Mobile Internet Access and Desire to Migrate*†

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Abstract

How does internet access affect migration intentions and behavior? To answer to this question, we combine a unique survey data set from 120 countries comprising more than 600,000 individuals with data on worldwide 3G mobile internet roll-out spanning from 2008 to 2018. We find a positive effect on plans to migrate (both in general and internationally) within 12 months and the preparation to migrate. We furthermore argue that pre-trends in migration intentions prior to expansion of 3G networks are unlikely to be present. Importantly, no statistically significant effects of 2G coverage have been found, excluding communication as a possible mechanism, allowing for internet access and use-related explanations.

Keywords: Migration intentions; Internet access; 3G

JEL Codes: F22

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1 Introduction

The internet and mobile phones have changed how people live, work, connect, and exchange information. The global number of internet users increased from 0.41 billion 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 positively affects employment in Africa. Falck, Gold and Heblich (2014) find a negative effect of broadband internet availability on voter turnout in Germany. Guriev, Melnikov and Zhuravskaya (2020) establish that 3G expansion 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 broadband mobile (3G) internet roll-out causally affects desire and plans to emigrate. Previous research has already established that desire and plans to emigrate are strongly linked to subsequent actual migration flows (Tjaden, Auer and Laczko, 2019).

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.[2] Combining these allows us to use data from 600,000 individuals living in 2200 sub-regions and 120 countries, collected over 11 years. To derive causal effects on desire and plans to emigrate, we exploit variation in within-sub-national district 3G mobile internet penetration over time. We control for Two-Way (sub-national district and year) Fixed Effects (TWFE), linear district-level time trends, as well as various individual,

[1] More households in developing countries own a mobile phone than have access to electricity or clean water, and nearly 70 percent of the poorest quintile of the population in developing countries own a mobile phone (World Bank, 2016).

[2] We use three different measures of migration intentions and plans: (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.

district, and country-level characteristics. This implies that the estimates are identified by exploiting within-district variation in 3G coverage that has been stripped of any influence of constant and linearly changing district-level characteristics.

We find that 3G coverage has a sizable impact on desire and plans to emigrate: a 10 percentage points increase in 3G mobile coverage leads to a 0.29 percentage points increase in the desire to emigrate permanently and a 0.09 percentage points increase in the plans to emigrate permanently during the next 12 months.[3] The effect is about 13 (32) percent of the baseline average of desire (plans) to emigrate permanently.

Although our main econometric specification controls for regional fixed effects at the sub-national 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 desire to emigrate. Second, we use the alternative estimator by de Chaisemartin and D'Haultfœuille (2020). It enables us to assess pre-trends on a larger part 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 two-way fixed effects approach[4]. 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 as 3G roll-out could be potentially faster in swifter developing sub-national regions. Fourth, following the method proposed by Oster (2019), we show that our results are unlikely to be driven by the unobserved variation potentially related to omitted factors. Fifth, we find qualitatively similar results when we employ an instrumental variables strategy based on Guriev, Melnikov and Zhuravskaya (2020), which exploits exogenous variation in the regional frequency of lightning strikes to

[3]These estimates arise when weighting our observations using the within-country weights as provided by Gallup. Importantly, the estimated effects are larger 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.

[4]As pointed out by (Borusyak and Jaravel, 2018; de Chaisemartin and D'Haultfœuille, 2019; Abraham and Sun, 2018; Goodman-Bacon, 2018), among others.

predict the speed of the expansion of regional mobile broadband internet coverage.

Finally, our estimates are robust across a variety of specification checks, like excluding potentially bad controls, accounting for multiple hypothesis testing, ruling out the importance of influential observations by dropping one survey year or global region at a time, using more conservative assumptions about variance-covariance matrix by clustering standard errors on the country-level, and considering balanced countries and districts only.

The remainder of the paper is structured as follows. Section 2 reviews related literature and elaborates 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 and section 7 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 earnings potential in the destination country shapes migration behavior. However, Dustmann and Okatenko (2014) show that the relationship between the intention to move (both domestic and international) 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 relation 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 one of the most important driving forces of international migration intentions. Adsera and Pytlikova (2015) examine the importance of language in international migration using a dataset on immigration flows and stocks in 30 OECD destinations from all countries in the world. They find that migration rates increase with linguistic

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

proximity. (Ruyssen and Salomone, 2018) used the Gallup World Polls to study how intentions and preparations to migrate are affected by perceptions of gender discrimination of women. We contribute to this literature by providing new *causal* evidence on the impact of internet access on migration intentions and by identifying the underlying mechanisms at play. To the best of our knowledge, only one paper has explored the global relationship between the internet and migration-related outcomes. Pesando et al. (2021) provide descriptive evidence using data on migration intentions from Gallup World Polls and Arab Barometer and data on actual migration from the Italian Statistical Institute as well as Sant'Anna Cara reception center. Across both levels of analysis, the authors find a positive *association* between internet access (measured as a percentage of population using the internet) and both willingness to emigrate and actual migration decisions.

Third, we build on the recent literature on the impact of mobile internet technologies on economic and political behavior. In the most closely related study, Guriev, Melnikov and Zhuravskaya (2020) analyze political implications of 3G internet roll-out using Gallup World Polls (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. Manacorda and Tesei (2020) use granular dataset for the entire African continent on the 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. We complement these studies by showing how 3G internet access also affects a non-political outcome — international migration intentions and plans.[6]

[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 positively affects 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 had a substantial negative effect on turnout in parliamentary elections in Italy until 2008 but this pattern was reversed afterwards.

Our main contribution is to the literature on the determinants of migration intentions, using the methodological advances from Guriev, Melnikov and Zhuravskaya (2020). Our data and empirical setting provide some unique advantages that allow us to provide new evidence on desire and plans to migrate in several dimensions. First, we use a granular (1-by-1 kilometer grid level) data on *mobile* 3G network coverage to calculate population-averaged coverage on the sub-national region, which means that our treatment variable is much less noisy (compared to the country-level *share of population with internet access* measure). The mobile internet is also more relevant with respect 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 migration intentions, we provide *causal* evidence using two alternative empirical strategies.

3 Theoretical Framework

There are two countries, denoted by 0 and 1. We analyze decisions of residents of country 0 on whether to invest in acquiring information on opportunities abroad and whether to migrate to country 1 if being 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 case of being mobile. Vector $\boldsymbol{x_j}$ has a constant term 1 that can be used to capture wage and 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, but also factors like gender and 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 has also 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 Gallup World Polls has no questions regarding on how many destinations respondents have gathered information. The information costs could be related to issues like 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 includes also 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 3G network eases 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, and that the probability of being mobile is θ, $0 < \theta < 1$. Individual mobility depends on both external constraints, like immigration rules in the destination, and on psychological and social components, like the effect of family members. It is individually optimal to invest in information acquisition if

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

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:

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

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

Proof 1 *Follows by differentiating 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 small. 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 also assume that $\beta^1_{3G,0} = 0$, implying that 3G coverage in the origin region has no effect on wages in the destination region. 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 information acquisition can migrate. Therefore, there is a linear link between desire to migrate and migration plans, giving as 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 a caveat that the 3G coverage should not boost local wages substantially. In the empirical analysis, we estimate the net effect of 3G coverage, and if this is positive then it already implies that the effect boosting local wages should not be very strong. A negative effect, instead, would suggest as a potential explanation within the model that the 3G coverage may have boosted local wages. Furthermore, we later analyze directly how 3G coverage is related to subsequent wages.

4 Data and Descriptive Statistics

The main data used in this paper come from the Gallup World Polls 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 intentions and plans come from the 2008-2018 Gallup World Polls (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 outcome variables of interest come from questions asked to Gallup respondents about their (international) migration desires, plans, preparations and likelihood.[8] The outcomes of interest, their time span, the wording of the underlying question and the answer possibilities are:[9]

1. Migration Desire (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*

[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 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 probing the same type of migration intention 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, question (2) and (3) are asked only during a specific time span and when the respondent answered positively to (1). Thus, (2) and (3) are imputed 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.

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, or not?* (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 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 of the fact that the frictions and hurdles preventing its translation into actual migration could be pervasive and hard to reduce (for detailed discussion, see Docquier, Ozden and Peri (2014)).

Questions (2), (3), and (4) reveal more concrete intentions and arrangements that people 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 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

[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 of 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 2000 origin-destination pairs. This suggest that plans and preparations 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.

