3G Internet and Human Capital Development*

Ronak Jain† Samuel Stemper‡

April 23, 2025

Abstract

We study the impact of global expansions in mobile internet access between 2000 and 2018 on student outcomes. We link geospatial data on the rollout of 3G mobile technology with over 2.5 million student test scores from 82 countries. Our findings indicate that the introduction of 3G coverage leads to substantial increases in smartphone ownership and internet usage among adolescents. Changes in 3G coverage lead to significant declines in test scores in math, reading, and science, with magnitudes roughly equivalent to the loss of one-quarter of a year of learning. We also find evidence of a reduction in the ease of making friends and a sense of belonging.

JEL Codes: I21, I25, I31, J24, L86, O33

*First version: June 20, 2023. This version: April 23, 2025. We are grateful to Emily Breza, Jishnu Das, David Dorn, Edward Glaeser, Joshua Goodman, Maya Eden, David Hémous, Anders Jensen, Asim Khwaja, Gautam Rao, Hans-Joachim Voth, Ulf Zölitz and seminar participants at the Asian and Australian Society of Labour Economics 2023 Conference, the 2024 Australia and New Zealand Econometrics Study Group, the 2024 Annual Conference of the New Zealand Association of Economists, FIW Research Conference International Economics, and Harvard University Development Seminar for helpful comments and suggestions. All remaining errors are ours.

†University of Zurich. Email: ronak.jain@econ.uzh.ch

‡University of Auckland. Email: sam.stemper@auckland.ac.nz

1 Introduction

The rapid proliferation of internet access and advancements in technology have impacted nearly every aspect of children's lives. Globally, estimates show that one in three internet users is a child.[1] In the United States, 95 percent of teenagers have access to a cell phone; nearly half say they use the internet "almost constantly".[2] Given that adolescence is a crucial time for human capital development (Cunha and Heckman, 2007), parents and educators have grown increasingly concerned about the impact of unrestricted internet use on academic outcomes (The New York Times, 2024; The Boston Globe, 2024; BBC News, 2024b; UNESCO, 2024). The availability of mobile internet raises pressing questions about how this constant connectivity affects students' human capital development. In this paper, we ask how has this massive change in mobile internet connectivity affected adolescents' behavior and their development of skills.

Theoretically, the answer to this question is ambiguous. On the one hand, information technology can broaden access to information and resources that enhance student learning and engagement (The Economist, 2017). On the other hand, these technologies may have adverse effects on mental health, sleep, and various other facets of students' lives, which could impede their learning and skill development (Braghieri et al., 2022; Billari et al., 2018; Golin, 2022). Allcott et al. (2022) propose that a model of "digital addiction," in which habit formation and self-control problems play a large role in the use of digital technologies, may explain these adverse health impacts. Reflecting these concerns, governments worldwide are debating the need for stricter regulations on smartphones in schools (BBC News, 2024a; Al Jazeera, 2024). Policymakers in some countries, including the United States and Australia, have already proposed bans on smartphones and social media use in classrooms (The New York Times, 2024; Guardian, 2024; France 24, 2024; UNESCO, 2024). More broadly, Pritchett (2022) argues that despite the massive increase in computing power, the relatively unimpressive test score growth suggests that digitization is not a "key element of improving learning in schools."

This study investigates the impact of 3G internet, a mobile network technology that provides wireless internet connectivity, on the human capital development of adolescents. In particular, we

[1] This estimate is based on a "survey results of 11 countries, from across which more than 14,000 internet-using children were interviewed about their online experiences" (UNICEF, 2019).

[2] "Teens, Social Media and Technology 2022," Pew Research Center, August 10, 2022.

leverage the staggered rollout of 3G technology to investigate the causal impact of 3G internet on student academic performance. We use test score data from over 2.5 million students from the Programme for International Student Assessment (PISA), an international assessment conducted by the Organisation for Economic Co-operation and Development (OECD). Since 2000, PISA has measured 15-year-old students' mathematics, reading, and science skills across multiple countries. This data allows us to explore the relationship between 3G coverage, internet access, use, and student test scores over nearly two decades, quantifying changes in adolescent skill development across regions and time. The data also allows us to capture various other aspects of students' lives, including social connectedness and mental well-being. For identification, we leverage the expansion of 3G coverage within and across countries, combining geospatial data on 3G coverage with PISA data on school-level urbanicity and student outcomes.

Mobile internet access, particularly via smartphones, differs significantly from previous forms of internet access, such as broadband through desktop computers, in both accessibility and its potential to influence adolescent behavior and learning outcomes. Unlike fixed broadband, 3G-enabled mobile internet can be accessed anywhere and at any time, making it more pervasive in students' daily lives. It enables continuous exposure to online content, social media, and gaming—often through personal devices with less parental oversight compared to family-shared devices like a desktop computer.[3] By focusing on 3G-enabled mobile internet, our study seeks to explore the broader implications of this technological shift, which facilitates constant connectivity and raises unique concerns for adolescent learning and development.

We find evidence that expansions in 3G lead to large increases in student technology use. Students living in areas with 3G coverage are 4 percentage points more likely to browse the internet daily, 10 percentage points more likely to have a smartphone at home, and report spending an additional 5 hours on the internet each week. In turn, 3G expansion results in reductions in student test scores. The magnitude of these reductions—approximately 0.04 to 0.08 standard deviations—is on par with roughly one-quarter of a year of schooling. Our results are robust to specifications that account for potential bias in difference-in-differences estimates, and we find no evidence for pre-trends in test scores before 3G introduction in event study estimates.

[3]While both 3G and its predecessor, 2G, provide basic voice and text communication services, 3G networks offer faster data speeds, increased data capacity, and enhanced support for multimedia applications such as online gaming and video calling.

To understand the mechanisms driving the negative effects of 3G coverage on student learning, we examine its impact on social connectedness, study behaviors, self-efficacy, and absenteeism. We find that 3G expansion is associated with declines in students' sense of belonging and ease of making friends, suggesting that worsening social connectedness could be one channel affecting academic performance. Interestingly, while students report spending more time on homework, this increase coincides with higher internet browsing, indicating that digital distractions likely reduce study efficiency. At the same time, we find little evidence that school absenteeism plays a major role in driving test score declines. Finally, while we observe a negative association between 3G coverage and academic self-efficacy, this effect is not statistically significant. Taken together, these findings suggest that 3G mobile internet access has reshaped students' academic experiences not just by altering the quality of their study time, but also their sense of belonging and connection within the school environment.

Our main specification leverages within-country variation in 3G coverage, employing a rich set of fixed effects to isolate the impact of differential 3G adoption on test score variation in the same country-year. We include country-by-urbanicity fixed effects to control for time-invariant differences between urban and rural areas (e.g., infrastructure, school quality). Country-by-year fixed effects absorb national, time-varying factors (e.g., policy changes, economic shocks), and urbanicity-by-year fixed effects capture time-varying differences between urban and rural areas (e.g., migration, economic shifts). We also control for key student demographic characteristics, parental education, and school type.

Several additional pieces of evidence further provide strong support for a causal interpretation of our results. First, to address potential bias from staggered rollouts in the presence of heterogeneous treatment effects, we use two alternative two-stage difference-in-differences estimators proposed in Gardner et al. (2023) and Borusyak et al. (2024) and find similarly sized estimates. Second, event study estimates reveal no pre-trends in test scores prior to 3G introduction. Third, we also conduct permutation tests that randomly reassign 3G histories across country-urbanicity pairs to generate a distribution of placebo estimates. Our actual treatment effects lie far outside this placebo distribution, providing clear evidence that our results are not driven by chance.

Our research contributes to the literature on mobile internet connectivity and human capital development by providing the first large-scale, global evidence on the effects of mobile internet

(via 3G) on educational outcomes. While Vigdor et al. (2014) examine home computer use in North Carolina and find a modest negative effect, their study is limited to one US state and covers only the early broadband expansion period (2000–2005). In contrast, we leverage data on a representative student sample from 82 countries over a much longer period (2000–2018), allowing us to estimate the impact of mobile internet across diverse educational and institutional settings. Importantly, 3G-enabled mobile internet differs fundamentally from previous technologies like fixed broadband or internet access at home through a computer or laptop, which might typically be shared within households and subject to parental oversight. Mobile internet allows anytime-anywhere, continuous, and often unsupervised exposure to online content, which makes digital engagement much more pervasive in students' daily lives, potentially altering learning behaviors and cognitive development in historically unprecedented ways. With over 95% of the tested students in 2018 reporting having a smartphone at home and over 70% reporting browsing the internet daily, understanding the implications of mobile internet for student outcomes is more urgent than ever.

The closest study to ours, Bessone et al. (2020) (unpublished), examines 3G expansion in Brazil and finds no significant impact on public school test scores. However, their study relies on aggregate municipal-level student test scores, rather than individual student-level data and lacks detailed data on internet use and non-academic outcomes, limiting its ability to holistically capture the broader impacts on student lives. Our study goes much further by quantifying the impact of mobile internet on not only test scores but also students' internet use, social connectedness, well-being, homework, and absenteeism. The longer time horizon (2000–2018) and global coverage of the data significantly improve the statistical power and generalizability of our findings. The worldwide scope of our analysis enables us to study impacts across low-, middle- and high-income countries, offering critical and timely evidence for ongoing policy debates on smartphone use and digital inclusion (United Nations, 2019).

Our study also advances the literature on communications technology and education by providing large-scale evidence on the general equilibrium effects of (3G) mobile internet expansion. Unlike prior research, which primarily focuses on targeted interventions in single-country settings, we examine how widespread, unrestricted 3G internet access affects adolescent learning and behavior across diverse contexts. Previous studies have found mixed effects of technology

on education—ranging from small positive impacts of early television exposure (Gentzkow and Shapiro, 2008) to negative or null effects of home computers in Romania and California (Malamud and Pop-Eleches, 2011; Fairlie and Robinson, 2013) and null effects of giving a laptop to students in Peru (Beuermann et al., 2015; Malamud et al., 2019). Recent experimental studies highlight the benefits of targeted remedial programs (Muralidharan et al., 2019) and computer-assisted learning (Ganimian et al., 2020; Escueta et al., 2020; Rodriguez-Segura, 2022), but they do not capture the widespread and potentially disruptive effects of unrestricted mobile internet access. Unlike structured learning tools, mobile internet provides constant, unregulated connectivity, raising the potential for distraction at an unprecedented scale. Moreover, while useful for identifying direct treatment effects, these studies miss capturing broader social externalities—which can be substantial in the case of widespread mobile internet and social media use (Bursztyn et al., 2023)—potentially limiting their ability to fully address the wider implications of changes in communications technology and skill development.

Finally, our research contributes to the nascent literature on the socioeconomic and political effects of internet expansion. Recent work has shown that 3G rollouts have increased child injuries (Palsson, 2017), changed political attitudes, voting behavior (Guriev et al., 2021; Melnikov, 2021; Falck et al., 2014; Golin and Romarri, 2022) and increased female labor force participation (Chiplunkar and Goldberg, 2022). Previous studies have linked broadband internet access to the widening of the gender gap in youth mental health (Golin, 2022; Donati et al., 2022), increased distractions and lower sleep quality (Billari et al., 2018), reduced social capital (Geraci et al., 2022) and increased employment (Hjort and Poulsen, 2019). We add to this literature by focusing on education—a crucial but underexplored dimension of digital expansion—offering new insights into how mobile internet is transforming student behavior and learning worldwide.

The rest of the paper proceeds as follows. In Section 2 describes the data. Section 3 outlines our methodology and Section 4 presents our results. Section 5 discusses potential mechanisms and 6 concludes.

2 Data

2.1 PISA Data

Our primary data source is the OECD's Programme for International Student Assessment (PISA). Administered every three years in countries across Europe, Asia, the Americas, and Africa, PISA measures 15-year-olds' mathematics, reading, and science skills.[4] In each country, PISA aims to obtain a sample that is representative of "15-year-old students attending educational institutions in grades 7 and higher."[5] We use student-level PISA data from seven rounds of testing between 2000 to 2018.[6]

Our main outcomes are test-based measures of student achievement in reading, mathematics, and science. The OECD transforms student scores such that they have a mean of 500 and a standard deviation of 100. We standardize all reported student scores by subtracting 500 and dividing by 100. Tests administered as part of PISA are "not directly linked to the school curriculum," and are meant to test students' "ability to use their reading, mathematics and science knowledge and skills to meet real-life challenges."[7] To allow for comparisons across years, the OECD performs an equating process based, in part, on common test questions between tests in different years.

In addition to measures of student achievement, OECD data includes several additional variables relating to the characteristics of participating students, as well as the characteristics of their families and schools. We therefore have data on student gender, age, immigration status, parental education, and whether the student's school is public, private and government-dependent, or private and government-independent. Appendix A provides a detailed description of the construction of the main variables used in our analyses. We make a small number of sample restrictions; we exclude students with missing test scores in any subjects, students with missing data on age, gender, or urbanicity, and students who are below 13 years old or above 17 years old.[8]

As PISA does not collect precise location coordinates for participating schools, we use another geographic identifier available in the data. In every PISA round, representatives from participat-

[4] A full list of participant countries can be found at PISA Country List.
[5] PISA 2018 Technical Report, Chapter 4.
[6] In response to the COVID-19 pandemic, the OECD postponed the scheduled PISA 2021 assessment.
[7] What is PISA?, About PISA.
[8] These restrictions on age, gender, and urbanicity lead us to drop 5 percent of observations with non-missing test scores. Most of these observations are dropped due to missing urbanicity data (which is necessary for our research design); among observations with non-missing urbanicity, only 0.06 percent (6 in 10,000) observations are dropped.

ing schools are asked “Which of the following definitions best describes the community in which your school is located?". Eligible responses are village, town, city, or large city. We refer to this variable as urbanicity. In combination with the geospatial data described below, we use this variable to capture student exposure to 3G coverage over time. Appendix Figure E.1 shows the number of responses in each year-by-country-by-urbanicity cell.

To gain insights into students' engagement, social connectedness, and mental well-being within the school context, we additionally consider student homework hours, absenteeism, ease of making friends, sense of belonging, and self-efficacy. Not all of these variables are available for all years. In cases where the scales vary across the years, Appendix A details how we harmonize measures across PISA rounds. In addition, Appendix Figures E.3 to E.8 summarize annual missingness by country-by-urbanicity pair for variables for which full data is not available.

Finally, we capture several student responses to the Information Communication Technology (ICT) questionnaire. This questionnaire is optional among countries participating in PISA and includes a set of survey questions related to students' use of technology, their digital competencies, and their attitudes toward information and communication technologies. Because these questions are not administered to every student in each PISA round, we define two samples in our analysis of PISA data: our “full sample," which includes all student observations for which data is available for our main testing and control variables, and our “ICT sample," which additionally requires that observations have non-missing responses to our main ICT variables. These samples include over 2.6 million and 1.5 million student-level observations, respectively. Appendix Figure E.2 displays the set of country-by-urbanicity pairs that appear in the ICT sample in each PISA round.

2.2 Geospatial Data

PISA data includes a categorical measure of urbanicity at the school level, which serves as our primary geographic identifier, since PISA does not collect precise location coordinates for participating schools. This urbanicity measure classifies schools into four categories based on the population of their location: villages (fewer than 3,000 residents), towns (3,000 to 100,000), cities (100,000 to 1,000,000), and large cities (more than 1,000,000). Using a combination of data sources described below, we calculate the share of the population with 3G coverage for each country-by-urbanicity cell and year (e.g. for large cities in Australia or villages in Italy). This procedure

involves combining data on which areas of each country fall into which urbanicity category (using global population and urbanicity data) and which areas of each country had 3G coverage each year. Figure 1 illustrates this aggregation process for five large countries in our sample: Australia, Canada, Italy, Mexico, and Spain.

2.2.1 3G Coverage Data

We use data on 3G coverage from Collins Bartholomew. This data ranges from 2007 to 2018, excluding 2010 and 2017. In each year, Collins Bartholomew data comes in the form of shapefiles, organized into 1-by-1-kilometer grid cells, that indicate which areas of each country have 3G coverage. While the underlying 3G coverage data is available at this granular 1-by-1 kilometer resolution, we aggregate it to the country-by-urbanicity-by-year level to match the geographic detail available in the PISA data.

2.2.2 Global Population and Urbanicity Data

To classify areas into PISA's urbanicity categories, we combine two complementary datasets. We use data from the Gridded Population of the World (CIESIN, 2018) and the Global Human Settlement Layer (Florczyk et al., 2019) to identify the area as either a village, town, city, or large city.

Gridded Population of the World data reports population counts and population density for each 2.5 arc-minute point on Earth. We combine this data with the Global Human Settlement Layer's Urban Centre Database to identify points that fall in cities and large cities. Consistent with the definitions used in PISA, we classify any urban center with a population greater than 1,000,000 as a large city, and any urban center with a population larger than 100,000 as a city. (For both Gridded Population of the World and the Urban Centre Database, we use population estimates as of 2015 throughout our calculations.[9])

Finally, we use population density data from the Gridded Population of the World to distinguish between towns and villages. We define villages as points with fewer than 100 people per square kilometer. The remaining points—those with more than 100 people per square kilometer

[9]Gridded Population of the World estimates are available for 2000, 2005, 2010, 2015, and 2020. Global Human Settlement Layer's Urban Centre Database provides population estimates as of 1975, 1990, 2000, and 2015.

that do not fall in a city or large city—are treated as towns.

This procedure generates a set of 291 unique country-by-urbanicity combinations found in PISA data, within which we calculate 3G coverage. We acknowledge that our data lacks the geographical precision seen in other studies on global 3G expansions. For example, the data used in Manacorda and Tesei (2020) includes 10,409 unique geographic cells, while the data in Guriev et al. (2021) includes 2,232 international subregions.

2.2.3 Calculating 3G Coverage

For each round of Collins Bartholomew data, we calculate 3G coverage for each country-by-urbanicity pair as the share of gridded points that have 3G, weighted by the total population. Many areas had non-zero 3G coverage in 2007. For these countries, we identify the month that commercial 3G coverage was introduced in the country, and assume that coverage in the month prior was zero.[10]

We combine these datasets to estimate 3G coverage for each country-by-urbanicity pair monthly, using linear interpolation between the yearly observations. For each PISA examination, we then calculate $3G_{cut}$ as the average level of 3G coverage in a student's country-by-urbanicity area during the 12 months preceding their exam. We follow the same procedure using Collins Bartholomew's 2007 data on global 2G coverage to construct an instrument for our instrumental variables estimates.

2.2.4 Lightning Data

In our instrumental variables estimation, we use lightning frequency as an instrument for 3G coverage. To calculate lightning frequency, we use Gridded Lightning Climatology Data available through NASA (Cecil, 2015). This data reports the average lightning flash rate for each point on the earth's 0.5-degree by 0.5-degree latitude-longitude grid. Similar to the 3G calculations above,

[10]This data is sourced from the OECD (OECD, 2004) and public press releases accompanying the introduction of 3G. These months are as follows: Australia: 10/2002, Austria: 4/2003, Belgium: 4/2004, Brunei: 8/2005, Croatia: 2/2005, Czech Republic: 12/2005, Denmark: 10/2003, Estonia: 10/2005, Finland: 1/2002, France: 5/2004, Germany: 2/2004, Greece: 7/2003, Hong Kong-China: 1/2004, Hungary: 5/2005, Indonesia: 9/2006, Ireland: 5/2003, Israel: 8/2004, Italy: 3/2003, Japan: 10/2001, Korea: 5/2002, Malaysia: 5/2005, Netherlands: 9/2003, New Zealand: 11/2004, Norway: 12/2004, Philippines: 2/2006, Poland: 9/2004, Portugal: 6/2004, Romania: 4/2005, Singapore: 12/2004, Slovak Republic: 1/2006, Slovenia: 12/2003, Sweden: 5/2003, Switzerland: 11/2004, Taiwan: 5/2005, United Kingdom: 3/2003, United States: 1/2002.

we calculate each country-by-urbanicity cell's average population-weighted lightning frequency by calculating the lightning frequency for each 2.5 arc-minute point on the earth and weighting these values by each point's total population.

