is the case discussed in the first paragraph of Online Appendix A.3.

The black line shows the event study estimates of 3G expansion around a sharp increase and confirms that the treatment is sharp, showing little change in 3G coverage before and after the steep increase. Prior to the event, 3G coverage increases on average by 4 percentage points in the three periods before treatment, as districts could have nonzero treatment prior to a treatment corresponding to an increase of at least 50 %. The 3G coverage rises, on average, by around 75 percentage points during the sharp increase in 3G coverage. After the first period, treatment only rises by 6 percentage points in the subsequent 4 periods. This justifies the use of an event study. The light blue line displays the event study estimates of the desire to emigrate. The instantaneous coefficient is around 0.2 and statistically significant. The first and second dynamic effects are just insignificant, but in magnitude comparable to the instantaneous estimator. In contrast, the third estimator is almost double as large, whereas the fourth dynamic effect is close to 0 and insignificant. As the treatment is very sharp, we can interpret the dynamic estimators as actual dynamic effects, compared to combinations of instantaneous and dynamic results as in section 6.2. These results thus further suggest that the first three dynamic effects after receiving 3G coverage are positive as well, implying that the found effect is not just transitory in nature. None of the pre-event estimates are significantly different from 0. The p-value of a joint test for significance of any of the pre-event estimators is 0.22.

***Controlling for Nighttime Light Density as an Alternative Measure of Regional Development***

To alleviate concerns that 3G expansion and regional development coincide and that the coefficient on 3G coverage is biased because it captures regional development, we control for the mean of subnational-district-year level of per capita income in the household. However, as this is a self-reported measure of income and a mean of a relatively small number of observations, we show that using other measures of regional development does not alter the main result. More specifically, we use the nighttime light density as an alternative measure of regional development in Column 1 of Appendix Table A4 and the median, instead of the mean, of district-year personal income in Column 2. Our results remain similar.

***Robustness to Excluding Potentially Bad Controls***

One might worry that some of the individual characteristics (life satisfaction and local amenities) are themselves affected by the 3G rollout. Therefore, we omit sets of controls in Columns 3, 4 and 5 of Appendix Table A4. Excluding life satisfaction and living standard-related controls (Column 3), satisfaction with amenities (Column 4) and whether someone can count on friends (Column 5) separately hardly alters the coefficient on 3G coverage.

***Robustness to Including an Extensive Set of Additional Controls***

As many questions in GWP are only covered for a part of the sample, we omitted some potentially relevant controls. However, adding controls for employment status in Column 6 of Appendix Table A4, financial support from home country or abroad in Column 7 of Table A4 and the aforementioned extra controls and various other controls (related to views about hard work, life satisfaction in five years, whether the current region is good for immigrants and whether the respondent has health problems) in Column 8 of Table A4 do barely change the estimated effect of 3G coverage.

***Falsification Exercise: Using 2G Expansion as a Treatment***

The main function of 2G technology is the transmission of information via voice signals while that of 3G technologies is internet browsing, data transfer and downloading. At the same time, the expansion of cellular 2G and 3G networks is strongly correlated because of the technologies' shared infrastructure. This raises as a potential concern that the estimated effect of the 3G expansion could have arisen, at least partially, already because of improved communication allowed by the coinciding expansion of 2G networks. However, in Column 1 of Appendix Table A5, we find that 2G coverage has no statistically significant effect on the desire to emigrate, which is consistent with the idea that 3G affects the desire to emigrate through improved internet access and is not driven by an improved ability for mobile bilateral communication. In Column 2, we find that inclusion of 2G in the main specification does not alter the point estimate of 3G coverage.

***Falsification Exercise: Using Leads as Treatments***

By regressing the desire to emigrate on leads in 3G coverage, we can assess whether future increases in 3G coverage predict previous changes in desire to emigrate. If this is the case, the parallel trends assumption may be violated or treatment may be anticipated.

Appendix Table A5 shows that the instantaneous value of 3G coverage (Column 5) has an effect on the desire to emigrate while lags (Columns 3 and 4) and leads of 3G (Column 6 and 7) have no effect on the desire to emigrate.[58] The insignificance of the leads alleviates the concern that both 3G coverage and the desire to emigrate may be related to a (slowly moving) omitted variable and therefore display non-parallel trends. If the main result would be driven by different longer-run pre-trends for treated and untreated units, we would expect the leads to have a significant effect on the outcome. Therefore, the insignificance of the first lead of 3G coverage renders it implausible that non-parallel pre-trends in desire to emigrate are present.

***Ruling Out Influential Observations***

We rule out the importance of influential observations by showing the coefficients of our preferred specifications by omitting one year at a time. Appendix Table A6 shows that our coefficient estimates are quite stable even as a specific survey year is excluded from our main sample in each iteration.

[58]Please note that using the $n^{\text{th}}$ lag (lead) disregards the observations in the $n$ earliest (last) years of the sample.

We repeat a similar analysis in Appendix Table A7, in which we exclude one global region at a time in each estimation and again find that our estimates are not driven by a single global region. We classify OECD countries outside Europe and Latin America as one group.

***Robustness to Excluding Top 10 Refugee-origin Countries and Countries with High or Low Desire to Emigrate***

In order to alleviate concerns that the found results are driven by a few countries in distress, we omit the 10 countries of origin with the most refugees.[59] Additionally, we omit countries where a large (≥40 %) proportion of GWP respondents desires to emigrate and those where a small (≤10 %) proportion desires to emigrate. Appendix Table A8 reports the baseline results for these three omissions. The coefficient on 3G is robust to omission of these country groups.

***Measurement Error in Mobile Coverage Data***

As the data on mobile network coverage is based on reports of mobile network operators, it may be susceptible to various kinds of measurement error. First of all, reporting may be delayed. Second, coverage is not necessarily reported by all network operators, possibly underestimating the network coverage. As both of those sources of measurement error may be related to mobile network operator, industry structure, as well as country- or district-level characteristics, these may potentially bias the results we reported. To alleviate concerns about such measurement errors affecting our estimates, we omit groups of countries in Appendix Table A9 based on several criteria, which are:

- Countries with large initially reported 3G coverage:

  We omit countries for which the first year of nonzero 3G coverage is 2009 or later, and more than 20% of population is covered already in the first year.[60] In this case, we deem it plausible that, prior to that year, the country already had nonzero 3G coverage.[61]

- Countries with much lower 3G coverage than mobile broadband subscriptions in 2015:

  Countries that have at least four times as many mobile broadband subscriptions per capita than population-averaged 3G coverage in 2015. In this case, it is plausible that 3G coverage is under-reported.[62]

[59] We consider the 10 countries with the largest number of refugees under the UN High Commissioner for Refugees mandate in 2015. These include Syria, **Afghanistan**, Somalia, South Sudan, **Sudan**, **Democratic Republic of the Congo**, Central African Republic, Myanmar, Eritrea and **Colombia**. The countries in bold are part of out baseline sample. For the raw data, see: `https://www.unhcr.org/refugee-statistics/download/?url=738dpE`

[60] We do not omit countries that show such increases before 2009, as it does not affect our sample period.

[61] This is the case in Armenia, Burkina Faso, Cameroon, Dominican Republic, Ecuador, Ghana, India, Kuwait, Malta, Mauritius, Montenegro, Qatar and Tunisia.