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 percent) is substantially lower than the share of those who reported having migration intentions (22 percent).

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 dataset on origin-destination-level migration intention and plan rates. We then combine these data with yearly actual flow rates to examine whether our outcomes convey meaningful information (see Section 4.2 for a detailed discussion).[12]

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.

Additionally, the GWP respondents are asked about their access to the internet. We use the following questions:

a. *Does your home have access to the Internet? (2008-2015)*

b. *Do you have access to the Internet in any way, whether on a mobile phone, a computer, or some other device? (2016-2018)*

c. *Have you used the Internet in the past seven days, whether on a mobile phone, a computer, or some other device? (2016-2018)*

We combine a) and b) to measure individuals' access to the internet. We use these questions to check the internal validity of our treatment variable (that is, 3G coverage at the

[12]The fraction of individuals answering positively on (1) but not mentioning a destination country is less than 7 percent. Similarly, less than 4 percent 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.

sub-national region level).[13]

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 night-time light data (explained below). Furthermore, to control for the age structure of the country, we compute the share of respondents 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 come from the Collins Bartholomew.[14] The data provide information on signal coverage at 1-by-1 kilometer grid level, as submitted by network operators to the GSM Association. That is, we know whether a given 1-by-1 kilometer grid has a 2G or 3G signal or not. However, we don't observe any information about the strength of the signal. The network coverage data is available on the yearly level, but the timing of data collection differs. Between 2011 and 2017 data is provided for the month December, whereas in 2007, 2008 and 2009 it is provided in the first quarter of the year.[15] We use the reported coverage in year $t-1$ to represent the network coverage in year t.[16]

To calculate the share of population that is covered by the 2G and 3G, we use 1-by-1 kilometer population data from the Gridded Population of the World (GPW) for 2015, which is distributed by the Center for International Earth Science Information Network.[17] We first

[13] In 2008, around 28 percent of world population had access to the internet and in 2018 this was about 50 percent. The usage rate in the last seven days, conditional upon access, is about 85 percent between 2016 and 2018. In Sub-Saharan Africa this figure is lower than in any other global region: around 75 percent of those with access to the internet have used it in the last seven days.

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

[15] Due to the change in data provider, 2010 data are missing. We overcome this challenge by linearly interpolating the missing information using the data from 2010 and 2012.

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

[17] Since 2012, 4G network coverage is recorded as well. 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

calculate each grid point's population coverage and then aggregate this information over the sub-national regions as provided in the GWP. The constructed population-weighted coverage of 3G networks is our main treatment variable.[18]

WWLLN Lightning Incidents Data

We obtain global data on geo-coded lightning strikes from the World Wide Lightning Location Network (WWLLN).[19] In particular, we use these data to construct an instrumental variable following Manacorda and Tesei (2020) and Guriev, Melnikov and Zhuravskaya (2020). The intuition is that cloud-to-ground (CG) lightning is likely to damage 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 a 1-by-1 kilometer grid) and calculate the intensity per square kilometer at the sub-national level.

Country Level Variables and the Other Datasets

- 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 DMSP-OLS and VIIRS.[20] 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 on the district level. As the nighttime light density data come from different sources (and thus are not directly

is covered by 4G and not by 3G, which is not likely to bias our results.

[18]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 sub-national districts (i.e. states) in GWP are too large for calculating meaningful 3G coverage of the GWP respondents

[19]The WWLLN network detects lightning not through optical, but Very Low Frequency (VLF) signals, which has the advantage of carrying longer than optical signals and thus requiring less detectors.

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

comparable), we normalize each value to a 0-1 range within each year.

- OECD: To compare bilateral migration intention and migration plan rates with actual migration flows, we obtain migration flow data between 2007 and 2017 (from more than 200 origin countries to 47 OECD countries). In particular, we use the inflows of foreign population by nationality.

- The World Bank: To control for country-level development, we obtain real PPP GDP per capita per year, expressed in constant 2011 U.S. 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 dataset. Polity score ranges from -10 to +10, with -10 to -6 corresponding to autocracies, -5 to 5 corresponding to anocracies, and 6 to 10 to democracies.[21]

4.2 Evidence that Our Treatment and Outcome Variables Convey Meaningful Information

An issue that is key to the interpretation of our results is whether our treatment variable (3G) and outcome variables (migration intentions and plans) 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. Appendix Table A3 shows that a one percentage point increase in district-level 3G coverage leads to a statistically significant 4.9 percentage points increase in the likelihood of having access to the internet — this effect is about 18 percent of the baseline average (in 2008, 28 percent of respondents reported to have access to the internet).

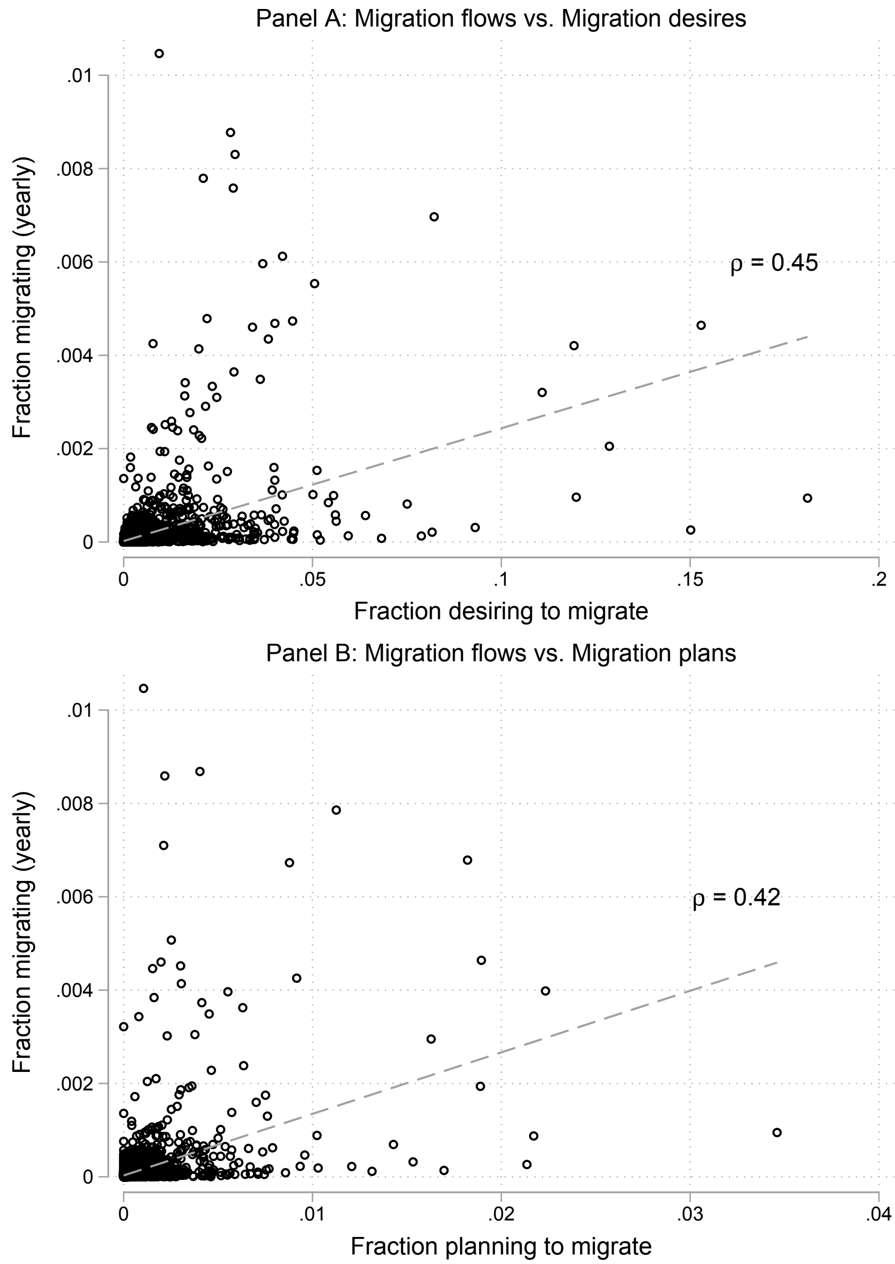
[21] For more details on the Polity IV project, see: https://www.systemicpeace.org/polityproject.html

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 intention and planning rates between origin and destination countries.[22] We then match our *intended* migration-flow matrix with data on actual migration flows to OECD countries between 2007 and 2018.[23]

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

[23]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 *intention* to migrate. As ideal and realistic destination countries may differ, we omit the data from 2008 and 2009.

