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Mobile Internet Access and the Desire to Emigrate*

Abstract

How does mobile internet access affect the desire to emigrate and migration plans? To answer this question, we combine survey data on more than 600,000 individuals from 110 countries with data on worldwide 3G mobile internet rollout. We show that an increase in mobile internet access increases desire to emigrate. This effect is particularly strong for higher-income individuals in low-income countries. We identify three potential mechanisms. Access to the mobile internet lowers the cost of acquiring information and leads to a drop in perceived material well-being and trust in government. Using municipal-level data from Spain, we also document that 3G rollout increased actual migration flows.

JEL Code: F22
Keywords: Migration intentions; desire to emigrate; internet access; 3G internet

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1 Introduction

The internet and mobile phones have changed how people live, work, connect, and exchange information. The number of internet users increased globally from 410 million in 2000 to nearly 4.1 billion in 2019, and is expected to continue double-digit growth (ITU, 2019). 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).[1] 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 open a new research front by studying how 3G mobile internet rollout causally affects desire and plans to emigrate.[2]

We estimate the effect of mobile internet access on desire and plans to emigrate by combining two unique data sets: Gallup World Polls (GWP) and Collins Bartholomew's Mobile Coverage Explorer.[3] Combining these allows us to use data from 600,000 individuals living in 2,200 sub-regions in 110 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

[1] 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 (WB16, 2016).

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

[3] 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.

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 0.29 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.[4] When moving from no to full 3G coverage, the implied aggregate effect is about 13 (32) percent of the baseline average of desire (plans) to emigrate permanently. 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 intending to emigrate by 90,000.

Although our main econometric specification controls for regional fixed effects at the subnational district level and annual fixed effects at the global level, as well as linear district-level 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. 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 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

[4]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 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 population weights.

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 following Guriev, Melnikov and Zhuravskaya (2021), which exploits exogenous variations in the regional frequency of lightning strikes to predict the speed of the expansion of regional mobile broadband internet coverage.

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 (balance test, using leads as treatments to assess whether future increases in 3G coverage predict previous changes in the desire to emigrate, using 2G network expansion as a placebo treatment, excluding potentially bad controls, multiple hypothesis testing, ruling out the importance of influential observations, excluding top 10 refugee-origin countries as well as districts with very high and low emigration desires, alternative levels of clustering, using different weights, omitting time trends, and using a balanced sample of countries and districts that are included in all years). We also explore heterogeneity of the effects using Causal Forests approach.

We then explore the mechanisms behind our results. We begin by showing that the effect of 3G 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 (suggesting that the internet offers access to information that is similar to the information offered by personal networks). We also show that 3G coverage does not worsen 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 emigration desires, 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 internationallly) 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).[5]

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

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

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 economic and 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 other non-political outcomes — international migration aspirations and intentions.[6]

Our data and empirical setting also provide some unique advantages that allow us to provide new evidence on the desire and plans to migrate in three more dimensions. First, we use granular (1x1 kilometer grid level) data on *mobile* 3G network coverage to calculate population-averaged coverage 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 using two alter-

[6]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 until 2008, but this pattern has reversed since.

native empirical strategies. Third, we show that 3G penetration not only affects migration intentions but also actual migration behavior.

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 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 1 that can be used to capture wage 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 education, $\boldsymbol{x_j}$ includes age and experience, and also factors such as gender, the family situation and 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 opportunities 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 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 term α_{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 ϵ_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 γ_{3G}, with $\gamma_{3G} < 0$ as a 3G network facilitates communication. Individual-specific component ϵ_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 θ, $0 < \theta < 1$. Individual mobility 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

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

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:

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

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

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

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

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, $\left(\beta^1_{3G,0} - \beta^0_{3G,0} - \gamma_{3G} - \frac{\alpha_{3G}}{\theta}\right)$, 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, $\phi\left((\boldsymbol{\beta_1} - \boldsymbol{\beta_0} - \boldsymbol{\gamma} - \boldsymbol{\alpha}/\theta) \cdot \boldsymbol{x_j}\right)$, 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 fraction θ of those investing in the acquisition of information can migrate. Therefore, there is a linear link between the desire to migrate and migration plans, 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 migration desires, instead, would suggest, as a potential explanation within the model, that the 3G coverage may have boosted local wages. In section 7 we analyze directly whether 3G coverage is related to subsequent earnings.

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 information on country-level indicators (ranging from urbanization rate to political regime), district-level nighttime light density and population, which we describe in detail in Appendix A.

4.1 Data

Gallup World Polls

Our primary data on migration 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.[7] Our resulting main sample includes about 600,000 respondents from 110 countries.

The survey's outcome variables of interest were identified by questions asked to Gallup respondents about their (international) migration desires, plans, preparations and likelihood thereof.[8] The outcomes of interest, their time span, the wording of the underlying question and possible responses are:[9]

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. International Migration Plans (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. International Migration Preparations (2009 – 2015): *Have you done any preparation*

[7]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

[8]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 Table A1. Moreover, questions (2) and (3) are asked only during a specific time span and when the respondent answered positively to (1). Thus, (2) and (3) automatically assigned with a negative answer for those observations in the right time span that answered negatively to (1)

[9]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.

for this move? (asked only of those who are planning to move to another country in the next 12 months) **Yes/No/Don't know/Refused to answer**

4. Self-assessed Migration Likelihood (2008 – 2018): *In the next 12 months, are you likely or unlikely to move away from the city or area where you live in?*[10]
   Likely/Unlikely/Don't know/Refused to answer

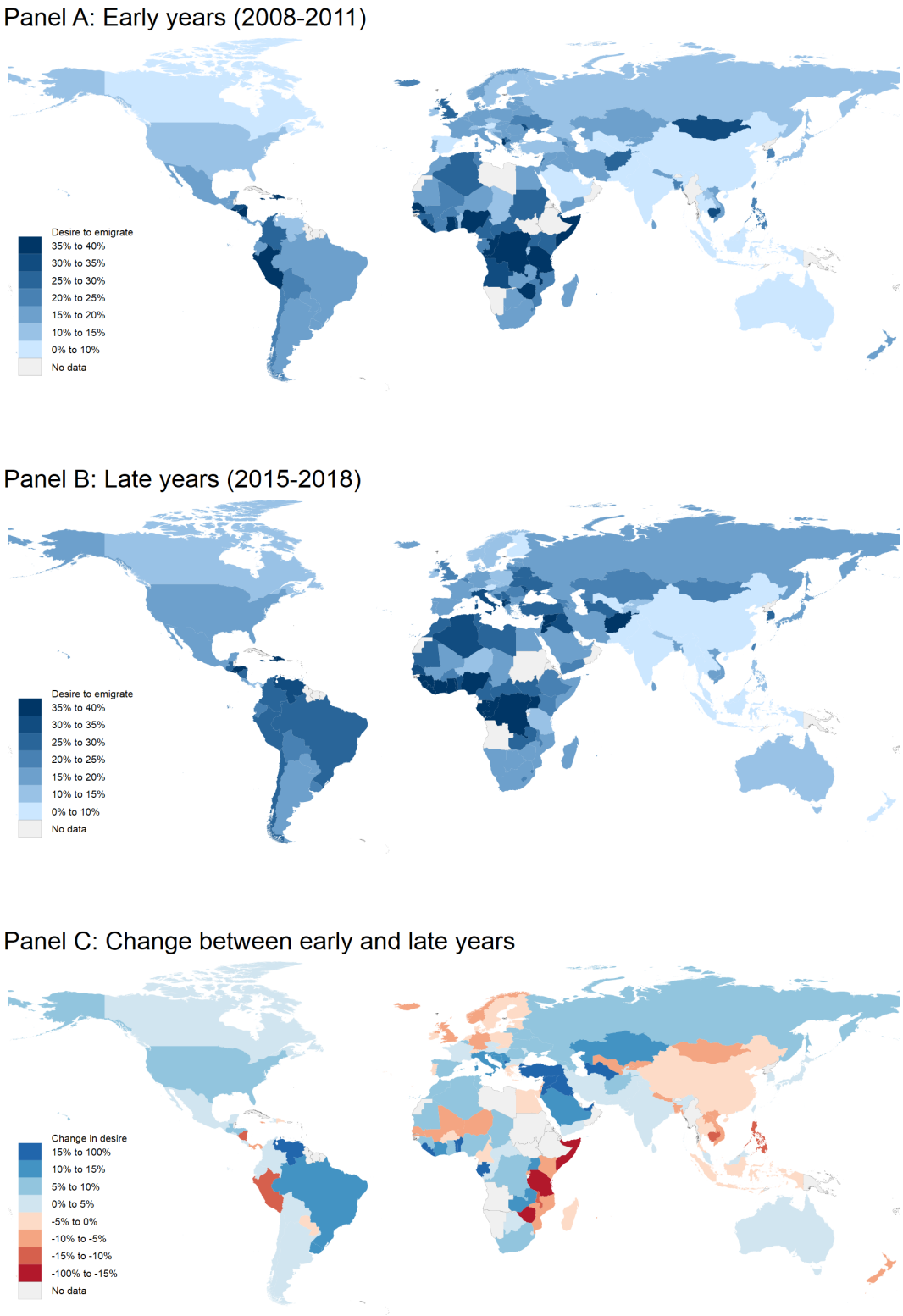
(1) captures "wishing to move abroad", which can simply reflect a general aspiration of the respondent. In our paper, we consider this group as potential migrants who look for migration opportunities but are also aware that the hurdles and frictions preventing its translation into actual migration could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

Questions (2), (3) and (4) reveal more concrete intentions and arrangements that individuals may undertake before leaving. (Tjaden, Auer and Laczko, 2019) document that questions (2) and (3) are strongly related to actual migration flows.[11] The emphasis on a relatively short time window of 12 months make it likely that only individuals with serious and developed migration plans answer affirmatively (Dustmann and Okatenko, 2014). In other words, (2) and (3) filter respondents who have the means to achieve and are taking steps towards migrating domestically or 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 %) or are preparing to move abroad (around 1 %) is substantially lower than the share of those who reported having desire to emigrate (22 %).

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

[11] (Tjaden, Auer and Laczko, 2019) combine (2) and (3) with a question from the GWP to which country one intends to migrate, and regress actual bilateral migration rates on the share planning (2) and preparing (3) to migrate and find a slope of around 0.8 in a cross section of more than 2,000 origin-destination pairs. This suggest that plans and preparations to migrate to a given country are indicative of current levels of out-migration. However, as no bilateral flow data is available for all destination countries, it relies on (prospective) migration to OECD countries. Furthermore, they omit dyads without an actual flow.

Figure 1: Share of respondents desiring to migrate in early (2008 – 2011) and late (2015 – 2018) years and the change between early and late years, by country.