2.3 Descriptive Statistics

2.3.1 Summary Statistics

Table 1 displays summary statistics for our sample, separately for our full sample (in Panel A) and our ICT sample (in Panel B). For both samples, roughly half of the students are male, and the average age is between 15 and 16 years. Over the whole panel, which spans from 2000 to 2018, average levels of 3G exposure (as measured by the share of their population with 3G coverage) are roughly 0.58. This figure is larger—at 0.61—for the ICT sample in Panel B. Raw PISA scores are originally scaled to have a mean of 500 and standard deviation of 100, benchmarked against OECD performance when each subject was first tested as the major focus (reading in 2000, mathematics in 2003, and science in 2006). We standardize these scores by subtracting 500 and dividing by 100, yielding scores that represent deviations from the OECD benchmark in standard deviation units. The average standardized scores in our sample are approximately -0.3 in the full sample and -0.1 in the ICT sample, with these scores representing simple unweighted averages across students.

Both panels additionally summarize a set of variables related to homework, attendance, social connectedness, and well-being. As noted above, these questions were not administered universally across countries and PISA rounds; Appendix Figures E.3 to E.8 summarize annual missingness by country-by-urbanicity pair for variables for which full data is not available.

Panel B also includes measures of technology access and use. 53 percent of interviewed students browse the internet daily. Among students interviewed in 2012, 2015, and 2018, 90 percent had a smartphone at home, and the average reported weekly hours spent on the internet was 29.4, or roughly 4 hours per day.

2.3.2 Trends in Internet Usage and Scores Over Time

Figure 2 shows over-time trends among OECD countries for 3G coverage, test scores, and other measures of technology access and use.[11] After increasing between 2000 and 2009, student test scores in math, reading, and science declined between 2009 and 2018. These test score declines occurred after a rapid expansion in 3G coverage between 2003 and 2009, which accompanied large changes in students' reported internet access and use.

Our estimated 3G coverage at the country level for 2006 and 2018 is displayed in Figure 3 . In line with the global trends depicted in Figure 2, the top panel of Figure 3 shows that in 2006, only a small portion of the population in the sample countries had 3G access. By 2018, as shown in the bottom panel, nearly every country in our sample had over 75 percent of their population covered by 3G.

Establishing a causal link between technology use and test scores is challenging not only due to selection bias, but also because the observational patterns differ across different measures of technology use. To illustrate this, Appendix Table D.1 displays the observed relationship between test scores and various measures of technology use in PISA data from 2012, 2015, and 2018. These results demonstrate that the associative relationships between technology use and test scores vary substantially across different measures of technology use. Specifically, reported internet use in hours is associated with significantly lower test scores, while smartphone ownership and daily internet browsing are associated with higher scores. Our methodology, described in the section below, seeks to establish a causal link between 3G coverage and student skills and behavior. To do so, we leverage the global expansion in 3G coverage between 2000 and 2018 and assess the relationship between local 3G availability and technology use, test scores, and student behavior.

[11] For consistency with published OECD figures, the countries in Figure 2 are the OECD 23; the 23 countries with non-missing test scores in PISA exams from 2003 to 2022. These countries are those included in over-time statistics in the most recent OECD PISA Publications (2022). To construct the figure, we calculated country-level averages of each variable in each year, weighted by PISA sampling weights. Points in Figure 2 reflect the average of these average country-level values in each year.

3 Methodology

3.1 Difference-in-Differences

To measure the effect of 3G coverage on student-level outcomes, we use a difference-in-differences specification that compares differences in test scores across areas with and without 3G coverage over time. Our baseline two-way fixed effects ("TWFE") specification is

$$y_{icut} = \gamma 3G_{cut} + X_{icut}\theta + \phi_{cu} + \kappa_{ut} + \tau_{ct} + \varepsilon_{icut} \tag{1}$$

where i indexes students, c indexes countries, u indicates urbanicity, and t indexes years. As we lack information on each student's location in the PISA data, $3G_{cut}$ measures the average level of 3G coverage in a student's country-by-urbanicity area 12 months preceding the exam.[12]

We include fixed effects for country-by-urbanicity (ϕ_{cu}) which account for any time-invariant differences between urban and rural areas within each country. For example, urban areas may differ from rural areas in terms of access to infrastructure, quality of schooling, socioeconomic composition, or historical development levels. By including these fixed effects, we ensure that our estimates reflect within-country differences in test scores over time, rather than cross-country or cross-urbanicity differences.

Additionally, we include country-by-year fixed effects (τ_{ct}) which absorb any time-varying factors that affect all areas (urban and rural) within a country in the same year. For instance, national education reforms, changes in curriculum, fluctuations in education budgets, country-level macroeconomic shocks, or political events that influence educational outcomes systematically across urban and rural areas are accounted for. This ensures that the variation used to identify the effects of 3G arises from within-country differences between urban and rural areas rather than cross-country or global trends.

We also add urbanicity-by-year fixed effects (κ_{ut}) controls for time-varying factors that may affect urban and rural areas differently across all countries. Examples include demographic shifts and migration patterns (e.g., due to urbanization), and changes in economic opportunities in ur-

[12] While some countries in PISA use sampling based on subnational regions or states, the inconsistency in regional data across countries and time periods makes such geographic analysis impractical for this study. Therefore, we rely on PISA's school-level urbanicity classifications as our primary geographic indicator.

ban and rural areas.

The set of baseline controls, X_{icut}, include student gender, age, and immigration status, whether the student's school is public, private with government funding, or private without government funding, mother's and father's education level, and dummy variables identifying students with missing father's education, missing mother's education, missing immigration status, and missing school type.[13] In some specifications, we additionally interact student-level controls with country-by-urbanicity, allowing the effect of each variable to vary across different areas.

Our coefficient of interest, γ, is identified using variation in the timing of 3G coverage across areas within a country, that is, by comparing trends across areas that received 3G coverage relatively earlier versus those that received it later. This variation allows us to estimate the causal impact of 3G coverage under the parallel trends assumption: in the absence of 3G arrival, these areas would have followed similar trends in student outcomes. In other words, identification requires (i) the timing of the rollout of 3G coverage must be uncorrelated with other factors that could simultaneously influence student test scores, and (ii) the expansion of 3G coverage should not itself be driven by expectations of future changes in educational outcomes or any unobserved factors that may simultaneously affect both test scores and 3G availability. While these assumptions are not directly testable, we show evidence of parallel trends prior to 3G entry using event study estimates in the Appendix Figure B.2.

For all regressions, we cluster standard errors at the country-by-urbanicity level. In addition, to account for PISA's sampling regime, we weight each observation by $w_{ict} / \sum_{i \in ct} w_{ict}$, where w_{ict} is individual i in country c in year t's sampling weight, and $\sum_{i \in ct} w_{ict}$ denotes the sum of sampling weights in country c in year t.

3.2 Alternative Difference-in-Difference Estimators

Recently, many researchers have drawn attention to potential bias that arises in estimating two-way fixed effects models in the presence of staggered treatment and treatment effect heterogeneity (e.g. Goodman-Bacon, 2021; Sun and Abraham, 2021; De Chaisemartin and d'Haultfoeuille, 2020; Borusyak et al., 2024). In these settings, the use of two-way fixed effects models entails using already-treated units as controls for newly-treated ones, which generates bias if treatment effects

[13] When the father's or mother's education is missing, we replace these observations with the mean.

vary over time.

To ensure that our main results are not driven by biases arising from the typical two-way fixed effects estimator, we estimate dynamic and static specifications using the estimators introduced in Borusyak et al. (2024) and Gardner et al. (2023). Both of these estimators avoid comparisons between already-treated units and newly treated ones via an imputation procedure. Specifically, the methods use untreated units to first estimate group and period effects and then estimate treatment effects by comparing treated and untreated units after removing these group and period effects. This procedure allows for event-study as well as static treatment effect estimates. We describe our procedure and accompanying results in more detail in Appendix B.

3.3 Instrumental Variables Estimates

While our main (and preferred) estimates are those in the difference-in-differences specification in equation 1 above, we note that many prior studies have used instrumental variables estimation to estimate the effect of 3G coverage on various outcomes. In particular, two instruments have been used most extensively: local lightning strike frequency (Manacorda and Tesei, 2020; Chiplunkar and Goldberg, 2022; Guriev et al., 2021; Jiang et al., 2022) and prior 2G coverage (Harm Adema et al., 2022). The logic behind these two instruments is as follows. First, electrical surges caused by frequent lightning strikes increase the cost of installing and maintaining 3G equipment. Thus, ceteris paribus, areas with more frequent lightning strikes exhibited slower diffusion of 3G availability. In contrast, prior 2G coverage has been associated with faster expansion of 3G coverage. Prior infrastructure for 2G can be repurposed or shared with 3G infrastructure. Specifically, cell towers used for 2G can be shared by a 3G base transceiver station. Thus, the expansion of 3G coverage was less costly in areas with preexisting 2G coverage.

Even though these instruments are widely used in the literature, there are important concerns about their validity. As McKenzie (2024) notes, lightning strikes have been used as instruments for numerous types of infrastructure, including electricity, fixed telephone lines, computer infrastructure, internet, and various mobile network generations. This raises concerns that any estimated effects might operate through these other infrastructure channels rather than through 3G coverage specifically. Moreover, while individual lightning strikes are random, systematic variation in lightning intensity is predictable and may have influenced historical patterns of development and

urbanization. Similar concerns may arise concerning 2G coverage, as the placement of initial mobile infrastructure likely responded to local economic conditions that could persist over time and affect our outcomes of interest. Nevertheless, given the widespread use of these instruments and their documented first-stage relationship with 3G expansion, we present instrumental variables estimates as a complementary approach to our main difference-in-differences specification, while maintaining appropriate caution in their interpretation.

We provide additional details and results in Appendix C, noting that our data lacks geographical precision when compared to other papers that have used these instruments previously. As noted above, the data used in Manacorda and Tesei (2020) includes 10,409 unique geographic cells, and the data used in Guriev et al. (2021) includes 2,232 international subregions. By comparison, our main test score data includes 291 unique country-by-urbanicity combinations. Consequently, the relatively small number of geographic cells in our data renders our instrumental variables estimates less precise compared to studies with more granular geographical data.

4 Results

In this section, we first explore the relationship between 3G availability and technology access and use among adolescents. Then, we investigate how changes in 3G affect student test scores in math, reading, and science. We also test for heterogeneous effects across different groups of students as well as effects on social connectedness, mental well-being, homework, and absenteeism.

4.1 Effects on Technology Access and Use

Table 2 shows our TWFE estimates of the effect of 3G coverage on technology access and use. These results come from the ICT sample, which includes roughly 1.6 million student-level observations and amounts to slightly more than half of our full sample. Throughout, regressions in Column 1 include country-by-urbanicity, urbanicity-by-year, and country-by-year fixed effects, in addition to our set of baseline controls. Regressions in Column 2 additionally include interactions between all baseline controls and country-by-urbanicity fixed effects. Intuitively, the addition of these interaction terms allows the effect of student demographics (e.g. gender, age) to vary across geographies.

Panel A of Table 2 reports effects on the likelihood that students report browsing the internet

daily. Estimates indicate that 3G coverage is associated with a 4 percentage-point increase in the likelihood of daily internet browsing. These estimates are economically significant amounting to 7.5% of the sample mean of 0.53 (shown in Table 1).

Panels B and C of Table 2 show estimates of the effect of 3G on smartphone ownership. The PISA ICT questionnaire included questions about smartphone ownership starting in 2012. We test for effects on smartphone ownership in two ways. First, we exclude observations prior to 2012, for which there are no responses. The results, shown in Panel B, are very imprecise and statistically insignificant, but point estimates suggest that 3G coverage increases smartphone ownership by roughly 4 percentage points. However, these estimates omit data prior to 2012, the period in which most 3G expansions occurred. To allow us to include this data, we alternatively estimate the effects on smartphone ownership after assuming that none of the students participating in 2000 and 2003 had a smartphone. These estimates, shown in Panel C, are much larger and suggest that 3G coverage increases smartphone ownership by 10 percentage points.

Finally, Panel D of Table 2 shows estimates on weekly internet use, limiting the sample to the years for which this data is available: 2012, 2015, and 2018. Consistent with the effects described above, access to 3G increases the amount of time students report spending on the internet. Effect sizes are reasonably large across specifications, suggesting that 3G coverage increases time on the internet by 5 hours weekly or roughly 40 minutes daily. PISA data also allows us to distinguish between reported time spent online both in and out of school. Appendix Table D.2 reports results separately for these two measures, indicating that 3G is associated with a 4-hour increase in reported weekly internet use out of school. Effects on internet use in school are positive but not statistically significant.

Overall, these results suggest that 3G coverage accelerated ownership of smartphones and, in turn, internet use among adolescents. Next, we consider the effect of 3G access on human capital development, as measured by PISA test scores.

4.2 Effects on PISA Test Scores

4.2.1 Main Results

Table 3 shows TWFE estimates of the effect of 3G coverage on test scores. Panels A, B, and C report effects on math, reading, and science scores, respectively. Similar to the results in Table 2,

Columns 1 and 2 differ in their inclusion of interactions between all baseline controls and country-by-urbanicity fixed effects.

Across both specifications in all subjects, the estimated effects of 3G access on test scores are negative. Focusing on the effects in Column 2, which include the most expansive set of controls, we find that 3G coverage leads to reductions in math scores of roughly 0.07 standard deviations ($p < 0.01$). The estimates for reading and science show slightly smaller declines of 0.05 ($p < 0.05$) and 0.04 ($p < 0.1$) standard deviations, respectively. Our results are consistent with Vigdor et al. (2014), which also finds that the negative test score effects of high-speed internet access are larger in math (0.027 standard deviations, $p < 0.05$) than in reading (0.01 standard deviations, $p > 0.1$).

These effect sizes qualify as medium in size, according to the schema put forth by Kraft (2020). More concretely, Bloom et al. (2008) and Evans and Yuan (2019) find that a year of schooling typically increases test score performance by 0.3 and 0.2 standard deviations, respectively.[14] Our estimates fall between one-sixth and just over one-third of these estimates.

4.2.2 Test Score Effects in the ICT Sample

Our main test score results in Table 3 use our full sample. In Appendix Table D.3, we show that we obtain similar test score results if we restrict ourselves to using the ICT sample, that is, those students for whom we have measures of internet use and access.

4.3 Robustness

We subject our main estimates on technology access and use and test scores to a set of robustness checks. These are briefly described below.

4.3.1 Permutation Test

We employ a permutation test to assess the robustness of our estimated treatment effects. Specifically, we reassign 3G histories across countries-by-urbanicity pairs, estimate the effect of 3G on test scores, and compare our estimates to the distribution of these placebo estimates after completing this procedure 100 times. This procedure allows us to compare the distribution of these placebo

[14] Bloom et al. (2008) use nationally representative data from the United States. The 0.3 estimate refers to the effect of a year of schooling for 8th to 9th graders, who are typically 13 to 15 years old. Evans and Yuan (2019) use a sample of test scores from low- and middle-income countries.

estimates to our actual estimates, allowing us to assess the reliability of our estimates relative to those that would be produced under alternative scenarios.

We perform the placebo reassignments in two ways. First, we reassign 3G histories across all country-by-urbanicity pairs in our sample. For example, under this approach, the 3G history for towns in Italy could be assigned to large cities in the United States. Alternatively, we consider restricting these assignments to within levels of urbanicity. Under this approach, towns in Italy will only be matched to towns in our sample.

Our results are shown in Appendix Figure D.1. As can be seen in Appendix Figure D.1, our actual estimates for test scores fall well outside of the normal range of estimates under placebo 3G histories. Specifically, only 2 out of 600 placebo estimates (2 reassignment approaches times 3 subjects times 100 iterations) yield a coefficient estimate more negative than our actual estimate.

4.3.2 Alternative Difference-in-Difference Estimators

In Appendix B, we repeat our difference-in-differences analyses using difference-in-differences estimators proposed by Borusyak et al. (2024) and Gardner et al. (2023), which are robust to potential bias associated with two-way fixed effects estimation. (As described further in Appendix B, we perform these analyses on data collapsed at the country-by-urbanicity-by-year level to simplify computation.) With respect to test scores, our estimates are consistently negative and slightly larger than our difference-in-differences results.

These estimators allow us to estimate event studies, which show estimated treatment effects as a function of years since treatment. Importantly, these estimates do not exhibit pre-trends in test scores prior to 3G arrival; the introduction of 3G is not preceded by differential test score trends between treated and control groups.

4.3.3 Instrumental Variables Estimates

We describe our instrumental variables methodology and results in Appendix C. Broadly, our estimates for effects on technology access and use are similar in size but less precise than the difference-in-differences effects. With respect to test scores, our instrumental variables estimates are slightly larger— lying between -0.1 and -0.3 standard deviations—but much less precise: 90% confidence intervals include zero for all subjects.

4.4 Heterogeneity

Next, we explore treatment effect heterogeneity across different subsets of students. To do so, we fully interact our TWFE models with student, family, or other characteristics. Consistent with Feigenberg et al. (2023), this approach allows for the effect of 3G, as well as the effect of control variables and fixed effects, to vary across these subgroups. We explore three dimensions of heterogeneity: gender, parental education, and level of economic development.[15]

Appendix Tables D.4 and D.5 display our results concerning technology access and use, and test scores, respectively. With respect to gender and test scores, we find some suggestive evidence for a larger negative response among female students. Coefficients on 3G $\times$ Female interaction terms fall between 0 and -0.06, but are generally not statistically significant. While noisy, these results suggest that test scores of female students may exhibit more negative responses to 3G availability than their male counterparts.

On parental education, our results are also noisy, but suggest that students who have at least one parent with a tertiary education exhibit smaller test score declines in response to 3G coverage. In other words, test scores for students coming from less educated families are more negatively affected by the presence of 3G coverage. These effects raise the possibility that more highly educated families may be better equipped to shield their children from the negative effects of mobile internet technology.

Finally, we test whether students in high-income countries respond differently to the rollout of 3G than other students. Our set of high-income countries includes countries classified as high-income by the World Bank in 2000: 33 of the 82 countries in our sample are in this group. The results suggest that the negative effects of 3G on test scores are concentrated among non-high-income countries; the interaction terms on our high-income indicator are roughly equal and opposite-signed as our main coefficients. This could be driven by several factors. First, it is possible that these non-high-income countries are those for which 3G had significant effects on internet access and use, while high-income countries expanded internet access and use among adolescents before the arrival of 3G. Alternatively, these differences may capture differences in teachers' and

[15] PISA does not collect data on individual students' prior test scores, so we are unable to test for heterogeneity with respect to prior test scores.

parents' ability to utilize technology productively for educational purposes.[16] Finally, differences across countries could simply reflect better parental awareness of the potential downsides of internet connectivity and stronger supervision of technology use at home in high-income countries.[17]

5 Potential Mechanisms

We now examine possible mechanisms that could drive the negative effects by studying how 3G coverage affected students' social connectedness, well-being, homework hours, self-efficacy, and attendance.