Figure 1: Scatter plot of yearly migration flow rate versus 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 1 confirm that our outcome variables are strongly associated with the official migrant flows 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 fraction of respondents planning to migrate from a specific origin to a specific destination is 0.42.[24] 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 in cross-border movements of people.

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

5 Empirical Strategy

In this section we describe the three complementary estimation strategies (difference-in-differences, de Chaisemartin and D'Haultfœuille estimator, and instrumental variables) we use in our paper.

Main Estimation Model

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

$$Outcome_{idt} = \beta_1 3G_{dt} + \alpha^{'} \mathbf{X_{idt}} + \phi_d + \theta_t + \gamma_d \cdot t + \epsilon_{idt} \tag{5}$$

where i indexes the individual, d the sub-national district, and t the year.

We use outcomes (1), (2), and (4) from Section 4 for individuals' migration intention and plans: (1) whether individual would like to move permanently to another country; (2) whether individual is planning to move abroad permanently in the next 12 months; and (4) whether individual is likely to move away from the city or area he or she lives in during the next 12 months. Responses to all three questions are coded as dummy variables, with one representing a positive answer and zero representing a negative answer. We estimate linear probability models for ease of interpretation.

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

To measure 3G internet coverage, our treatment variable, we follow (Guriev, Melnikov and Zhuravskaya, 2020) and calculate the share of the district's territory covered by 3G networks in a given year, weighted by population density at each 1-by-1 kilometer grid-level.[25]

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 (whether the respondent is satisfied with their life, experienced a lack of money for shelter, satisfied with the environment, public transport, roads, education, healthcare, and housing, whether the respondent had money of property stolen recently, and whether the respondent can count on friends or relatives);
- 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 check for potential "bad controls" (Angrist and Pischke, 2008) by adding these characteristics gradually. Doing so does barely change the point estimates for our variables of interest. Nevertheless, we keep these controls in our main specification to alleviate concerns related to omitted variable bias.[26]

[25] 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 a further discussion about the 3G data and its timing, see Section 4.

[26] 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.

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 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 sub-national 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 to 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 migration intentions prior to and not prior to an increase in 3G internet coverage. We provide evidence in an event-study framework by constructing of an indicator variable for large increases (at least 50 percentage points in one year) in 3G coverage [27]. 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 intentions and plans.

[27] We focus on the sub-national districts with a large increase in 3G coverage, although this constitutes 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. In the first period treated, the treatment is small and potential pre-trends are smaller and harder to detect than around a large increase in 3G coverage. Nevertheless, in section 6.2 we provide a pre-trend test that focuses on the trends prior to *any* first increase in 3G coverage. In the later periods, the unit is already treated and pre-trend testing is polluted with dynamic effects from previous treatment, rendering pre-trend tests meaningless.

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 intentions. Without 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 sharp 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.[28]

3. Several other factors could potentially affect 3G internet access and migration intentions simultaneously, net of a linear local time trend. We, therefore, control for 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 expansion of 2G infrastructure can also affect individuals migration intentions and plans. Although it is implausible since 2G technology only

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

allows texting and a very limited internet connectivity, we directly show that 2G network coverage has no impact on our outcomes.

6. To rule out any problem related to the simultaneous inference of multiple hypotheses, we re-estimate our main results using the randomization inference technique 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 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.

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

An Alternative to Two-Way Fixed Effects Estimators

In both the baseline model (Equation 5) and in the event study excluding the never-treated (Equation 7), we use a Two-Way Fixed Effects (TWFE) estimator which includes fixed effects on the group (sub-national regions) and time (years) level to estimate the average treatment effects on the treated (ATT) in the difference-in-differences setting with many groups and several time periods. By decomposing the TWFE estimator under various assumptions, a recent literature has showed that the TWFE estimator is problematic in the presence of heterogeneous[29] and dynamic[30] treatment effects (Abraham and Sun, 2018;

[29] 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 where many groups are treated and to groups treated for many periods (de Chaisemartin and D'Haultfœuille, 2019). In a staggered adoption design (A staggered adoption design is 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 and Jaravel, 2018).

[30] Another issue of TWFE regressions and event studies is the presence of dynamic effects. When considering the setting with two time periods and one treatment (treatment status changes by 1 unit) and one control group (treatment status is unchanged), the possibility of dynamic effects requires one to account for past treatment 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, (Abraham and Sun, 2018) 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.

Borusyak and Jaravel, 2018; Goodman-Bacon, 2018; de Chaisemartin and D'Haultfœuille, 2019; Callaway and Sant'Anna, 2018).

As treatments effects of 3G internet coverage on migration intentions are likely to be heterogeneous for various reasons, negative weights on group-time level treatment effects bias our TWFE results. Slow adoption of 3G mobile internet, the gradual formation of migration intentions and subsequent migration, make it plausible that the treatment effects of 3G on migration intentions and plans do not settle within one year, further questioning the use of TWFE models.

To be able to estimate 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 (2018) and de Chaisemartin and D'Haultfœuille (2019) use both never treated and not-yet treated groups to assess the contemporaneous and dynamic treatment effect.[31] de Chaisemartin and D'Haultfœuille (2020) implement an alternative estimator that identifies an average treatment effect on the treated 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 (2018); Borusyak and Jaravel (2018) and (Abraham and Sun, 2018), as it allows for non-binary treatments. We discuss the implementation of this estimator in the following section.

Explanation of de Chaisemartin and D'Haultfœuille Estimator

In the staggered adoption case with binary treatment, DiD_l is an estimator consisting of a weighted average of $DiD_{t,l}^{d=0}$, which is the difference (between first time 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 ($d=0$) 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.[32] Although this estimator is robust

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

[32] Importantly, to calculate the DID_l using all available groups one needs a treatment variable that is

to those, just as the TWFE estimator, we have to rely on a common trends assumption, which can be assessed using the placebo estimators.[33]

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. [34] 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.[35] Nevertheless, we can still apply the principle of units switching into treatment for the first time to identify difference-in-differences between treatment and clean controls. The elementary building block is now differentiated over initial (status quo) treatment status d and we calculate the $DiD^d_{t,l}$ within this group d. As 3G coverage is continuous, this urges to bin the status quo treatments d, 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.[36] When those bins become wider, treatment and control groups with fairly different status quo levels

balanced on the unit level, as knowledge of a unit's past treatment status is essential for determining whether 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 Gallup World Poll, we do observe the value of 3G coverage in the gaps in the sample. In 2008 and 2009 only 600 districts are surveyed, compared to on average 1600 in the later years. Around 200 districts have gaps in the sample.

[33]The placebo estimators DiD^{pl}_l calculate the difference-in-differences between the treatment and control units between $l+2$ periods before and 1 period before the treated unit is treated for the first time. These estimators are important assessments of differential pre-trends between treatment and control units prior to first treatment.

[34]Covariate adjustment of the elementary building blocks $DiD^{d=0}_{t,l}$ is done 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 l^{th} temporal difference in outcome using the coefficients of step (1) multiplied by the l^{th} temporal difference in covariates. The $DiD^{d=0}_{t,l}$ are then the (treatment-control) differences in (relative time l) differences unexplained by the covariates. This has implications for the feasibility of the estimator as there may be less observations in the regression then there are covariates in step (1).

[35]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. This happens in only around 100[check] out of 2000[check] districts in the main sample. Therefore, we leave out the discussion related to designs in which treatment can decrease here, although the estimators are robust to this.