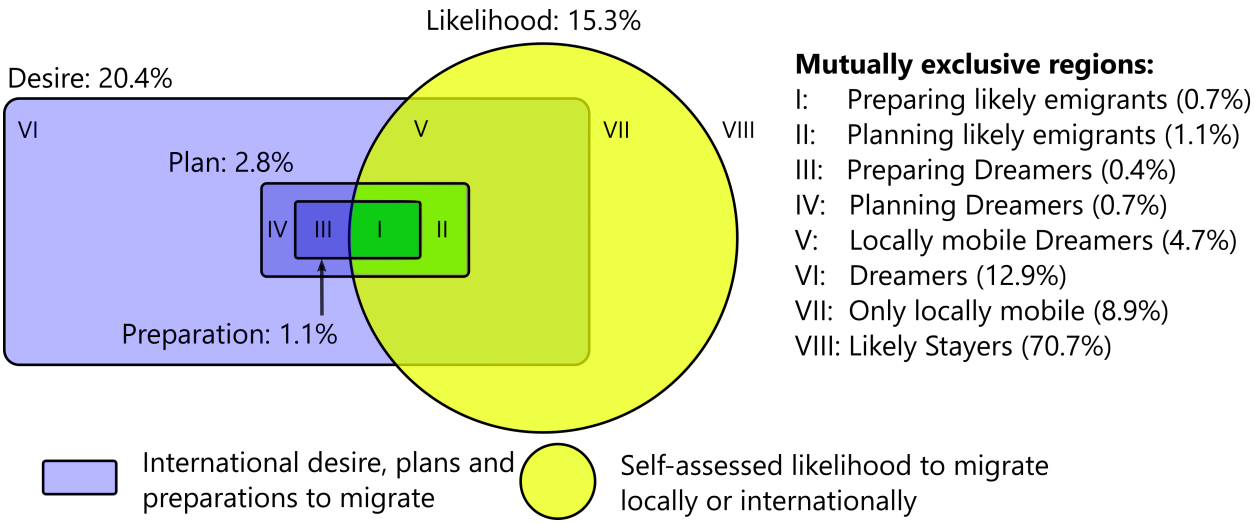
There is significant heterogeneity in migration aspirations within and across countries. Panel A of Figure 1 shows the averaged levels of the desire to emigrate in the 2008 – 2011 period, and Panel B for the 2015-2018 period. Panel C of Figure 1 displays the changes in average reported desire to emigrate between early (2008 – 2011) and late years (2015 – 2018). [12] Notable patterns can be summarized as follows: (i) less than 20 percent desires to emigrate in most developed countries; (ii) less than 10 % in many East Asian countries; and (iii) there is substantial variation in the share of people desiring to emigrate within global regions over time — in the Middle East and North Africa (an increase from 16 % in 2008 to 26 % in 2018), Sub-Saharan Africa (an increase from 30 % in 2008 to 36 % in 2018) and South America excluding Venezuela (an increase from 16 % in 2010 to 32 % in 2018).[13]

We visually summarize our outcomes in a Venn diagram in Figure 2, which identifies eight mutually-exclusive regions for migration aspirations and plans (ranging from preparing to move to likely to stay). Regions I and II are of particular interest as they combine narrow definitions of migration intentions with a self-assessed likelihood of moving away within 12 months. Therefore, they are likely to capture more developed preparations (I) and plans (I+II) to migrate, in comparison with general preparations (I+III) and plans to migrate (I+II+III+IV). Among those *planning* to migrate (2.8 %), about two-thirds (1.8 %) report that they are likely to move within 12 months. Similarly, among those *preparing* to migrate (1.8 %), about two-thirds (1.1 %) are likely to move within 12 months. Therefore, it is plausible that plans to migrate within 12 months are a better proxy for actual migration behavior. Moreover, region VII identifies those deeming migration likely, but do not desire to emigrate. Although not a perfect measure, VII predominantly captures those that intend to migrate domestically.

[12]Note that we take the latest and earliest available year in early year periods, as some countries are not included in GWP for all years.

[13]The GWP question on desire to emigrate was not asked in South America prior to 2010.

Figure 2: Venn diagram of the four migration-related outcomes (1 – 4), identifying eight mutually exclusive regions. Note that all regions are only defined for 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 positively to questions 1,2 and 3 (boxes) and 4 (circle) from the main text, whereas the list on the right-hand side gives the proportion of respondents belonging to each of the mutually exclusive groups. $N = 317,520$.

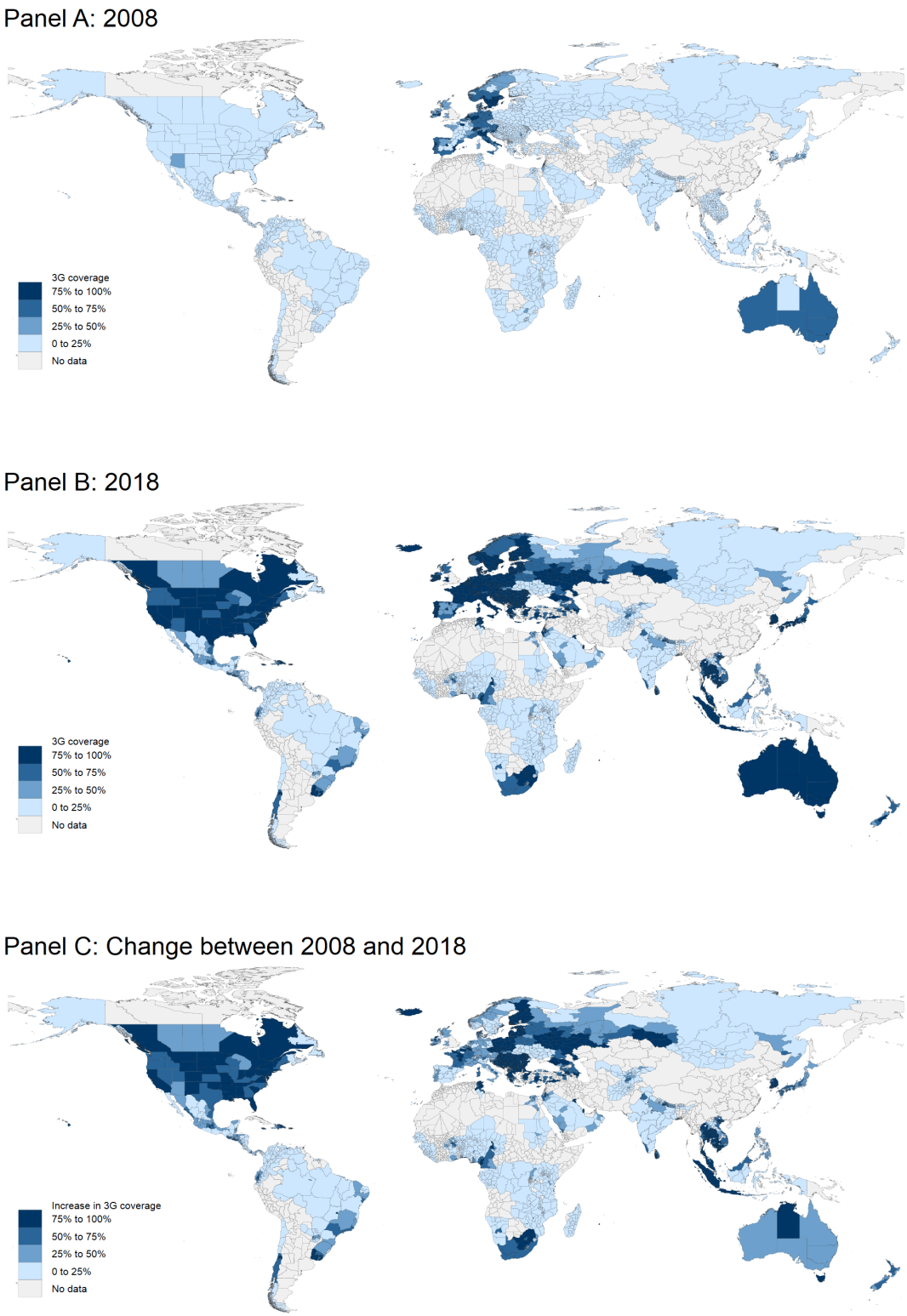


Respondents are also asked about which country they *desire* to move to (for question (1)) and which country they *plan* to move to (for question (2)). We use this information to construct a yearly data set on origin-destination-level rates of the desire to emigrate and plans to emigrate. We then combine 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).[14]

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.

[14]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. This suggests that individuals desiring to migrate seriously form ideas about possible destinations.

Figure 3: Population-averaged 3G coverage in 2008, 2018, and the change between 2008 and 2018, by subnational region.



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.[15] The data provide information on signal 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. 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.[16] We use the reported coverage in year $t-1$ to represent the network coverage in year t.[17]

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.[18] We first calculate each grid point's population coverage and then aggregate this information

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

[16]Due to the change in data provider, data between the first quarter of 2010 and December 2011 are missing. We overcome this challenge by linearly interpolating the missing information using the data from 2009 and 2011.

[17]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.

[18]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.

over the subnational regions as provided in the GWP. The constructed population-weighted coverage of 3G networks is our main treatment variable.[19]

Figure 3 illustrates the increase in 3G internet coverage at the subnational region level over time.[20] In particular, Panel A of Figure 3 shows population averages of 3G internet coverage in 2008, Panel B in 2018 and Panel C shows the increase between 2008 and 2018. 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. Conversely, many Latin American and Sub-Saharan African countries have coverage levels of below 25 %. Nevertheless, several 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.

WWLLN Lightning Incidents Data

We obtain global data on geo-coded lightning strikes from the World Wide Lightning Location Network (WWLLN).[21] In particular, we use these data to construct an IV following Manacorda and Tesei (2020) and Guriev, Melnikov and Zhuravskaya (2021). The intuition is that cloud-to-ground (CG) lightning is likely to damage the electrical equipment of mobile network towers, which implies a cost of reparation as well as the cost of using additional lightning-protection hardware. This provides us with a possible source of exogenous district-level variation in 3G expansion.

To construct our instrument, we weight every lightning strike with the local population density (in each one square kilometer cell) and calculate the intensity per square kilometer

[19]The data are not available for large countries such as Algeria, Angola, Argentina, Bangladesh, China, Ethiopia, Iran, Iraq, Kazakhstan, Myanmar, Morocco, Pakistan, Peru and Yemen. We also exclude Australia, Canada and the United States as the subnational districts (i.e. states) in GWP are too large for calculating meaningful 3G coverage of the GWP respondents

[20]The data availability is somewhat limited for some countries. For example, both Canada and Australia are removed from the final data set as the subnational regions are much larger than in other countries. Data for some countries with large migration aspirations, intentions and flows in the Middle East and North Africa (MENA) region are also missing.

[21]The WWLLN network detects lightning not through optical, but very low frequency (VLF) signals, which has the advantage of carrying further than optical signals and thus requiring fewer detectors.

at the subnational level.

Additional Data Sets for Main Specification

- Nighttime Light Density: To control for district-level economic development, we 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.[22] 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 2007 and 2017 (from more than 200 origin countries to 47 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).

- 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.[23]

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

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

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 subsample.[24] Appendix Table A3 shows that a 10 percentage point increase in district-level 3G coverage leads to a statistically significant 0.49 percentage point increase in the likelihood of having access to the internet — this effect is about 18 % of the baseline average (in 2008, 28 % of respondents reported having access to the internet). This effect is probably an underestimation of the effect of 3G on internet access, as prior to 2016 the question about internet access probes access *at home* only.

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 bilateral desire and plans to migrate rates between origin and destination countries.[25] We then match our *desired* and *planned* migration-flow matrix with data on actual migration flows to OECD countries between 2007 and 2018.[26]

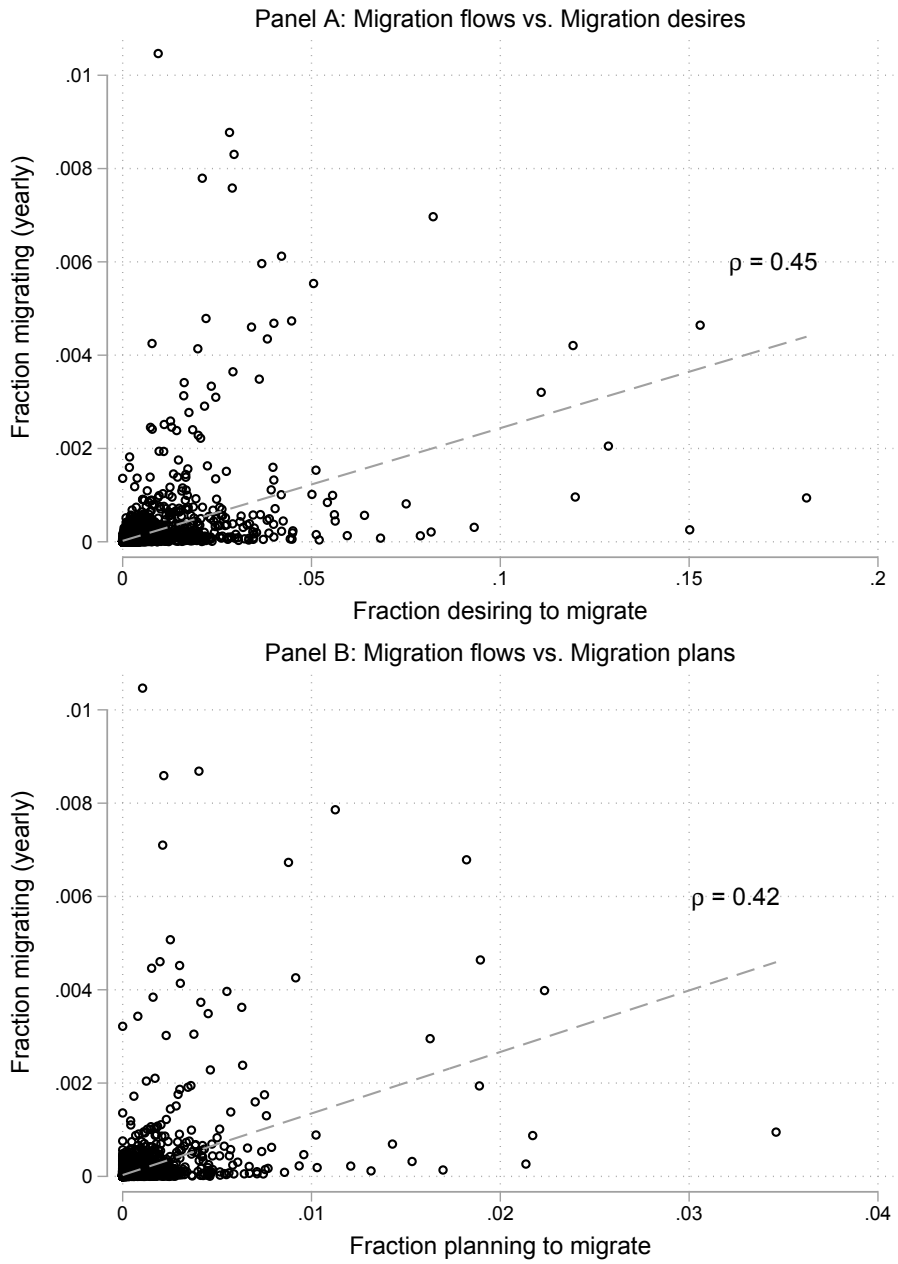
html

[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)*

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

[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.