[62] We calculate country-level averages of population-weighted 3G coverage and we compare this to the number of mobile broadband subscriptions in 2015 as indicated by the International Telecommunication Union (ITU) `https://tcdata360.worldbank.org/indicators/h1e032144`. This is the case in the following countries: Belize, Bhutan, Colombia, Costa Rica, El Salvador, India, Kyrgyzstan, Mozambique, Namibia, Nepal, Nigeria, Oman, Senegal, Thailand, Trinidad and Tobago and Venezuela.

- Districts that report report sharp decreases (defined as a drop of 10 percentage points or more) in 3G coverage. It is unlikely that coverage drops sharply within one year. This may be the caused by a reporting error.[63]

Excluding these country groups individually in Columns 1, 2 and 3 of Appendix Table A9, and all of them simultaneously in Column 4, does not change our results qualitatively.

***Balancing Test***

3G expansion depends on the choices by network operators and authorities giving permissions to network expansion. If these choices are correlated with the characteristics of local population, our econometric analysis risks associating parts of the estimated effects of endogenous network expansion to control variables with which it is correlated. To address this concern, we ran a balancing test to check whether our treatment variable is correlated with respondents' observable demographic and socio-economic characteristics, with results shown in Appendix Table A10. In line with our identification assumption, none of the estimates is statistically significant at a 5 % level. Furthermore, the p-value on the joint insignificance of all covariates equals 0.11.

***Multiple Hypothesis Testing***

We also conducted multiple hypothesis testing based on a randomization inference technique, as recently suggested by Young (2019). This helps to establish the robustness of our results, both for individual treatment coefficients in separate estimations and also for the null hypothesis that our treatment does not have any effect across any of the outcome variables (i.e., treatment is irrelevant). The method builds on estimating the distribution of treatment effects by randomizing the treatment assignment under the null hypothesis that the treatment effect is 0 for all observations, and comparing the pool of randomized estimates to the estimates derived in the baseline specification. Using 500 iterations, the results presented in Appendix Table A11 show that our three findings in Column 4 of Table 1 remain robust. The null hypothesis of the Westfall-Young test for irrelevance for the 3G treatment in all three regressions is also rejected, with a p-value of 0.014.

***Robustness to Alternative Levels of Clustering***

In our main specification, we cluster the standard errors in two ways: at the district level (2120 groups) and at country-year level (791 groups). We show that our results are robust to using alternative assumptions about the variance-covariance matrix: the results remain significant when clustering at gender-education-country level (assuming that residuals move collectively within these units) as well as clustering at country-level (see Appendix Table A12).

[63] This happens in 109 districts in the baseline sample, most of which are located in Europe (31 in six countries) or in the former Soviet Union (36 in five countries). A striking example is Finland, where six districts reported decreases greater than 50 % in 2016, to (more than) fully recover in 2017.

***Are the Results Driven by Non-comparable Samples?***

Not all countries and districts are consistently included in GWP between 2008 and 2018, especially in earlier years in our sample. Thus, the results could conceivably be biased by heterogeneous, non-comparable samples. We therefore consider the baseline result on the sample of countries and districts that are included in all years. The results reported in Appendix Table A13 confirm that our findings are robust across balanced samples.

***Robustness to Using Population Weights and Using No Weights***

We weight our observations in the baseline using the within-country weights based on the inverse probability of being included in the Gallup surveys. These weights are based on the demographic characteristics of the respondent and of the country of residence.[64]

We show that found results are robust to the choice of weights in Table A14. Column 1 reports the results for the unweighted baseline regression, whereas Column 2 reports Gallup weights only (our baseline). We find that the effect size is largest when using individual-level population weights (Column 3). Although the estimate using population-weighted observations provides truly global evidence, we have chosen as our baseline the more conservative Gallup weights only, due to a concern that a few large countries could drive the found effect when using population weights. That the qualitative effects are similar is an important robustness test, as the preferred population and Gallup weights vary significantly between countries and, to a lesser extent, between individuals.

***Robustness to Alternative District-specific Trends***

In our baseline regressions, we use district-specific time trends to alleviate concerns about spurious correlations between district-level 3G coverage and desire to emigrate driven by unobserved drifts on the district level. However, to show that our results do not critically depend on the inclusion of these linear time trends, we consider alternative specifications in Appendix Table A15. Omitting the time trend reduces the effect size found by around one standard deviation (Column 2), whereas adding a quadratic time trend does not alter the results by much (Column 3).

***Robustness to Omission of Phone Interviews***

[64]GWP supplies a within-country weight variable based on unequal inverse probability of selection, calculated from (among others) national demographics, number of phone connections per household and number of household members. This allows the calculation of average statistics on the national level and to weight regressions accordingly. We refer to those weights as Gallup weights. Moreover, GWP aims to cover each country with at least 1,000 interviews per country-year. This implies that small countries are oversampled in GWP with regard to their populations. One can calculate population-adjusted country weights by using the Gallup weights $w_i^{Gallup}$, country-level population data obtained from the World Bank in 2015, $N_c$, and the total number of respondents between 2008 and 2018 in GWP per country, $N_c^{Gallup}$:

$$w_{ic}^{pop} = w_i^{Gallup} \cdot \frac{N_c}{N_c^{Gallup}} \tag{9}$$

We refer to $w_{ic}^{pop}$ as the individual-level population weights.

The Gallup World Polls are conducted in-person, except when countries have a phone penetration exceeding 80 %. As reaching this threshold may be correlated with mobile network rollout, differences in answers between in-person and phone interviews could drive our effects. Therefore, we check whether our results are robust to the omission of phone interviews. Table A16 shows that omitting phone interviews or omitting countries with at least one phone interview completely does not alter our results.

***Robustness across Sub-Periods***

As the internet and its contents developed rapidly in the period of study, we are interested in knowing whether the effect of 3G coverage is driven by either early or late time periods. Table A17 shows that this is not the case: when interacting 3G coverage with a dummy for years 2014 and later in our main specification, we obtain very comparable estimates.

## A.3 Implementation of the de Chaisemartin-D'Haultfœuille Estimator

In this section we discuss the use of the estimator proposed by de Chaisemartin and D'Haultfœuille (2020$a$) as an alternative to a TWFE regression using a continuous treatment variable. First, we introduce the estimator in the case of a binary staggered treatment. Thereafter, we discuss the adaptation of this estimator to the case where treatment is continuous and can change more than once over time. For a full discussion of this estimator and further extensions, we refer the reader to de Chaisemartin and D'Haultfœuille (2020$a$).

***dCDH Estimator for a Binary Treatment***

In the staggered adoption case with binary treatment, $DiD_l$ is an estimator comprising a weighted average over groups $g$ of $DiD_{g,l}$. This elementary building block is the difference (between first-treated units and a weighted average of suitable not-yet-treated units) in differences (over the length of $l$ periods after being treated) of those units first treated at time $F_g$ and being untreated prior to that. $g$ indexes the unit receiving treatment, in our case a subnational region. As this estimator computes $DiD_{g,l}$ at group $g$ level, all variables are aggregated on the group-time (indexed by group $g$ and time $t$) level prior to estimation.[65] The weights on $DiD_{g,l}$ are proportional to the number of observations in group $g$.