[36]Except for those districts with $d = 0$, as those comprise approximately 40% of our sample.

of treatment are compared. In order to estimate the DiD_l^d in an unbiased way, we have to assume that the treatment effects between the binned treatments are not varying over time.[37] Furthermore, as 3G coverage for many groups increases at least somewhat 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 status quo treatment levels d 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 d may not have a single switching group and DiD_l^d is not defined. 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 increased since 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:

$$\hat{\delta}^L = \frac{\sum_{l=0}^{L} w_l DiD_l^Y}{\sum_{l=0}^{L} w_l DiD_l^{3G}} \tag{6}$$

$\hat{\delta^L}$ is the treatment effect per unit of treatment which be calculated using the ratio of the D-I-D on the outcome and the D-I-D on the treatment path itself, weighted by the fraction

[37] If this is not the case, the counterfactual of remaining in treatment d is not exactly the counterfactual treatment of staying in d' and the elementary building block $DiD_{t,l}^d$ is biased through its control term (in symbols for all l: $Y_t^d - Y_{t-l-1}^d = Y_t^{d'} - Y_{t-l-1}^{d'}$ only holds if $TE_t^{d \to d'} = Y_t^d - Y_t^{d'} = Y_{t-l-1}^d - Y_{t-l-1}^{d'} = TE_{t-l-1}^{d \to d'}$). This bias is plausibly larger for (1) larger l, as treatment effects likely vary slowly as well as for (2) larger bins, such that the treatment effect $TE^{d \to d'}$ between d and d' 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 status quo levels, this implies that if we use groups with d as controls for first switchers from d' as often (weighted with the number of observations) as d' for first switches from d, the two contributions cancel out, and $DiD_{t,l}^{d,d'}$ is unbiased. As the (adoption of f) 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 status quo groups thus needs to be justified.

of observations in the lth effect. de Chaisemartin and D'Haultfœuille (2020) shows that this is equivalent in interpretation to an IV estimator as the numerator in Equation 6 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 average treatment effect on the treated.[38]

Instrumental Variable Strategy

To further address the concerns about omitted variables bias and reverse causality, we use an instrumental variables strategy following Manacorda and Tesei (2020) and Guriev, Melnikov and Zhuravskaya (2020).[39] In particular, Manacorda and Tesei (2020) use spatially differential incidence 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.[40] In the global context, Guriev, Melnikov and Zhuravskaya (2020) adopt a similar instrument using worldwide lightning data from Very Low Frequency (VLF) radiation detectors on a 1-by-1 km resolution from the World Wide Lightning Location Network (WWLLN) project.

The intuition of the instrument is that electromagnetic discharge due to lightning in or around a Base Transciever Station (BTS) can damage the antenna and telecommunications equipment, 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

[38]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.

[39]Instruments for traditional cable internet connections are often based on the positioning of main ('backbone') 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 moment internet became available. Unfortunately, a similar instrument, using the interaction of $2G$ network coverage on the sub-national 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.

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

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 (2020), we focus on lightning strikes from WWLLN between 2005 and 2011 to alleviate concerns of later time periods' lightning patterns being modified by climate change.[41] Importantly, WWLLN has a good detection efficiency for Cloud-to-Ground (CG) lightning, which is an advantage over space-based optical detection of lightning, which is most sensitive to Intra-Cloud (IC) lightning.[42]

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 region level.[43] Using these data, we construct the instrument as follows.

We first determine whether a lightning strike occurred in a 1-by-1 km 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-by-1 km grid in a given *day* in a given year in a subnational region r. $P_{box,r}$ is the population in the 1-by-1 km grid in region r in 2005 and $P_r = \sum_{box} P_{box,r}$ is the total population of the region. Then, we sum over all days of the years 2005 to 2011 and all 1-by-1 km boxes 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

[41] 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 networks operators, it is most likely that network operators base such decisions on historical patterns.

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

[43] The WWLLN network uses only several 10s of detectors worldwide, as the VLF radiation in the kHz range is detectable 1000s of kilometers away. Nowadays, the detection efficiency of powerful (discharges exceeding 30 kA) lightning strikes lies around 30% and the typical spatial accuracy is on the order of a few kilometer. 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).

we operationalize by assuming a logarithmic relation. We interact $\log(\mathcal{L}_r)$ with a linear time trend to construct our instrument: high-lightning 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 strongly coincide with initial levels of 3G coverage.[44]

The construction of instruments separately for income groups is important for two reasons:

Relevance: As the potential financial benefits from extending 3G coverage are higher in wealthier regions than in poor 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. Before the start of our sample in 2008, wealthier countries may have expanded 3G coverage predominantly in low-lightning districts. Therefore, high-lightning districts may see a stronger increase to catch-up to the surrounding districts. This can be partially controlled for by controlling for initial 3G in 2008 interacted with a linear time trend, but low initial 3G coverage may be driven by many unobserved factors that are more fundamentally limiting factors than high lightning frequency.

Lightning patterns are likely to be correlated to geography and demography, which both impact mobile network expansion differently.[45] Therefore, it is necessary to control for the effect on 3G expansion of factors such as population density, area size, and the share of deserts and mountains.

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

[45]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.

6 Results

In this section, we present four sets of results. First, we present the baseline results on the effects of 3G expansion on migration desire and plans, followed by non-binary de Chaisemartin-D'Haultfœuille estimator, IV results and heterogeneity analysis.

6.1 Main Results

Table 1 reports estimates of Equation 5. The dependent variables are a dummy 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). 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, Columns 5 and 6 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 intentions 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 10 percentage points increase in 3G coverage leads to .29 percentage points increase in international migration desire, 0.09 percentage points increase in international migration plans in the next 12 months, and 0.26 percentage points 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 percent, the effect is 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 A3) drive the results.[46] The dependent variables are a dummy indicating that the respondent "has any desire to emigrate locally or internationally" (first panel), that the respondent "plans to emigrate in the next 12 months" (second panel), that the respondent "has done 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 respondents' general desire to migrate to another country without considering any preparations. The next two are actual migration-related outcomes that take plans and preparations into account. The last one solely focuses on domestic migration intentions in the next 12 months.

We find that 3G internet has a positive, sizable and statistically significant effect on almost all of these outcome variables with the exception of domestic migration intentions. 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 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 two-way fixed effects regressions as the effect of 3G expansion is likely to vary across

[46]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 Intentions and Plans

	(1)	(2)	(3)	(4)	(5)
Outcome:	Intention to migrate internationally				
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 migrate internationally in the next 12 months				
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:	Likelihood to migrate in the next 12 months				
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 to be financially better off in 5 years, whether the respondent has sufficient means for food and shelter, and whether the respondent experienced something stolen in the past year. Income controls include the log of sub-regional average per person income and the log of per person income in the household of the respondent. Country-level controls include: the log of real GDP per capita, polity2 score, and the share of respondents under 30. The standard errors are clustered two-way on the country-year and district level.

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

	(1)	(2)	(3)	(4)
Outcome:	Any desire to emigrate locally or internationally (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:	Has plans to emigrate in the next 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:	Has done preparations to emigrate in the next 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 to migrate only domestically in the next 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 combination 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 A3. The fourth measure comprises those answering positively to (4) but negatively to (1) and covers years between 2008 and 2018. The standard errors are clustered two-way on the country-year and district level.

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.[47] To investigate whether our results are driven by this potential bias, we use a novel estimator by de Chaisemartin and D'Haultfœuille (2020), 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 have a large number of control groups for every switcher, we assign treated sub-regions (that is, $d > 0$) into bins; (ii) we set the treatment threshold, Δ_{3G}, for a (first) switch to be 3 percentage points increase in 3G coverage and drop all districts that experience a drop of 3 percentage points or larger. By doing so, we ensure to include only districts where treatment is monotonously increasing.

In Figure 2, we show the instant and dynamic estimators, DiD_l^Y, and three long difference placebo estimators, DiD_l^{pl}, for our treatment variable.[48] The placebo estimators should be equal to zero to satisfy common trends assumption conditional on covariates.[49] Notably, the results reported in all panels of Figure 2 provide *no evidence* for pre-trends.