Figure 4: Scatter plot of yearly migration flow rate versus the constructed rate of (Panel A) emigration desire (N = 5,346) and (Panel B) emigration plans (N = 4,711) from 155 origin countries to 47 OECD countries between 2007 and 2017. Every data point is an origin-destination pair.



The results presented in Figure 4 confirm that our outcome variables are strongly associated with the official migrant flow data. We find that the correlation on the origin-destination level between annual migration flow rates and the share of respondents desiring to migrate

from a specific origin to a specific destination is 0.45. The raw correlation between yearly migration flow rates and the proportion of respondents planning to migrate from a specific origin to a specific destination is 0.42.[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 (Two-Way Fixed Effects (TWFE, de Chaisemartin and D'Haultfœuille estimator and IV) that we use to study the effect of 3G coverage on migration desires, plans and preparations.

5.1 Main Estimation Model

We estimate the effect of 3G internet access on individuals' migration aspirations and intentions using a difference in differences methodology. Our models take the following form:

$$Outcome_{idt} = \beta 3G_{dt} + \alpha^{'}\mathbf{X_{idt}} + \phi_d + \theta_t + \gamma_d \cdot t + \epsilon_{idt} \tag{5}$$

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

We use outcomes (1), (2) and (4) 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 (4) 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

[27] As the correlation between migration desires and migration plans on the origin-destination level is 0.83, they largely capture the same variation.

and 0 representing a negative answer. 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]

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
- 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, θ_t, (to capture the impact of global shocks that affect all countries simultaneously), district dummies, ϕ_d, (to control for time-invariant

[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.

[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.

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.

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 and (ii) are not yet or will not be treated. We provide evidence in an event-study framework by constructing an indicator variable for large increases (at least 50 percentage points in one year) in 3G coverage.[30] The results indicate parallel trends prior to 3G adoption. We also show that leads (that is, future levels of 3G coverage) do not affect current migration aspirations.

2. We carefully handle the issue of possible violations of parallel trends. By including district-specific linear time trends, we capture possible downward- or upward-trending common causes that correlate to both 3G coverage and migration aspirations. Without

[30] 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. Nevertheless, in section 6.2 we provide a pre-trend test that focuses on the trends prior to *any* first increase in 3G coverage.

inclusion of such linear time trends, such common causes may bias our estimates. This makes our specification more demanding by capturing part of the variation in 3G coverage, as 3G coverage expanded gradually over the period of study across the world. 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.[31]

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 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 (see, for example, Hombrados, Ciacci and Zainudeen (Forthcoming)). 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

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

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 the number of 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 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 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[32] and dynamic[33] treatment effects (Sun and Abraham, 2021; Borusyak, Jaravel and Spiess, 2021; Goodman-Bacon, 2018; De Chaisemartin and D'Haultfoeuille, 2020*b*; Callaway and Sant'Anna, 2021).

[32]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).

[33]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 difference in differences regression does not control for 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.

To enable the estimation of the 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 groups to assess the contemporaneous and dynamic treatment effect.[34] 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 $DiD_{t,l}^{ini}$. This is a 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 t and where treated and control units have initial treatment ini. In other words, treated units are only compared to control units in the same bin ini. In a similar fashion, we calculate the pre-treatment 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 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 $DiD_{t,l}^{ini}$, we need to consider units that have received minimal treatments as untreated.

[34] Similarly, a treatment group that has been treated previously may carry dynamic treatment effects and may thus be unsuitable.

- **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 $DiD_{t,l}^{ini>0}$, we bin treatment in groups $ini = 0$ and $ini \neq 0$.

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 being treated 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 β 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*) 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 Strategies

Lightning Incidence and Delayed 3G Rollout

To further address the concerns about omitted variables bias and reverse causality, we use an IV strategy following Manacorda and Tesei (2020) and Guriev, Melnikov and Zhuravskaya (2021).[35] In particular, Manacorda and Tesei (2020) use spatially differential incidence

[35]Instruments for traditional cable internet connections are often based on the positioning of main ('back-

rates of lightning strikes as a source of exogenous variation in mobile network expansions to study the role of mobile communication in political mobilization in Africa.[36] In the global context, Guriev, Melnikov and Zhuravskaya (2021) adopt a similar instrument using worldwide lightning data from very low frequency (VLF) radiation detectors on a 1x1 km resolution from the WWLLN project.

The intuition of the instrument is that electromagnetic discharge due to lightning in or around a base transceiver station (BTS) can damage the antenna and telecommunications equipment, thus requiring repair. Appropriate earthing and shielding of electrical equipment and the use of power surge-protection devices can mitigate this, but come at a substantial cost. Both the cost of repair and the cost of protective measures increase the cost of operating mobile networks. As the expected likelihood of lightning in a given region is known, it is plausible that investments in mobile network coverage by operators is deterred in areas with a higher incidence of lightning.

Following Guriev, Melnikov and Zhuravskaya (2021), we focus on lightning strikes from WWLLN between 2005 and 2011 to alleviate concerns of lightning patterns in later time periods being affected by climate change.[37] Importantly, WWLLN has a good detection efficiency for cloud-to-ground (CG) lightning, which is advantageous over space-based optical detection of lightning, which is most sensitive to intra-cloud (IC) lightning.[38]

The WWLLN project documents lightning at the single geo-coded and time-stamped lightning strike level, which we weight by population density and aggregate to the subnational

bone') internet cables that offer large bandwidth (Hjort and Poulsen, 2019; Porcher, 2020). Romarri (2020) constructs an instrument for broadband internet by using the interaction between the coverage of telephone landlines before the period of interest (in the 1990s) and the point in time internet became available. Unfortunately, a similar instrument, using the interaction of $2G$ network coverage on the subnational level in the year 2000 and expansion of $3G$ coverage on the global region level (excluding the country of interest), is too weak in the presence of district-specific linear time trends.

[36] Manacorda and Tesei (2020) use optical detection-based NASA data, which is available on a 55x55 km spatial resolution, but this is unavailable for higher latitudes.

[37] As the sign of the effect of climate change on global lightning rates is subject to academic debate (Finney et al., 2018) and thus plausibly not anticipated by mobile network operators, it is most likely that network operators base such decisions on historical patterns.

[38] IC and CG lightning are not very strongly correlated, and the IC-to-CG ratio varies greatly over latitude (Prentice and Mackerras, 1977).

region level.[39] Using these data, we construct the instrument as follows.

We first determine whether a lightning strike occurred in a 1 square kilometer box in the grid of the GPW population density data. $L_{box,day,r}$ is a dummy variable indicating whether a lightning strike occurred in a 1 square kilometer grid cell in a given *day* in a given year in a subnational region r. $P_{box,r}$ is the population in the 1 square kilometer grid cell in region r in 2005 and $P_r = \sum_{box} P_{box,r}$ is the total population of the region. Then, we aggregate the lightning incidence over all days of the years 2005 to 2011 and all 1 square kilometer cells in the region:

$$\mathcal{L}_r = \frac{1}{P_r} \sum_{box} \sum_{day} L_{box,day,r} P_{box,r}$$

Assuming that protection measures largely mitigate the damage of lightning strikes, the cost of lightning for a given location is a concave function of lightning strike intensity, which we operationalize by assuming a logarithmic relation (all regions are large enough to have at least one lightning strike during our period of analysis). We interact $\log(\mathcal{L}_r)$ with a linear time trend to construct our instrument: high lightning frequency districts expand 3G networks more slowly because of the expected additional cost of power surge protection and repairs from lightning damage. Exploiting the differential response of regions with different levels of development, we construct three separate instruments for the districts by upper-, middle- and lower-tercile of district-level average income, as measured in the GWP. These terciles coincide strongly with initial levels of 3G coverage.[40] The construction of instruments separately for income groups to identify a local average treatment effect (LATE) is important for two reasons:

Relevance: As the potential financial benefits from extending 3G coverage are greater in

[39] The WWLLN uses only several tens of detectors worldwide, as the VLF radiation in the kHz range is detectable thousands of kilometers away. Nowadays, the detection efficiency of powerful (discharges exceeding 30kA) lightning strikes is around 30% and the typical spatial accuracy is in the order of a few kilometers. The detection efficiency of CG lightning by WWLLLN improved during the time span 2005 – 2011 from 4% to 10% due to an increase in the number of VLF sensors (Abarca, Corbosiero and Galarneau Jr., 2010).

[40] In our main sample, the upper tercile of districts had an average 3G coverage in excess of 40%, the middle tercile 2.5%, and the lowest tercile less than 0.1%

wealthier regions than in poorer regions, a higher level of anticipated lightning-induced cost is less likely to lead to lower investment in 3G network in wealthier regions than in poorer regions. It thus improves the relevance of the instrument.

Monotonicity: Allowing the effect of lightning to vary for various groups is important for satisfying the monotonicity assumption to identify a LATE (Angrist and Pischke, 2008). For example, before the start of our sample in 2008, wealthier countries may have expanded 3G coverage predominantly in districts with lower lightning frequency. Therefore, high lightning frequency districts may even see a stronger increase to catch up to the surrounding districts, given all other characteristics. It is thus important to allow the slope of the instrument to differ for different groups in the first stage.

Lightning patterns are likely to be correlated to geography and demography, both of which plausibly impact mobile network expansion.[41] Therefore, it is necessary to control for the effect on 3G expansion of factors such as population density, area size, and the share of land are covered by deserts and mountains.

Pre-existing 2G Infrastructure and Faster 3G Rollout

High lightning incidence is one possible cause of delayed 2G and 3G network expansion. However, many other reasons could have contributed to variation in 2G and 3G networks prior to our period of study. When 2G network infrastructure is absent, expansion of 3G networks is more costly: the cell tower infrastructure can be shared by a 2G and a 3G BTS. Therefore, we employ a second IV that is less likely than the IV based on lightning to suffer from issues related to weak instruments. We use the variation in 2G networks prior to the period we study (2006) as a measure of pre-existing infrastructure. The larger the infrastructure, the stronger the predicted 3G expansion is in our period of study.[42] We use 2006 as the time stamp for pre-period infrastructure, as 2G networks in earlier years are

[41]For an overview of the effects of geography and demography on 3G and 4G network expansion in the United Kingdom, see: https://www.ofcom.org.uk/__data/assets/pdf_file/0027/146448/Economic-Geography-2019.pdf.