As it uses only clean control units (meaning that they have never been treated yet at $t$), this estimator is robust to treatment effect heterogeneity and dynamic effects.[66] Although this estimator is robust to those, for identification of a causal effect we still have to rely on a common trends assumption, which can be assessed using the placebo estimators.[67]

[65]Symbolically, we can write this as: $DiD_{g,l} = Y_{g,F_g+l} - Y_{g,F_g-1} - \sum_{g':D_{g',t=1}=0,F_{g'}>F_g+l} \frac{N_{g',F_g+l}}{N_{F_g+l}} \left(Y_{g',F_{g'}+l} - Y_{g',F_{g'}-1}\right)$. $Y_{g,t}$ is the (weighted) group-time level average outcome of the individual outcome $Y_{i,g,t}$. Groups $g^{'}$ are suitable control groups if they are untreated at period 1 ($D_{g',t=1}$) and remain untreated until at least $l$ periods after $g$ receives treatment for the first time ( $F_{g'} > F_g + l$). $N_{g',F_g+l}$ are the number of observations in the suitable treatment group $g'$. $N_{F_g+l}$ is the sum of the number of suitable groups $g'$ ($\sum_{g':D_{g',t=1}} N_{g',F_g+l}$), such that the weights on the outcome differences of the control groups sum to 1.

[66]Importantly, to calculate the $DiD_l$ using all available groups, one needs a treatment variable that is balanced on the group level, as knowledge of a group's past treatment status is essential for determining if it is a clean control group and whether the unit switches into treatment for the first time. Although we do not observe every district every year in the GWP, we do observe the value of 3G coverage in the gaps of the GWP sample. We leverage this information, which is not used in a TWFE setting, to identify the exact timing of switching into treatment.

[67]The placebo estimators $DiD_l^{pl}$ calculate the difference-in-differences between the treatment and control units between $l+2$ periods before and 1 period before the treated unit is treated for the first time. To ensure that we calculate the placebo estimators on (a subset of) the same observations as we calculate dynamic effects, we restrict the sample for the $l$th placebo estimators to the groups that are used for calculation of the $l$th dynamic effect. If we would not restrict the placebo estimators accordingly, the earlier (larger $l$) placebo estimators would predominantly cover later treated (larger $F_g$) units that have not been used for the later (larger $l$) dynamic estimators, which rely of earlier treated (smaller $F_g$) units. This problem arises due to the finite panel length: we do not observe outcomes (i) many periods after treatment for groups treated late in the panel and many periods

One can modify the estimator to allow for the inclusion of relevant covariates.[68] Including covariates allows for a weaker common trends assumption: common trends of treatment and control groups only needs to hold after conditioning on covariates.

***Extending to the Case of Non-Binary Treatments***

In our main empirical strategy, we use the population-averaged 3G coverage for every sub-national region. This is a non-binary treatment that gradually increases over time.[69] Nevertheless, we can still apply the principle of units switching into treatment for the first time to identify difference-in-differences between treatment and clean control groups. Some units receiving treatment for the first time during our sample period may already have a stable level of nonzero treatment for several periods (in our case, at least since the beginning of the sample period in 2008). We refer to the initial level of treatment in 2008 as $ini$. The elementary building block $DiD_{g,l}^{ini}$ is now differentiated over initial treatment status $ini$ and we calculate the $DiD_{g,l}^{ini}$ using treatment and control groups with the same $ini$. As 3G coverage is continuous, it is necessary to bin the initial treatments $ini$, as otherwise all districts are in different groups and we are unable to find a control group for a group that switches to a higher treatment.[70]

If those bins become too wide, treatment and control groups with fairly different initial levels of treatment are compared. In order to estimate the $DiD_{l}^{ini}$ unbiasedly, we have to assume that the treatment effects between the binned treatments do not vary over time.[71] As the (adoption

before treatment of groups treated early. Therefore, these estimators are important assessments of differential pre-trends between treatment and control units prior to the first treatment.

[68]Covariate adjustment of the elementary building blocks $DiD_{g,l}$ is performed in two steps. First, we run an OLS regression of the first differences in outcome on the first differences in covariates and time fixed effects on the sample of all never treated and treated groups prior to first treatment. Secondly, we residualize the $l^{\text{th}}$ temporal difference in outcomes using the coefficients of the first step multiplied by the $l^{\text{th}}$ temporal difference in covariates. The covariate-adjusted $DiD_{g,l}$ are then the differences between treatment and control in the difference over time relative to first treatment $l$ unexplained by the covariates. Covariate adjustment has implications for the feasibility of the estimator as there may be fewer observations in the regression than there are covariates in the first step.

[69]It is important to note that our treatment $3G$ is not exactly monotonically increasing, as the level of 3G coverage is allowed to decrease between two periods. In only 234 out of 2,120 districts in the main sample the coverage decreases by more than 3 % of population between any two years. As we omit these districts from our econometric analysis due to concerns about data reliability, we do not discuss here designs in which treatment can decrease. For a discussion of estimators when such declines are prevalent, see de Chaisemartin and D'Haultfœuille (2020*a*).

[70]Except for those districts with $ini = 0$, which constitute approximately 40 % of our sample.

[71]If this is not the case, the counterfactual of remaining in treatment $ini$ is not exactly the counterfactual treatment of staying in a slightly different initial treatment status $ini' \neq ini$ and the elementary building block $DiD_{g,l}^{ini}$ is biased through the differences in outcomes for control units (in symbols for all $l$: $Y_{g,F_g+l}^{ini} - Y_{g,F_g-1}^{ini} = Y_{g,F_g+l}^{ini'} - Y_{g,F_g-1}^{ini'}$ only holds if $TE_{g,F_g+l}^{ini \to ini'} = Y_{g,F_g+l}^{ini} - Y_{g,F_g+l}^{ini'} = Y_{g,F_g-1}^{ini} - Y_{g,F_g-1}^{ini'} = TE_{g,F_g-1}^{ini \to ini'}$). This bias is greater for (1) larger $l$, as treatment effects likely vary slowly as well as for (2) larger bins (implying larger $|ini - ini'|$), such that the treatment effect $TE^{ini \to ini'}$ is larger. This issue is mitigated if there is a balance in the various binned levels and their period of first treatment, as the biases cancel each other. In the simplified case of binning observations into two distinct treatment levels $ini$ and $ini'$, we use both groups with $ini$ as control groups for first switches from $ini'$ as well as groups with $ini'$ as control units for first switches from $ini$. In this case, the two contributions counteract, and the estimator binning observations with $ini$ and $ini'$ together ($DiD_{l}^{ini \cup ini'}$) is less biased.

of) internet and the activity of users changed considerably between 2008 and 2018, it is likely that treatment effects are heterogeneous over time. Any binning of initial treatment groups thus requires justification. As 3G coverage for many groups increases at least somewhat in most years between 2008 and 2018, it is helpful to define a stable treatment as an increase exceeding some threshold (which we call $\Delta_{3G}$) for an increase in 3G coverage between two subsequent years. Without this adjustment, for some initial treatment levels $ini$, it is impossible to find control groups, as most of the sub-national regions have changing levels of 3G coverage during the time span studied. As such a threshold biases the control group somewhat towards the treatment group, this is a conservative adjustment. However, if $\Delta_{3G}$ is too large, some levels of $ini$ may not have a single group switching into treatment and $DiD_l^{ini}$ is not defined.