When it comes to evolution of post-treatment effects, in Panel A, we find that 3G coverage increases steadily overtime after the initial jump. In Panel B, we observe that the desire to migrate internationally increases immediately after a first increase in 3G coverage and remains stable. The average effect of all observations following a first increase in 3G expansion is 0.057, which is relatively larger than our OLS estimate. Panel C presents the results for plans to emigrate internationally in the next 12 months and Panel D shows results for the self-assessed likelihood to emigrate in the next 12 months (both domestically

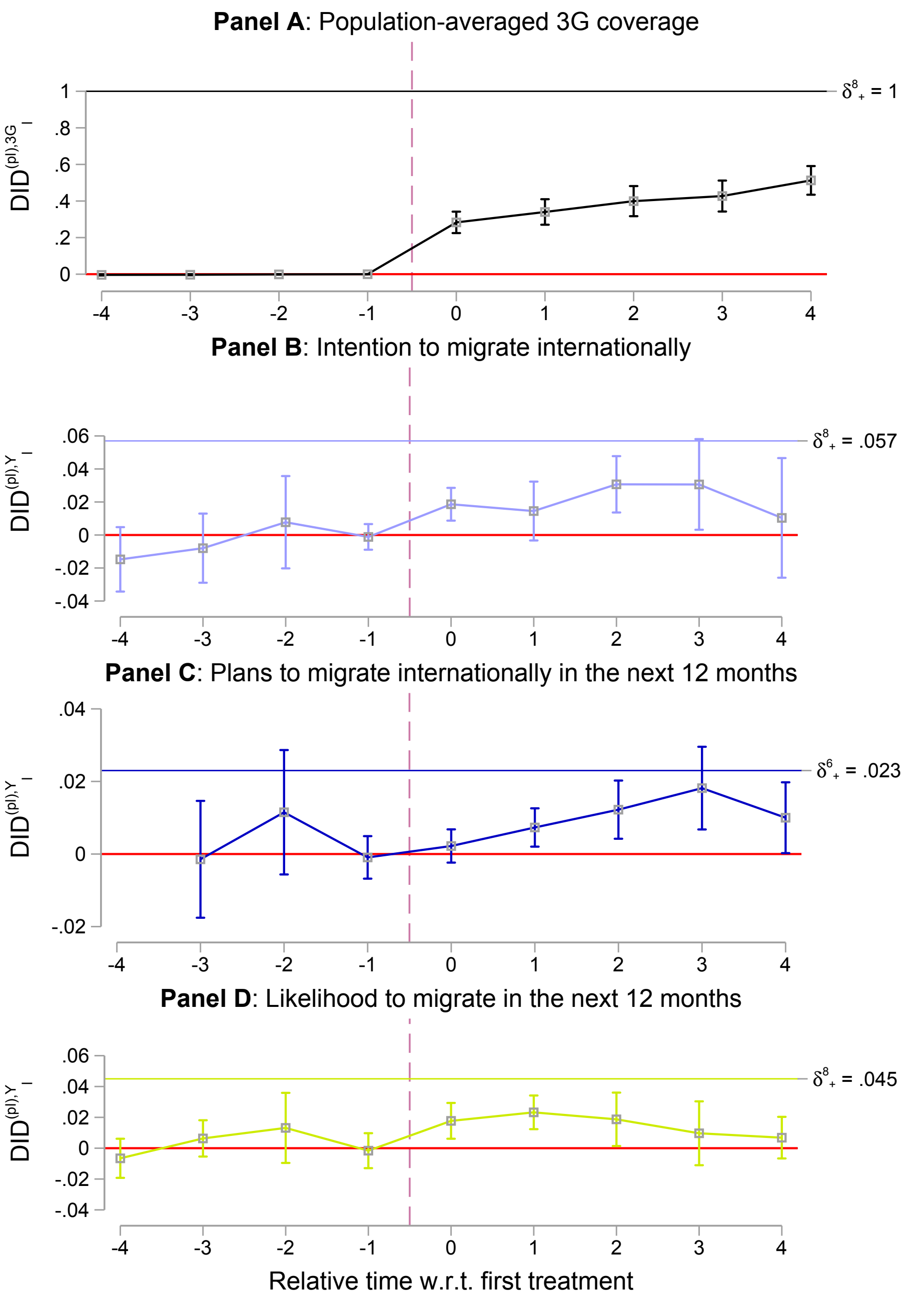
[47] de Chaisemartin and D'Haultfœuille (2019) developed a procedure (TWOWAYFEWEIGHTS) to calculate how many of the weights on the group-time-level treatment effects are negative and what the sum of negative weights is (where all weights sum to unity). Using TWOWAYFEWEIGHTS while allowing for heterogeneous 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.

[48] 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.

[49] To assess whether pre-trends between treatment are insignificant over the 1 to $l + 1$ periods before treatment, we consider the null hypothesis that any of the placebo estimators is nonzero.

and internationally). In both panels, the point estimates increases directly after treatment, but decays after $t+3$, which may be due to the lack of sufficient data.

Figure 2: de Chaisemartin-D'Haultfouille Estimates for 3G and Migration Intentions



Note: $DID_l^{(pl)}$ of the effect of first switchers in 3G coverage in Panel A on the three main outcomes in 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 Panel C and D. δ_+^n denotes the estimated average effects using the instantaneous effects and 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 $d = 0$ or those with $d \neq 0$ in 2008. Observations are weighted using the district-year average of Gallup weights and the number of respondents. After a switch, a group can not 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 IV Results

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 by a linear time trend. As our baseline includes regional-level linear time trends, we omit those in the IV estimations.

Table 3 reports the IV 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 large frequencies of lightnings expand their 3G coverage less. For the middle tercile and upper tercile, the first stage coefficients are statistically insignificant. The F-statistic is 14.23, which suggests a sufficiently strong first stage.

In line with the our baseline results, IV estimates also indicate that 3G expansion leads to an increase in desire to migrate. The IV estimate is much larger in magnitude than the OLS estimate, because of two reasons. First, as the effect of mobile internet coverage is likely to be heterogeneous, we identify a Local Average Treatment Effect (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 larger the coverage of 2G is, the more infrastructure exists (e.g., cell towers and cabling) that is also essential to 3G internet. We construct the instrument in a similar fashion to the lightning-based instrument: we inter-

Table 3: Lightning IV Results

	(1)	(2)	(3)	(4)
Dependent variable:	International desire to migrate			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 on 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 sub-national 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.

act the 2G coverage in 2006, $2G_{2006,r}$, with a linear time trend. Importantly, we use the 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) show 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 higher than the OLS result.

Table 4: 2G Infrastructure IV Results

	(1)	(2)	(3)	(4)
Dependent variable:	International desire to migrate			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]*	
$2G_{2006}$ × 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 on 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 Gallup World Poll. 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 sub-national 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.

7 Conclusion

Combining worldwide data on 3G internet roll-out and global surveys over a decade allows us to estimate causally the effect of 3G internet expansion on migration intentions and plans. We show that increases in 3G internet coverage causally increases the intention to emigrate, as well as plans to migrate. This effect is robust to inclusion of district-level time trends, demographic controls, controls probing satisfaction with local amenities and own live, employment status controls, controls probing social networks and remittances and various alternative measures of regional development.

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A Online Appendix

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

Outcome Variables from GWP

GWP contains multiple questions that ask about desires, likelihoods, plans, and preparations to migrate and a question to probe potential destination countries. Appendix Table A1 shows the relevant questions as they were asked 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 consists of two questions that are slightly different. Individuals that did not name a country in WP3120 are not asked WP6880 and thus are flagged as not planning to migrate. However, it is unlikely a respondent planning to migrate is unable to mention the country in the preceding question. A larger threat is posed by individuals planning to move to a realistic destination might not have answered that country in WP3120, but rather a less realistic country 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.

Visual Summary of the Intersection of Our Outcome Variables

We visually summarize our outcomes in a Venn diagram in Appendix Figure A3, which identifies eight mutually-exclusive regions for migration behavior (ranging from preparing to move to likely to stay). Regions I and II are of our 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 and plans to migrate, in comparison with general preparations (I+III) and plans to migrate (I+II+III+IV).

Among those *planning* to migrate (2.8 percent), about two-thirds (1.8 percent) report that they are likely to move within 12 months. Similarly, among those *preparing* to migrate

(1.8 percent), about two-thirds (1.1 percent) are likely to move within 12 months. Therefore, it is likely that migration intentions within 12 months are a better proxy for actual migration behavior.

Other Covariates

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 percent of respondents report that they would like to move permanently to another country, while only 3 percent report that they are planning to move permanently to their intended destination country in the next 12 months. 17 percent report that they are likely to move away from the city or area where they live in the next 12 months.

There is also large heterogeneity within and across countries. Figure A6 displays changes in average reported international migration intention between early (2008-2011) and late years (2016-2018), whereas Figure A7 shows the averaged levels in the 2016-2018 period.[50] Notable patterns can be summarized as follows: (i) migration intention is less than 20 percent in most developed countries; (ii) migration intention is below 10 percent in many East Asian countries; and (iii) there is substantial variation in intention to migrate within global regions over time — in the Middle East and North Africa (an increase from 16 percent in 2008 to 26 percent in 2018), Sub-Saharan Africa (an increase from 30 percent in 2008 to 36 percent in 2018) and South America excluding Venezuela (an increase from 16 percent in 2010 to 32 percent in 2018).[51]

46 percent of survey respondents are men. The average age of respondents is 40, 15 percent have tertiary education, and 58 percent are partnered.