[42]Note that the variation in 2G networks in 2006 also crediby includes the variation in lightning incidence that affects rollout of mobile networks.

absent in poorer regions in our sample. Along the same lines as for the lightning-based IV, we construct our instrument by interacting the 2G network in 2006 with a linear time trend. A major weakness of this approach in comparison with the lightning IV is that it is unknown what is exactly driving the variation in pre-existing 2G networks. This unobserved characteristic could be related to trends in migration aspirations and intentions.

However, the lightning IV identifies a LATE on a non-random subsample that complies with the variation in lightning incidence. As we will show in the results, this only concerns low-income districts. As the variation in pre-existing 2G networks may be more pluriform, an IV based on such variation identifies a treatment effect that is more representative of the global sample we have at our disposal.

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 TWFE estimator. Thereafter, we focus on the desire to migrate and we present results for the de Chaisemartin-D'Haultfœuille estimator for non-binary treatment, and two complementary IV strategies. Ultimately, we conduct a heterogeneity analysis using the recently developed causal forest procedure.

6.1 Main Results

Table 1 reports estimates of Equation 5 for the three main outcomes. The dependent variables are binary variables indicating that the respondent "would like to move permanently to another country" (first panel), that the respondent "has plans to migrate internationally 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 life satisfaction-related controls 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.

Column 1 shows a positive, statistically significant relationship between 3G mobile internet expansion and migration desire and plans. Column 5 restricts all variation to within country income tercile and country educational attainment observations, and reports conservative estimates that are similar in magnitude and still significant at conventional levels.

In our preferred model (Column 4), we find that a 10 percentage point increase in 3G coverage leads to 0.29 percentage point increase in the desire to emigrate, a 0.09 percentage point increase in international migration plans in the next 12 months, and a 0.26 percentage point increase in local or international migration likelihood in the next 12 months. Given that the mean levels of these outcome variables are 19, 2.8 and 17%, the effects are sizable. Importantly, 3G internet expansion not only has an impact on *desire* but also shapes *actual plans* to emigrate.

Table 2 reports estimates for four additional dependent variables to illustrate which of the groups (as described in Figure 2) drive the results.[43] The dependent variables are a dummy indicating that the respondent "has any desire to migrate internationally or deems it likely to migrate locally or internationally in the upcoming 12 months" (first panel), that the respondent "plans to emigrate in the next 12 months" (second panel), that the respondent "has made preparations to emigrate in the next 12 months" (third panel); and that the respondent "is likely to migrate domestically in the next 12 months" (fourth panel).

The first outcome measures whether respondents desire to migrate internationally or deem

[43] We can only conduct this analysis for the 2010 – 2015 period due to data unavailability.

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

	(1)	(2)	(3)	(4)	(5)
Outcome:	Desire to emigrate (I-V)				
3G	0.030**	0.028**	0.030***	0.029**	0.028**
	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)
Observations	606,827	606,827	606,827	606,827	606,827
R^2	0.12	0.16	0.19	0.19	0.19
Average dependent variable	0.222	0.222	0.222	0.222	0.222
First year	2008	2008	2008	2008	2008
Last year	2018	2018	2018	2018	2018
Outcome:	Plans to emigrate in the next 12 months (I-IV)				
3G	0.009*	0.009*	0.009**	0.009*	0.008*
	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
Observations	376,801	376,801	376,801	376,801	376,801
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+II+V+VI)				
3G	0.026**	0.025**	0.027***	0.027***	0.027***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Observations	553,849	553,849	553,849	553,849	553,849
R^2	0.10	0.12	0.16	0.16	0.16
Average dependent variable	0.17	0.17	0.17	0.17	0.17
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 5 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.

it likely to move away from their current residence. The next two are actual migration-related outcomes that take plans and preparations into account. The last focuses solely on domestic migration intentions in the next 12 months.

We find that 3G internet coverage has a positive, sizable and statistically significant effect on the desire, plans and preparations to emigrate, but no statistically significant effect on the perceived likelihood of domestic migration. This finding suggests that 3G expansion shapes international migration 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.[44] 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 groups to include all baseline covariates and a large number of control groups for every unit first switching into treatment, we assign already treated (prior to 2008) subnational regions (that is, $ini > 0$) into bins; (ii) we set the treatment threshold, Δ_{3G}, for a (first) switch to be a 3 percentage point increase in

[44] 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 estimations in Table 1 are -0.77,-0.44 and -0.78 for the three featured TWFE regressions. This suggests that our baseline results may be biased.

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

	(1)	(2)	(3)	(4)
Outcome:	Any desire or plans to migrate (I-VII)			
3G	0.043***	0.041***	0.043***	0.041***
	(0.014)	(0.014)	(0.013)	(0.013)
Observations	541,644	541,644	541,644	541,644
Average dependent variable	0.311	0.311	0.311	0.311
Outcome:	Preparing or planning likely emigrant within 12 months (I+II)			
3G	0.011***	0.011***	0.011***	0.011***
	(0.004)	(0.003)	(0.003)	(0.004)
Observations	368,388	368,388	368,388	368,388
Average dependent variable	0.018	0.018	0.018	0.018
Outcome:	Preparing likely emigrant within 12 months (I)			
3G	0.006***	0.006***	0.006***	0.006**
	(0.002)	(0.002)	(0.002)	(0.002)
Observations	317,520	317,520	317,520	317,520
Average dependent variable	0.007	0.007	0.007	0.007
Outcome:	Likely internal migrant within 12 months (VII)			
3G	0.009	0.008	0.009	0.008
	(0.008)	(0.008)	(0.008)	(0.008)
Observations	541,644	541,644	541,644	541,644
Average dependent variable	0.093	0.093	0.093	0.093
District and year fixed effects	✓	✓	✓	✓
District-year trends	✓	✓	✓	✓
Demographic controls		✓	✓	✓
Life satisfaction-related controls			✓	✓
Income controls			✓	✓
Country-level controls				✓

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 (4) and covers years between 2008 and 2018. The second measure is the likely plans to migrate (positive to question (2) and (4), whereas the third is the likely preparations to migrate internationally (positive to question (3) and (4)) and covers 2008 to 2015, as defined in Figure 2. The fourth measure comprises those answering positively to (4) but negatively to (1), and covers the years between 2008 and 2018. The standard errors are clustered two-way on the country-year and district level.

3G coverage and exclude all districts that experience a decrease of 3 percentage points or more between any two years from the sample. By doing so, we include only districts where treatment is monotonically increasing. We chose a threshold of 3 percentage points as the largest proportion of small increases is concentrated below 3 percentage points; results are qualitatively similar if using 2 or 5 percentage points as the threshold.

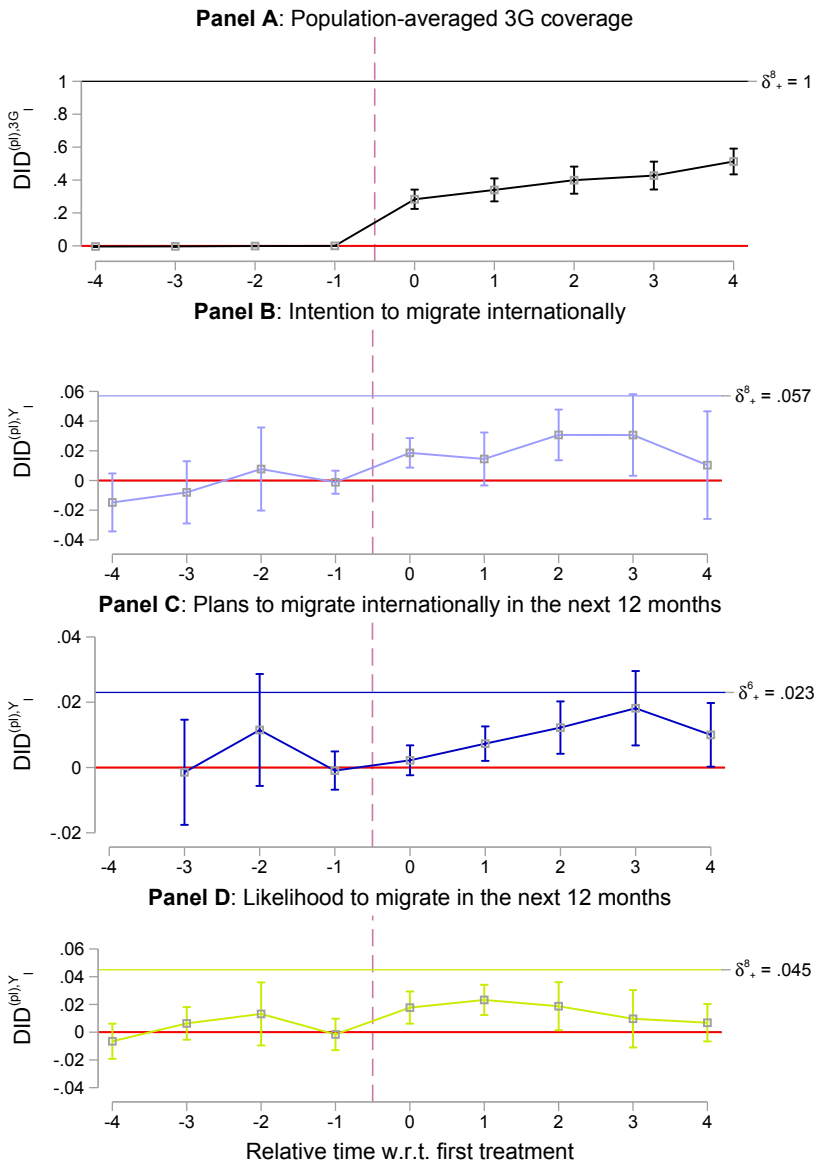
In Figure 5, we show the instant and three dynamic estimators (referring to two, three or four years after the expansion), DiD_l^Y, and three placebo estimators, DiD_l^{pl}, for our treatment variable (referring to two, three or four years before the expansion).[45] The confidence interval of the placebo estimators should enclose 0 to support the parallel trends assumption.[46] Notably, the results reported in all panels of Figure 5 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 migrate internationally increases immediately after an initial increase in 3G coverage and then remains stable. The average effect of all observations following a first increase in 3G expansion (δ) is 0.057, which exceeds our TWFE estimate. 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 significant from 0. 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.

[45] 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-year level. Note that, in Table A12, we find that clustering at the country level gives somewhat smaller standard errors than our baseline estimates.

[46] 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 any of the placebo estimators is nonzero.

Figure 5: De Chaisemartin-D'Haultfouille Estimates for 3G and Migration Intentions



Notes: $DiD_l^{(pl)}$ of the effect of first switchers in 3G coverage in Panel A on the three main outcomes in Panels B, C and D. p-value for jointly insignificant time trends equal 0.575 in Panel B, 0.06 in Panel C and 0.04 in Panel D and suggest a downward pre-trend two time periods before the first switch in Panels C and D. δ^n denotes the estimated average effects using the instantaneous effects and the n dynamic effects. The threshold for a switch is a coverage of 3% of the population. Treatment and control groups are matched within two groups, either those with initial treatment level $ini = 0$ or those with initial treatment level $ini \neq 0$ in 2008. Observations are weighted using the district-year average of Gallup weights and the number of respondents. After a switch, a district can no longer be part of a control group anymore and is only considered for the lth dynamic effect of its first switch. Standard errors are calculated using 50 bootstrap replications, clustered on the country-year level, 95% confidence intervals are shown.

6.3 Instrumental Variables

To alleviate concerns about the endogeneity of 3G network coverage, we instrument 3G expansion by the logarithm of regional population-weighted lightning-strike frequency interacted with a linear time trend. As our baseline includes regional-level linear time trends, we omit those in the IV estimations.

Table 3 reports the two-stage least squares (2SLS) estimates at the individual level. Column (1) shows the baseline result from Table 1 for comparison purposes, Column (2) reports the reduced form results, Column (3) reports the second-stage results, and Column (4) shows the first stage coefficients of 3G coverage on the three instruments. Among the three income terciles, the lowest-income districts drive the first stage: districts with high frequencies of lightning strikes expand their 3G coverage less. For the middle and upper terciles, the first-stage coefficients are statistically insignificant. The F-statistic is 14.23, which suggests a sufficiently strong first stage.

In line with our baseline results, IV estimates also indicate that 3G expansion leads to an increase in desire to migrate. The IV estimate is much greater in magnitude than the TWFE estimate, for two reasons. First, as the effect of mobile internet coverage is likely to be heterogeneous, we identify a LATE which may be higher for those regions complying with the instrument. Second, measurement error in 3G coverage may be substantial and cause a bias towards zero in the difference in differences estimates.