5.1 Social Connectedness and Well-Being

Panels A, B, and C of Table 4 examine whether 3G coverage affects students' sense of social connectedness and well-being. We standardize two indicators of this—the Belonging Index, which captures students' perceptions of social integration at school, and the Friendship Index, which measures the ease of making friends—to have a mean of zero and a standard deviation of one. Across both the measures, the effects are uniformly negative and statistically significant at the 5% level, with effect sizes ranging from 0.09 to 0.16 standard deviations. As a reference point, Braghieri et al. (2022) find that Facebook's arrival on college campuses reduced a mental health index by 0.085 standard deviations. Fletcher (2010) finds significant effects of depressive symptoms on educational attainment, underscoring the link between mental well-being and student learning outcomes. These findings suggest that a decline in students' social connectedness and well-being may contribute to the observed test score declines.

5.2 Distraction and Productivity of Study Time

The arrival of mobile internet potentially changes both the opportunity cost of studying as well as the productivity of study time. Both of these effects may lead students to alter their homework time in response to the arrival of 3G coverage. Panel D of Table 4 tests whether changes in 3G coverage are associated with changes in time spent doing homework. The results suggest that 3G coverage is associated with increases in homework time of about 20 minutes per week (statistically

[16] For instance, in the United Kingdom, MyMaths, an online platform for students to practice math problems and commonly used in classrooms, was launched in 1999.

[17] For example, Ramey and Ramey (2010) document large increases in time spent on childcare among parents in the United States and Canada in the mid-1990s.

significant at the 10% level).

At first glance, this finding may seem surprising and counterintuitive given the negative impact of 3G on test scores. However, this pattern could be explained by the rise in digital distractions: as previously discussed, we find that out-of-school internet browsing increases by roughly four hours per week. Therefore, while students may be allocating more time to homework, they could be studying less effectively due to frequent multitasking or interruptions.

Moreover, the decline in test scores is most pronounced among students who browse more, particularly those from non-high-income countries and students whose parents do not have a college education (Appendix Tables D.4 and D.5). These findings align with recent evidence on the importance of cognitive endurance (Brown et al., 2022) and the adverse effects of digital distractions on learning efficiency (Allcott et al., 2022).

5.3 Self-Efficacy

Given the observed negative effects of 3G on social connectedness and well-being, as well as the potential for negative social comparison, we next investigate whether perceived academic self-efficacy—students' confidence in their ability to complete school-related tasks—is affected by 3G coverage. In Panel C of Table 4, we find that academic self-efficacy is also negatively associated with 3G coverage but this effect is not statistically significant.

5.4 Other School Engagement Related Factors

We find little evidence that 3G affects school attendance. Panel E of Table 4 analyzes how 3G affects school absenteeism. Results suggest that students are slightly more likely to have skipped nearly 0.03 of a school day in the past two weeks, which amounts to about 18 minutes per month. Using estimates from Lavy (2015), where each additional hour of instructional time per week raises test scores by 0.06 standard deviations, we estimate that missed school days could only explain around 4-6% of our observed declines in Math, Reading, and Science scores (ranging from 0.04 to 0.08 standard deviations; see Appendix Table D.10). Relatedly, Panel F shows that school administrators were 3 percentage points more likely to report that student absenteeism hindered learning "A lot" after the arrival of 3G. However, both these estimates are not statistically significant. Therefore, the decline in test scores is unlikely due to increased student absenteeism.

5.5 Heterogeneity in Mechanisms

Finally, we examine whether these mechanisms differ by student demographics (Appendix Tables D.6 and D.7). However, we find no strong evidence of systematic heterogeneity in how 3G impacts social connectedness, well-being, or academic behaviors across different student subgroups.

6 Conclusion

The proliferation of information technology has rapidly changed the lives of adolescents worldwide. Do these changes improve or hamper learning? Public discourse surrounding this question spans a large spectrum. Proponents argue that information technology can expand access to information and educational resources, potentially enhancing student learning. Conversely, others express concerns about the adverse effects of these technologies on sleep, mental health, and other aspects of students' lives, which could impede skill development. The evidence presented here suggests that these concerns about the impact of internet use on student achievement are not unwarranted; leveraging test score data from 82 countries, we show that the expansion of internet access via 3G coverage is causally associated with reductions in student achievement.

PISA remains the largest and most comprehensive international assessment, enabling analysis of educational outcomes, student behavior, and access to technology globally. Even though this paper specifically examines the impacts of internet expansion in PISA countries, which do not include South Asia, China, and sub-Saharan Africa, this paper's findings are of broader significance for global policymaking. Most directly, the study provides valuable insights into how increasing mobile internet penetration and use affect educational outcomes among adolescents, particularly in high- and middle-income countries where the introduction of 3G mobile internet enabled constant connectivity. Beyond these contexts, understanding the implications of greater availability and use of mobile internet is crucial for designing policies suited to low-income settings, where internet infrastructure and mobile internet penetration are in the process of expansion. In such environments, the introduction of 3G—or other high-speed mobile internet technologies—could have an even more transformative effect by substantially increasing access to the internet as well as improving the connectivity to the internet. As such, this study offers important and timely insights for policymakers in low- and middle-income countries (LMICs) striving to bridge the

digital divide and ensure equitable benefits from mobile internet expansion.

Our results warrant many avenues for future research; we highlight two in particular. First, given that technology plays a complex and changing role in adolescents' lives, future research can help to understand potential mechanisms and circumstances that allow technology to augment, rather than hamper student learning. Second, we note that the set of skills measured in PISA exams is limited to math, reading, and science, and our set of behavioral and mental health measures is quite limited. Future research that quantifies the myriad of skills and factors that contribute to students' overall well-being and success in the modern world will be extremely valuable.

References

Adema, Joop Age Harm, Cevat Giray Aksoy, and Panu Poutvaara, "Mobile internet access and the desire to emigrate," 2022.

Al Jazeera, "Why Are US States, School Districts Banning Smartphones in Schools?," 2024. Accessed: 2024-07-06.

Allcott, Hunt, Matthew Gentzkow, and Lena Song, "Digital addiction," *American Economic Review*, 2022, *112* (7), 2424–2463.

BBC News, "The debate: Should smartphones be banned for under 16s?," 2024. Accessed: 2024-11-28.

_ , "Fight begins to make mobile-free schools law," 2024. Accessed: 2024-11-28.

Bessone, Paul, Ricardo Dahis, and Lisa Ho, "The impact of 3G mobile internet on educational outcomes in Brazil," 2020.

Beuermann, Diether W, Julian Cristia, Santiago Cueto, Ofer Malamud, and Yyannu Cruz-Aguayo, "One laptop per child at home: Short-term impacts from a randomized experiment in Peru," *American Economic Journal: Applied Economics*, 2015, *7* (2), 53–80.

Billari, Francesco C, Osea Giuntella, and Luca Stella, "Broadband internet, digital temptations, and sleep," *Journal of Economic Behavior Organization*, 2018, *153*, 58–76.

Bloom, Howard S, Carolyn J Hill, Alison Rebeck Black, and Mark W Lipsey, "Performance trajectories and performance gaps as achievement effect-size benchmarks for educational interventions," *Journal of Research on Educational Effectiveness*, 2008, *1* (4), 289–328.

Borusyak, Kirill, Xavier Jaravel, and Jann Spiess, "Revisiting event-study designs: robust and efficient estimation," *Review of Economic Studies*, 2024, p. rdae007.

Braghieri, Luca, Ro'ee Levy, and Alexey Makarin, "Social media and mental health," *American Economic Review*, 2022, *112* (11), 3660–3693.

Brown, Christina L, Supreet Kaur, Geeta Kingdon, and Heather Schofield, "Cognitive endurance as human capital," Technical Report, National Bureau of Economic Research 2022.

Bursztyn, Leonardo, Benjamin R Handel, Rafael Jimenez, and Christopher Roth, "When product markets become collective traps: The case of social media," Technical Report, National Bureau of Economic Research 2023.

Cecil, D. J., "LIS/OTD Gridded Lightning Climatology Data Collection," 2015.

Chaisemartin, Clément De and Xavier d'Haultfoeuille, "Two-way fixed effects estimators with heterogeneous treatment effects," *American Economic Review*, 2020, *110* (9), 2964–2996.

Chiplunkar, Gaurav and Pinelopi Koujianou Goldberg, "The Employment Effects of Mobile Internet in Developing Countries," Report, National Bureau of Economic Research 2022.

CIESIN, "Gridded Population of the World, Version 4 (GPWv4): Population Density, Revision 11," 2018.

Cunha, Flavio and James Heckman, "The technology of skill formation," *American economic review*, 2007, *97* (2), 31–47.

Donati, Dante, Ruben Durante, Francesco Sobbrio, and Dijana Zejcirovic, "Lost in the net? Broadband Internet and youth mental health," 2022.

Escueta, Maya, Andre Joshua Nickow, Philip Oreopoulos, and Vincent Quan, "Upgrading education with technology: Insights from experimental research," *Journal of Economic Literature*, 2020, *58* (4), 897–996.

Evans, David and Fei Yuan, "Equivalent years of schooling: A metric to communicate learning gains in concrete terms," *World Bank Policy Research Working Paper*, 2019, (8752).

Fairlie, Robert W and Jonathan Robinson, "Experimental evidence on the effects of home computers on academic achievement among schoolchildren," *American Economic Journal: Applied Economics*, 2013, *5* (3), 211–240.

Falck, Oliver, Robert Gold, and Stephan Heblich, "E-lections: Voting Behavior and the Internet," *American Economic Review*, 2014, *104* (7), 2238–2265.

Feigenberg, Benjamin, Ben Ost, and Javaeria A Qureshi, "Omitted variable bias in interacted models: A cautionary tale," *Review of Economics and Statistics*, 2023, pp. 1–47.

Fletcher, Jason M, "Adolescent depression and educational attainment: results using sibling fixed effects," *Health economics*, 2010, *19* (7), 855–871.

Florczyk, A., C. Corbane, M. Schiavina, M. Pesaresi, L. Maffenini, M. Melchiorri, P. Politis, F. Sabo, S. Freire, D. Ehrlich, T. Kemper, P. Tommasi, D. Airaghi, and L. Zanchetta, "GHS Urban Centre Database 2015, multitemporal and multidimensional attributes, R2019A," 2019.

France 24, "Australia Passes World's First Social Media Ban for Under-16s," 2024. Accessed: 2024-11-28.

Ganimian, Alejandro J, Emiliana Vegas, and Frederick M Hess, "Realizing the promise: How can education technology improve learning for all," *Center for Universal Education at Brookings*, 2020.

Gardner, John, Neil Thakral, Linh T Tô, and Luther Yap, "Two-Stage Differences in Differences," 2023.

Gentzkow, Matthew and Jesse M Shapiro, "Preschool television viewing and adolescent test scores: Historical evidence from the Coleman study," *The Quarterly Journal of Economics*, 2008, *123* (1), 279–323.

Geraci, Andrea, Mattia Nardotto, Tommaso Reggiani, and Fabio Sabatini, "Broadband internet and social capital," *Journal of Public Economics*, 2022, *206*, 104578.

Golin, Marta, "The effect of broadband internet on the gender gap in mental health: Evidence from germany," *Health Economics*, 2022, *31*, 6–21.

_ **and Alessio Romarri**, "Broadband Internet and Attitudes Towards Migrants: Evidence from Spain," 2022.

Goodman-Bacon, Andrew, "Difference-in-differences with variation in treatment timing," *Journal of Econometrics*, 2021, *225* (2), 254–277.

Guardian, The, "New York Lawmakers Introduce Smartphone Ban Bill for Schools," *The Guardian*, 2024. Accessed: 2025-02-10.

Guriev, Sergei, Nikita Melnikov, and Ekaterina Zhuravskaya, "3g internet and confidence in government," *The Quarterly Journal of Economics*, 2021, *136* (4), 2533–2613.

Hjort, Jonas and Jonas Poulsen, "The arrival of fast internet and employment in Africa," *American Economic Review*, 2019, *109* (3), 1032–1079.

Jiang, Erica Xuewei, Gloria Yang Yu, and Jinyuan Zhang, "Bank Competition amid Digital Disruption: Implications for Financial Inclusion," *Available at SSRN 4178420*, 2022.

Kraft, Matthew A, "Interpreting effect sizes of education interventions," *Educational Researcher*, 2020, *49* (4), 241–253.

Lavy, Victor, "Do differences in schools' instruction time explain international achievement gaps? Evidence from developed and developing countries," *The Economic Journal*, 2015, *125* (588), F397–F424.

Malamud, Ofer and Cristian Pop-Eleches, "Home computer use and the development of human capital," *The Quarterly journal of economics*, 2011, *126* (2), 987–1027.

_ **, Santiago Cueto, Julian Cristia, and Diether W Beuermann**, "Do children benefit from internet access? Experimental evidence from Peru," *Journal of Development Economics*, 2019, *138*, 41–56.

Manacorda, Marco and Andrea Tesei, "Liberation technology: Mobile phones and political mobilization in Africa," *Econometrica*, 2020, *88* (2), 533–567.

McKenzie, David, "Are lightning strikes the new rainfall IV?," https://blogs.worldbank.org/en/impactevaluations/are-lightning-strikes-the-new-rainfall-iv July 2024. Accessed: 2024-11-19.

Melnikov, Nikita, "Mobile internet and political polarization," *Available at SSRN 3937760*, 2021.

Muralidharan, Karthik, Abhijeet Singh, and Alejandro J Ganimian, "Disrupting education? Experimental evidence on technology-aided instruction in India," *American Economic Review*, 2019, *109* (4), 1426–1460.

OECD, "Development of Third-Generation Mobile Services in the OECD," Technical Report 85, OECD Publishing, Paris 2004.

_ , "Education at a Glance 2022:Indicators, OECD," *URL: https://www.oecd-ilibrary.org/education/education-at-a-glance-2023*$_8$*008b7a*1 − *en*, 2023.

Palsson, Craig, "Smartphones and child injuries," *Journal of Public Economics*, 2017, *156*, 200–213.

Pritchett, Lant, "The Future of Government: Basic Education and the (limited) role of technology in complex human endeavor," 2022. Published on Governance for Development.

Ramey, Garey and Valerie A Ramey, "The Rug Rat Race," *Brookings Papers on Economic Activity*, 2010, *2010* (1), 129–176.

Rodriguez-Segura, Daniel, "EdTech in developing countries: A review of the evidence," *The World Bank Research Observer*, 2022, *37* (2), 171–203.

Sun, Liyang and Sarah Abraham, "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects," *Journal of Econometrics*, 2021, *225* (2), 175–199.

The Boston Globe, "Should Cellphones Be Allowed in Schools?," *The Boston Globe*, 2024. Accessed: 2025-02-10.

The Economist, "Technology is transforming what happens when a child goes to school," 2017.

The New York Times, "California Moves to Ban Smartphones in Schools to Curb Distractions," *The New York Times*, 2024. Accessed: 2025-02-10.

UNESCO, "UNESCO Calls for Global Ban on Smartphones in Schools," 2024. Accessed: 2024-11-28.

UNICEF, "Growing Up in a Connected World Understanding Children's Risks and Opportunities in a Digital Age," *URL: https://www. unicef-irc. org/growing-up-connected*, 2019.

United Nations, "Digital Inclusion for a Sustainable Future: Summary of the Report of the UN Secretary-General's High-Level Panel on Digital Cooperation," 2019.

Vigdor, Jacob L, Helen F Ladd, and Erika Martinez, "Scaling the digital divide: Home computer technology and student achievement," *Economic inquiry*, 2014, *52* (3), 1103–1119.

Main Tables and Figures

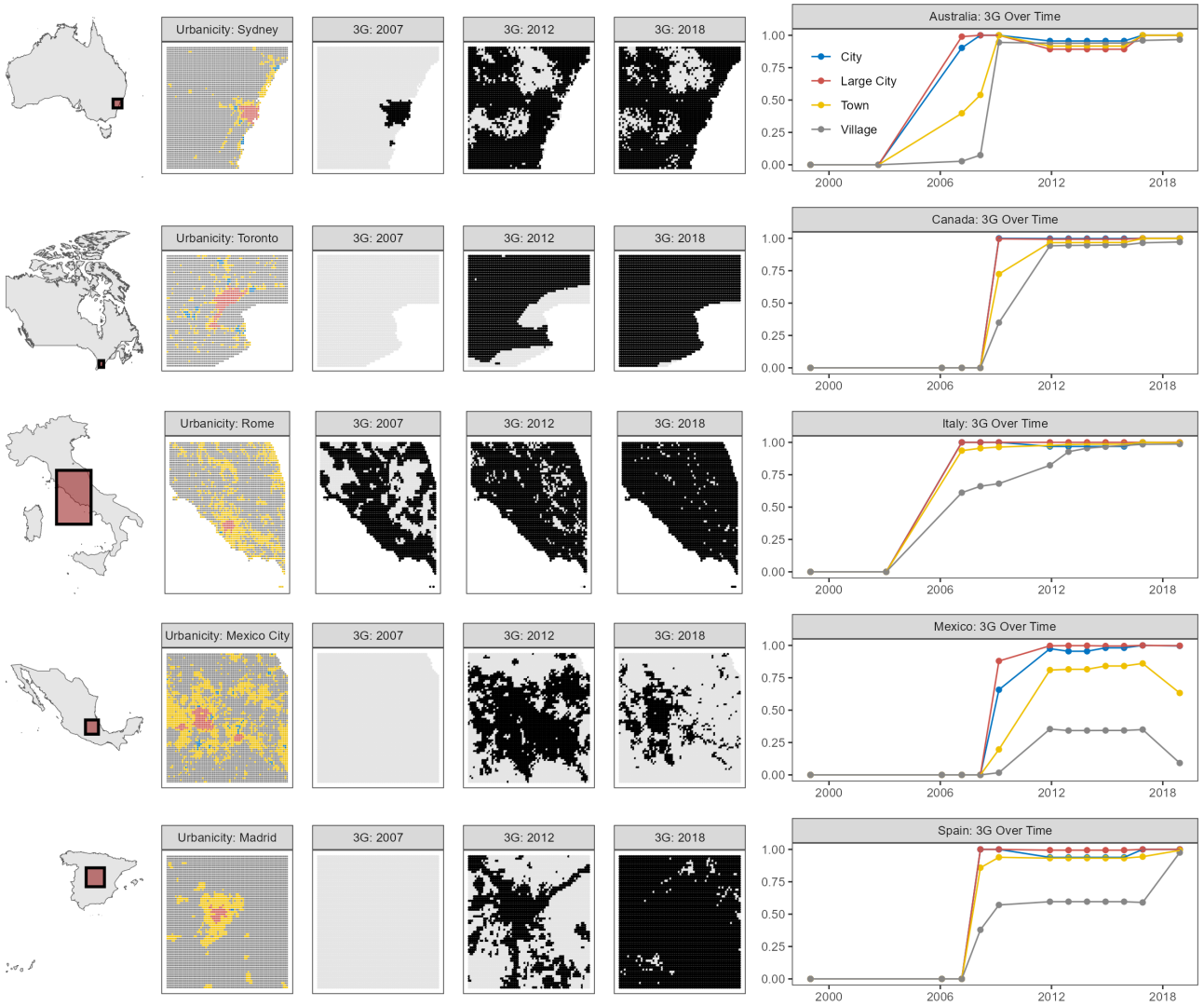


Figure 1: 3G Calculation Illustration

Note: Figure summarizes the calculations for 3G expansion in five countries: Australia, Canada, Italy, Mexico, and Spain. The maps show urbanicity and 3G coverage in 2007, 2012, and 2018 for each country's largest urban area. The line plots illustrate 3G coverage trends across different urbanicity levels at the national level.