3G Mobile Internet Coverage

Figures A4 and A5 illustrate 3G internet coverage at the sub-national region level.[52] In particular, Figure A4 shows the increase in 3G internet coverage between 2008 and 2018 and Figure A5 shows the levels in 2018. Perhaps not surprisingly, the levels of 3G internet coverage are highest in developed and densely populated countries, mostly achieving coverage levels of over 75 percent of the population. On the contrary, many Latin American and Sub-Saharan African countries have coverage levels of below 25 percent. Nevertheless, several non-OECD countries have showed expansions in excess of 25 percent over the 11 year period

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

[51]The GWP question on migration intentions was not asked in South America prior to 2010.

[52]The data availability is somewhat limited for some countries. For example, both Canada and Australia are removed from the final data set as the sub-national regions are much larger than in other countries. Data for some countries with large migration intentions and flows in the MENA region are also missing.

that we study. This offers relevant variation in 3G internet coverage on a global scale in the period studied.

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?

[TODO: implement a robust-to-heterogeneity and dynamic effects estimator here]

In Figure 2, 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 two-fold. 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 increase in 3G coverage. An advantage of focusing on sharp increases is that instantaneous and dynamic effects can be distinguished, as there is no large 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 differs.[53]

In the event study, we focus on districts that experienced an increase of 50% in their 3G coverage from year-to-year[54], and analyze how the desire to migrate develops with respect to this event, net of all baseline controls and fixed effects.

$$Outcome_{idt} = \sum_{l} \mu_l \mathbb{1}\{t_{id} - t_d^{'} - l\} + \alpha^{'}\mathbf{X_{idt}} + \phi_d + \theta_t + \epsilon_{idt} \tag{7}$$

The event study specification is shown in Equation 7. The year of interview is denoted by t_{id} and the first year after the rapid rise in 3G coverage is denoted by $t_d^{'}$. Then, 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

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

[54] A possible problem of 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 flagged as a treated area. This might imply that included areas already saw a substantial increase in 3G coverage before the recorded year of event.

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, n.d.). Thus, the coefficients μ_l are interpreted as the difference in outcome between the l^{th} period with respect to the first pre-event period.

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 5 or more periods after the event in one bin and the observations 5 or more periods before the event in another bin.

Appendix Figure A8 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 raises with 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 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 needed to fully explain our results by omitted variable bias.[55]

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 A9 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

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

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 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 (satisfaction with life and local amenities) are themselves affected by the 3G roll-out. 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 5 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 shows strong autocorrelation on the district-level, which may possibly 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[56]. 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

[56]Please note that using the n^{th} lag (lead) disregards the observations in the n earliest (last) years.

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

As the expansion of cellular 2G and 3G network expansion is strongly correlated because of the shared infrastructure both technologies use, the found effect of 3G on migration intentions may (partially) arise because of coinciding expansions of 2G (which enables bilateral communication through calling and texting) 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 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 plotting the coefficients of our preferred specifications as one year is omitted 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 we drop one global region at a time in each estimation and again find that our estimates are not driven by a single global region. [57]

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.[58] Additionally, we omit countries where a large (≥40%) proportion of GWP respondents desires to migrate and those where a small (≤10%) proportion desires to migrate. Appendix Table A8 reports the baseline results for those three omissions. The coefficient on 3G is robust to omission of these country groups.

Measurement and Potential Reporting Error in Mobile Coverage Data

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

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

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. Secondly, coverage is not necessarily reported by all network operators, possibly underestimating the network coverage. As both of those sources of measurement error may be related to mobile network operator, industry structure as well as country- or district-level characteristics, these may potentially bias the results we reported. To alleviate concerns about such measurement 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 percent population-averaged coverage of 3G in the first year that an operator in that country reports non-zero 3G coverage. In this case, we deem it plausible that prior to that year that country already had nonzero 3G coverage.[59]

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

  Countries that have at least 4 times as much per capita mobile broadband subscriptions than population-averaged 3G coverage in 2015. In this case, it is plausible that 3G coverage is under-reported.[60]

- 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.[61]

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

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

[60]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 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.

[61]This happens in 109 districts in the baseline sample, most of which located in Europe (31 in 6 countries) and in the former Soviet Union (36 in 5 countries). A striking example is Finland, where 6 districts experience decreases larger than 50% in 2016, to (more than) fully recover in 2017.

should be uncorrelated with respondents' observable demographic characteristics. To check 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.

[NOTE: here we only note the individual demographic controls, whereas two other variables are significant at the 10, but not 5% level: the share of persons under 30 years old on the country-year level (positive), whether the respondent can count on friends (negative) and the question whether the respondent had not enough money for food (positive). As we can not show all covariates (28 in total), we have to make a choice what to show. I included a very basic table for now.] [NOTE: Guriev et al. include a balancing test on a binary post-event dummy (as in the event study) and use a rebalancing adjustment. Should we do something similar?]

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 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 two-way: 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 co-move 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

In the baseline we weight our observations using the within-country weights based on the inverse probability of being included in the Gallup surveys based on the respondent's and country's demographics as provided by Gallup[62].

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 wildly differ over countries and to a lesser extent 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 intentions 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 1 standard deviation (Column 2), whereas adding a quadratic time trend does not alter the results by much (Column 3).

[62]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 approximately correct 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 respect to their population. One can calculate population adjusted country weights by using the Gallup weights w_i^{Gallup}, country-level population data obtained from the World Bank in 2015, N_c, and the total number of respondents between 2008 and 2018 in GWP per country, N_c^{Gallup}:

$$w_{ic}^{pop} = w_i^{Gallup} \cdot \frac{N_c}{N_c^{Gallup}} \tag{8}$$

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

Figures

Figure A3: 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 fraction of respondents belonging to questions 1,2, and 3 (boxes) and 4 (circle) from the main text, whereas the list on the right hand side gives the fraction of respondents belonging to each of the mutually exclusive groups. $N = 317,520$.

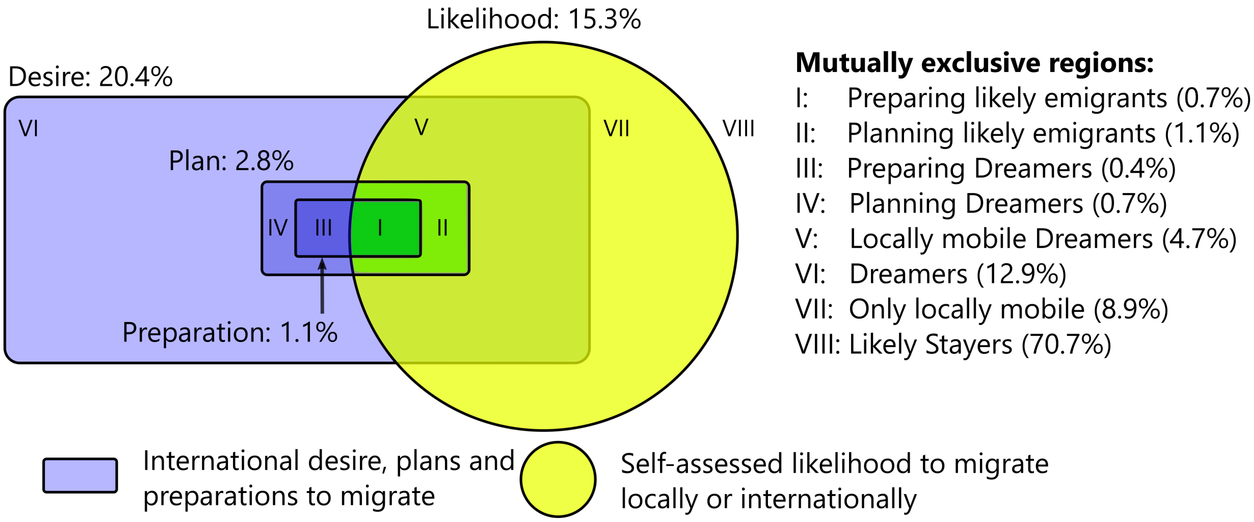


Figure A4: Increase of 3G population-averaged coverage between 2008 and 2018, aggregated per sub-national region