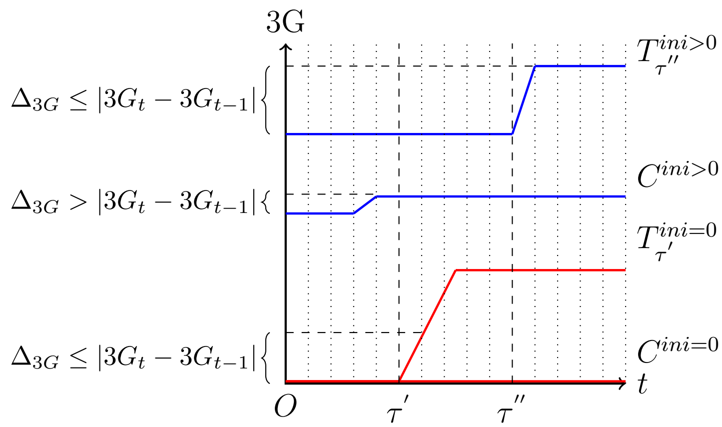
Figure A1 diagrammatically presents examples of time series of 3G coverage in the case of two initial treatment levels $ini = 0$ and $ini > 0$ and to which group they belong. Units that are never treated are indicated with $C$ and units that are treated are indicated by $T$. Treated units have a subscript $\tau$ to indicate the time period in which they switch into treatment. For $t < \tau$ ever treated units are not yet treated and thus also valid control groups. An illustration of an increase smaller than the threshold $\Delta_{3G}$ is given in the time series for $C^{ini>0}$, as well as increases larger than the threshold in the time series for $T^{ini=0}$ and $T^{ini>0}$.

As with the binary staggered adoption design, we calculate the dynamic effects $DiD_l$ where $l > 0$ are the cumulative effects of receiving treatment $l$ periods ago. The interpretation of the $DiD_l$ for the case of a monotonically increasing non-binary treatment is different from the binary staggered case. In the staggered case when $l \geq 1$, one can interpret $DiD_l$ as the cumulative effect of being treated for $l$ periods. However, as treatment may have increased further since the first time the region receives treatment (the 'first switch'), $DiD_l$ is a weighted average of the instantaneous effect of increased coverage in period $l$ and the dynamic effects of the first switch and the earlier period increases, respectively. Using the $DiD_l$, we can calculate the following quantity (de Chaisemartin and D'Haultfœuille, 2020*a*):

$$\hat{\delta}^L = \frac{\sum_{l=0}^{L} w_l DiD_l^Y}{\sum_{l=0}^{L} w_l DiD_l^{3G}} \tag{10}$$

$\hat{\delta}^L$ is the treatment effect per unit of treatment which is calculated using the ratio of the $DiD_l$ on the outcome of interest $Y$ and the $DiD_l$ on the treatment (3G), weighted by the share of observations in the $l$th effect. de Chaisemartin and D'Haultfœuille (2020*a*) shows that this is equivalent in interpretation to an IV estimator as the numerator in Equation 10 is the average treatment effect of a first switch, whereas the denominator is the average treatment following a first switch. Only if there would be no dynamic effects and treatment would be staggered, $\hat{\delta}^L$ denotes the ATT. Nevertheless, the estimator allows us to study the average treatment

Figure A1: Examples of Relevant Treatment and Control Groups for the de Chaisemartin and D'Haultfœuille Estimator

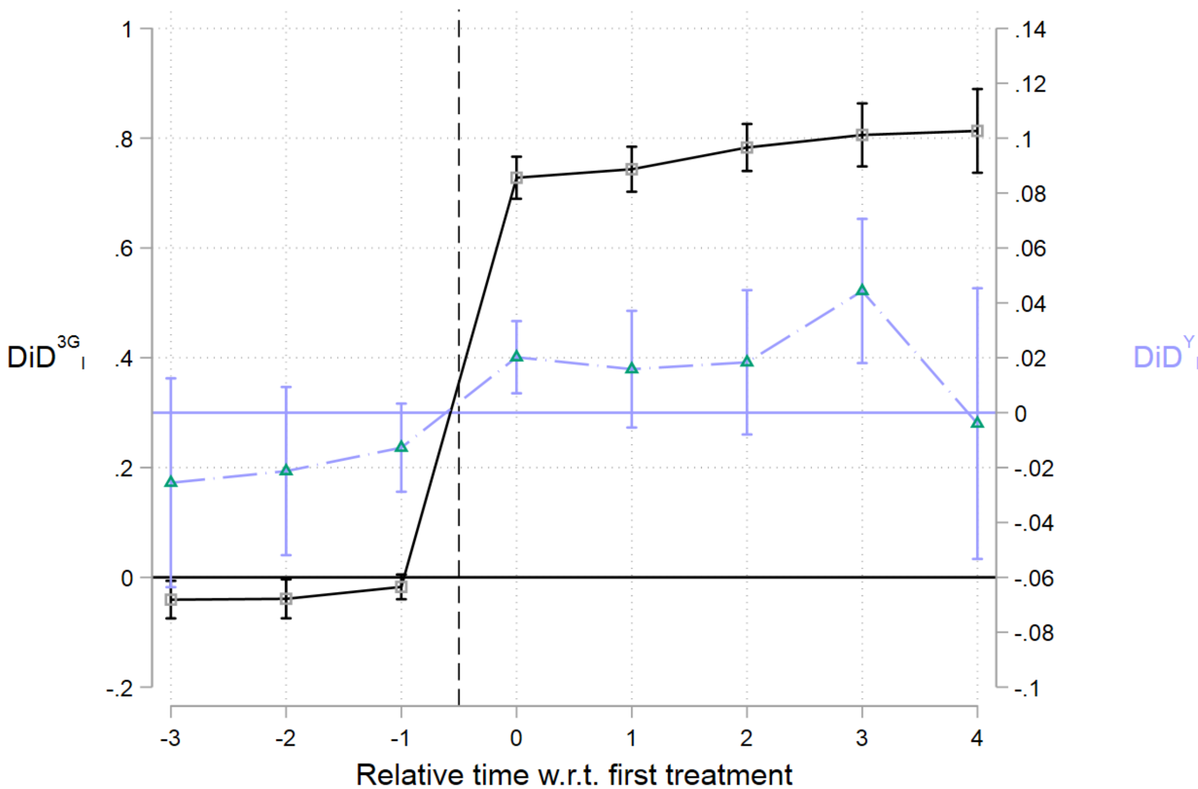



Notes: in this Figure, we show an example of a treatment (T) and control (C) unit for both $ini = 0$ (in red) and $ini > 0$ (in blue). Note that $C^{ini=0}$ overlaps with the horizontal axis and $T^{ini=0}$ overlaps with horizontal axis until $\tau'$. The control unit for $ini > 0$ is treated with less than the threshold $\Delta_{3G}$, so we consider it as a control unit also after the marginal treatment. The treated group for $ini = 0$ receives a treatment exceeding the threshold between $\tau'$ and $\tau' + 1$ and is considered a treated group from $\tau' + 1$ onwards. The treated group for $ini > 0$ receives treatment exceeding the threshold between $\tau''$ and $\tau'' + 1$ and is considered a treated group from $\tau'' + 1$ onwards.

effect using an estimator robust to heterogeneous and dynamic treatment effects. Therefore, $\hat{\delta^L}$ identifies a convex combination of (heterogeneous) instantaneous and dynamic treatment effects.