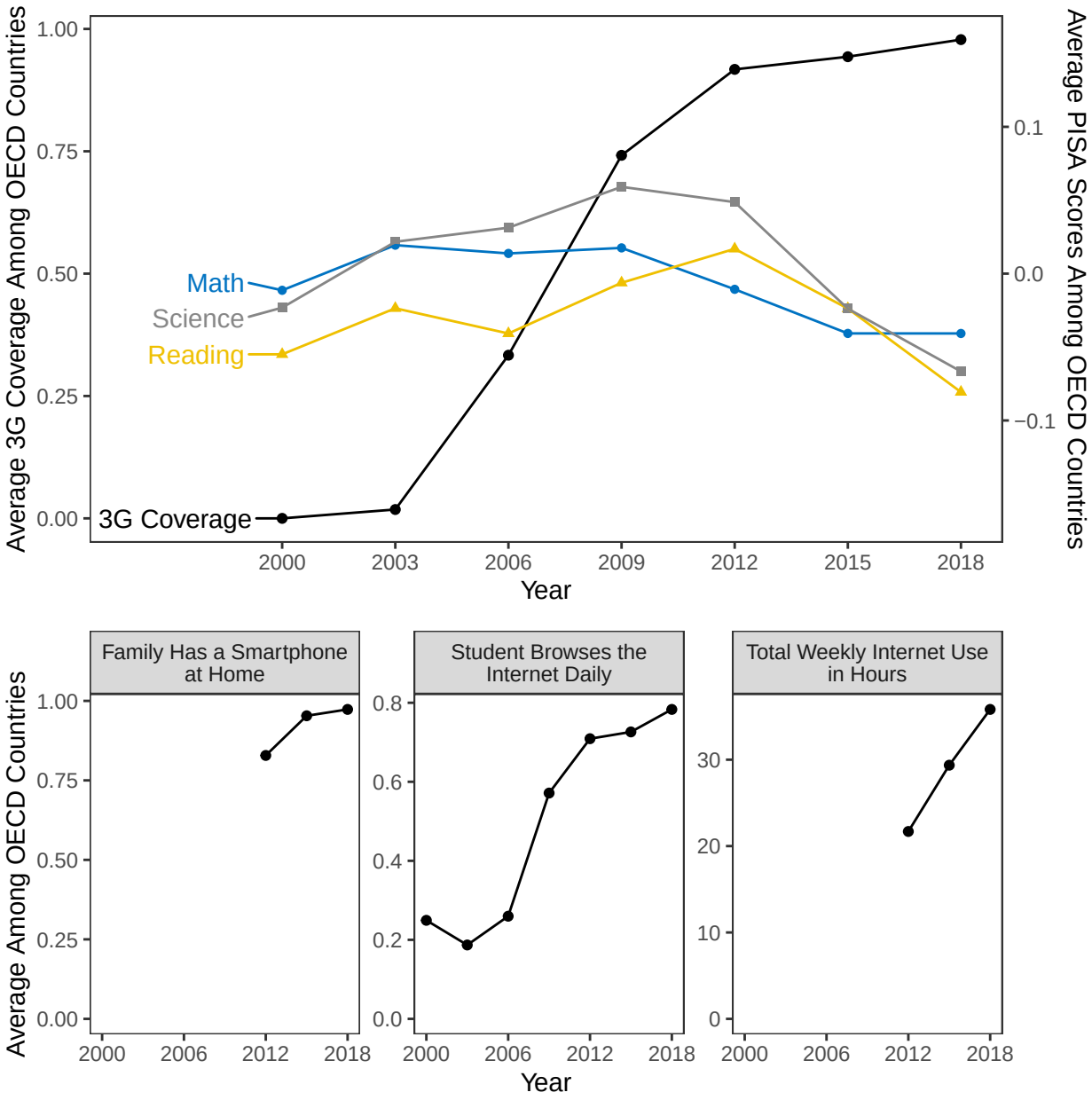
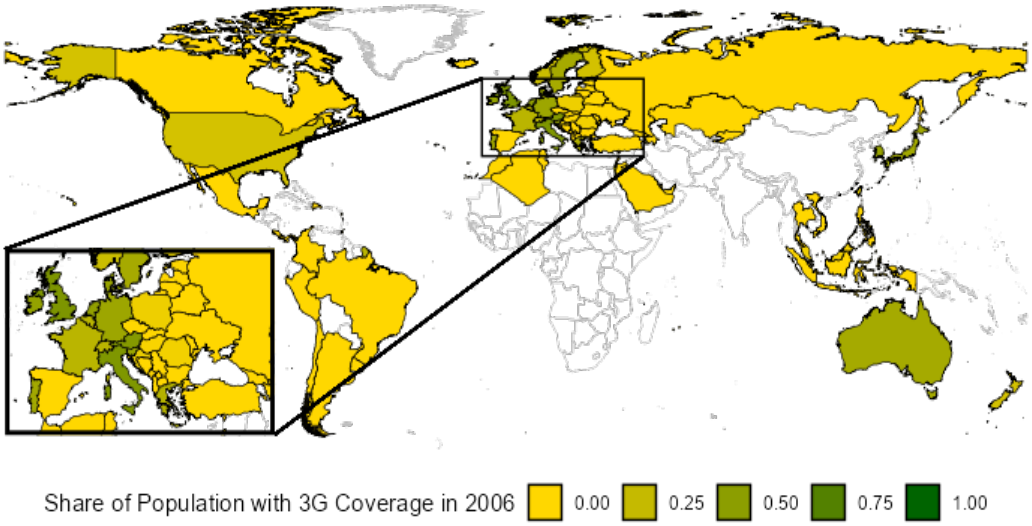


Figure 2: Trends in 3G Coverage, Test Scores, and Internet Access and Use in OECD Countries

Note: Figure displays trends in internet access and use in PISA countries. Sample countries are the OECD 23; the 23 countries with non-missing test scores in PISA exams from 2003 to 2022 and the set of countries used in most recent OECD PISA publications. Plotted points show averages of country-level values for each year in the sample.



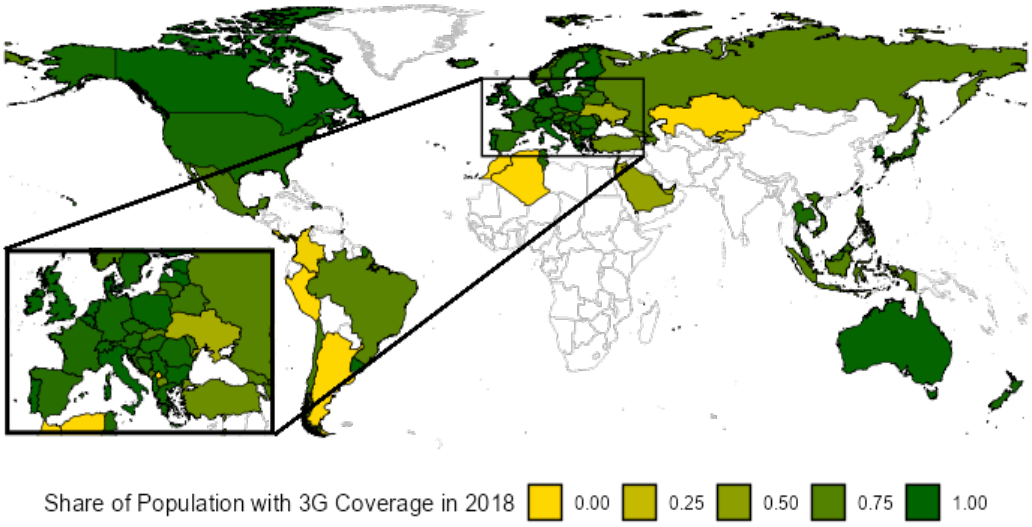


Figure 3: Country-Level 3G Coverage in 2006 and 2018

Note: Figure displays the estimated country-level share of the population with 3G coverage in 2006 and 2018.

Table 1: Summary Statistics

Statistic	N	Mean	St. Dev.
Panel A: Full Sample			
Male	2,600,134	0.496	0.500
Age	2,600,134	15.785	0.291
Is Immigrant	2,600,134	0.050	0.219
Father's Years of Education	2,600,134	13.059	4.138
Mother's Years of Education	2,600,134	13.022	4.251
School is Private, Independent	2,600,134	0.071	0.257
School is Private, Non-Independent	2,600,134	0.088	0.283
Weekly Homework Hours (2000-2006, 2012)	886,279	6.385	5.379
Days Skipped Past 2 Weeks (2012-2018)	1,251,516	0.827	1.476
Skipping Hinders Learning A Lot (2000-2003, 2009-2015)	2,192,073	0.076	0.265
Friendship Index (2000-2003, 2012-2018)	1,473,535	0.003	0.998
Belonging Index (2000-2003, 2012-2018)	1,482,513	−0.026	0.984
Self-Efficacy Index (2000-2006, 2012-2015)	1,340,914	−0.007	1.064
Math Score	2,600,134	−0.351	0.997
Reading Score	2,600,134	−0.362	1.018
Science Score	2,600,134	−0.309	0.992
Share of Population with 3G Coverage	2,600,134	0.575	0.424
Panel B: ICT Sample			
Male	1,595,699	0.494	0.500
Age	1,595,699	15.783	0.292
Is Immigrant	1,595,699	0.047	0.212
Father's Years of Education	1,595,699	13.281	3.883
Mother's Years of Education	1,595,699	13.327	3.942
School is Private, Independent	1,595,699	0.055	0.228
School is Private, Non-Independent	1,595,699	0.109	0.311
Weekly Homework Hours (2000-2006, 2012)	618,233	6.381	5.345
Days Skipped Past 2 Weeks (2012-2018)	790,297	0.749	1.395
Skipping Hinders Learning A Lot (2000-2003, 2009-2015)	1,327,988	0.070	0.255
Friendship Index (2000-2003, 2012-2018)	940,620	−0.010	0.989
Belonging Index (2000-2003, 2012-2018)	945,772	0.0001	1.002
Self-Efficacy Index (2000-2006, 2012-2015)	894,549	0.013	1.054
Math Score	1,595,699	−0.118	0.945
Reading Score	1,595,699	−0.135	0.950
Science Score	1,595,699	−0.086	0.942
Share of Population with 3G Coverage	1,595,699	0.614	0.421
Browses the Internet Daily	1,595,699	0.530	0.499
Has a Smartphone at Home (2012-2018)	838,077	0.899	0.301
Total Weekly Internet Use in Hours (2012-2018)	838,077	29.439	22.436

Note: Table displays summary statistics for PISA data. Panel A displays summary statistics for the full PISA sample, which includes all student observations for which data is available for main testing and control variables. Panel B displays summary statistics for the ICT sample, which additionally requires that observations have non-missing responses to the main ICT variables.

Table 2: TWFE Estimates: Effect of 3G on Technology Access and Use

	(1)	(2)
Panel A: Browses the Internet Daily		
3G	0.035*	0.039**
	(0.019)	(0.018)
Num.Obs.	1595699	1595699
R2	0.268	0.275
Panel B: Has a Smartphone at Home (2012-2018)		
3G	0.037	0.043
	(0.037)	(0.038)
Num.Obs.	838077	838077
R2	0.124	0.133
Panel C: Has a Smartphone at Home (2012-2018; 2000, 2003 Set to 0)		
3G	0.105**	0.104**
	(0.048)	(0.048)
Num.Obs.	1067892	1067892
R2	0.800	0.802
Panel D: Total Weekly Internet Use in Hours (2012-2018)		
3G	5.130***	4.882**
	(1.947)	(1.917)
Num.Obs.	838077	838077
R2	0.130	0.147
Country-by-Urbanicity FEs	✓	✓
Urbanicity-by-Year FEs	✓	✓
Country-by-Year FEs	✓	✓
Baseline Controls	✓	✓
Controls Interacted with Country-by-Urbanicity		✓

Note: Table displays TWFE results estimating the effect of 3G coverage on technology access and use. Dependent variables are indicated in panel labels. Baseline controls include student gender, age, immigrant status, fixed effects for private (with government funding) and private (without government funding) school attendance, mother's and father's education level, and dummy variables identifying students with missing father's education, missing mother's education, missing immigration status, and missing school type. Robust standard errors clustered at the country-by-urbanicity level in parentheses. Observations are weighted by $w_{ict} / \sum_{i \in ct} w_{ict}$, where w_{ict} is individual i in country c in year t's sampling weight, and $\sum_{i \in ct} w_{ict}$ denotes the sum of sampling weights in country c in year t. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: TWFE Estimates: Effect of 3G on PISA Test Scores

	(1)	(2)
Panel A: Math		
3G	-0.076***	-0.073***
	(0.025)	(0.025)
Num.Obs.	2600134	2600134
R2	0.385	0.412
Panel B: Reading		
3G	-0.045*	-0.047**
	(0.024)	(0.024)
Num.Obs.	2600134	2600134
R2	0.361	0.388
Panel C: Science		
3G	-0.047**	-0.041*
	(0.024)	(0.023)
Num.Obs.	2600134	2600134
R2	0.342	0.372
Country-by-Urbanicity FEs	✓	✓
Urbanicity-by-Year FEs	✓	✓
Country-by-Year FEs	✓	✓
Baseline Controls	✓	✓
Controls Interacted with Country-by-Urbanicity		✓

Note: Table displays TWFE results estimating the effect of 3G coverage on test scores. Dependent variables are scaled student test scores in math (Panel A), reading (Panel B), and science (Panel C). Baseline controls include student gender, age, immigrant status, fixed effects for private (with government funding) and private (without government funding) school attendance, mother's and father's education level, and dummy variables identifying students with missing father's education, missing mother's education, missing immigration status, and missing school type. Robust standard errors clustered at the country-by-urbanicity level in parentheses. Observations are weighted by $w_{ict} / \sum_{i \in ct} w_{ict}$, where w_{ict} is individual i in country c in year t's sampling weight, and $\sum_{i \in ct} w_{ict}$ denotes the sum of sampling weights in country c in year t. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table 4: TWFE Estimates: Effect of 3G on Student Attitudes and Behaviors

	(1)	(2)
Panel A: Friendship Index (2000-2003, 2012-2018)		
3G	-0.145***	-0.152***
	(0.028)	(0.030)
Num.Obs.	1473535	1473535
R2	0.041	0.048
Panel B: Belonging Index (2000-2003, 2012-2018)		
3G	-0.067**	-0.089**
	(0.034)	(0.035)
Num.Obs.	1482513	1482513
R2	0.055	0.064
Panel C: Self-Efficacy Index (2000-2006, 2012-2015)		
3G	-0.056	-0.048
	(0.036)	(0.037)
Num.Obs.	1340914	1340914
R2	0.071	0.084
Panel D: Weekly Homework Hours (2000-2006, 2012)		
3G	0.300*	0.365*
	(0.178)	(0.195)
Num.Obs.	886279	886279
R2	0.161	0.177
Panel E: Days Skipped Past 2 Weeks (2012-2018)		
3G	0.014	0.026
	(0.084)	(0.081)
Num.Obs.	1251516	1251516
R2	0.127	0.139
Panel F: Skipping Hinders Learning A Lot (2000-2003, 2009-2015)		
3G	0.026	0.027
	(0.018)	(0.018)
Num.Obs.	2192073	2192073
R2	0.110	0.127
Country-by-Urbanicity FEs	✓	✓
Urbanicity-by-Year FEs	✓	✓
Country-by-Year FEs	✓	✓
Baseline Controls	✓	✓
Controls Interacted with Country-by-Urbanicity		✓

Note: Table displays TWFE results estimating the effect of 3G coverage on student attitudes and behaviors. Dependent variables in Panels A, B, and C are indices with a mean of 0 and a standard deviation of 1. Baseline controls include student gender, age, immigrant status, fixed effects for private (with government funding) and private (without government funding) school attendance, mother's and father's education level, and dummy variables identifying students with missing father's education, missing mother's education, missing immigration status, and missing school type. Robust standard errors clustered at the country-by-urbanicity level in parentheses. Observations are weighted by $w_{ict} / \sum_{i \in ct} w_{ict}$, where w_{ict} is individual i in country c in year t's sampling weight, and $\sum_{i \in ct} w_{ict}$ denotes the sum of sampling weights in country c in year t. These variables are not available for all countries in all years; see Appendix Figures E.6, E.7, E.8, E.3 E.4 and E.5. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A PISA Variables Construction

In this Appendix, we describe our process of quantizing and harmonizing variables across different PISA rounds. We use the `EdSurvey` package in `R` to import and append PISA data.

A.1 Mother's and Father's Years of Education

PISA data records the 1997 ISCED code of the highest level of education for each student's mother and father. We convert ISCED categories into years of education, assuming that ISCED code 0 (pre-primary education) corresponds to 0 years, ISCED code 1 (primary education) corresponds to 6 years, ISCED code 2 (lower secondary education) corresponds to 9 years, ISCED codes 3 (upper secondary education) and 4 (post-secondary non-tertiary education) correspond to 13 years, and ISCED codes 5 (first stage of tertiary education) and 6 (second stage of tertiary education) correspond to 17 years. We use this grouping because some rounds of PISA group these ISCED codes together.

A.2 Test Scores

PISA test scores are reported as plausible values, which provide a range of scores that are consistent with the observed responses. For simplicity, we use the average of plausible values for all subjects.[18]

A.3 Internet Browsing Frequency

Between 2000 and 2018, PISA asked students various questions about student internet use. For each year, we identify the question that comes closest to gauging the respondent's frequency of internet use. These questions are listed below, separately by year.

- **2000**: How often do you read these materials because you want to: [...] Emails and Web pages
- **2003** How often do you use: [...] the Internet to look up about people, things, or ideas?
- **2006**: How often do you use computers for the following reasons? [...] Browse the Internet for information about people, things, or ideas
- **2009**: How often do you use a computer for following activities at home? [...] Browse the Internet for fun (such as watching videos, e.g. <YouTube™>)
- **2012**: How often do you use a computer for the following activities outside of school? [...] Browsing the Internet for fun (such as watching videos, e.g. <YouTube™>).
- **2015**: How often do you use digital devices for the following activities outside of school? [...] Browsing the Internet for fun (such as watching videos, e.g. <YouTube™>).

[18] We note that the number of reported plausible values varies from year to year. Prior to 2015, 5 plausible values were reported. In 2015 and later, 10 plausible values were reported. We average across all available plausible values.

- **2018**: How often do you use digital devices for the following activities outside of school? [...] Browsing the Internet for fun (such as watching videos, e.g. <YouTube™>).

For simplicity, we measure these responses in the form of a binary indicator identifying whether the respondent uses the internet daily. In one year, 2000, the PISA questionnaire did not include "daily" as a response. In this case, we code responses of "several times a week" as equal to one.

A.4 Length of Weekly Internet Use

In 2012, 2015, and 2018, PISA's ICT questionnaire asked students the following questions:

- During a typical weekday, for how long do you use the Internet at school?
- During a typical weekday, for how long do you use the Internet outside of school?
- On a typical weekend day, for how long do you use the Internet outside of school?

For each question, students choose between the following responses:

- No time
- 1-30 minutes per day
- 31-60 minutes per day
- Between 1 and 2 hours per day
- Between 2 and 4 hours per day
- Between 4 hours and 6 hours per day
- More than 6 hours per day

We calculate the total amount of time associated with each response as the mid-point between the upper and lower bounds. For example, we assume that "Between 1 and 2 hours per day" corresponds to 90 minutes. We additionally assume that "More than 6 hours per day" corresponds to 8 hours. Finally, we calculate the average amount of time spent weekly according to the equation below.

$$\begin{aligned}\text{Avg. Weekly Internet} =& 5 * \text{Weekday Int. Time} + 2 * \text{Weekend Int. Time} \\ =& 5(\text{Weekday School Int. Time + Weekday Non-School Int. Time}) \\ &+ 2(\text{Weekend Int. Time})\end{aligned}$$

A.5 Homework Time

PISA questionnaires in 2000, 2003, 2006, 2012, and 2015 asked about weekly time spent on homework. In 2003, 2012, and 2015, questionnaires asked about total homework time in hours and allowed students to respond freely. In other years, PISA asked about total homework time separately by subject (math, reading, science, other) and gave students a set of time ranges (e.g. “Less than 1 hour per week," “1 to 3 hours per week," “More than 3 hours per week") from which to choose. In these instances, we convert these values based on the midpoint of the range (assuming more than 3 hours corresponds to 4 hours) and sum across subjects. This means that, in some years, the largest possible value for student homework time is 12 hours. For consistency, we recode responses above 12 hours per week to be equal to 12 hours per week. Finally, we exclude data from 2015 due to extreme values: many students report spending more than 40 hours per week on homework and in some cases as much as 70 hours.