Alternative IV Estimation Based on Pre-Existing 2G Infrastructure

We also construct an alternative 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 construct the instrument in a similar fashion to the lightning-based instrument: we interact the 2G coverage in 2006, $2G_{2006,r}$, with a linear time trend. Importantly, we use the

Table 3: Lightning IV Results

	(1)	(2)	(3)	(4)
Dependent variable:	Desire to emigrate			3G
Stage:	Baseline	Reduced	IV: second	IV: first
3G	0.029**	0.028***	0.143**	
	(0.010)	(0.005)	(0.025)	
Anderson-Rubin 95% Confidence Interval			*[0.044,0.313]*	
Lowest-income tercile districts × $\log(\mathcal{L}_d + 1)$ × year				-0.012***
				(0.000)
Middle-income tercile districts × $\log(\mathcal{L}_d + 1)$ × year				-0.003
				(0.289)
Highest-income tercile districts × $\log(\mathcal{L}_d + 1)$ × year				0.006
				(0.151)
First-stage F-statistic				14.23
Observations	606,827	606,827	606,827	606,827
R^2	0.188	0.177	0.176	0.880
Mean dep. var	0.223	0.223	0.223	0.371
District-level time trends	✓			
IV-related controls		✓	✓	✓
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 control variables. 3G expansion is instrumented by the logarithm of population-weighted lightning density on the district level between 2005 and 2011, interacted by a yearly time trend for each of the three between district-level income groups. Column (1) shows our baseline estimate, which includes district-level time trends. To include the instrument at the district *times* (linear) 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, mean altitude of the district, the share of mountains, the initial population-weighted 3G coverage, a dummy for 3G coverage being 0 on the district level 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 on the country-year and district level.

same controls as in the lightning instrument and, in addition, we control for time-varying 2G coverage ($2G_{t,r}$) to alleviate concerns about the validity of the exclusion restrictions.

Table 4 shows the 2SLS estimates. The first two columns are slightly different than the first two columns of Table 3, 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 45.21, suggesting a very strong relation between 2G coverage in 2006 and the expansion of 3G. Unsurprisingly, this exceeds the F-statistic of the lightning-based instrument, as the 2G coverage in 2006 likely also reflects reduced coverage due to high lightning intensity, as well as other causes that impact the cost of mobile network expansion and the direct effect of pre-existing 2G infrastructure on the ease of expanding 3G networks.

The second stage in Column (3) shows a statistically significant point estimate of 0.095, which is lower than the lightning-based IV estimate, but still considerably greater than the TWFE result found previously.

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, based on a

Table 4: 2G Infrastructure IV Results

	(1)	(2)	(3)	(4)
Dependent variable:	Desire to emigrate			3G
Stage:	Baseline	Reduced	IV: second	IV: first
3G	0.029**	0.027***	0.095**	
	(0.011)	(0.006)	(0.040)	
Anderson-Rubin 95% Confidence Interval			*[0.013,0.195]*	
$2G_{2006} \times$ year				0.041***
				(0.000)
First-stage F-statistic				45.21
Observations	606,827	606,827	606,827	606,827
R^2	0.188	0.177	0.177	0.883
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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. P-values in parentheses. See notes to Table 1 for details of control variables. 3G expansion is instrumented by the logarithm of population-weighted lightning density on the district level between 2005 and 2011, 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, which includes district-level time trends. To include the instrument at the district *times* (linear) 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 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.

criterion that maximizes treatment effect heterogeneity.[47] Regression trees aim to predict an outcome variable building on the mean outcome of observations with similar characteristics. Similar to bootstrapping processes, variance is 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:

$$\widetilde{Y_{ict}} = \alpha_i(X_{it}^{'}) + \tau_i(X_{it}^{'})3G_{c,t} + u_{ict} \tag{6}$$

where $\widetilde{Y_{ict}}$ is a dummy indicating that the respondent i in country c and interview year t "would like to move permanently to another country", and $X_{it}^{'}$ is the full set of baseline covariates.[48] However, as we have many (2,200) subnational districts, we have many fixed effects, which may be problematic in a method based on regression trees.[49] 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).[50] 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.

The CF approach allows us to calculate Conditional Average Treatment Effects (CATE)

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

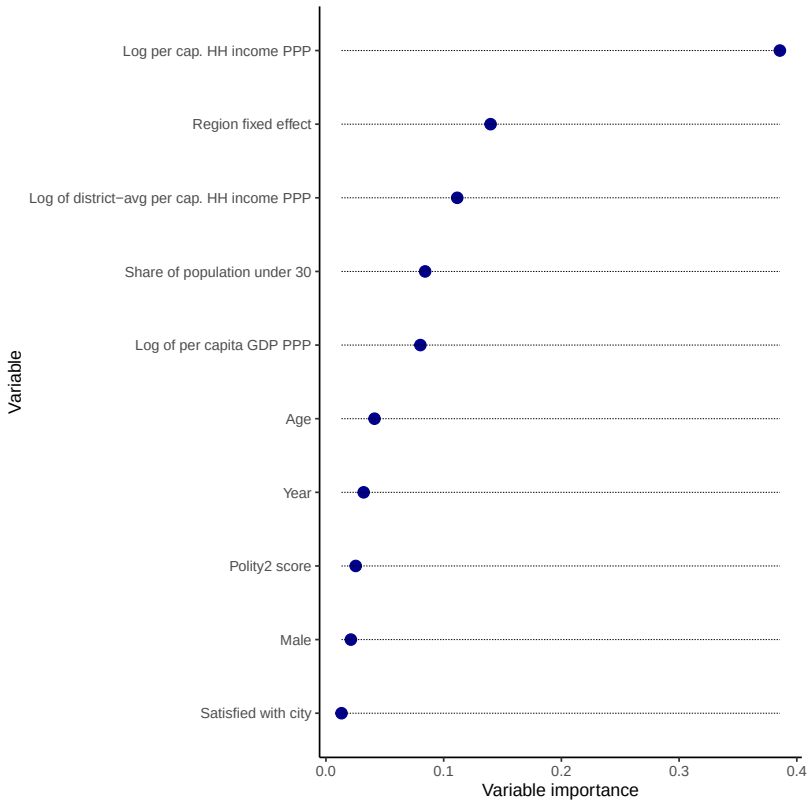
[48]To use the outcome variable with greatest coverage and simplify the analyses, we only conduct the causal forest analysis for the outcome variable "desire to migrate internationally."

[49]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, but this faces the drawback that we do not account for unobserved subnational region level heterogeneity in the treatment effect.

[50]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.

based on the covariates of all observations. Encouragingly, the arithmetic mean of the CATE (0.0253) 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 6.[51] We find that all income-related covariates are important in explaining the heterogeneity, but also the subnational-level fixed effect.

Figure 6: Variable Importance for Treatment Effect Heterogeneity of the Causal Forest Algorithm.



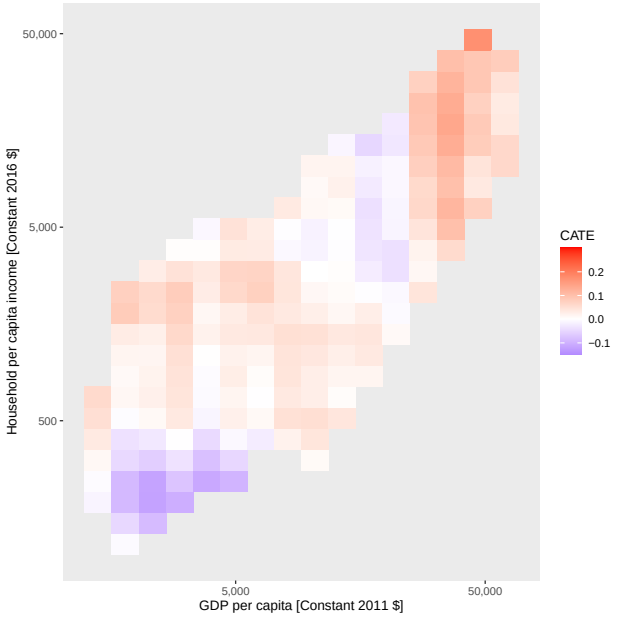
To consider in what way personal and regional income levels affect treatment effect heterogeneity, we consider the level of the CATE in Figure 7. We find a striking pattern, which

[51]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.

indicates strongest treatment effects for high-income individuals in high-income regions, and lowest effects for low-income individuals in low-income regions. Table 5 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 the quartiles of GDP per capita. These groups are based on the patterns in Figure 7. We find that treatment effects are positive and statistically significant for high-income countries and for higher-income households in lower-middle-income countries. The lower-middle-income countries (the second quartile of GDP) include large countries such as Egypt, India, Indonesia, Kenya, Nigeria, Philippines and Vietnam.[52]

Higher-income individuals in low-income countries may have the means to afford mobile internet access (compared to lower-income individuals in the same countries) and a large potential benefit from migration.

Figure 7: Heatmap of Conditional Average Treatment Effect (CATE) for 3G internet expansion on the desire to migrate over GDP per capita and personal per capita household income.



[52]For the full classification of countries in these four quartiles, see Appendix Table A16.

[52]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.

Table 5: Estimates of Heterogeneous Effects over Country and Household Per Capita Income Levels

	Median of per capita household income within GDP quartile	
Quartiles of GDP per capita on the country level	Below median	Above median
Lowest quartile	-0.022	0.031
	(0.061)	(0.048)
Second quartile	-0.024	0.131***
	(0.055)	(0.049)
Third quartile	-0.018	0.005
	(0.040)	(0.047)
Highest quartile	0.075*	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 quartiles 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). Every cell contains either N = 75,853 or 75,854 observations. 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 consider the reduction of migration and information costs by internet access. We assess this by considering how potential destinations change and whether the effect is driven by those who do not have close personal networks abroad. 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 Emigration and Networks Abroad

Does internet access change preferred destinations?

Internet access could reduce migration and information costs. However, these reductions are not equal for all destinations. 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 share 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 share of people desiring to migrate. Moving from no to full 3G coverage increases desired migration rates by 29%, on average. This estimate is larger than our baseline estimate, which suggests an increase of around 15%, 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

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

	(1)	(2)	(3)
	Desired bilateral emigration rate		
$3G_{ot}$	0.293***	0.895***	
	(0.062)	(0.176)	
$3G_{ot} \times \ln(stock_{od,2005} + 1)$		-0.055***	-0.078***
		(0.017)	(0.017)
Observations	48,003	48,003	48,003
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 number of people migrating on the the origin-destination-year level. $3G_{ot}$ is the $\ln(stock_{od,2005} + 1)$ is the log of the stock of migrants (plus one) in origin country o in destination d in 2005. The origin-year level controls include the unemployment rate, the population and the polity score in the origin country, and the ratio of GDP between the origin and destination country. We estimate the models in Columns $1 - 3$ using the Pseudo-Poisson Maximum Likelihood (PPML) estimator (see Silva and Tenreyro (2006)).

emigration is reduced for those dyads with a large prior stock of migrants. In Column 3, we include origin-year fixed effects to control for unobserved time-varying country-level 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 want to migrate. The reduction of costs associated with finding information about prospective destinations and actual migration likely mediate this effect.

Does internet access substitute for personal networks abroad?

In addition to the result that 3G increases the desire to migrate, Table 6 shows that bilateral desired migration rates have changed towards less connected destinations. A possible explanation for these findings is that those who do not have prior networks abroad are now able to obtain information abroad, which may be less directed towards countries hosting a diaspora. 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 large part of the desire to migrate and Manchin and Orazbayev (2018).

Table 7 shows that the effect of 3G on the desire to emigrate is strong for the group without any close personal network abroad, and insignificant for the group with such a network abroad. This suggests the internet may offer access to information that is similar to the information offered by personal networks.