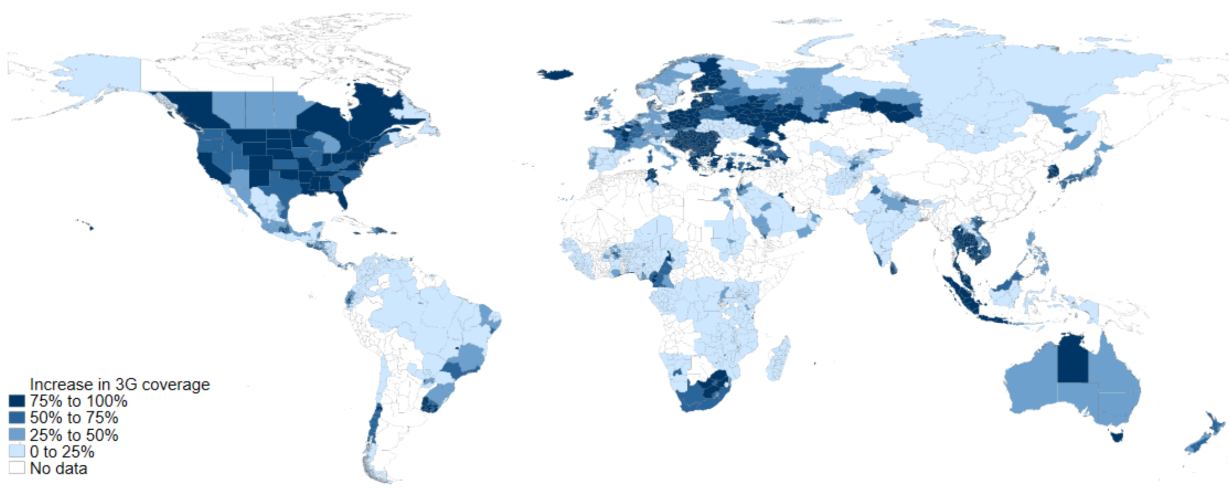


Figure A5: 3G population-averaged coverage in 2018, aggregated per sub-national region

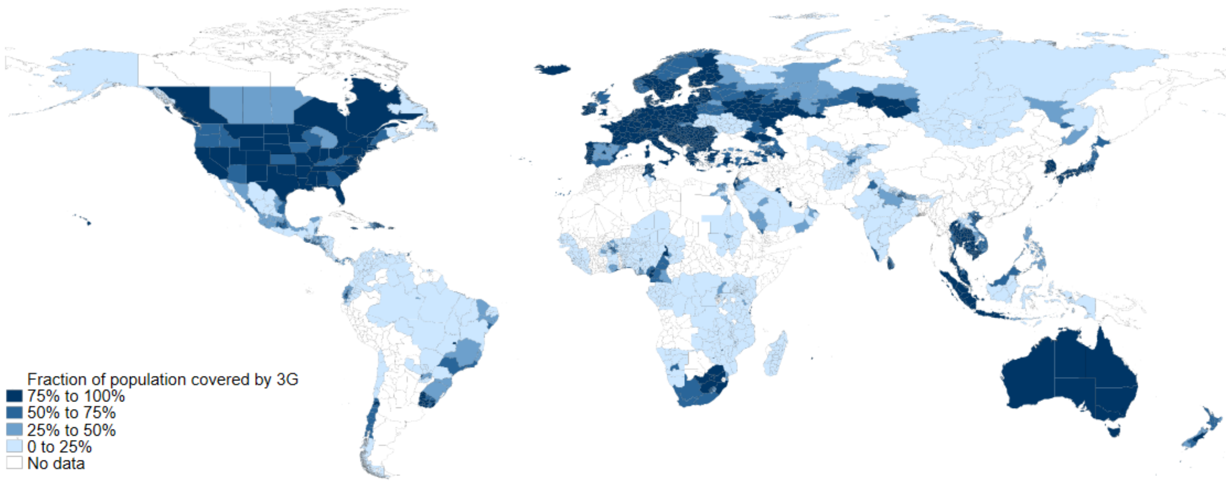


Figure A6: Increase in intention to migrate between early (2008-2011) and late (2016-2018) years, on the country level

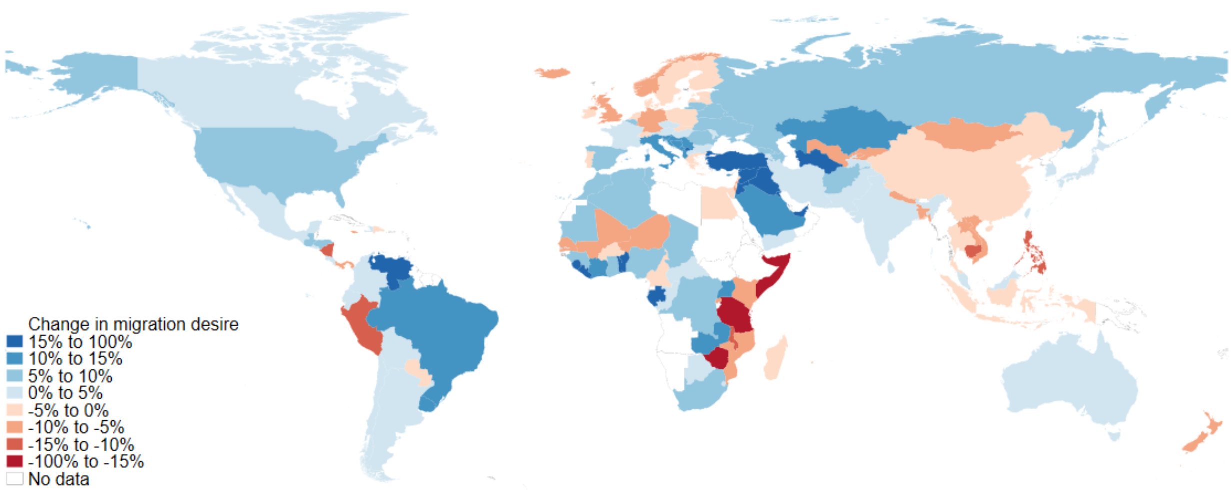


Figure A7: Average intention to migrate in the late years (2015-2018) on the country level

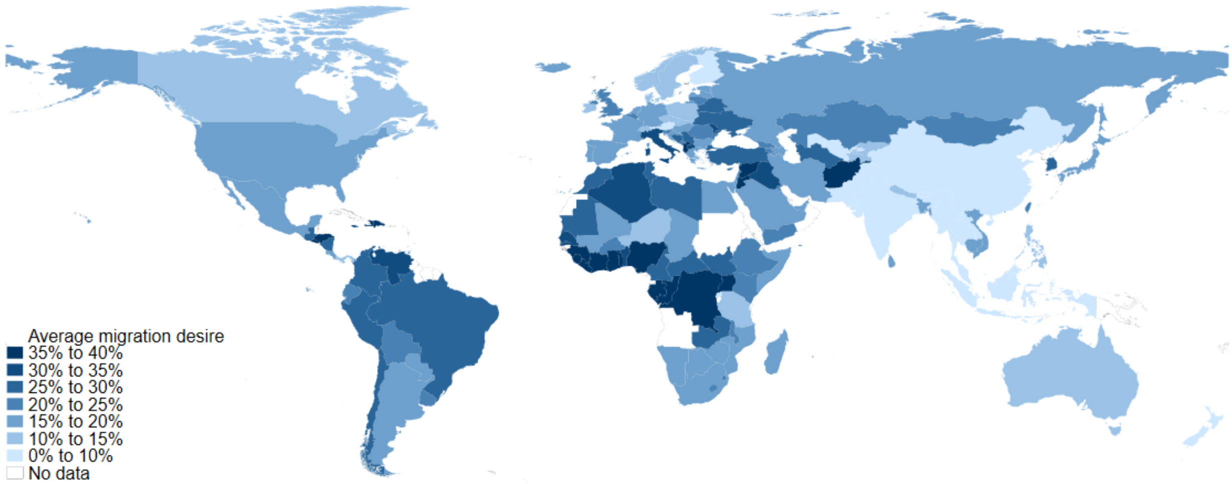


Figure A8: Event study estimates around treatment of 50 percentage points increase in 3G in a year with a 95% confidence interval. The black (blue) line depicts the event study estimates with 3G coverage (desire to migrate) as dependent variable. The omitted pre-event dummy is the last period before treatment. Endpoints are binned for 5 periods before treatment and earlier and for 5 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.