# Figures

Figure A2: Event Study around Sharp Increases in 3G Coverage




Notes: Event study estimates around treatment of 50 percentage point increase in 3G in a year with a 95 % confidence interval using the estimator proposed by de Chaisemartin and D'Haultfœuille (2020*a*). The black (blue) line depicts the event study estimates with 3G coverage (desire to emigrate) as dependent variable. All units that experience a decrease of more than 10 percentage points in 3G coverage between any two subsequent years are omitted, to exclude districts with possibly poor data quality. The sample covers 116,413 respondents in 380 districts with a sharp increase in 3G coverage. A test of joint insignificance of the pre-treatment period (placebo) estimators for the desire to emigrate gives a p-value of 0.22.

Figure A3: Oster's $\delta$ for Increasing Values of Maximally Admissible $R^2_{max}$

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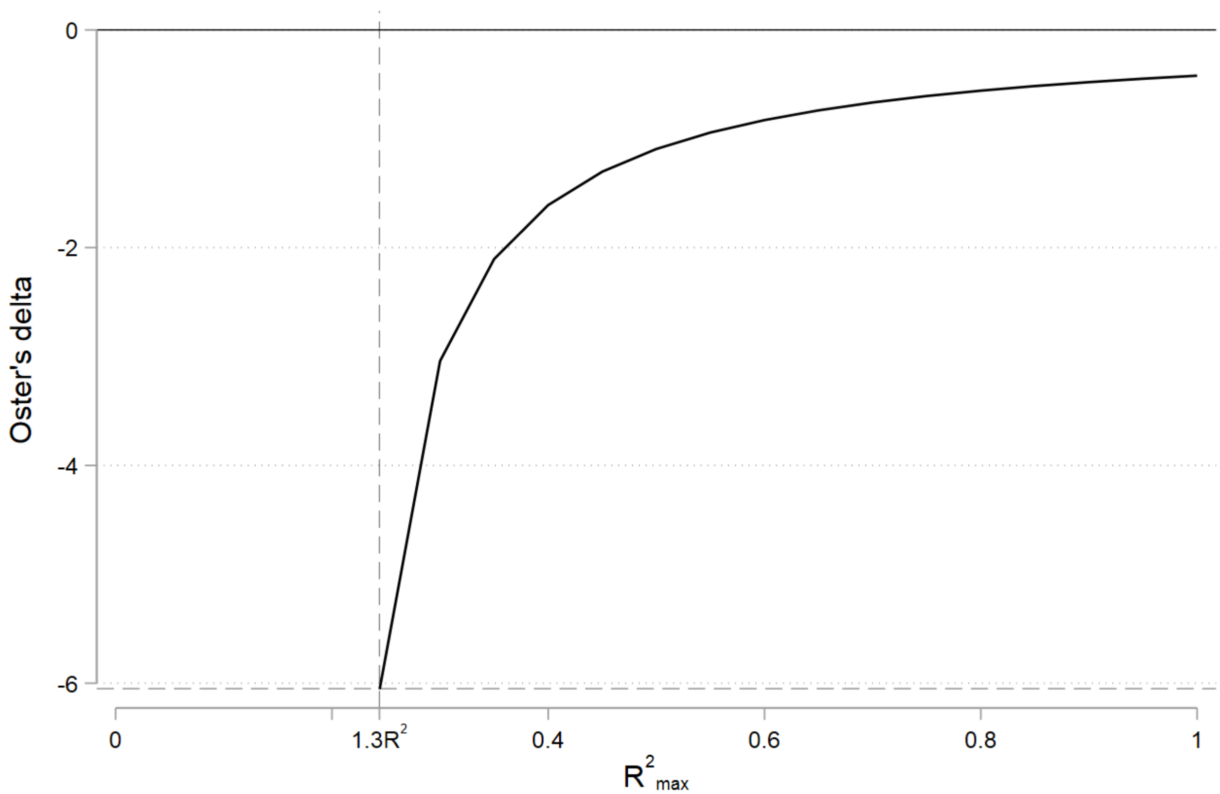


Notes: This Figure shows Oster's Delta (Oster, 2019) for different values of the maximum allowed variation in outcome that covariates can explain. Oster's Delta indicates how much stronger (and with what sign) the selection of unobservables should be compared to selection on observables to fully explain the found effect. The analysis here is based on our main specification, as found in Column 4 of Table 1. Oster's $\delta$ is equal to -6.05 for the value recommended by Oster (2019) of $R^2_{max} = 1.3R^2$ and Oster's $\delta$ is still equal to -0.4 when we allow unobservables to explain all remaining variation.

# Tables

Table A1: Questions in GWP relating to Respondents' Aspirations and Intentions to Migrate

| Variable | GWP ID | Question / construction | Coverage |
|---|---|---|---|
| *Panel A* | | | |
| (1): Desire to emigrate | WP1325 | Ideally, if you had the opportunity, would you like to move permanently to another country, or would you prefer to continue living in this country? | (2008 – 2018) |
| $(1C)$ | WP3120 | To which country would you like to move? (Asked only of those who would like to move to another country (WP1325)) | (2008 – 2018) |
| *Panel B* | | | |
| $Mig10252$ | WP10252 | Are you planning to move permanently to another country in the next 12 months, or not? (Asked only of those who would like to move to another country - WP1325) | (2010 – 2015) |
| $Mig6880$ | WP6880 | Are you planning to move permanently to that country in the next 12 months, or not? (Asked only of those who specified a country to which they would like to move. - WP3120) | (Mostly 2008/09) |
| (2): Plan to emigrate | WP10252& WP6880 | $Mig10252$, $Mig6880$ if $Mig10252$ unavailable | (2008 – 2015) |
| $(2C)$ | WP3120& WP10253 | WP3120 if question (2) answered positively (2008 – 2009) and WP10253 (2010 – 2015) | (2008 – 2018) |
| *Panel C* | | | |
| (3): Likely to move | WP85 | In the next 12 months, are you likely or unlikely to move away from the city or area where you live? | (2008 – 2018) |