7.2 Perceived Material Well-being, Trust in Institutions, and Life Satisfaction

To explore possible mechanisms, we consider the direct effect of 3G rollout on outcomes that may affect migration behavior. We use various indices as constructed by Gallup, supplemented with reported log household income, a constructed aggregate index of material prospects and the first principal component of trust in the government as constructed by

Table 7: 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	0.032**	0.046***	0.018
	(0.015)	(0.017)	(0.026)
Demographic controls	✓	✓	✓
Amenities, satisfaction, and income controls	✓	✓	✓
Country-level controls	✓	✓	✓
Observations	382,780	249,058	133,657
R^2	0.19	0.18	0.21
Average dependent variable	0.211	0.162	0.302

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 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.

Guriev, Melnikov and Zhuravskaya (2021).

Does internet access affect the sense of material well-being?

The first mechanism is related to the sense of material well-being. In particular, we test whether respondents' perceived economic and financial conditions change after accessing the mobile internet. To do so, we consider four outcome variables in Panel A of Table 8. 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 and household income (an objective measure of material well-being). 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 3G internet has a negative effect on the financial well-being index (measures respondents' personal economic situations and the economic situation of the community in which they live) but it is not statistically

Table 8: The effect of 3G on Various Gallup Items and Indices

Panel A: Material well-being				
	(1)	(2)	(3)	(4)
Dependent variable:	Household income (log)	Material prospects first principal component	Job climate index	Financial well-being index
3G	-0.019	-0.030**	-0.040**	-0.085
	(0.036)	(0.015)	(0.018)	(0.064)
Observations	606,765	559,762	603,796	170,857
R^2	0.26	0.28	0.19	0.25
Panel B: Institutional satisfaction				
	(1)	(2)	(3)	(4)
Dependent variable:	Law and order index	Corruption index	Community basics index	Trust in government first principal component
3G	0.014	-0.017	0.007	-0.038**
	(0.009)	(0.015)	(0.010)	(0.015)
Observations	606,144	579,294	606,765	477,395
R^2	0.19	0.22	0.26	0.23
Panel C: Life satisfaction and optimism				
	(1)	(2)	(3)	(4)
Dependent variable:	Optimism index	Daily experience index	Life evaluation index	Life purpose index
3G	-0.019	-0.004	0.001	-0.024
	(0.015)	(0.007)	(0.024)	(0.069)
Observations	606,583	605,354	570,187	170,663
R^2	0.24	0.13	0.24	0.22

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 – C 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. All dependent variables in this table 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 construction of the GWP indices, see https://www.oecd.org/sdd/43017172.pdf (Last accessed on 08-12-2021).

significant due to lack of power.

Overall, these results suggest that individuals' perceived material well-being decline after mobile internet penetration, while there is no effect on their household income.

Does internet access affect institutional satisfaction?

To investigate whether a fall in institutional satisfaction can also explain our results, we use a wide range of outcomes from Gallup, the results of which are reported in Panel B of Table 8. The outcome variables across the columns in the middle panel are as follows: "law and 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 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 affect on trust in government.

Does internet access affect views about life?

In the bottom panel of Table 8 we explore the impact of 3G internet 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.

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 can explain the relationship

between mobile internet access and 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 in- and outflows are recorded by person based on municipal registrations, including supplementary information such as country of origin[53]. Data is published for all municipalities with more than 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.

Using the Mobile Coverage Explorer and the GPW population density, we calculated the share of population covered by 3G in these municipalities. Although the maps are provided on a yearly basis, this does not mean that actual coverage is updated. The first time nonzero coverage is reported to the Mobile Coverage Explorer was December 2008.[54] 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 smaller municipalities 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.

[53]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=1254736177013&menu=resultados&idp=1254734710990#!tabs-1254736195469

[54]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)

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:

$$m_{dt} = \beta_1 3G_{d(t-1)} + \phi_d + \theta_t + \epsilon_{dt} \tag{7}$$

where m_{dt} is the emigration rate of Spanish-born individuals from muncipality d in year t. Our sample contains 657 municipalities, of which 29 have a population exceeding 200,000 in 2008. We restrict the sample to the years 2010 to 2020, as prior years have no information on 3G coverage.

Table 9 reports the estimation results of Equation 7. We find that a 100 percentage point increase in 3G coverage on the municipality level increases emigration by 0.008 percentage points. For these small municipalities, the average yearly emigration rate is about 0.09%, implying an increase in migration of about 10%.

Table 9: The effect of 3G Rollout on Emigration of Spanish-born individuals from Spain

	(1)	(2)
Dependent variable:	Emigration rate (× 100)	
Population in 2008:	≤200,000	> 200,000
3G Coverage$_{t-1}$	0.008**	0.012
	(0.0038)	(0.017)
Observations	6,908	319
R^2	0.83	0.94
Average emigration rate (× 100)	0.091	0.102
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 2020, 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 with a population of less than 200,000 in 2008, Column 2 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

Combining worldwide data on 3G internet rollout and global surveys over a decade allows us to estimate causally the effect of 3G internet expansion on desire and plans to emigrate. We show that increasing 3G internet coverage increases the desire and plans to emigrate. This effect is robust to a comprehensive set of specification tests and the use of alternative estimation methods.

Heterogeneity analysis suggests that the effects of 3G rollout vary widely depending on regional and personal income levels. Treatment effects for the desire to emigrate are particularly sizable for higher-income individuals in low-income countries, as well as for high-income individuals in high-income countries.

When it comes to mechanisms, we document that preferred destinations change in an important way: destination countries with lower stocks of migrants from a specific origin country become more popular destinations for prospective migrants from that country. This supports the intuitive idea that internet reduces the costs of migration (e.g. by making it easier to apply for a visa or to reduce the perceived distance from those left behind following migration) and the costs of acquiring information (e.g. on wages and living standards) about potential destinations; obtaining access to the internet is a substitute to an accommodating diaspora. This has the potential to change global migration patterns to be more dispersed than pre-existing migrant stocks. In addition, we find that access to the mobile internet led to a decrease in perceived material well-being and trust in government, which can explain the relationship between mobile internet access and desire to emigrate.

An important question on any survey data is to what extent reported plans translate into action. In our setting, this concern is alleviated by two additional results. First of all, there is a strong correlation between reported desire and plans to emigrate and actual migration flows. Second, we use annual data on emigration and 3G coverage from Spanish municipalities to study whether mobile internet access affects actual emigration. We find that increased municipal-level 3G coverage increases emigration from Spain. Our point estimate

suggests that moving from no 3G coverage to full coverage is followed by an increase of emigration of about 10 percent of the initial emigration rate.

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A Online Appendix

A.1 Additional Information on Outcome Variables, and Descriptive Characteristics

Construction of Outcome Variables from GWP

GWP contains multiple questions about desires, likelihoods, plansand preparations to migrate and a question 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 migrate internationally. Variable (2) refers to international migration 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 migrate. However, it is unlikely a respondent planning to migrate is unable to identify the intended destination country in the preceding question. A greater issue is posed by individuals planning to move to a feasible destination country. They might have identified another country in WP3120, which they only desire to migrate to. These individuals then would answer negatively to WP6880, as they do not plan to migrate to the country mentioned in WP3120. Therefore, for some individuals we might underestimate their plans to move when considering WP6880. However, within-country positive rates of WP10252 and WP6880 are comparable, suggesting that the questions are interpreted in a similar way. By combining WP10252 and WP6880, we are able to obtain a measure of plans to migrate 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 low 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 to their intended destination country 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. When it comes to satisfaction with amenities, more than half of the respondents report being satisfied with public transport, roads, education, healthcare and housing.

A.2 Robustness Checks

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

Do Districts Prior to Large Increases in 3G Coverage Display Pre-trends?

In Figure 5, we have no significant pre-trend between the controls and the not yet and never treated using the de Chaisemartin-D'Haultfœuille estimator. Additionally, one can assess the pre-trends prior to large increases in 3G coverage in an event study design. The difference between the event study and the de Chaisemartin-D'Haultfœuille estimator is twofold. First of all, in the former estimator, we focus on the less than 20% of the sample that lives in a district with a sharp expansion of 3G coverage. 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 it comes at the cost of observations. Secondly, in the latter estimator, the control group also contains never-treated units, whereas the event study contains only treated units and control units thus exist on not yet and already treated units. As never treated units may be on a different trend then not yet and already treated units, a test on the presence of pre-trends on the two estimators is complementary.[55]

In the event study, we focus on districts that experienced an increase of 50% in their 3G coverage between two subsequent years[56], and analyze how the desire to migrate develops with regard to this event, net of all baseline controls and fixed effects.

$$Outcome_{idt} = \sum_{l} \mu_l \mathbb{1}\{t_{id} - t_d^{'} - l\} + \alpha^{'}\mathbf{X_{idt}} + \phi_d + \theta_t + \epsilon_{idt} \tag{8}$$

The event study specification is shown in Equation 8. The year of interview is denoted by t_{id} and the first year after the rapid rise in 3G coverage is denoted by $t_d^{'}$. The binary variables $\mathbb{1}\{t_{id} - t_d^{'} - l\}$ indicate that an individual i in district d is interviewed l years after the first post-event period (or, if $l < 0$, before the event). The coefficients of interest are the μ_l on the pre- and post-event dummies. To prevent multicollinearity of the dummies, we omit the first pre-event period, which is a commonly made choice in the event study literature (Roth, 2019). Thus, the coefficients μ_l are interpreted as the difference in outcome between the lth period with regard to the first pre-event period.

[55] However, results of event studies in the presence of heterogeneous and dynamic treatments need to be assessed carefully, as discussed in the empirical strategy.

[56] A potential problem with the mobile network data is the possible reporting lag of coverage by the network providers. In an event study design this may be exacerbated, as areas with a reporting intermittence are more likely to be identified as treated areas. This might imply that included areas have already seen a substantial increase in 3G coverage before the recorded year of event.

Furthermore, we bin the endpoints in the event study to be able to restrict the number of pre- and post-event dummies to identify the model even in the absence of never treated units (Schmidheiny and Siegloch, 2019). We bin observations five or more periods after the event in one bin and the observations five or more periods before the event in another bin.

Appendix Figure A2 shows the results from a canonical event study design. The black line shows the event study estimates of 3G expansion after an event. As we focus on increases in population-averaged 3G coverage of 50 percentage points or higher, the 3G coverage rises by around 75 percentage points in the first post-treatment period and stays stable thereafter. Prior to the event, we see no evidence of pre-trend. The light blue line displays the event study estimates of the desire to migrate. 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 is 0.35.

Robustness to Omitted Variables Bias

Although we control for various observable characteristics and fixed effects, one still might be concerned as to whether our results are driven by omitted unobservable factors. To investigate this concern formally, we perform a rigorous robustness check following the method proposed by (Oster, 2019). The Oster's δ indicates the degree of selection on unobservables relative to observables that would be required to fully explain our results by omitted variable bias.[57]

We define R^2_{max} upper bound as 1.3 times the R^2 in specifications that control for observables following Oster (2019). At R^2_{max}, we find Oster's δ to be equal to 57.4, which is reassuring; given the wide range of controls we include in our models, it seems implausible that unobserved factors are 57.4 times more important than the observables included in our preferred specification.

Appendix Figure A3 also shows the Oster's δ as a function of R^2_{max}. Even at $R^2_{max} = 1$ (instead of 1.3), Oster's δ still equals 2.9, which makes it highly unlikely that our results can be explained by omitted variables bias.

Robustness to Controlling for 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 group, we show that other measures of regional development do not alter the main result. More specifically, we use the nighttime light density as an alternative measure of regional development in

[57]The rule of thumb to be able to argue that unobservables cannot fully explain the treatment effect is for Oster's δ to be over the value of one.

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 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 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 Leads as Treatments

By regressing the desire to migrate on leads in 3G coverage, we can assess whether future increases in 3G coverage predict previous changes in desire to migrate. If this is the case, the parallel trends assumption may be violated or treatment may be anticipated. 3G coverage displays strong autocorrelation at the district level, which may falsely render coefficients on lags and leads significant. This concern is alleviated in our case, as by including district-level time trends we capture the trend of 3G coverage, reducing the autocorrelation and total variation in the residual 3G coverage.

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

Falsification Exercise: Using 2G Expansion as a Treatment

[58]Please note that using the nth lag (lead) disregards the observations in the n earliest (last) years.

As the expansion of cellular 2G and 3G networks is strongly correlated because of the technologies' shared infrastructure, the found effect of 3G on the desire to emigrate may (partially) arise because of coinciding expansion of 2G and 3G 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 migrate through improved internet access and is not driven by an improved ability for mobile bilateral communication.