A.6 Absenteeism

In 2012, 2015, and 2018, PISA questionnaires asked students how frequently they skipped a whole day or some of a day of school over the past two weeks. Eligible responses were “None," “One or two times," “Three or four times," “Five or more times." We convert this to a numeric value by taking the midpoint of each range and assuming “Five or more times" corresponds to 5. Finally, we calculate the total number of school days missed as the sum of ‘whole days skipped’ and one-half times ‘some days skipped’.

In all rounds except 2006, the school administrators were also asked "In your school, to what extent is the learning of students hindered by the following phenomena? — Students Skipping Classes. The responses were: ’Not at all’, ‘A little’, ‘To some extent’, and ‘A lot’. We report the effect of 3G arrival on their likelihood of reporting that skipping classes hinders learning (a) ‘A lot’ and (b) ‘To some extent’, or ‘A lot’.

A.7 Social Connectedness and Well-being Measures

Some rounds of PISA data include two constructed indices—a “belonging" index and a “self-efficacy" index—that summarize responses to a number of mental health-related questions. The belonging index captures student responses to statements about feeling like an outsider, making friends easily at school, feeling a sense of belonging, feeling awkward and out of place, and how well-liked the student feels by other students. The self-efficacy index is available periodically for different subjects over time, with slightly different definitions. As an example, in 2012, the OECD measured self-efficacy in math as “the extent to which students believe in their own ability to solve specific mathematics tasks." We harmonize this index over time by taking overall self-efficacy in 2000, math self-efficacy in 2003 and 2012, and science self-efficacy in 2006 and 2015. Finally, many rounds of PISA asked students the degree to which they agreed with the statement “I make friends easily at school." Eligible responses were “strongly agree," "agree," “disagree," and “strongly disagree." We transform these responses into integers 0 through 3 and standardize this value such that it is mean 0 and standard deviation 1.

B Additional Difference-in-Difference Estimators

As described briefly in the main text, we use two alternative difference-in-differences estimators—from Borusyak et al. (2024) and Gardner et al. (2023)—to assess whether our main results are driven by potential bias from two-way fixed effects estimation. Both estimators use imputation to estimate treatment effects, using only untreated observations to estimate a model of $\hat{Y}_{it}(0)$—the "period-t stochastic potential outcome of unit i if it is never treated"—and construct estimated treatment effects based on the difference between observed treated observations and $\hat{Y}_{it}(0)$.

Two related challenges arise in applying these estimators to our setting. First, because our measure of treatment—3G coverage—is continuous, the set of observations that are deemed untreated depends on the threshold used to distinguish treated versus untreated observations. Second, when using a discrete indicator to determine which country-by-urbanicity pairs are treated in a given year, models that include country-by-year fixed effects have little residual variation in treatment timing; the inclusion of country-by-year fixed effects absorbs nearly all of the variation in the timing of treatment.

To illustrate this second challenge, Appendix Figure B.1 displays, for all country-by-urbanicity pairs in our data, the first year in which 3G coverage exceeded 10%. As can be seen in Figure B.1, for most countries, all levels of urbanicity exceeded 10% coverage within 3 years of one another. This is a challenge for any model using country-by-year fixed effects and a binary treatment indicator in our setting: the fixed effects absorb much of the variation in treatment timing, leaving limited within-country variation to identify the treatment effect. (This is not the case in our main, two-way fixed effects estimates; there, using a continuous measure of 3G allows us to use all of the variation in 3G coverage across country-by-urbanicity pairs and over time, even when all pairs within a country-year are above or below 10% 3G coverage.)

We estimate these models without country-by-year fixed effects. Instead, we include either (a) year fixed effects or (b) region-by-year fixed effects, where regions are defined according to the World Bank classifications: East Asia and Pacific, Europe and Central Asia, Latin America and the Caribbean, Middle East and North Africa, North America, and Sub-Saharan Africa. (The seventh World Bank region, South Asia, does not appear in our data.) We use two-stage difference-in-differences models with year fixed effects or region-by-year fixed effects. However, when applying the Borusyak et al. (2024) estimator, including region-by-year fixed effects leads to matrix singularity issues, so these models are estimated with year fixed effects only. Throughout, we define the binary treatment as equal to one for all years following the first year in which a country-by-urbanicity pair exceeds 10% 3G coverage.

Even with these less stringent specifications—with lower-dimensional fixed-effects and less granular measures of 3G coverage—we find effects similar to our main results, and importantly, our event study estimates are generally flat, and don't indicate large differential pre-trends between treated and untreated country-by-urbanicity pairs.

In these settings, it is common to present event studies to show estimated treatment effects as a function of time since treatment. We use the Borusyak et al. (2024) and Gardner et al. (2023) es-

timators to estimate event studies for treatment effects for each period before and after treatment. We estimate these effects for each three-year period before and after treatment (e.g. 0 to 2 years, 3 to 5 years, etc.).

We perform these analyses on data collapsed at the country-by-urbanicity-by-year level to simplify computation. Specifically, we first regress test score outcomes on our set of baseline covariates and capture the residuals of this regression. We refer to these as "demographic-adjusted outcomes." Next, we compute average demographic-adjusted outcomes at the country-by-urbanicity-by-year level, weighted by PISA sampling weights. These average demographic-adjusted outcomes are our dependent variables in all analyses in this Appendix. Across all analyses, we weight each observation by w_{cut}/w_{ct}, where w_{cut} is the sum of sampling weights in country c and urbanicity u and year t, and w_{ct} denotes the sum of sampling weights in country c and year t.

In our results below, we present both static and dynamic estimates using the Borusyak et al. (2024) and Gardner et al. (2023) estimators. All regressions include country-by-urbanicity and urbanicity-by-year fixed effects. As noted above, in some specifications we additionally include region-by-year fixed effects.

Appendix Figure B.2 displays our dynamic event study estimates with respect to 3G coverage (in the top panels) and test scores (in the bottom panels).

Estimates in the top panel indicate that, in the years after 3G entry (defined by passing 10% coverage, 3G coverage rises by roughly 50% in the first three years, and nearly 100% in the years thereafter. The estimates in the bottom panel indicate that 3G coverage is associated with lower scores on PISA exams, consistent with the estimates provided in the body of the paper. Importantly, these changes are not preceded by systematic differences in the trends between treated and untreated groups; event study estimates for periods prior to 3G arrival are generally small and insignificant.

The corresponding static difference-in-difference estimates are shown in Figure B.3. Across subjects and estimators, these estimates are consistently negative and statistically significant. Magnitudes are slightly larger than our main TWFE

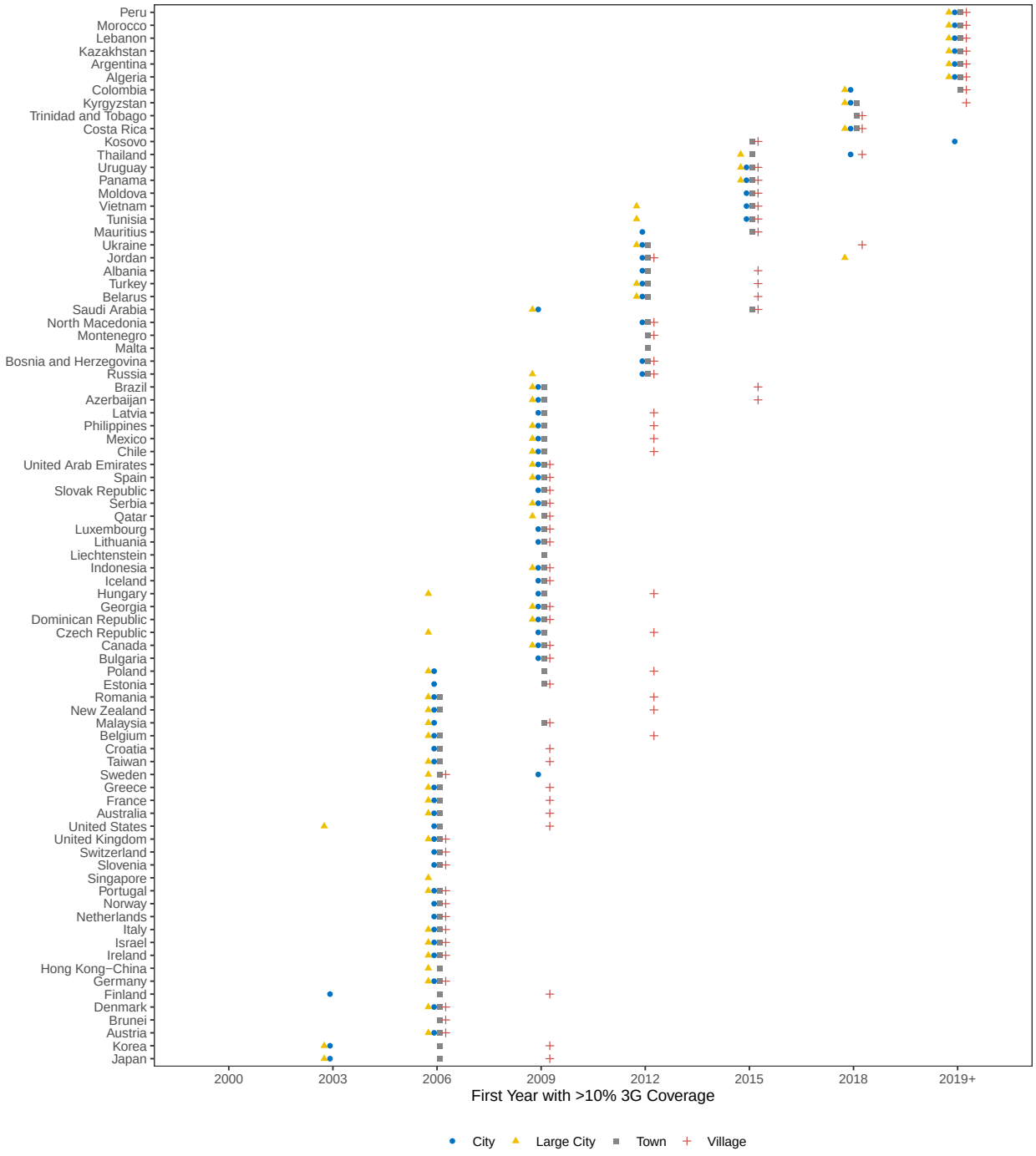


Figure B.1: Distribution of 3G Entry Dates within and Across Countries

Note: Figure displays, for each country-by-urbanicity combination, the first year in which 3G coverage exceeded 10%.

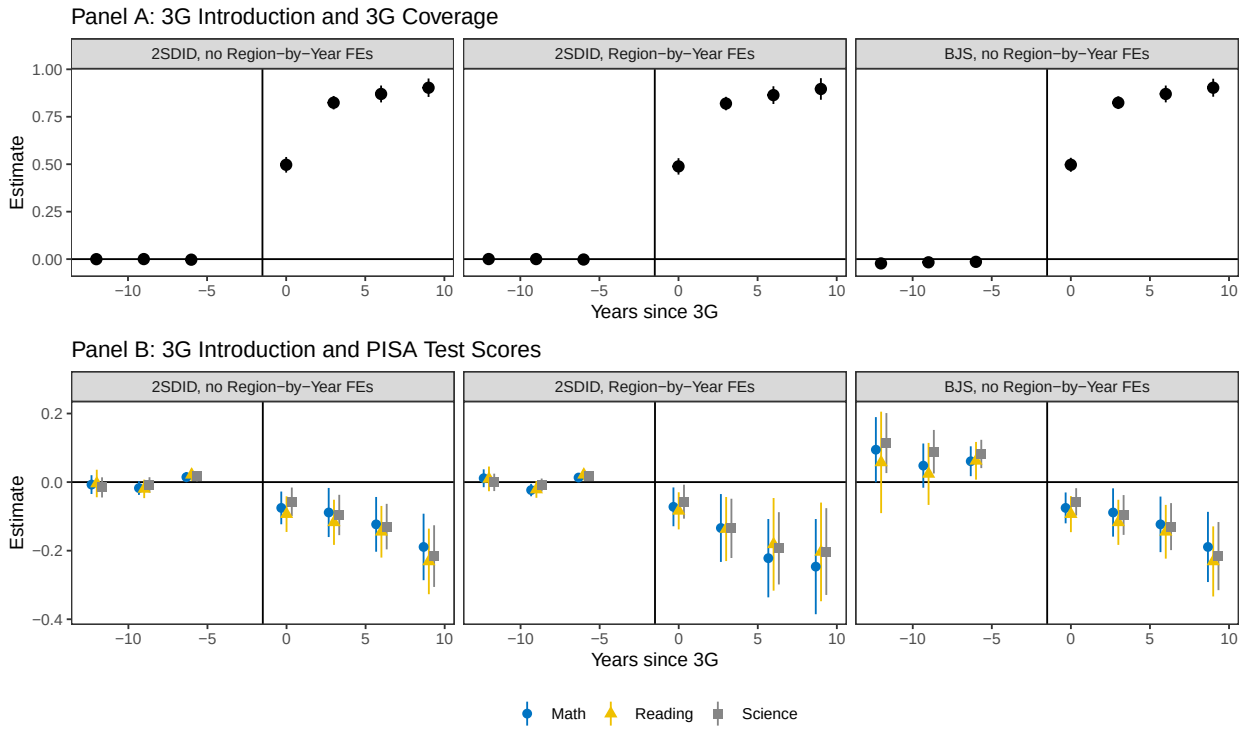


Figure B.2: Dynamic Effect of 3G Entry on PISA Test Scores: Borusyak et al. (2024) and Gardner et al. (2023) Estimators

Note: Figure displays event study estimates using estimators from Borusyak et al. (2024) ("BJS") and Gardner et al. (2023) ("2SDID"). The top panel shows the effect on 3G coverage over time and the bottom panel shows effect on test scores. Robust standard errors clustered at the country-by-urbanicity level in parentheses. Analyses are performed on data collapsed at the country-by-urbanicity-by-year level. Observations are weighted by w_{cut}/w_{ct}, where w_{cut} is the sum of sampling weights in country c and urbanicity u and year t, and w_{ct} denotes the sum of sampling weights in country c and year t.

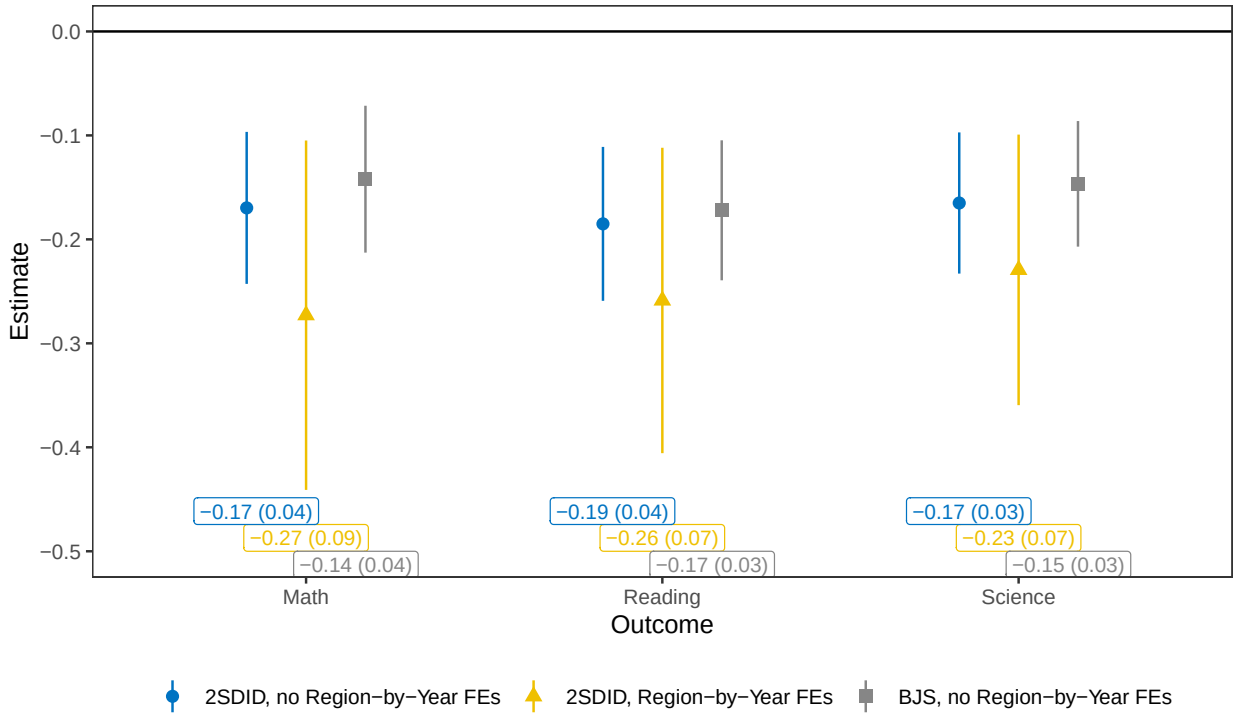


Figure B.3: Static Effect of 3G Entry on PISA Test Scores: Borusyak et al. (2024) and Gardner et al. (2023) Estimators

Note: Figure displays difference-in-difference estimates using estimators from Borusyak et al. (2024) ("BJS") and Gardner et al. (2023) ("2SDID"). Robust standard errors clustered at the country-by-urbanicity level in parentheses. Analyses are performed on data collapsed at the country-by-urbanicity-by-year level. Observations are weighted by w_{cut}/w_{ct}, where w_{cut} is the sum of sampling weights in country c and urbanicity u and year t, and w_{ct} denotes the sum of sampling weights in country c and year t.

C Instrumental Variables Estimates

As discussed briefly in the main text, in addition to our difference-in-differences estimates, we use instrumental variables estimation to estimate the effect of 3G coverage on student achievement. In these specifications, we use two instruments for 3G coverage: local lightning strike frequency and 2G coverage as of 2007. Manacorda and Tesei (2020) first used lightning strike frequency as an instrument for 3G coverage. Since then, it has since been used much more broadly (Chiplunkar and Goldberg, 2022; Guriev et al., 2021; Jiang et al., 2022). Electrical surges caused by frequent lightning strikes increase the cost of installing and maintaining 3G equipment. Thus, ceteris paribus, areas with more frequent lightning strikes exhibited slower diffusion of 3G availability. Oppositely, prior 2G coverage has been associated with faster expansion of 3G coverage (Harm Adema et al., 2022). Prior infrastructure for 2G can be repurposed or shared with 3G infrastructure. Specifically, cell towers used for 2G can be shared by a 3G base transceiver station. Thus, the expansion of 3G coverage was less costly in areas with preexisting 2G coverage.