Table A2: Summary Statistics and the Data Sources

| **Panel A: Baseline** | Mean | S.D. | Observations | Source | Level |
|---|---|---|---|---|---|
| Desire to emigrate | 0.22 | 0.42 | 617,402 | GWP | Individual |
| Plan to emigrate | 0.03 | 0.16 | 376,801 | GWP | individual |
| Likely to move | 0.17 | 0.37 | 544,022 | GWP | Individual |
| Regional 3G coverage | 0.37 | 0.39 | 617,402 | Collins Bartholomew | District-Year |
| Regional 2G coverage | 0.77 | 0.30 | 617,402 | Collins Bartholomew | District-Year |
| Male | 0.46 | 0.50 | 617,402 | GWP | Individual |
| Age | 40.10 | 17.02 | 617,402 | GWP | Individual |
| Urban | 0.39 | 0.49 | 617,402 | GWP | Individual |
| With partner | 0.58 | 0.49 | 617,402 | GWP | Individual |
| Separated/divorced | 0.06 | 0.24 | 617,402 | GWP | Individual |
| Presence of children | 0.56 | 0.50 | 617,402 | GWP | Individual |
| Secondary education | 0.53 | 0.50 | 617,402 | GWP | Individual |
| Tertiary education | 0.15 | 0.36 | 617,402 | GWP | Individual |
| Born in country of interview | 0.96 | 0.19 | 617,402 | GWP | Individual |
| Log of HH per capita income | 7.74 | 1.51 | 617,402 | GWP | Individual |
| Log of district per capita income | 8.15 | 1.15 | 617,402 | GWP | District-Year |
| Life satisfaction | 0.46 | 0.50 | 617,402 | GWP | Individual |
| Can count on friends/relatives | 0.82 | 0.39 | 617,402 | GWP | Individual |
| Satisfied with living standard | 0.62 | 0.48 | 617,402 | GWP | Individual |
| Living standard is getting better | 0.46 | 0.50 | 617,402 | GWP | Individual |
| Lack of money for food | 0.35 | 0.48 | 617,402 | GWP | Individual |
| Lack of money for shelter | 0.25 | 0.43 | 617,402 | GWP | Individual |
| Satisfied with the city | 0.78 | 0.41 | 617,402 | GWP | Individual |
| Satisfied with public transport | 0.62 | 0.49 | 617,402 | GWP | Individual |
| Satisfied with roads | 0.55 | 0.50 | 617,402 | GWP | Individual |
| Satisfied with education | 0.68 | 0.47 | 617,402 | GWP | Individual |
| Satisfied with healthcare | 0.58 | 0.49 | 617,402 | GWP | Individual |
| Satisfied with housing | 0.52 | 0.50 | 617,402 | GWP | Individual |
| Had money or property stolen | 0.16 | 0.37 | 617,402 | GWP | Individual |
| Log of GDP per capita | 8.44 | 1.40 | 617,402 | World Bank | Country-Year |
| Polity 2 | 5.44 | 5.01 | 617,402 | Center for Systemic Peace | Country-Year |
| Share of respondents below 30 | 0.32 | 0.13 | 617,402 | GWP | Country-Year |

Notes: All individual-level variables are binary apart from log of per capita income, with 1 denoting yes and 0 denoting no. All income-related variables are measures in USD PPP terms.

Table A3: The Effects of 3G Expansion on Access to the Internet

| | (1) | (2) |
|---|---|---|
| Outcome: | Internet Access | |
| 3G | $0.049^{***}$ | $0.051^{***}$ |
| | (0.015) | (0.014) |
| Broadband subscription rate | | ✓ |
| Observations | 614,945 | 606,541 |
| $R^2$ | 0.52 | 0.52 |
| Average dependent variable | 0.435 | 0.435 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. See notes to Table 1 for details on the specification. Standard errors are clustered two-way: at the district and country-year level.

Table A4: Robustness to Including Extensive Set of Additional Controls

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Outcome: | Desire to emigrate | | | | | | | |
| 3G | 0.029*** | 0.027** | 0.028*** | 0.024** | 0.027** | 0.029*** | 0.030*** | 0.026** |
| | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) | (0.011) | (0.012) | (0.013) |
| Nighttime light density | -0.000 | | | | | | | |
| | (0.001) | | | | | | | |
| Log of district-year median per capita HH income | | 0.003 | | | | | | |
| | | (0.005) | | | | | | |
| Log of district-year mean per capita HH income | | | 0.003 | 0.002 | 0.003 | 0.004 | 0.004 | 0.004 |
| | | | (0.003) | (0.003) | (0.003) | (0.003) | (0.003) | (0.004) |
| Demographic and country-level controls | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| District-level trend and district and year FEs | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Can count on friends/relatives | ✓ | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ |
| Satisfaction with local amenities | ✓ | ✓ | ✓ | | ✓ | ✓ | ✓ | ✓ |
| Satisfaction with life situation | ✓ | ✓ | | ✓ | ✓ | ✓ | ✓ | ✓ |
| Employment status | | | | | | ✓ | | ✓ |
| Received money/goods (from home country and abroad) | | | | | | | ✓ | ✓ |
| Additional controls | | | | | | | | ✓ |
| Observations | 606,712 | 617,402 | 617,402 | 617,402 | 617,402 | 579,507 | 566,873 | 471,622 |
| $R^2$ | 0.19 | 0.19 | 0.18 | 0.17 | 0.19 | 0.19 | 0.19 | 0.20 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Column (1) and (2) includes the baseline controls, except for the log of average per capita income in the household on the district-year level. Column (1) includes the nighttime light density, whereas Column (2) includes the log of median per capita income in the household on the district-year level. Column (3), (4) and (5) include the baseline controls, except for life satisfaction, satisfaction with living standards, whether the respondent believes to be financially better off in five years, whether the respondent has sufficient means for food, for shelter, and whether the respondent had something stolen in the past year in Column (3), satisfaction with housing, healthcare, education, roads, transportation and the city in Column (4), and whether the respondent can count on family or friends in Column (5). Column (6), (7) and (8) includes the baseline controls and additionally include a dummy for unemployment, involuntarily part-time employment and being out of the workforce in Column (6), whether the respondent received money or goods from abroad and whether the respondent received money or goods domestically in Column (7), and whether the respondent believes people can get ahead in life by working hard, expect to have higher life satisfaction in five years, whether the respondent believes his or her current living area to be good for immigrants, and whether the respondent has health problems in Column (8).

Table A5: Effect of 2G Internet and Lags/Leads of 3G Internet on the Desire to Emigrate

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Outcome: | Desire to emigrate | | | | | | |
| $2G_t$ | 0.019 | 0.018 | | | | | |
| | (0.014) | (0.013) | | | | | |
| $3G_t$ | | 0.027** | | | 0.027** | | |
| | | (0.011) | | | (0.011) | | |
| $3G_{t-2}$ | | | 0.017 | | | | |
| | | | (0.012) | | | | |
| $3G_{t-1}$ | | | | 0.001 | | | |
| | | | | (0.012) | | | |
| $3G_{t+1}$ | | | | | | 0.010 | |
| | | | | | | (0.013) | |
| $3G_{t+2}$ | | | | | | | -0.001 |
| | | | | | | | (0.015) |
| Observations | 617,402 | 617,402 | 551,021 | 581,401 | 617,402 | 548,152 | 473,783 |
| $R^2$ | 0.19 | 0.19 | 0.19 | 0.19 | 0.19 | 0.19 | 0.18 |
| Average dependent variable | 0.214 | 0.214 | 0.214 | 0.214 | 0.214 | 0.214 | 0.216 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. See notes to Table 1 for details on control variables. Standard errors are clustered two-way: on the district and the country-year levels.

Table A6: Robustness to Omission of Single Years from Sample

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Outcome: | Desire to emigrate | | | | | | | | | | |
| Omitted year: | 2008 | 2009 | 2010 | 2011 | 2012 | 2013 | 2014 | 2015 | 2016 | 2017 | 2018 |
| 3G | 0.028** | 0.024** | 0.018 | 0.040*** | 0.025** | 0.030*** | 0.031*** | 0.031*** | 0.028** | 0.031** | 0.016 |
| | (0.011) | (0.012) | (0.012) | (0.011) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.012) | (0.011) |
| Observations | 590,636 | 586,273 | 565,156 | 551,182 | 558,148 | 565,846 | 562,549 | 547,900 | 546,699 | 541,586 | 558,045 |
| $R^2$ | 0.19 | 0.19 | 0.19 | 0.19 | 0.19 | 0.19 | 0.19 | 0.19 | 0.19 | 0.19 | 0.19 |
| Average dependent variable | 0.222 | 0.221 | 0.222 | 0.221 | 0.223 | 0.221 | 0.220 | 0.221 | 0.218 | 0.218 | 0.215 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. See notes to Table 1 for details on control variables. Standard errors are clustered two-way: on the district and the country-year levels.