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 eliminated from our main sample in each iteration.

We repeat a similar analysis in Appendix Table A7, in which exclude drop one global region at a time in each estimation and again find that our estimates are not driven by a single global region. [59]

Robustness to Excluding Top 10 Refugee-origin Countries and High- and Low Migration Desire Districts

In order to alleviate concerns that the found results are driven by few countries in distress, we omit the 10 countries of origin with the most refugees.[60] Additionally, we omit countries where a large ($\geq$40%) proportion of GWP respondents desires to migrate and those where a small ($\leq$10%) proportion desires to migrate. Appendix Table A8 reports the baseline results for these three omissions. The coefficient on 3G is robust to omission of these country groups.

Measurement and Potential Reporting 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

[59] The global regions are mutually exclusive. MENA stands for the Middle East and North Africa. Turkey and Israel are included in MENA. Oceania (in our sample this only covers New Zealand) is included in Asia.

[60] 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

concerns about such measurement error 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 that have a more than 20% population-averaged coverage of 3G in the first year that an operator in that country reports nonzero 3G coverage. 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 2005:

  Countries that have at least four times as much 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]

- Districts that report report sharp decreases (defined as a drop of 10 percentage points) in 3G coverage. It is unlikely that coverage drops sharply within one year. This may be the artefact of a reporting error or a network operating only a part of the year reported.[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

One of our key identifying assumptions is that the 3G expansion is exogenous to socio-demographic characteristics of the local population. If this is the case, our treatment variable should be uncorrelated with respondents' observable demographic characteristics. To verify the validity of this argument, we provide a direct evidence 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. Overall, the results presented in Appendix Table A10 show that the 3G expansion is a plausibly exogenous process.

[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, Senegal, Thailand, Venezuela, Trinidad and Tobago, Costa Rica, El Salvador, India, Mozambique, Kyrgyzstan, Namibia, Nepal, Nigeria and Oman.

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

Multiple Hypothesis Testing

We also conducted multiple hypothesis testing by employing 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), taking into account the multiplicity of the hypothesis testing procedure. The method builds on repeatedly randomizing the treatment variable in each estimation under the null hypothesis that the treatment effect is 0 for all observations, and comparing the pool of randomized estimates to the estimates derived via the true treatment variable. Based on 500 iterations, the results presented in Appendix Table A11 show that our findings remain robust, both for the individual coefficients and the joint tests of treatment significance. The null hypothesis of the Westfall-Young test for irrelevance for the 3G treatment in all three regressions is rejected with a p-value of 0.034.

Robustness to Alternative Levels of Clustering

In our main specification, we cluster the standard errors in two ways: at the district level (2209 groups) and at country-year level (791 groups). We establish robustness of our results using alternative assumptions about the variance-covariance matrix: the results are robust to 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).

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 heterogenous, 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 characteristic of the respondent and of the country of residence.[64]

[64]GWP supplies a within-country weight variable based on unequal inverse selection 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

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 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 migration aspirations driven by unobserved drifts on the district level. However, to show that our results do not critically depend on 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).

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.

A.3 Alternative Estimator

In this section we discuss the use of the De Chaisemartin-D'Haultfœuille estimator as an alternative for a TWFE regression using a continuous treatment variable.

dCDH Estimator for a Binary Treatment

In the staggered adoption case with binary treatment, DiD_l is an estimator comprising a weighted average of $DiD_{t,l}^{ini=0}$, which is the difference (between first-treated units and not yet treated units) in differences (over the length of l periods after being treated) of those units first treated at $t-l$ and being untreated ($ini = 0$) prior to that. As it uses only clean controls (meaning that they have never been treated at or before t), this estimator is robust to treatment effect heterogeneity and dynamic effects.[65] Although this estimator is robust to those, however, for identification of a causal effect we still have to rely on a common trends assumption, which can be assessed using the placebo estimators.[66]

The estimators are averaging outcomes and covariates on the unit-year level. One can modify the estimator to allow for the inclusion of relevant covariates. [67] 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

However, the population-averaged 3G coverage differs from a treatment that is adopted in a staggered fashion across groups, as it is a non-binary treatment that increases gradually over time.[68] Nevertheless, we can still apply the principle of units switching into treatment

[65]Importantly, to calculate the DiD_l using all available groups, one needs a treatment variable that is balanced on the unit level, as knowledge of a unit'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. In 2008 and 2009 only 600 districts are surveyed, compared to, on average, 1,600 in the later years. Around 200 districts have gaps in the sample.

[66]The placebo estimators DiD_l^{pl} calculate the difference-in-differences between the treatment and control units between $l+2$ periods before and one period before the treated unit is treated for the first time. These estimators are important assessments of differential pre-trends between treatment and control units prior to first treatment.

[67]Covariate adjustment of the elementary building blocks $DiD_{t,l}^{ini=0}$ is performed in two steps: (1) OLS regression of the first differences in outcome on the first differences in covariates on the sample of all never treated and treated groups prior to first treatment, and (2) residualizing the lth temporal difference in outcome using the coefficients of step (1) multiplied by the lth temporal difference in covariates. The $DiD_{t,l}^{ini=0}$ are then the differences between treatment and control in the difference over relative time l unexplained by the covariates. This has implications for the feasibility of the estimator as there may be fewer observations in the regression then there are covariates in step (1).

[68]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 221 out of 2,105 districts in the main sample the coverage decreases by more than 3% of population. Therefore, we omit the discussion related to designs in which treatment can decrease here, although the estimators are robust to this.

for the first time to identify difference-in-differences between treatment and clean controls. The elementary building block is now differentiated over initial treatment status ini and we calculate the $DiD_{t,l}^{ini}$ within this group ini. As 3G coverage is continuous, we should 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.[69] When those bins become wider, treatment and control groups with fairly different initial levels of treatment are compared. In order to estimate the DiD_l^{ini} in an unbiased way, we have to assume that the treatment effects between the binned treatments are not varying over time.[70] Furthermore, 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 less than some threshold Δ_{3G}. Without this adjustment, for some initial treatment levels ini, it is impossible to find control groups, as all of the groups are treated during the time span studied. As this biases the control group somewhat towards the treatment group, this is a conservative adjustment. However, if Δ_{3G} is too large, some levels of ini may not have a single switching group 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 $ini \neq 0$ 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 τ 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 Δ_{3G} is given in the time series for $C^{ini \neq 0}$.

As with the 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 than that of the 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

[69] Except for those districts with $ini = 0$, that approximately 40% of our sample.

[70] If this is not the case, the counterfactual of remaining in treatment ini is not exactly the counterfactual treatment of staying in ini' and the elementary building block $DiD_{t,l}^{ini}$ is biased through its control term (in symbols for all l: $Y_t^{ini} - Y_{t-l-1}^{ini} = Y_t^{ini'} - Y_{t-l-1}^{ini'}$ only holds if $TE_t^{ini \to ini'} = Y_t^{ini} - Y_t^{ini'} = Y_{t-l-1}^{ini} - Y_{t-l-1}^{ini'} = TE_{t-l-1}^{ini \to ini'}$). This bias is plausibly greater for (1) larger l, as treatment effects likely vary slowly as well as for (2) larger bins, such that the treatment effect $TE^{ini \to ini'}$ between ini and ini' is larger. This issue is mitigated if there is a balance in the various binned levels and their first treated period. In the case of binning two initial levels, this implies that, if we use groups with ini as controls for first switchers from ini' as often (weighted with the number of observations) as ini' for first switches from ini, the two contributions cancel out, and $DiD_{t,l}^{ini,ini'}$ is unbiased. As the (adoption 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.

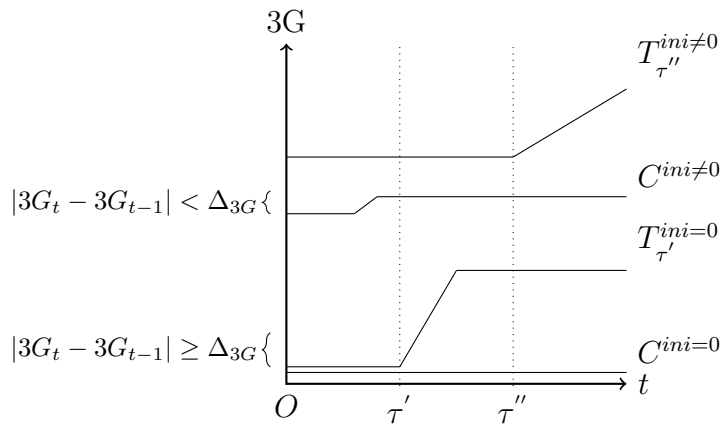


Figure A1: Relevant Treatment and Control groups for the de Chaisemartin and D'Haultfœuille Estimator

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:

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

$\hat{\delta}^L$ is the treatment effect per unit of treatment which be 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 lth 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, δ^L denotes the ATT.[71]

[71] However, without further assumptions on the absence of interactions between subsequent treatments or the heterogeneity of treatment effects, it is impossible to estimate the true dynamic effects, contrary to the case of staggered adoption.

Figures

Figure A2: Event study estimates around treatment of 50 percentage point increase in 3G in a year with a 95% confidence interval. The black (blue) line depicts the event study estimates with 3G coverage (desire to migrate) as dependent variable. The omitted pre-event dummy is the last period before treatment. Endpoints are binned for five periods before treatment and earlier, and for five periods after treatment and later. All units that experience a decrease of more than 10 percentage points between 2008 and 2018 are omitted, to prevent inclusion of previously treated regions in the event study and its effect through dynamic effects.