Following the literature, we operationalize these observations by multiplying each area's population-weighted lightning frequency, $Lightning_{cu}$, with a time trend t. Similarly, we use 2007 as our base year for constructing 2G coverage, and interact this measure, $2G^{2007}$, with a time trend t. The first-stage equation is below.

$$3G_{cut} = \delta_1[Lightning_{cu} \times t] + \delta_2[2G_{cu}^{2007} \times t] + X_{icut}\mu + \phi_{cu} + \tau_t + \varepsilon_{icut} \tag{2}$$

Here, δ_1 captures the differential rate of 3G availability between areas with relatively higher versus relatively lower levels of lightning frequency. If areas with more lightning exhibited slower diffusion of 3G availability, δ_1 should be negative. Oppositely, δ_2 captures the differential rate of 3G availability between areas with relatively higher versus relatively lower levels of 2007 2G coverage. If areas with more 2G coverage in 2007 exhibited faster diffusion of 3G availability, δ_2 should be positive.

In Appendix Table C.1, we show results using our instrumental variables methodology. Due to the smaller size of our ICT sample, and the limited availability of some variables, the standard errors of our estimates are quite large. Still, we document positive and statistically significant effects on smartphone ownership using this approach. Effects on other measures of internet use are noisily estimated, but generally consistent with the TWFE results described above.

Next, we describe results using instrumental variables to estimate the effects of 3G on student test scores. Table C.2 shows instrumental variables estimates of the effect of 3G on test scores in math, and science. Column 1 of Table C.2 displays TWFE estimates of the effect of 3G on test scores. These results are identical to those displayed in Column 1 of Table 3, and suggest small and negative effects on student test scores.

Column 2 of Table C.2 displays evidence of the first stage effect of lightning strike frequency and 2007 2G coverage on 3G coverage. In these regressions, the outcome is the share of the population with 3G access. The coefficient on the interaction term Lighting $\times$ Year measures the effect

of a 1 standard deviation increase in lightning strike frequency on the yearly growth of 3G access. The estimated effect size suggests that a 1 standard deviation increase in lightning strike frequency decreases the annual growth rate of 3G coverage by 0.7 percentage points. Similarly, the coefficient on the interaction term 2G $\times$ Year indicates that areas with full 2G coverage, relative to those with no 2G coverage, expanded their 3G coverage by 1 percentage point more per year.

Column 3 of Table C.2 displays reduced form effects of lightning frequency and 2G coverage on test scores. Across all subjects, areas with more frequent lightning strikes exhibit higher rates of test score growth. The magnitudes of these effects are small: 0.001 to 0.003 student standard deviations. Still, relative to the first stage effects in Column 2, these suggest very large effects of 3G on student achievement. Areas with 2G coverage display the opposite pattern; higher 2G coverage is associated with lower test score growth in math and reading.

Finally, Column 4 of Table C.2 shows two-stage least squares estimates of the effect of 3G access on test scores. These estimates are somewhat noisy relative to TWFE estimates in Table 3; 95% confidence intervals include 0 for all subjects. Still, point estimates are all negative and fall between 0.15 and 0.3 standard deviations. Appendix Tables C.3 and C.4 display results that repeat this analysis using only the lightning and 2G instruments, respectively, with qualitatively similar results.

Table C.1: IV Estimates: Effect of 3G on Technology Access and Use

	(1)	(2)	(3)
	FS	RF	IV
Dep. Var	3G	Tech. Use	Tech. Use
Panel A: Browses the Internet Daily			
Lightning × Year	-0.006***	0.000	
	(0.002)	(0.001)	
2G × Year	0.005*	0.011***	
	(0.003)	(0.002)	
$\widehat{3G}$			0.195
			(0.162)
F-Stat			7.71
Num.Obs.	1595699	1595699	1595699
R2	0.878	0.246	0.244
Panel B: Has a Smartphone at Home (2012-2018)			
Lightning × Year	0.021*	0.002	
	(0.012)	(0.002)	
2G × Year	0.012**	0.017***	
	(0.005)	(0.002)	
$\widehat{3G}$			0.132**
			(0.054)
F-Stat			2.84
Num.Obs.	838077	838077	838077
R2	0.883	0.114	0.112
Panel C: Has a Smartphone at Home (2012-2018; 2000, 2003 Set to 0)			
Lightning × Year	-0.005***	-0.002***	
	(0.001)	(0.001)	
2G × Year	0.006***	0.000	
	(0.002)	(0.001)	
$\widehat{3G}$			0.288**
			(0.114)
F-Stat			12.38
Num.Obs.	1067892	1067892	1067892
R2	0.950	0.795	0.793
Panel D: Total Weekly Internet Use in Hours (2012-2018)			
Lightning × Year	0.021*	0.043	
	(0.012)	(0.098)	
2G × Year	0.012**	0.341	
	(0.005)	(0.269)	
$\widehat{3G}$			3.046
			(4.511)
F-Stat			2.84
Num.Obs.	838077	838077	838077
R2	0.883	0.124	0.124
Country-by-Urbanicity FEs	✓	✓	✓
Year FEs	✓	✓	✓
Baseline Controls	✓	✓	✓

Note: Table displays instrumental variables results estimating the effect of 3G coverage on technology access and use. Column 1 displays the first stage results. Column 2 displays reduced form results. Column 3 displays two-stage least squares results. Dependent variables in Columns 2 and 3 are indicated in panel labels. The dependent variable in Column 1 is 3G coverage. Baseline controls include student gender, age, immigrant status, fixed effects for private (with government funding) and private (without government funding) school attendance, mother's and father's education level, and dummy variables identifying students with missing father's education, missing mother's education, missing immigration status, and missing school type. Robust standard errors clustered at the country-by-urbanicity level in parentheses. Observations are weighted by $w_{ict} / \sum_{i \in ct} w_{ict}$, where w_{ict} is individual i in country c in year t's sampling weight, and $\sum_{i \in ct} w_{ict}$ denotes the sum of sampling weights in country c in year t. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table C.2: IV Estimates: Effect of 3G on PISA Test Scores

	(1)	(2)	(3)	(4)
	TWFE	FS	RF	IV
Dep. Var	Score	3G	Score	Score
Panel A: Math				
3G	-0.034			
	(0.034)			
Lightning × Year		-0.008***	0.005***	
		(0.002)	(0.001)	
2G × Year		0.005	0.009	
		(0.004)	(0.006)	
$\hat{3G}$				-0.550**
				(0.224)
F-Stat				12.50
Num.Obs.	2600134	2600134	2600134	2600134
R2	0.370	0.850	0.371	0.363
Panel B: Reading				
3G	-0.046			
	(0.034)			
Lightning × Year		-0.008***	0.003**	
		(0.002)	(0.001)	
2G × Year		0.005	0.006	
		(0.004)	(0.006)	
$\hat{3G}$				-0.325*
				(0.192)
F-Stat				12.50
Num.Obs.	2600134	2600134	2600134	2600134
R2	0.345	0.850	0.345	0.343
Panel C: Science				
3G	-0.005			
	(0.030)			
Lightning × Year		-0.008***	0.004***	
		(0.002)	(0.001)	
2G × Year		0.005	0.012**	
		(0.004)	(0.005)	
$\hat{3G}$				-0.329**
				(0.162)
F-Stat				12.50
Num.Obs.	2600134	2600134	2600134	2600134
R2	0.329	0.850	0.329	0.326
Country-by-Urbanicity FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓

Note: Table displays instrumental variables results estimating the effect of 3G coverage on test scores. Column 1 displays TWFE results. Column 2 displays the first stage results. Column 3 displays reduced form results. Column 4 displays two-stage least squares results. Dependent variables in Columns 1, 3, and 4 are scaled student test scores in math (Panel A), reading (Panel B), and science (Panel C). The dependent variable in Column 2 is 3G coverage. Baseline controls include student gender, age, immigrant status, fixed effects for private (with government funding) and private (without government funding) school attendance, mother's and father's education level, and dummy variables identifying students with missing father's education, missing mother's education, missing immigration status, and missing school type. Robust standard errors clustered at the country-by-urbanicity level in parentheses. Observations are weighted by $w_{ict} / \sum_{i \in ct} w_{ict}$, where w_{ict} is individual i in country c in year t's sampling weight, and $\sum_{i \in ct} w_{ict}$ denotes the sum of sampling weights in country c in year t. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3: IV Estimates: Effect of 3G on PISA Test Scores (Lightning Instrument Only)

	(1)	(2)	(3)	(4)
	TWFE	FS	RF	IV
Dep. Var	Score	3G	Score	Score
Panel A: Math				
3G	-0.034			
	(0.034)			
Lightning × Year		-0.008***	0.005***	
		(0.002)	(0.001)	
$\widehat{3G}$				-0.638**
				(0.250)
F-Stat				22.23
Num.Obs.	2600134	2600134	2600134	2600134
R2	0.370	0.850	0.371	0.360
Panel B: Reading				
3G	-0.046			
	(0.034)			
Lightning × Year		-0.008***	0.003**	
		(0.002)	(0.001)	
$\widehat{3G}$				-0.380*
				(0.208)
F-Stat				22.23
Num.Obs.	2600134	2600134	2600134	2600134
R2	0.345	0.850	0.345	0.342
Panel C: Science				
3G	-0.005			
	(0.030)			
Lightning × Year		-0.008***	0.003***	
		(0.002)	(0.001)	
$\widehat{3G}$				-0.430**
				(0.184)
F-Stat				22.23
Num.Obs.	2600134	2600134	2600134	2600134
R2	0.329	0.850	0.329	0.323
Country-by-Urbanicity FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓

Note: Table displays instrumental variables results estimating the effect of 3G coverage on test scores. Column 1 displays TWFE results. Column 2 displays the first stage results. Column 3 displays reduced form results. Column 4 displays two-stage least squares results. Dependent variables in Columns 1, 3, and 4 are scaled student test scores in math (Panel A), reading (Panel B), and science (Panel C). The dependent variable in Column 2 is 3G coverage. Baseline controls include student gender, age, immigrant status, fixed effects for private (with government funding) and private (without government funding) school attendance, mother's and father's education level, and dummy variables identifying students with missing father's education, missing mother's education, missing immigration status, and missing school type. Robust standard errors clustered at the country-by-urbanicity level in parentheses. Observations are weighted by $w_{ict} / \sum_{i \in ct} w_{ict}$, where w_{ict} is individual i in country c in year t's sampling weight, and $\sum_{i \in ct} w_{ict}$ denotes the sum of sampling weights in country c in year t. * p $<$ 0.1, ** p $<$ 0.05, *** p $<$ 0.01.

Table C.4: IV Estimates: Effect of 3G on PISA Test Scores (2G Instrument Only)

	(1)	(2)	(3)	(4)
	TWFE	FS	RF	IV
Dep. Var	Score	3G	Score	Score
Panel A: Math				
3G	-0.034			
	(0.034)			
2G × Year		0.007	0.008	
		(0.005)	(0.006)	
$\widehat{3G}$				1.060
				(1.105)
F-Stat				2.53
Num.Obs.	2600134	2600134	2600134	2600134
R2	0.370	0.844	0.371	0.336
Panel B: Reading				
3G	-0.046			
	(0.034)			
2G × Year		0.007	0.005	
		(0.005)	(0.006)	
$\widehat{3G}$				0.676
				(0.902)
F-Stat				2.53
Num.Obs.	2600134	2600134	2600134	2600134
R2	0.345	0.844	0.345	0.331
Panel C: Science				
3G	-0.005			
	(0.030)			
2G × Year		0.007	0.011**	
		(0.005)	(0.005)	
$\widehat{3G}$				1.519
				(1.255)
F-Stat				2.53
Num.Obs.	2600134	2600134	2600134	2600134
R2	0.329	0.844	0.329	0.261
Country-by-Urbanicity FEs	✓	✓	✓	✓
Year FEs	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓

Note: Table displays instrumental variables results estimating the effect of 3G coverage on test scores. Column 1 displays TWFE results. Column 2 displays the first stage results. Column 3 displays reduced form results. Column 4 displays two-stage least squares results. Dependent variables in Columns 1, 3, and 4 are scaled student test scores in math (Panel A), reading (Panel B), and science (Panel C). The dependent variable in Column 2 is 3G coverage. Baseline controls include student gender, age, immigrant status, fixed effects for private (with government funding) and private (without government funding) school attendance, mother's and father's education level, and dummy variables identifying students with missing father's education, missing mother's education, missing immigration status, and missing school type. Robust standard errors clustered at the country-by-urbanicity level in parentheses. Observations are weighted by $w_{ict} / \sum_{i \in ct} w_{ict}$, where w_{ict} is individual i in country c in year t's sampling weight, and $\sum_{i \in ct} w_{ict}$ denotes the sum of sampling weights in country c in year t. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D Additional Figures and Tables

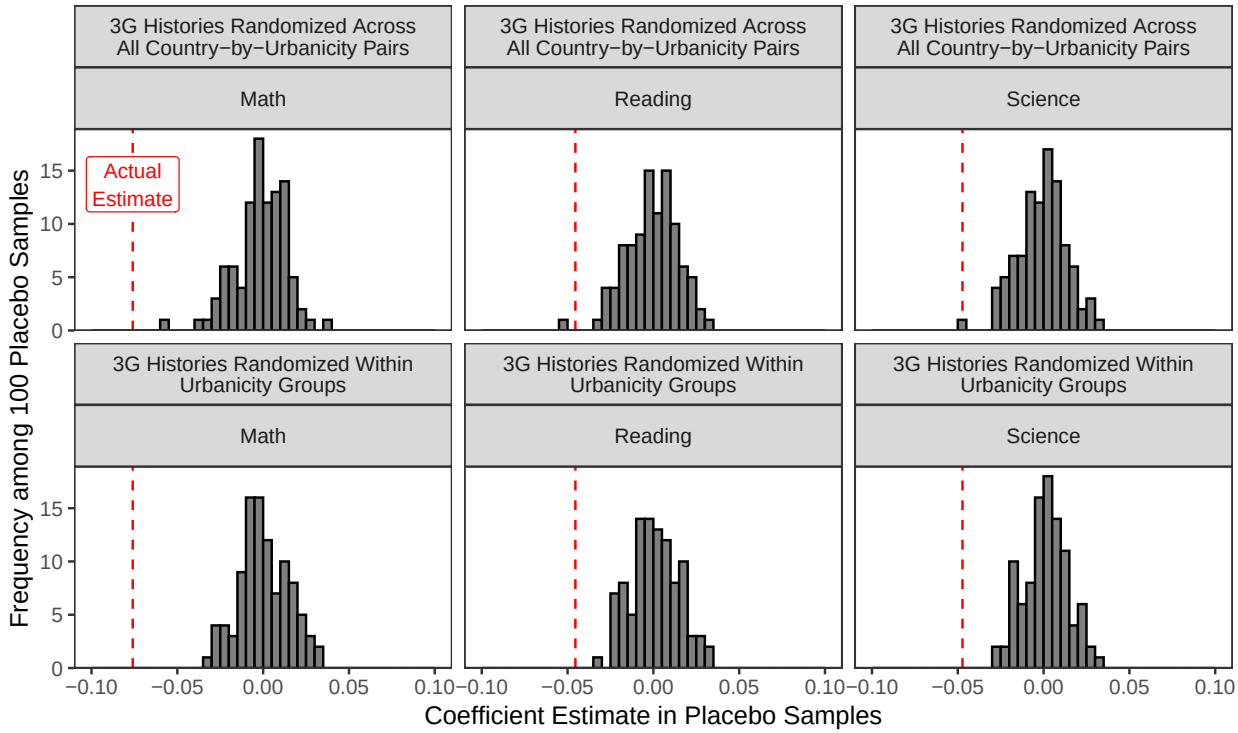


Figure D.1: Permutation Test: Distribution of Estimated Test Score Effects under Placebo 3G Histories vs. Actual Estimates

Note: Figure displays the distribution of the estimated effect of 3G coverage on test scores under placebo 3G histories. Actual estimates indicated by the dashed red line. All estimates include fixed effects for country-by-urbanicity, urbanicity-by-year, country-by-year, and baseline controls. Baseline controls include student gender, age, immigrant status, fixed effects for private (with government funding) and private (without government funding) school attendance, mother's and father's education level, and dummy variables identifying students with missing father's education, missing mother's education, missing immigration status, and missing school type. Observations are weighted by $w_{ict} / \sum_{i \in ct} w_{ict}$, where w_{ict} is individual i in country c in year t's sampling weight, and $\sum_{i \in ct} w_{ict}$ denotes the sum of sampling weights in country c in year t.

Table D.1: Associations Between Digital Technology Use and PISA Test Scores

	(1)	(2)	(3)	(4)
Panel A: Math				
Total Weekly Internet Use in Hours	-0.005***			-0.006***
	(0.000)			(0.000)
Has a Smartphone at Home		0.028**		0.036***
		(0.011)		(0.010)
Browses Internet Daily			0.154***	0.211***
			(0.013)	(0.012)
Num.Obs.	838077	838077	838077	838077
R2	0.334	0.324	0.329	0.343
Panel B: Reading				
Total Weekly Internet Use in Hours	-0.004***			-0.005***
	(0.000)			(0.000)
Has a Smartphone at Home		0.046***		0.041***
		(0.012)		(0.010)
Browses Internet Daily			0.231***	0.285***
			(0.014)	(0.013)
Num.Obs.	838077	838077	838077	838077
R2	0.284	0.277	0.288	0.301
Panel C: Science				
Total Weekly Internet Use in Hours	-0.004***			-0.006***
	(0.000)			(0.000)
Has a Smartphone at Home		0.016		0.019*
		(0.011)		(0.010)
Browses Internet Daily			0.191***	0.249***
			(0.013)	(0.012)
Num.Obs.	838077	838077	838077	838077
R2	0.293	0.284	0.292	0.306
Country-by-Urbanicity FEs	✓	✓	✓	✓
Urbanicity-by-Year FEs	✓	✓	✓	✓
Country-by-Year FEs	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓

Note: Table displays OLS results estimating the association between technology use and test scores. Data includes all observations in the ICT sample from years 2012, 2015, and 2018. Dependent variables are indicated in panel labels. Baseline controls include student gender, age, immigrant status, fixed effects for private (with government funding) and private (without government funding) school attendance, mother's and father's education level, and dummy variables identifying students with missing father's education, missing mother's education, missing immigration status, and missing school type. Robust standard errors clustered at the country-by-urbanicity level in parentheses. Observations are weighted by $w_{ict} / \sum_{i \in ct} w_{ict}$, where w_{ict} is individual i in country c in year t's sampling weight, and $\sum_{i \in ct} w_{ict}$ denotes the sum of sampling weights in country c in year t. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table D.2: TWFE Estimates: Effect of 3G on Reported Weekly Internet Use

	(1)	(2)
Panel A: In-School Weekly Internet Use in Hours (2012-2018)		
3G	0.999	0.870
	(0.758)	(0.792)
Num.Obs.	838077	838077
R2	0.095	0.107
Panel B: Out-of-School Weekly Internet Use in Hours (2012-2018)		
3G	4.130***	4.012***
	(1.523)	(1.461)
Num.Obs.	838077	838077
R2	0.113	0.133
Panel C: Total Weekly Internet Use in Hours (2012-2018)		
3G	5.130***	4.882**
	(1.947)	(1.917)
Num.Obs.	838077	838077
R2	0.130	0.147
Country-by-Urbanicity FEs	✓	✓
Urbanicity-by-Year FEs	✓	✓
Country-by-Year FEs	✓	✓
Baseline Controls	✓	✓
Controls Interacted with Country-by-Urbanicity		✓

Note: Table displays TWFE results estimating the effect of 3G coverage on reported weekly internet use. Dependent variables are indicated in panel labels. Baseline controls include student gender, age, immigrant status, fixed effects for private (with government funding) and private (without government funding) school attendance, mother's and father's education level, and dummy variables identifying students with missing father's education, missing mother's education, missing immigration status, and missing school type. Robust standard errors clustered at the country-by-urbanicity level in parentheses. Observations are weighted by $w_{ict} / \sum_{i \in ct} w_{ict}$, where w_{ict} is individual i in country c in year t's sampling weight, and $\sum_{i \in ct} w_{ict}$ denotes the sum of sampling weights in country c in year t. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table D.3: TWFE Estimates: Effect of 3G on PISA Test Scores (ICT Sample)