Table A7: Robustness to Omission of Global Regions from Sample

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| Outcome: | Desire to emigrate | | | | | | |
| Global region omitted: | Europe | Former USSR | AUS+CAN+ISR+JPN+ KOR+NZL+TUR+USA | Middle East | Rest of Asia | Americas without CAN+USA | Africa |
| 3G | 0.022* | 0.027** | 0.024** | 0.032*** | 0.036*** | 0.022** | 0.027** |
| | (0.011) | (0.012) | (0.012) | (0.012) | (0.013) | (0.011) | (0.012) |
| Observations | 509,276 | 539,645 | 575,734 | 523,460 | 498,829 | 609,016 | 448,452 |
| $R^2$ | 0.19 | 0.19 | 0.19 | 0.19 | 0.19 | 0.19 | 0.17 |
| Average dependent variable | 0.229 | 0.224 | 0.225 | 0.211 | 0.245 | 0.220 | 0.183 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Results omitting a mutually exclusive global region at a time. Column 1 omits European countries (including the Baltic countries), Column 2 omits former USSR countries (excluding Baltic countries), Column 3 omits a group of developed non-European countries: Australia, Canada, Israel, Japan, New Zealand, South Korea, Turkey, and the United States, Column 4 omits the Middle East, Column 5 omits the remaining Asian countries (except the Middle East, former USSR, Israel, Japan, South Korea and Turkey), Column 6 omits the Americas (excluding USA and Canada), and Column 7 omits Africa. See notes to Table 1 for details on control variables. Standard errors are clustered two-way: on the district and the country-year levels.

Table A8: Robustness to Excluding Countries with Many Refugees and High or Low Share of Respondents Desiring to Emigrate

| | (1) | (2) | (3) |
|---|---|---|---|
| Outcome: | Desire to emigrate | | |
| Excluding countries: | Top 10 refugee | $\geq$40% desire to emigrate | $\leq$10% desire to emigrate |
| 3G | 0.026** | 0.023** | 0.036*** |
| | (0.011) | (0.011) | (0.013) |
| Observations | 599,017 | 565,042 | 515,940 |
| $R^2$ | 0.19 | 0.16 | 0.17 |
| Average dependent variable | 0.216 | 0.194 | 0.251 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. See notes to Table 1 for details on control variables. Standard errors are clustered two-way: on the district and the country-year levels. Column (1) omits respondents in Afghanistan, Sudan, Democratic Republic Congo and Venezuela. Column (2) omits countries where, on average, more than 40% of GWP respondents desires to migrate. Column (3) omits countries where, on average, less than 10% of respondents desire to migrate.

Table A9: Robustness to Dropping Observations with Potentially Poor-quality 3G Data

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Outcome: | Desire to emigrate | | | |
| Omits: | Districts with a more than 10 p.p. drop in 3G coverage between 2008 and 2018 | Countries where first-reported 3G coverage exceeds 20% | Countries where 3G coverage is less than one-quarter of the number of mobile broadband subscriptions in 2015 | All aforementioned |
| 3G | 0.031** | 0.031** | 0.032*** | 0.037** |
| | (0.012) | (0.012) | (0.012) | (0.014) |
| Observations | 580,253 | 522,958 | 501,979 | 427,062 |
| $R^2$ | 0.19 | 0.18 | 0.18 | 0.18 |
| Average dependent variable | 0.224 | 0.221 | 0.231 | 0.219 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. See notes to Table 1 for details on control variables. Standard errors are clustered two-way: on the district and the country-year levels. Column (1) omits districts that experience a sharp drop of more than 10 percentage points in 3G coverage anytime between 2008 and 2018, Column (2) omits districts in countries that report a country-average population coverage exceeding 20% in the first year of nonzero reported coverage, Column (3) omits regions with a population-averaged 3G coverage lower than one-quarter of the number of mobile broadband subscriptions in 2015, as reported by ITU. Column (4) omits all units omitted in Columns (1-3) compared to the baseline displayed in Table 1.

Table A10: Balancing Test of 3G on Baseline Demographic Covariates

| Outcome: | 3G × 100 |
|---|---|
| Male | 0.008 |
| | (0.032) |
| Age | -0.001 |
| | (0.006) |
| Age-squared | 0.000 |
| | (0.000) |
| Urban | 0.028 |
| | (0.147) |
| With partner | -0.102* |
| | (0.053) |
| Separated/divorced | -0.170* |
| | (0.099) |
| Presence of children | 0.100 |
| | (0.064) |
| Secondary education | -0.032 |
| | (0.087) |
| Tertiary education | -0.101 |
| | (0.121) |
| Not born in country of interview | -0.015 |
| | (0.142) |
| Log of personal income | -0.009 |
| | (0.050) |
| Log of district-year mean per capita HH income | -0.063 |
| | (0.555) |
| N | 617,402 |
| R2 | 0.932 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. p-value of the F-test of joint insignificance: 0.1154. Standard errors are clustered two-way: on the district and the country-year level.

Table A11: Robustness to Randomization Inference and Multiple Hypothesis Testing

| Outcome: | (1)<br>Desire to emigrate | (2)<br>Plans to emigrate | (3)<br>Likelihood to migrate | (4)<br>Joint test of irrelevance |
|---|---|---|---|---|
| 3G | $0.027^{**}$ | $0.009^{**}$ | $0.027^{***}$ | |
| *Young(2019) Randomized p-value* | (0.020) | (0.023) | (0.004) | (0.014) |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Young (2019) randomization inference p-values in parentheses, based on 500 bootstrap replications. Column 1-3 denote the point estimates of Table 1 and the standard errors corrected for multiple hypothesis testing. See notes to Table 1 for details on control variables.

Table A12: Robustness to Alternative Variance-Covariance Matrix Structure

| | (1) | (2) |
|---|---|---|
| Outcome: | Desire to emigrate | |
| 3G | 0.027*** | 0.027* |
| | (0.009) | (0.014) |
| Observations | 617,402 | 617,402 |
| $R^2$ | 0.19 | 0.19 |
| Level of clustering | Country-Education-Gender | Country |
| Number of clusters | 658 | 112 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. See notes to Table 1 for details on control variables. Standard errors are clustered two-way: on the district and the country-year levels.

Table A13: Robustness to Omission of Non-balanced Countries and Districts

| | (1) | (2) |
|---|---|---|
| Outcome: | Desire to emigrate | |
| 3G | 0.047*** | 0.059*** |
| | (0.018) | (0.020) |
| Observations | 202,378 | 179,138 |
| $R^2$ | 0.16 | 0.15 |
| Average dependent variable | 0.156 | 0.164 |
| Level of balancing | Country | District |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. See notes to Table 1 for details on control variables. Standard errors are clustered two-way: on the district and the country-year levels.