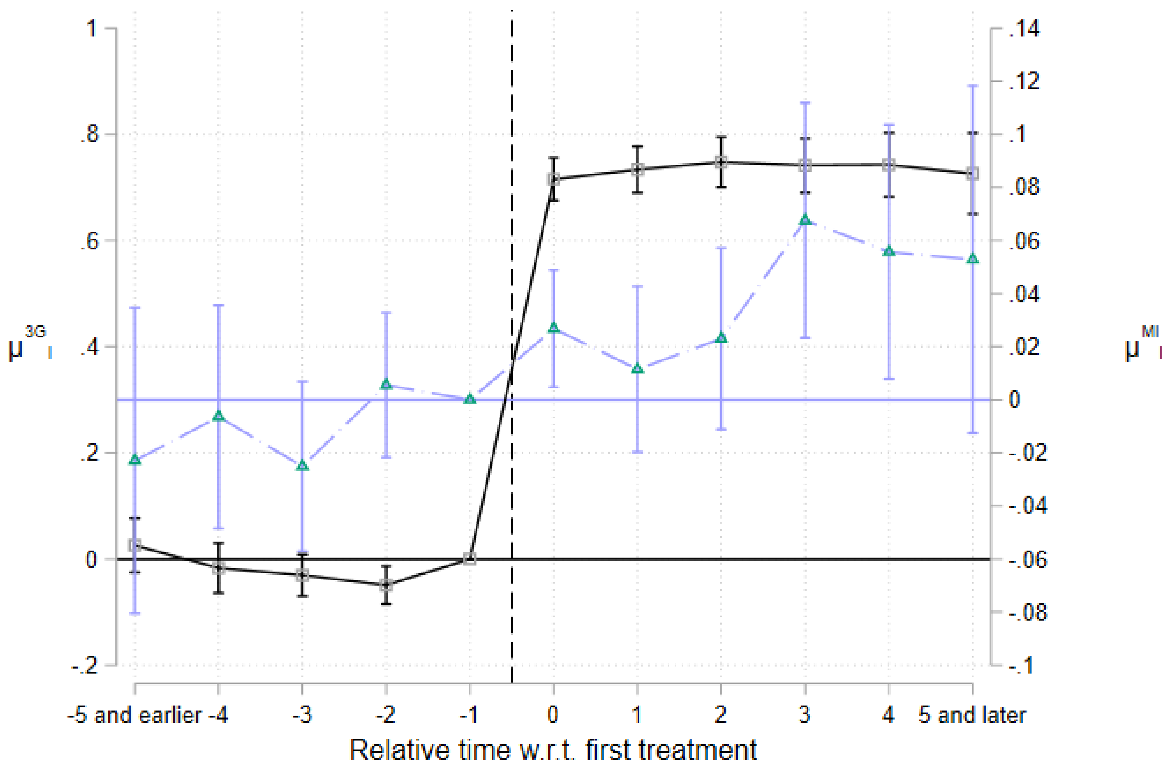
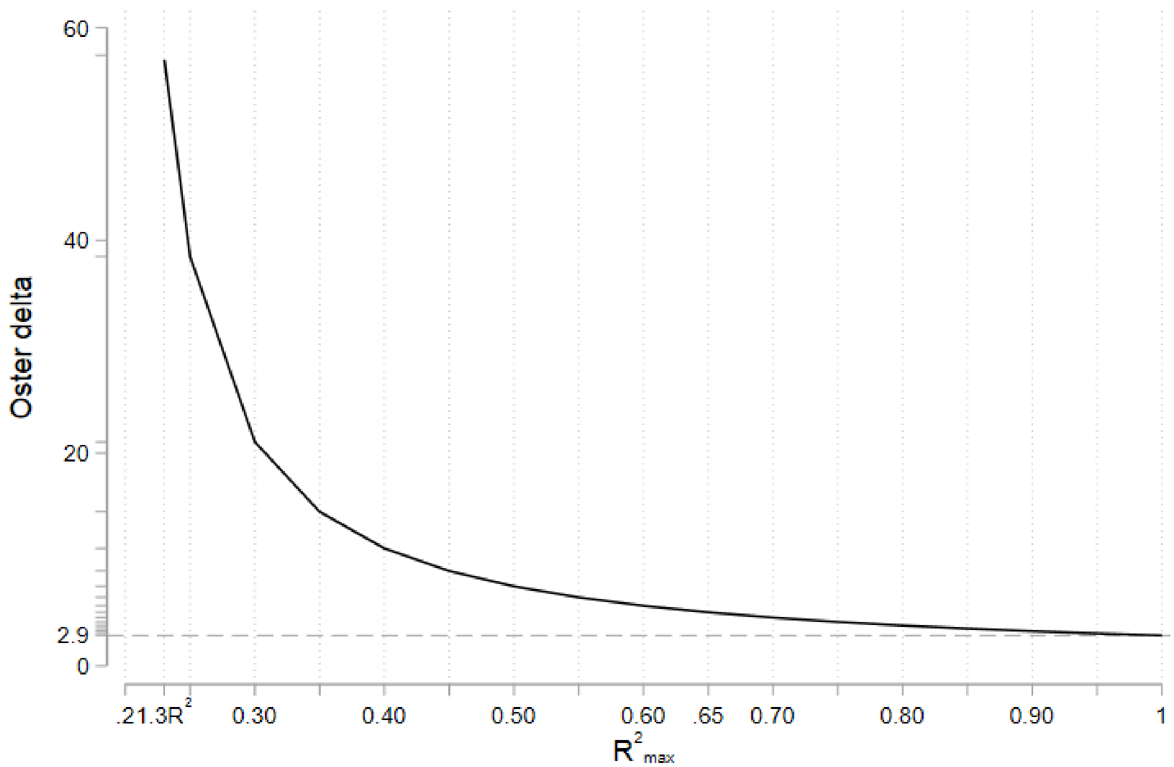


Figure A9: Oster's δ for increasing values of maximally admissible R^2_{max}. Oster's δ is equal to 57.4 for the R^2_{max}=$1.3R^2$.



Tables

Table A1: *Questions in GWP relating to respondents' desires and intentions to migrate*

Name	GWP ID	Question / construction	Coverage
Panel A			
(1): International migration desire	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): International migration plans	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): International migration preparations	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
International migration intention	0.22	0.42	606,827	GWP	Individual
International migration plans	0.03	0.16	366,089	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 (9 - 15 yrs)	0.53	0.50	606,827	GWP	Individual
Tertiary education (4 yrs college)	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 about 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

	(1)	(2)
Outcome:	Internet Access	
3G	0.047***	0.049***
	(0.002)	(0.001)
Baseline controls, FEs and district-level time trend	✓	✓
Broadband subscription rate		✓
Observations	636516	627815
R^2	0.52	0.52
Average dependent variable	0.432	0.432

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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 country or 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 5 years, whether the respondent has sufficient means for food, for shelter, and whether the respondent experienced 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 out of 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 5 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 Intentions

	(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

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 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 Dropping High and Low Migration (Intention) Countries

	(1)	(2)	(3)
Outcome:	Desire to emigrate		
Dropping countries:	Top 10 refugee	$\geq$40% desire to emigrate	$\leq$10% desire to emigrate
3G	0.028**	0.026**	0.036***
	(0.011)	(0.012)	(0.013)
Observations	588449	554462	516011
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 a 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 non-zero reported coverage, Column (3) omits regions with a population-averaged 3G coverage lower than a 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 (9 - 15 yrs)	-0.032
	(0.087)
Tertiary (4 yrs college degree)	-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

Standard errors in parentheses

Standard errors are clustered two-way: on the District and the Country-year level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A11: Robustness to Randomization Inference and Multiple Hypothesis Testing

	(1)	(2)	(3)	(4)
Outcome:	Desire to emigrate	Plans to emigrate	Likelihood to migrate	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

	(1)	(2)
Outcome:	Desire to emigrate	
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

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

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

	(1)	(2)
Outcome:	Desire to emigrate	
3G	0.055***	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

Standard errors in parentheses

Standard errors are clustered two-way: on the District and the Country-year level

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 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

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. 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.