	(1)	(2)
Panel A: Math		
3G	-0.101***	-0.083***
	(0.031)	(0.030)
Num.Obs.	1595699	1595699
R2	0.333	0.358
Panel B: Reading		
3G	-0.056*	-0.039
	(0.033)	(0.031)
Num.Obs.	1595699	1595699
R2	0.301	0.326
Panel C: Science		
3G	-0.071**	-0.052*
	(0.031)	(0.030)
Num.Obs.	1595699	1595699
R2	0.283	0.309
Country-by-Urbanicity FEs	✓	✓
Urbanicity-by-Year FEs	✓	✓
Country-by-Year FEs	✓	✓
Baseline Controls	✓	✓
Controls Interacted with Country-by-Urbanicity		✓

Note: Table displays TWFE results estimating the effect of 3G coverage on test scores within the ICT Sample. Dependent variables are scaled student test scores in math (Panel A), reading (Panel B), and science (Panel C). Baseline controls include student gender, age, immigrant status, fixed effects for private (with government funding) and private (without government funding) school attendance, mother's and father's education level, and dummy variables identifying students with missing father's education, missing mother's education, missing immigration status, and missing school type. Robust standard errors clustered at the country-by-urbanicity level in parentheses. Observations are weighted by $w_{ict} / \sum_{i \in ct} w_{ict}$, where w_{ict} is individual i in country c in year t's sampling weight, and $\sum_{i \in ct} w_{ict}$ denotes the sum of sampling weights in country c in year t. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table D.4: Heterogeneous Effects of 3G on Technology Access and Use

	Browses the Internet Daily		Has a Smartphone at Home (2012-2018)		Has a Smartphone at Home (2012-2018; 2000, 2003 Set to 0)		Total Weekly Internet Use in Hours (2012-2018)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: By Student Gender								
3G	0.031	0.030	0.056	0.056	0.099**	0.099**	5.584***	5.711***
	(0.020)	(0.020)	(0.034)	(0.035)	(0.045)	(0.046)	(2.063)	(2.099)
3G × Female	0.009	0.014	-0.036	-0.024	0.014	0.014	-0.704	-1.551
	(0.019)	(0.018)	(0.035)	(0.036)	(0.018)	(0.019)	(2.186)	(2.196)
Num.Obs.	1595699	1595699	838077	838077	1067892	1067892	838077	838077
R2	0.271	0.273	0.125	0.129	0.800	0.801	0.134	0.143
Panel B: By Parental Education								
3G	0.073***	0.072***	0.056*	0.054	0.126**	0.128**	5.276**	5.274**
	(0.024)	(0.024)	(0.033)	(0.034)	(0.053)	(0.053)	(2.249)	(2.301)
3G × Either Parent has Tert. Ed.	-0.074***	-0.070***	-0.046	-0.043	-0.089**	-0.097**	-2.039	-2.226
	(0.024)	(0.026)	(0.039)	(0.038)	(0.043)	(0.045)	(2.466)	(2.494)
Num.Obs.	1595699	1595699	838077	838077	1067892	1067892	838077	838077
R2	0.271	0.274	0.127	0.129	0.801	0.801	0.136	0.143
Panel C: By Country Income Level								
3G	0.052**	0.053**	0.054	0.051	0.097*	0.099*	6.256***	6.168***
	(0.020)	(0.021)	(0.039)	(0.043)	(0.050)	(0.051)	(2.153)	(2.197)
3G × Country is High Income in 2000	-0.114***	-0.095***	-0.099*	-0.106	-0.113**	-0.104*	-5.451	-3.790
	(0.029)	(0.030)	(0.056)	(0.065)	(0.053)	(0.056)	(5.903)	(5.927)
Num.Obs.	1595699	1595699	838077	838077	1067892	1067892	838077	838077
R2	0.270	0.267	0.125	0.122	0.800	0.800	0.134	0.138
Country-by-Urbanicity FEs	✓	✓	✓	✓	✓	✓	✓	✓
Urbanicity-by-Year FEs	✓	✓	✓	✓	✓	✓	✓	✓
Country-by-Year FEs	✓	✓	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓	✓	✓
Controls Interacted with Country-by-Urbanicity		✓		✓		✓		✓
Complete Interactions with Heterogeneous Variable	✓	✓	✓	✓	✓	✓	✓	✓

Note: Table displays TWFE results estimating the heterogeneous effect of 3G coverage on technology access and use. Dependent variables are indicated above column numbers. Baseline controls include student gender, age, immigrant status, fixed effects for private (with government funding) and private (without government funding) school attendance, mother's and father's education level, and dummy variables identifying students with missing father's education, missing mother's education, missing immigration status, and missing school type. All models include full interactions with the heterogeneous variable identified in each panel. Robust standard errors clustered at the country-by-urbanicity level in parentheses. Observations are weighted by $w_{ict} / \sum_{i \in ct} w_{ict}$, where w_{ict} is individual i in country c in year t's sampling weight, and $\sum_{i \in ct} w_{ict}$ denotes the sum of sampling weights in country c in year t. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table D.5: Heterogeneous Effects of 3G on PISA Test Scores

	Math		Reading		Science	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: By Student Gender						
3G	-0.062**	-0.082**	-0.027	-0.049	-0.021	-0.042
	(0.029)	(0.033)	(0.028)	(0.032)	(0.027)	(0.031)
3G × Female	-0.031	-0.008	-0.042	-0.012	-0.053**	-0.021
	(0.026)	(0.031)	(0.026)	(0.031)	(0.026)	(0.031)
Num.Obs.	2600134	2600134	2600134	2600134	2600134	2600134
R2	0.388	0.372	0.365	0.347	0.346	0.328
Panel B: By Parental Education						
3G	-0.076***	-0.079**	-0.054**	-0.064**	-0.059**	-0.064**
	(0.029)	(0.031)	(0.027)	(0.030)	(0.027)	(0.029)
3G × Either Parent has Tert. Ed.	0.016	0.028	0.032	0.052	0.039	0.060**
	(0.031)	(0.031)	(0.034)	(0.034)	(0.029)	(0.030)
Num.Obs.	2600134	2600134	2600134	2600134	2600134	2600134
R2	0.394	0.390	0.369	0.349	0.351	0.348
Panel C: By Country Income Level						
3G	-0.055	-0.063*	-0.043	-0.056	-0.058*	-0.067*
	(0.034)	(0.038)	(0.033)	(0.039)	(0.032)	(0.037)
3G × Country is High Income in 2000	0.025	0.050	0.062	0.082	0.085	0.120**
	(0.056)	(0.060)	(0.056)	(0.061)	(0.055)	(0.059)
Num.Obs.	2600134	2600134	2600134	2600134	2600134	2600134
R2	0.386	0.378	0.362	0.339	0.344	0.338
Country-by-Urbanicity FEs	✓	✓	✓	✓	✓	✓
Urbanicity-by-Year FEs	✓	✓	✓	✓	✓	✓
Country-by-Year FEs	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓
Controls Interacted with Country-by-Urbanicity		✓		✓		✓
Complete Interactions with Heterogeneous Variable	✓	✓	✓	✓	✓	✓

Note: Table displays TWFE results estimating the heterogeneous effect of 3G coverage on test scores. Dependent variables are scaled student test scores in math, reading, and science. Baseline controls include student gender, age, immigrant status, fixed effects for private (with government funding) and private (without government funding) school attendance, mother's and father's education level, and dummy variables identifying students with missing father's education, missing mother's education, missing immigration status, and missing school type. All models include full interactions with the heterogeneous variable identified in each panel. Robust standard errors clustered at the country-by-urbanicity level in parentheses. Observations are weighted by $w_{ict} / \sum_{i \in ct} w_{ict}$, where w_{ict} is individual i in country c in year t's sampling weight, and $\sum_{i \in ct} w_{ict}$ denotes the sum of sampling weights in country c in year t. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table D.6: Heterogeneous Effects of 3G on Social and Mental Well-Being

	Friendship Index (2000-2003, 2012-2018)		Belonging Index (2000-2003, 2012-2018)		Self-Efficacy Index (2000-2006, 2012-2015)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: By Student Gender						
3G	-0.120***	-0.120***	-0.082**	-0.086**	-0.085**	-0.104**
	(0.039)	(0.040)	(0.036)	(0.039)	(0.041)	(0.044)
3G × Female	-0.053	-0.053	0.019	0.017	0.061	0.085*
	(0.056)	(0.056)	(0.045)	(0.047)	(0.043)	(0.048)
Num.Obs.	1473535	1473535	1482513	1482513	1340914	1340914
R2	0.044	0.047	0.059	0.061	0.078	0.073
Panel B: By Parental Education						
3G	-0.111***	-0.109***	-0.012	-0.010	-0.045	-0.044
	(0.036)	(0.037)	(0.040)	(0.041)	(0.040)	(0.042)
3G × Either Parent has Tert. Ed.	-0.061	-0.094	-0.094	-0.122*	-0.011	0.000
	(0.057)	(0.059)	(0.063)	(0.068)	(0.040)	(0.048)
Num.Obs.	1473535	1473535	1482513	1482513	1340914	1340914
R2	0.042	0.047	0.057	0.061	0.076	0.077
Panel C: By Country Income Level						
3G	-0.124***	-0.122***	-0.053	-0.051	-0.080	-0.079
	(0.035)	(0.036)	(0.041)	(0.043)	(0.050)	(0.053)
3G × Country is High Income in 2000	0.065	0.096*	0.063	0.057	0.135*	0.116
	(0.062)	(0.057)	(0.070)	(0.069)	(0.071)	(0.078)
Num.Obs.	1473535	1473535	1482513	1482513	1340914	1340914
R2	0.041	0.044	0.056	0.057	0.074	0.072
Country-by-Urbanicity FEs	✓	✓	✓	✓	✓	✓
Urbanicity-by-Year FEs	✓	✓	✓	✓	✓	✓
Country-by-Year FEs	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓
Controls Interacted with Country-by-Urbanicity		✓		✓		✓
Complete Interactions with Heterogeneous Variable	✓	✓	✓	✓	✓	✓

Note: Table displays TWFE results estimating the heterogeneous effect of 3G coverage on social and mental well-being. Dependent variables are indices with a mean of 0 and a standard deviation of 1. Baseline controls include student gender, age, immigrant status, fixed effects for private (with government funding) and private (without government funding) school attendance, mother's and father's education level, and dummy variables identifying students with missing father's education, missing mother's education, missing immigration status, and missing school type. All models include full interactions with the heterogeneous variable identified in each panel. Robust standard errors clustered at the country-by-urbanicity level in parentheses. Observations are weighted by $w_{ict} / \sum_{i \in ct} w_{ict}$, where w_{ict} is individual i in country c in year t's sampling weight, and $\sum_{i \in ct} w_{ict}$ denotes the sum of sampling weights in country c in year t. These variables are not available for all countries in all years; see Appendix Figures E.6, E.7, and E.8. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table D.7: Heterogeneous Effects of 3G on Homework and Absenteeism

	Weekly Homework Hours (2000-2006, 2012)		Days Skipped Past 2 Weeks (2012-2018)		Skipping Hinders Learning A Lot (2000-2003, 2009-2015)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: By Student Gender						
3G	0.180	0.140	-0.031	-0.023	0.017	0.019
	(0.195)	(0.197)	(0.082)	(0.083)	(0.022)	(0.021)
3G × Female	0.241	0.408*	0.086	0.044	0.018	0.020
	(0.212)	(0.232)	(0.112)	(0.109)	(0.013)	(0.014)
Num.Obs.	886279	886279	1251516	1251516	2192073	2192073
R2	0.167	0.173	0.131	0.135	0.112	0.119
Panel B: By Parental Education						
3G	0.163	0.245	-0.120	-0.123	0.025	0.025
	(0.226)	(0.176)	(0.076)	(0.076)	(0.021)	(0.021)
3G × Either Parent has Tert. Ed.	0.102	0.000	0.224**	0.208**	0.002	0.004
	(0.293)	(0.000)	(0.101)	(0.104)	(0.021)	(0.020)
Num.Obs.	886279	886279	1251516	1251516	2192073	2192073
R2	0.164	0.164	0.129	0.134	0.113	0.121
Panel C: By Country Income Level						
3G	0.105	0.174	-0.073	-0.069	0.031	0.032
	(0.278)	(0.292)	(0.076)	(0.075)	(0.020)	(0.019)
3G × Country is High Income in 2000	0.175	0.057	0.327	0.307	-0.042	-0.047
	(0.374)	(0.398)	(0.199)	(0.207)	(0.038)	(0.037)
Num.Obs.	886279	886279	1251516	1251516	2192073	2192073
R2	0.162	0.160	0.128	0.129	0.110	0.113
Country-by-Urbanicity FEs	✓	✓	✓	✓	✓	✓
Urbanicity-by-Year FEs	✓	✓	✓	✓	✓	✓
Country-by-Year FEs	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓
Controls Interacted with Country-by-Urbanicity		✓		✓		✓
Complete Interactions with Heterogeneous Variable	✓	✓	✓	✓	✓	✓

Note: Table displays TWFE results estimating the heterogeneous effect of 3G coverage on homework and absenteeism. Dependent variables are indicated above column numbers. Baseline controls include student gender, age, immigrant status, fixed effects for private (with government funding) and private (without government funding) school attendance, mother's and father's education level, and dummy variables identifying students with missing father's education, missing mother's education, missing immigration status, and missing school type. All models include full interactions with the heterogeneous variable identified in each panel. Robust standard errors clustered at the country-by-urbanicity level in parentheses. Observations are weighted by $w_{ict} / \sum_{i \in ct} w_{ict}$, where w_{ict} is individual i in country c in year t's sampling weight, and $\sum_{i \in ct} w_{ict}$ denotes the sum of sampling weights in country c in year t. These variables are not available for all countries in all years; see Appendix Figures E.3, E.4 and E.5. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table D.8: Heterogeneous Effects of 3G on Reported Weekly Internet Use

	In-School Weekly Internet Use in Hours (2012-2018)		Out-of-School Weekly Internet Use in Hours (2012-2018)		Total Weekly Internet Use in Hours (2012-2018)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: By Student Gender						
3G	0.671	0.694	4.913***	5.018***	5.584***	5.711***
	(0.845)	(0.845)	(1.750)	(1.785)	(2.063)	(2.099)
3G × Female	0.808	0.512	-1.513	-2.062	-0.704	-1.551
	(0.936)	(0.903)	(1.690)	(1.774)	(2.186)	(2.196)
Num.Obs.	838077	838077	838077	838077	838077	838077
R2	0.097	0.104	0.118	0.128	0.134	0.143
Panel B: By Parental Education						
3G	0.426	0.432	4.850***	4.842***	5.276**	5.274**
	(0.940)	(0.945)	(1.667)	(1.715)	(2.249)	(2.301)
3G × Either Parent has Tert. Ed.	0.571	0.543	-2.610	-2.769*	-2.039	-2.226
	(1.302)	(1.310)	(1.582)	(1.584)	(2.466)	(2.494)
Num.Obs.	838077	838077	838077	838077	838077	838077
R2	0.098	0.105	0.120	0.128	0.136	0.143
Panel C: By Country Income Level						
3G	0.950	0.973	5.306***	5.196***	6.256***	6.168***
	(0.841)	(0.842)	(1.688)	(1.758)	(2.153)	(2.197)
3G × Country is High Income in 2000	0.393	0.776	-5.844	-4.566	-5.451	-3.790
	(2.192)	(2.177)	(4.942)	(5.045)	(5.903)	(5.927)
Num.Obs.	838077	838077	838077	838077	838077	838077
R2	0.096	0.102	0.119	0.122	0.134	0.138
Country-by-Urbanicity FEs	✓	✓	✓	✓	✓	✓
Urbanicity-by-Year FEs	✓	✓	✓	✓	✓	✓
Country-by-Year FEs	✓	✓	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓	✓	✓
Controls Interacted with Country-by-Urbanicity		✓		✓		✓
Complete Interactions with Heterogeneous Variable	✓	✓	✓	✓	✓	✓

Note: Table displays TWFE results estimating the heterogeneous effect of 3G coverage on reported weekly internet use. Dependent variables are indicated above column numbers. Baseline controls include student gender, age, immigrant status, fixed effects for private (with government funding) and private (without government funding) school attendance, mother's and father's education level, and dummy variables identifying students with missing father's education, missing mother's education, missing immigration status, and missing school type. All models include full interactions with the heterogeneous variable identified in each panel. Robust standard errors clustered at the country-by-urbanicity level in parentheses. Observations are weighted by $w_{ict} / \sum_{i \in ct} w_{ict}$, where w_{ict} is individual i in country c in year t's sampling weight, and $\sum_{i \in ct} w_{ict}$ denotes the sum of sampling weights in country c in year t. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table D.9: TWFE Estimates: Effect of 3G on School Administrator's Perceived Skipping Burden

	(1)	(2)
Panel A: Skipping Hinders Learning A Lot (2000-2003, 2009-2015)		
3G	0.026	0.027
	(0.018)	(0.018)
Num.Obs.	2192073	2192073
R2	0.110	0.127
Panel B: Skipping Hinders Learning Some or A Lot (2000-2003, 2009-2015)		
3G	0.049	0.047
	(0.030)	(0.031)
Num.Obs.	2192073	2192073
R2	0.169	0.195
Country-by-Urbanicity FEs	✓	✓
Urbanicity-by-Year FEs	✓	✓
Country-by-Year FEs	✓	✓
Baseline Controls	✓	✓
Controls Interacted with Country-by-Urbanicity		✓

Note: Table displays TWFE results estimating the effect of 3G coverage on the school administrator's perceived burden of student absenteeism. School administrators were asked each year whether "In your school, to what extent is the learning of students hindered by the following phenomena? — Students Skipping Classes. The responses were: 'Not at all', 'A little', 'To some extent', and 'A lot'. Panel A reports the regression of the responses 'A lot' and Panel B reports the results combining the responses 'To some extent' and 'A lot'. Dependent variables are indicated in panel labels. Baseline controls include student gender, age, immigrant status, fixed effects for private (with government funding) and private (without government funding) school attendance, mother's and father's education level, and dummy variables identifying students with missing father's education, missing mother's education, missing immigration status, and missing school type. Robust standard errors clustered at the country-by-urbanicity level in parentheses. Observations are weighted by $w_{ict} / \sum_{i \in ct} w_{ict}$, where w_{ict} is individual i in country c in year t's sampling weight, and $\sum_{i \in ct} w_{ict}$ denotes the sum of sampling weights in country c in year t. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table D.10: Calculation of 3G-Induced Decline in Test Scores due to Absenteeism

	Parameter	Value
A	Instruction Hours Per Year (OECD, 2023)	923
B	Number of Instruction Days Per Year (OECD, 2023)	184
C	Instruction Hours Per Day (A/B)	5.02
D	Estimated 3G Effect on Days Skipped in Two Weeks (Table 4)	0.02
E	Hours of Instruction Missed Per Week (C*D)	0.05
F	Effect of 1 Hour of Instruction on Test Scores (Lavy, 2015)	0.06
G	Effect of 3G-Induced Instruction Missed (-E*F)	-0.003

Note: Table displays calculations that estimate the impact of 3G-induced missed instruction on test scores.