Table A17: Interaction of 3G with Time Period Dummy

| Outcome: | Desire to emigrate |
|---|---|
| 3G × I(Year<2014) | 0.029** |
| | (0.012) |
| 3G × I(Year≥2014) | 0.024* |
| | (0.012) |
| N | 617,402 |
| R2 | 0.19 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered two-way: on the district and the country-year level.

Table A14: Robustness to Alternative Choices of Weighting Observations

| | (1) | (2) | (3) |
|---|---|---|---|
| Outcome: | | Desire to emigrate | |
| 3G | 0.033*** | 0.027** | 0.039*** |
| | (0.010) | (0.011) | (0.013) |
| Observations | 617,402 | 617,402 | 617,402 |
| $R^2$ | 0.19 | 0.19 | 0.22 |
| Average dependent variable | 0.222 | 0.222 | 0.222 |
| Weights | Unweighted | Gallup only (baseline) | Population and Gallup |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. See notes to Table 1 for details on control variables. Standard errors are clustered two-way: on the district and the country-year levels.

Table A15: Robustness to Different Specifications of District-specific Time Trends

| | (1) | (2) | (3) |
|---|---|---|---|
| Outcome: | | Desire to emigrate | |
| 3G | 0.027** | 0.018* | 0.032*** |
| | (0.011) | (0.009) | (0.012) |
| Observations | 617,402 | 617,402 | 617,402 |
| $R^2$ | 0.19 | 0.18 | 0.20 |
| Average dependent variable | 0.222 | 0.222 | 0.222 |
| District-level trend | Linear | - | Linear + Quadratic |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. Column 1 presents the baseline result. See notes to Table 1 for details on control variables. Standard errors are clustered two-way: on the district and the country-year levels.

Table A16: Robustness to Omission of Telephone Interviews

| | (1) | (2) | (3) |
|---|---|---|---|
| Outcome: | | Desire to emigrate | |
| 3G | 0.027** | 0.029** | 0.028** |
| | (0.011) | (0.012) | (0.013) |
| Observations | 617,402 | 514,637 | 506,326 |
| $R^2$ | 0.19 | 0.19 | 0.19 |
| Average dependent variable | 0.231 | 0.231 | 0.231 |
| Telephone Interviews | All | No | No (country) |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses. See notes to Table 1 for details on control variables. Column 2 omits all phone interviews, whereas Column 3 omits all countries with at least 1 phone interview in the sample. Standard errors are clustered two-way: on the district and the country-year levels.

Table A18: Country Income Groups used in Causal Forest Procedure

| Income Group | Country |
|---|---|
| Low-income countries | Afghanistan, Benin, Burkina Faso, Cambodia, Cameroon, Chad, Congo Brazzaville, Congo Kinshasa, Guinea, Haiti, Kyrgyzstan, Laos, Lesotho, Liberia, Madagascar, Malawi, Mozambique, Nepal, Niger, Rwanda, Senegal, Sierra Leone, Sudan, Tajikistan, Tanzania, Uganda, Zambia, Zimbabwe |
| Lower-middle-income countries | Armenia, Bhutan, Ecuador, Egypt, El Salvador, Georgia, Ghana, Guatemala, Honduras, India, Indonesia, Jordan, Kenya, Moldova, Mongolia, Namibia, Nicaragua, Nigeria, Philippines, Sri Lanka, Tunisia, Uzbekistan, Vietnam |
| Upper-middle-income countries | Azerbaijan, Belarus, Brazil, Bulgaria, Chile, Colombia, Costa Rica, Dominican Republic, Gabon, Latvia, Mauritius, Mexico, Montenegro, Panama, Paraguay, Romania, Russia, Serbia, South Africa, Thailand, Ukraine, Uruguay, Venezuela |
| High-income countries | Australia, Austria, Belgium, Canada, Cyprus, Croatia, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Israel, Italy, Japan, Lithuania, Luxembourg, Malaysia, Netherlands, New Zealand, Norway, Poland, Portugal, Saudi Arabia, Singapore, Slovak Republic, Slovenia, South Korea, Spain, Sweden, Switzerland, Trinidad and Tobago, Turkey, United Kingdom, United States |

Table A19: The Effect of Material Well-being and Satisfaction with Life and Institutions on the Desire to Migrate, prior to 3G coverage

| **Panel A: Material well-being** | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| X-variable: | Household income (log) | Material prospects first principal component | Job climate index | Financial well-being index |
| Outcome: Desire to migrate | 0.000 | -0.100*** | -0.076*** | 0.031*** |
| | (0.002) | (0.010) | (0.007) | (0.005) |
| Observations | 181,505 | 166,227 | 179,298 | 49,864 |
| $R^2$ | 0.19 | 0.19 | 0.19 | 0.21 |

| **Panel B: Life satisfaction and optimism** | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| X-variable: | Optimism index | Daily experience index | Life evaluation index | Life purpose index |
| Outcome: Desire to migrate | -0.078*** | -0.105*** | -0.017*** | 0.025*** |
| | (0.009) | (0.011) | (0.004) | (0.004) |
| Observations | 181,341 | 180,996 | 170,071 | 49,807 |
| $R^2$ | 0.19 | 0.19 | 0.17 | 0.21 |

| **Panel C: Institutional satisfaction** | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| X-variable: | Law and order index | Corruption index | Community basics index | Trust in government first principal component |
| Outcome: Desire to migrate | -0.113*** | 0.060*** | -0.117*** | -0.125*** |
| | (0.009) | (0.006) | (0.010) | (0.009) |
| Observations | 181,505 | 174,984 | 181,505 | 146,794 |
| $R^2$ | 0.19 | 0.19 | 0.19 | 0.18 |

| **Panel D: Mobile Banking and Remittances** | | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| X-variable: | Owns a bank account | Used cellphone to receive cash in the last 12 months | Received money or goods from friend/ family from same country | Received money or goods from friend/ family from another country |
| Outcome: Desire to migrate | 0.017*** | 0.053*** | 0.010*** | 0.068*** |
| | (0.006) | (0.014) | (0.004) | (0.007) |
| Observations | 53,658 | 53,268 | 162,009 | 162,009 |
| $R^2$ | 0.18 | 0.18 | 0.19 | 0.19 |

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors, clustered by district and country-year, in parentheses. The specification used in Columns 1 – 4 of Panels A – D is similar to that of Column 4 of Table 1. In Columns 1 and 2 of Panel D we omit the district-level time trends, as we only have 3 time periods available. We only exclude the control variables related to local amenities as some of these amenities are used in the construction of the GWP indices. All independent variables of interest in Panels A – C are GWP indices, except for "(log) household income" (which is the reported log of per capita household income), "material prospects" (a first principle component of the following questions (weights in parentheses): living comfortably on present income (0.69), now is a good time to find a job (0.34), and not having enough money to afford food (-0.65)), and "trust in goverment" (a first principle component of four questions related to trust in the government, as constructed by Guriev, Melnikov and Zhuravskaya (2021).). For all items in Panel A – C a higher value of the independent variable of interest implies a higher value of the item. For example, a higher value of "Material prospects first principal component" implies a better subjective evaluation of material well-being and a higher value of "Corruption index" implies a larger perception of corruption. For construction of the GWP indices, see https://www.oecd.org/sdd/43017172.pdf (Last accessed on 08-12-2021).