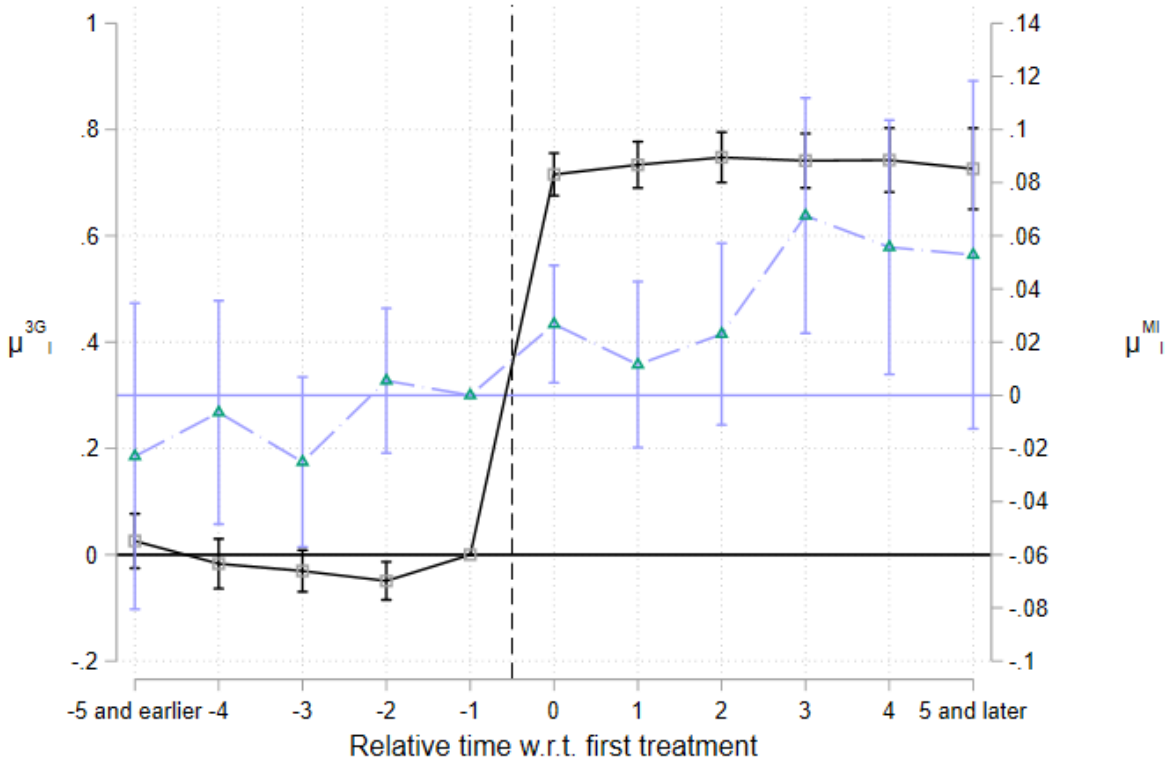
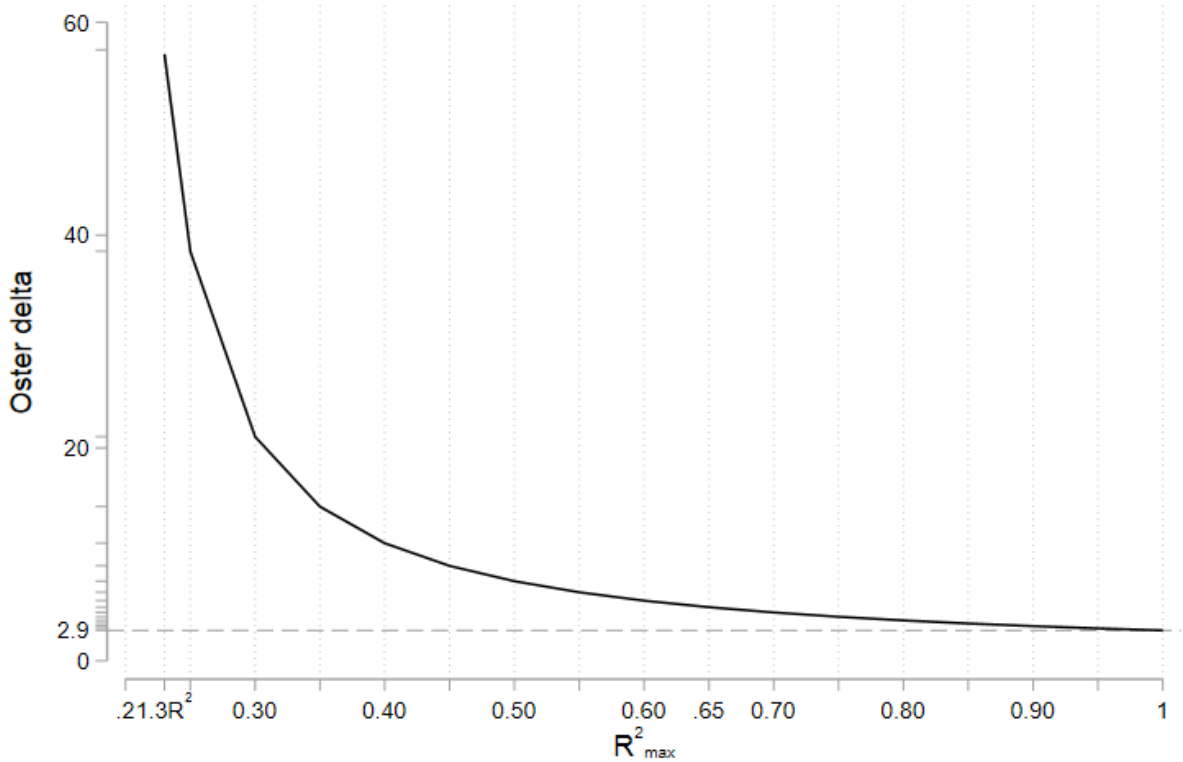


Figure A3: Oster's δ for increasing values of maximally admissible R^2_{max}. Oster's δ is equal to 57.4 for the R^2_{max}=1.3R^2.



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	WP2130 if question (2) answered positively (2008 – 2009) and WP10253 (2010 – 2015)	(2008 – 2018)
Panel C			
(3): Preparation to emigrate	WP9455	Have you done any preparation for this move (asked only of those who are planning to move to another country in the next 12 months)	(2009 – 2015)
(3C)	WP10253	WP10253 if $MigPrepI$ answered positively	(2009 – 2015)
Panel D			
(4): 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	606,827	GWP	Individual
Plan to emigrate	0.03	0.16	376,801	GWP	individual
Preparation to emigrate	0.01	0.16	317,520	GWP	individual
Likely to move	0.17	0.37	544,022	GWP	Individual
Regional 3G coverage	0.37	0.39	606,827	Collins Bartholomew	District-Year
Regional 2G coverage	0.77	0.30	606,827	Collins Bartholomew	District-Year
Male	0.46	0.50	606,827	GWP	Individual
Age	40.10	17.02	606,827	GWP	Individual
Urban	0.39	0.49	606,827	GWP	Individual
Partner	0.58	0.49	606,827	GWP	Individual
Separated/divorced	0.06	0.24	606,827	GWP	Individual
Presence of children	0.56	0.50	606,827	GWP	Individual
Secondary education	0.53	0.50	606,827	GWP	Individual
Tertiary education	0.15	0.36	606,827	GWP	Individual
Born in country of interview	0.96	0.19	606,827	GWP	Individual
Log of per capita income	7.74	1.51	606,827	GWP	Individual
Log of district per capita income	8.15	1.15	606,827	GWP	District-Year
Life satisfaction	0.46	0.50	606,827	GWP	Individual
Can count on friends/relatives	0.82	0.39	606,827	GWP	Individual
Satisfied with living standard	0.62	0.48	606,827	GWP	Individual
Living standard is getting better	0.46	0.50	606,827	GWP	Individual
Lack of money for food	0.35	0.48	606,827	GWP	Individual
Lack of money for shelter	0.25	0.43	606,827	GWP	Individual
Satisfied with the city	0.78	0.41	606,827	GWP	Individual
Satisfied with public transport	0.62	0.49	606,827	GWP	Individual
Satisfied with roads	0.55	0.50	606,827	GWP	Individual
Satisfied with education	0.68	0.47	606,827	GWP	Individual
Satisfied with healthcare	0.58	0.49	606,827	GWP	Individual
Satisfied with housing	0.52	0.50	606,827	GWP	Individual
Had money or property stolen	0.16	0.37	606,827	GWP	Individual
Log of GDP per capita	8.44	1.40	606,827	World Bank	Country-Year
Polity 2	5.44	5.01	606,827	Center for Systemic Peace	Country-Year
Share of respondents below 30	0.32	0.13	606,827	GWP	Country-Year

Table A3: The effects of 3G expansion on Access to the Internet

Outcome:	(1) Internet	(2) Access
3G	0.047***	0.049***
	(0.012)	(0.011)
Baseline controls, FEs and district-level time trend	✓	✓
Broadband subscription rate		✓
Observations	636,516	627,815
R^2	0.52	0.52
Average dependent variable	0.432	0.432

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: 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***	0.029**	0.030***	0.027**	0.029**	0.032***	0.033***	0.029**
	(0.011)	(0.011)	(0.012)	(0.012)	(0.011)	(0.012)	(0.013)	(0.014)
Nightlight luminosity	-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	0.003	0.003	0.004	0.005	0.004
			(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Demographic controls	✓	✓	✓	✓	✓	✓	✓	✓
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	606,827	606,827	606,827	606,827	606,827	571,023	557,787	464,497
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 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 Migration Aspirations

	(1)	(2)	(3)	(4)	(5)	(6)
Outcome:			Desire to emigrate			
2G	0.019					
	(0.014)					
$3G_{t+2}$		-0.000				
		(0.015)				
$3G_{t+1}$			0.010			
			(0.013)			
3G				0.029**		
				(0.011)		
$3G_{t-1}$					0.001	
					(0.012)	
$3G_{t-2}$						0.017
						(0.013)
Baseline controls, FEs and district-level trend	✓	✓	✓	✓	✓	✓
Observations	606,827	473,835	548,274	606,827	581,510	551,109
R^2	0.19	0.18	0.19	0.19	0.19	0.19
Average dependent variable	0.214	0.206	0.214	0.214	0.215	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	0.031***	0.025**	0.020*	0.043***	0.027**	0.033***	0.033***	0.034***	0.030**	0.033***	0.018
	(0.012)	(0.012)	(0.012)	(0.011)	(0.012)	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)	(0.012)
Observations	581,510	576,426	556,199	541,771	548,211	556,388	551,973	537,957	537,173	532,388	548,274
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	0.224	0.224	0.225	0.224	0.226	0.223	0.223	0.224	0.221	0.220	0.218

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)
Outcome:	Desire to emigrate					
Global region omitted:	Europe	Former USSR	Asia	The Americas	MENA	Sub-Saharan Africa
3G	0.023*	0.029**	0.033**	0.035***	0.029***	0.021*
	(0.012)	(0.013)	(0.014)	(0.012)	(0.011)	(0.013)
Observations	498,708	529,027	471,313	508,658	572,037	454,392
R^2	0.19	0.19	0.19	0.19	0.19	0.17
Average dependent variable	0.232	0.227	0.251	0.214	0.223	0.188

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

	(1)	(2)	(3)
Outcome:	Desire to emigrate		
Excluding countries:	Top 10 refugee	≥40% desire to emigrate	≤10% desire to emigrate
3G	0.028**	0.026**	0.036***
	(0.011)	(0.012)	(0.013)
Observations	588,449	554,462	516,011
R^2	0.19	0.16	0.17
Average dependent variable	0.218	0.196	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)
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)
Baseline controls	✓
District and year FE	✓
District-level time trend	✓
N	606,827
R2	0.933

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 A11: Robustness to Randomization Inference and Multiple Hypothesis Testing

Outcome:	(1) Desire to emigrate	(2) Plans to emigrate	(3) Likelihood to migrate	(4) Joint test of irrelevance
3G	0.028**	0.008**	0.027*	
Young(2019) p-value	(0.012)	(0.018)	(0.092)	(0.034)

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Young (2019) randomization inference p-values in parentheses, based on 500 bootstrap replications. See notes to Table 1 for details on control variables.

Table A12: Robustness to Alternative Variance-Covariance Matrix Structure

Outcome:	(1) Desire to emigrate	(2)
3G	0.029***	0.029**
	(0.001)	(0.050)
Baseline controls, FEs and district time trend	✓	✓
Observations	606,827	606,827
R^2	0.19	0.19
Level of clustering	Country-Education-Gender	Country
Number of clusters	658	110

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

Outcome:	(1) Desire to emigrate	(2)
3G	0.055***	0.069***
	(0.02)	(0.02)
Baseline controls, FEs and district time trend	✓	✓
Observations	240,283	222,617
R^2	0.16	0.17
Average dependent variable	0.189	0.192
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 A14: Robustness to Alternative Choices of Weighting Observations

	(1)	(2)	(3)
Outcome:		Desire to emigrate	
3G	0.034***	0.029**	0.041***
	(0.011)	(0.011)	(0.013)
Observations	606,827	606,827	606,827
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	0.029**	0.019**	0.035***
	(0.011)	(0.010)	(0.013)
Baseline controls, FEs and district time trend	✓	✓	✓
Observations	606,827	606,827	606,827
R^2	0.19	0.18	0.20
Average dependent variable	0.222	0.222	0.222
Trends	District-level, linear (baseline)	none	District-level, linear and quadratic

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 A16: Questions in GWP relating to respondents' aspirations and intentions to migrate

Quartile	Country
Lowest	Afghanistan, Benin, Burkina Faso, Cambodia, Cameroon, Chad, Congo Brazzaville, Congo Kinshasa, Guinea, Haiti, Honduras, Kenya, Kyrgyzstan, Laos, Lesotho, Liberia, Madagascar, Malawi, Mozambique, Nepal, Niger, Rwanda, Senegal, Sierra Leone, Sudan, Tajikistan, Tanzania, Uganda, Zambia, Zimbabwe
Lower middle	Armenia, Bhutan, Ecuador, Egypt, El Salvador, Georgia, Ghana, Guatemala, Honduras, India, Indonesia, Jordan, Kenya, Kyrgyzstan, Laos, Moldova, Mongolia, Namibia, Nicaragua, Nigeria, Philippines, Sri Lanka, Sudan, Tunisia, Uzbekistan, Vietnam
Higher middle	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
Highest	Austria, Belgium, Cyprus, Croatia, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Israel, Italy, Japan, Lithuania, Luxembourg, Malaysia, Netherlands, New Zealand, Norway, Poland, Portugal, Russia, Saudi Arabia, Singapore, Slovak Republic, Slovenia, South Korea, Spain, Sweden, Switzerland, Trinidad and Tobago, Turkey, United Kingdom, United States