E Data Availability

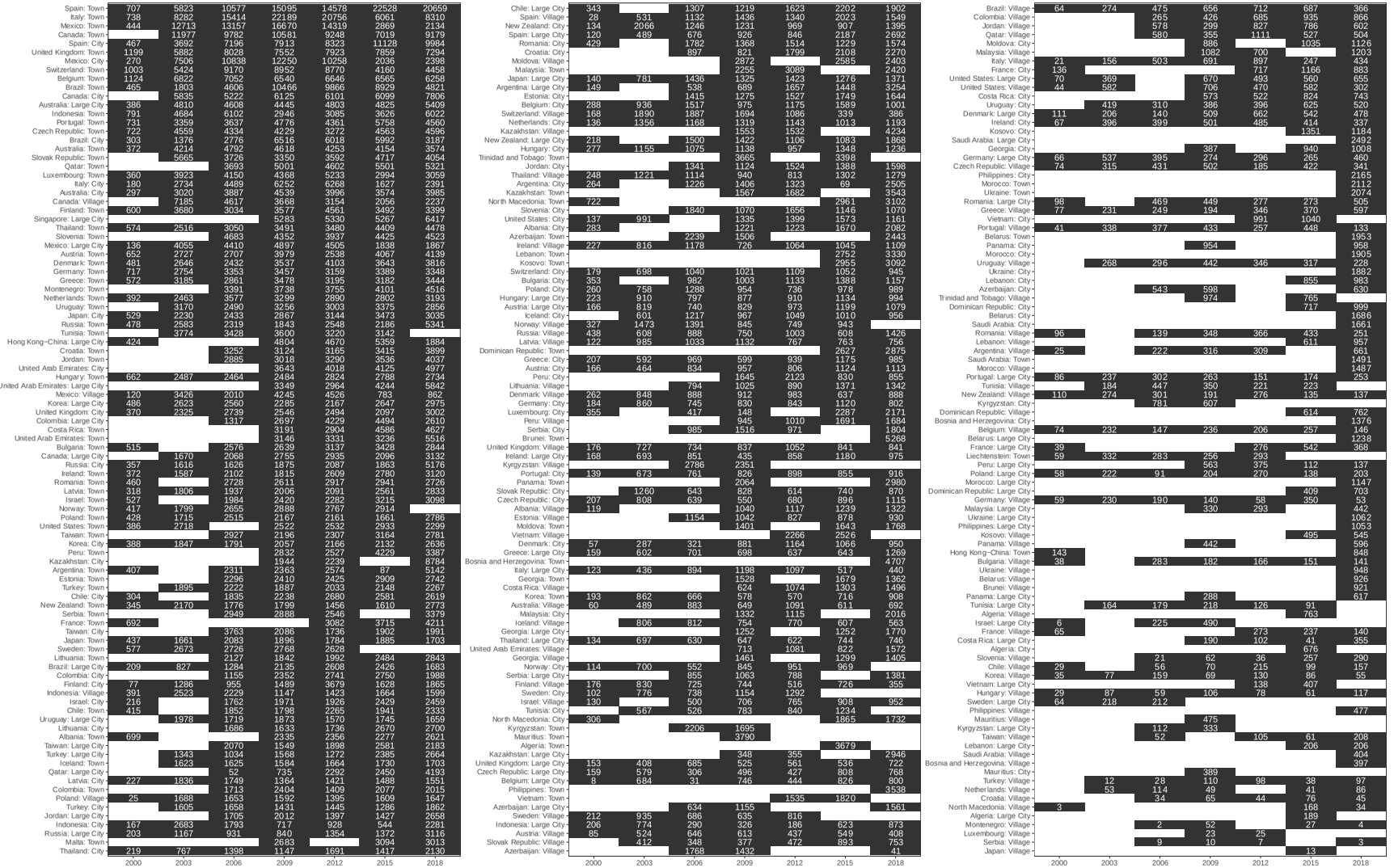


Figure E.1: Data Coverage by Country and Year

Note: Figure displays the number of observations in each country-by-urbanicity cell.

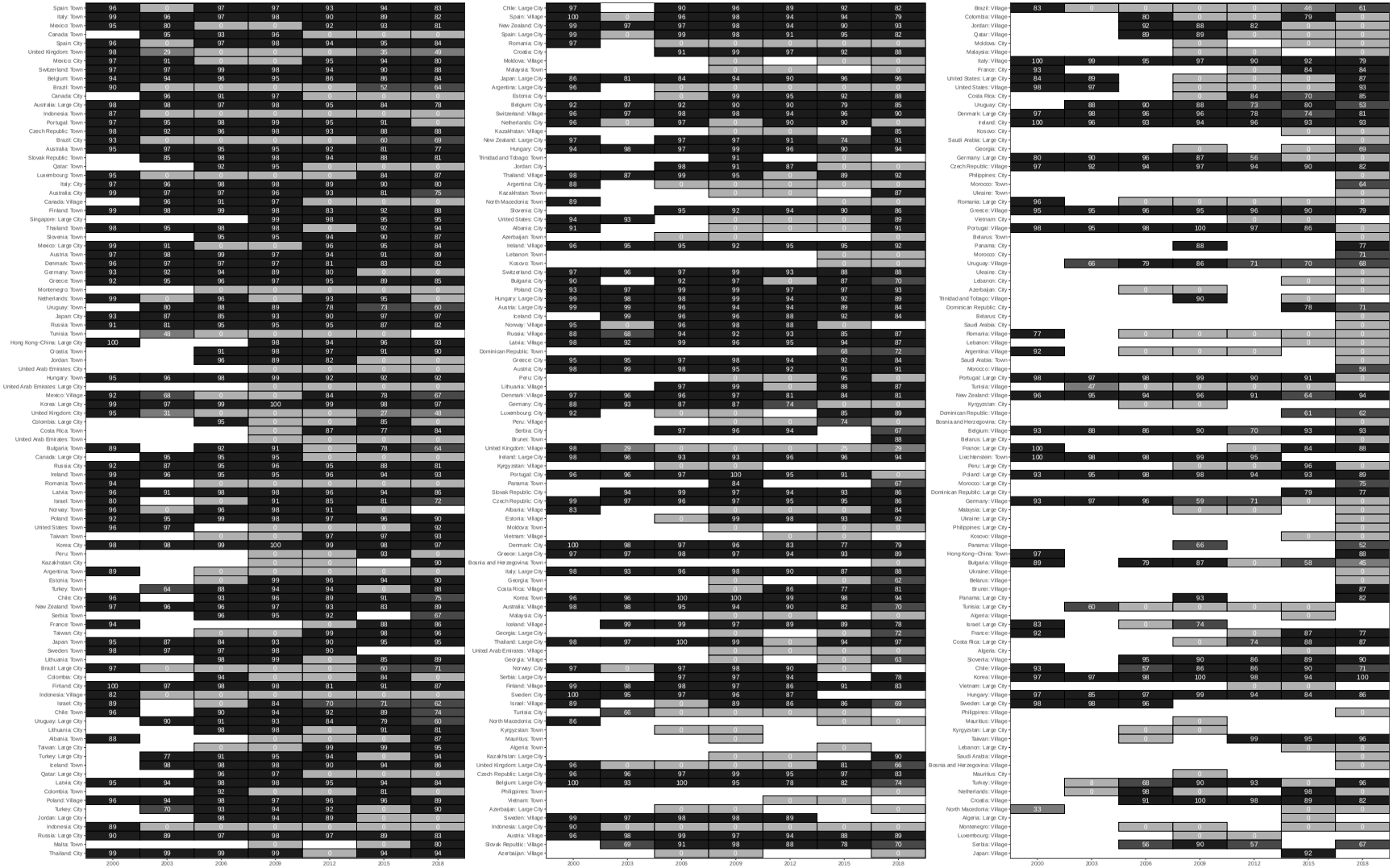


Figure E.2: ICT Sample: Data Availability by Country and Year

Note: Figure displays the share (in percentages ranging from 0 to 100) of full sample observations that appear in the ICT sample, separately for each country-by-urbanicity cell.

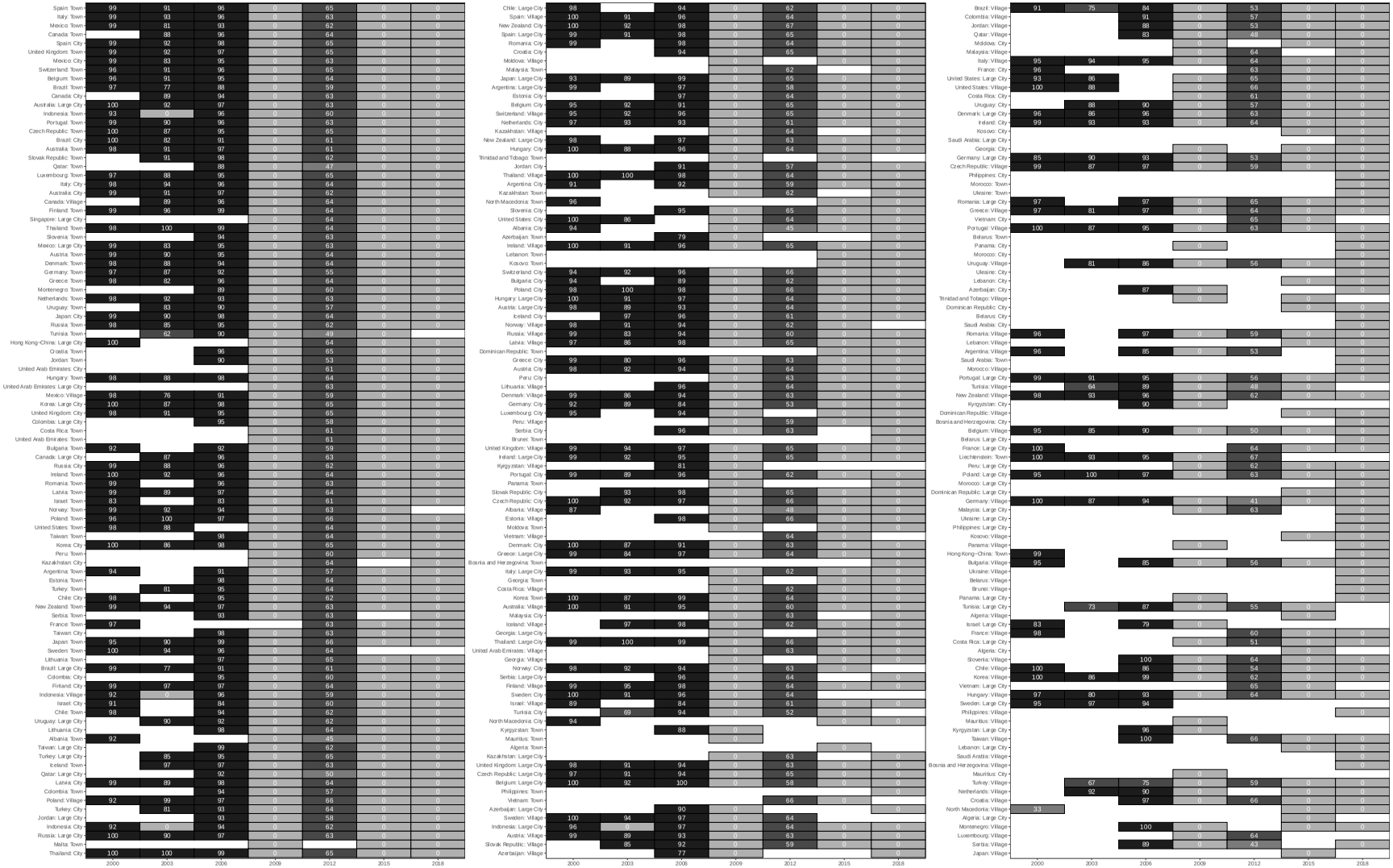


Figure E.3: Weekly Homework Hours: Data Availability by Country and Year

Note: Figure displays the share (in percentages ranging from 0 to 100) of full sample observations for which weekly homework hours data is available, separately for each country-by-urbanicity cell.

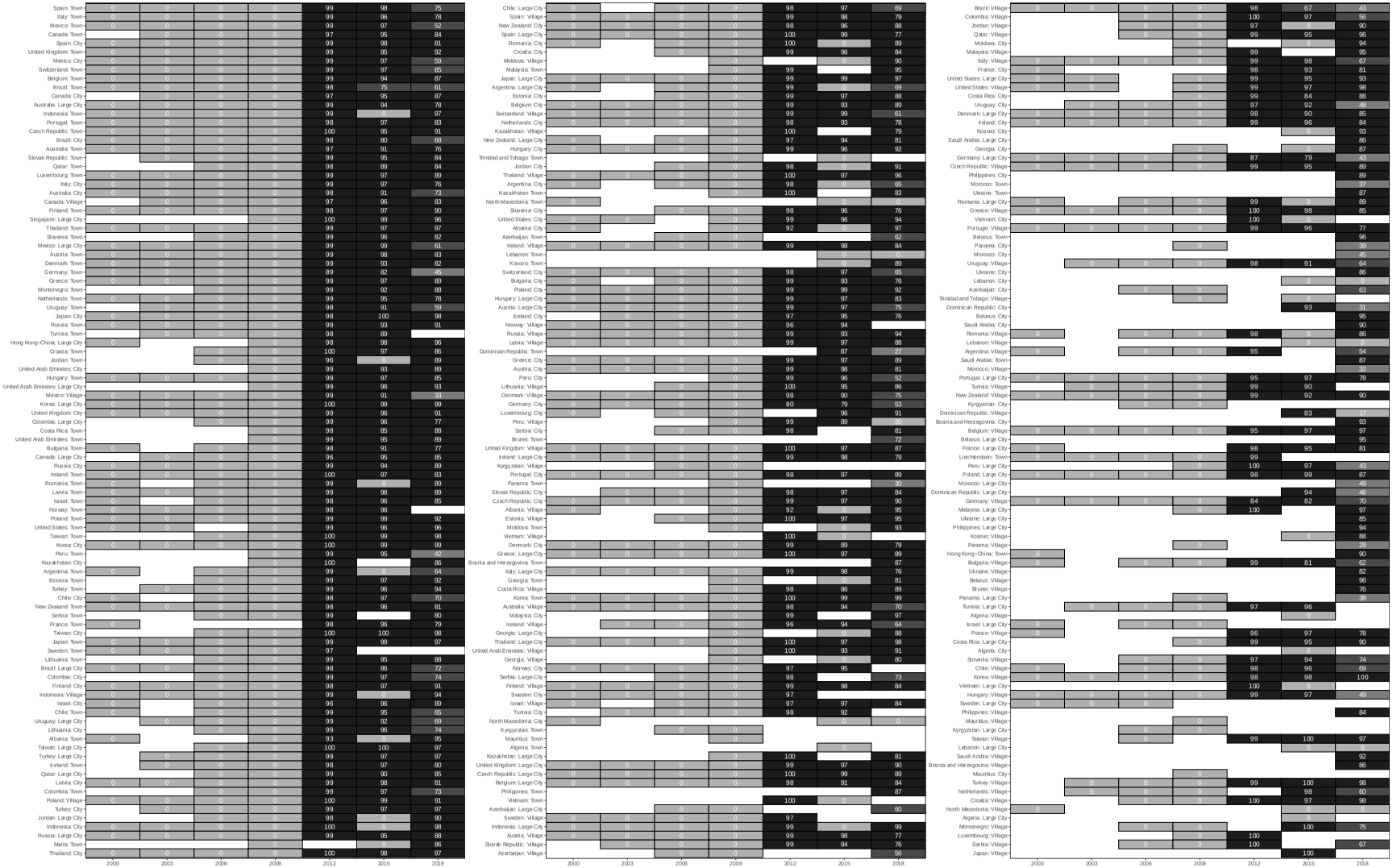


Figure E.4: Days Skipped Past 2 Weeks: Data Availability by Country and Year

Note: Figure displays the share (in percentages ranging from 0 to 100) of full sample observations for which days skipped past 2 weeks data is available, separately for each country-by-urbanicity cell.

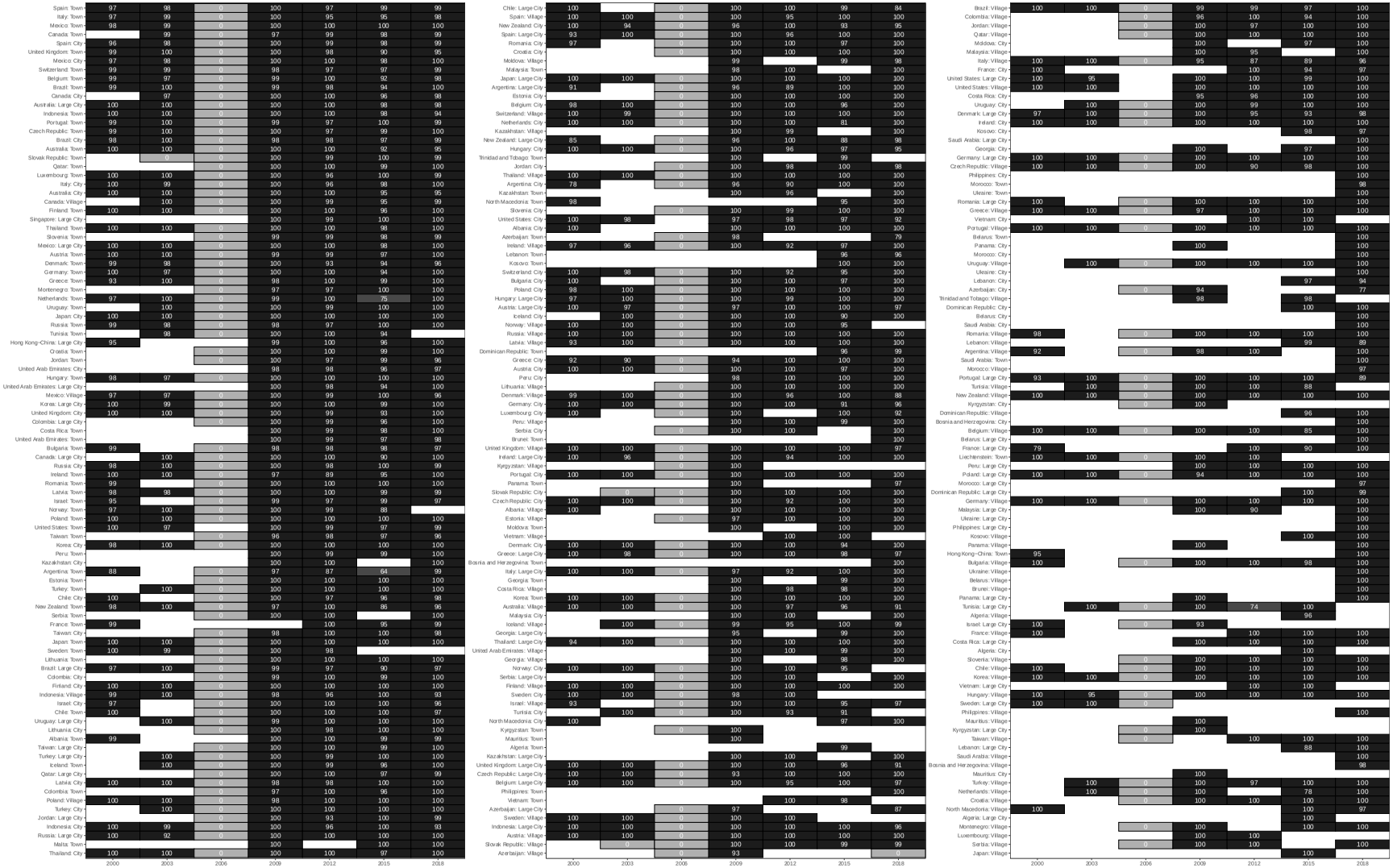


Figure E.5: Perceived Burden of Student Absenteeism: Data Availability by Country and Year

Note: Figure displays the share (in percentages ranging from 0 to 100) of full sample observations for which perceived burden of student absenteeism data is available, separately for each country-by-urbanicity cell.

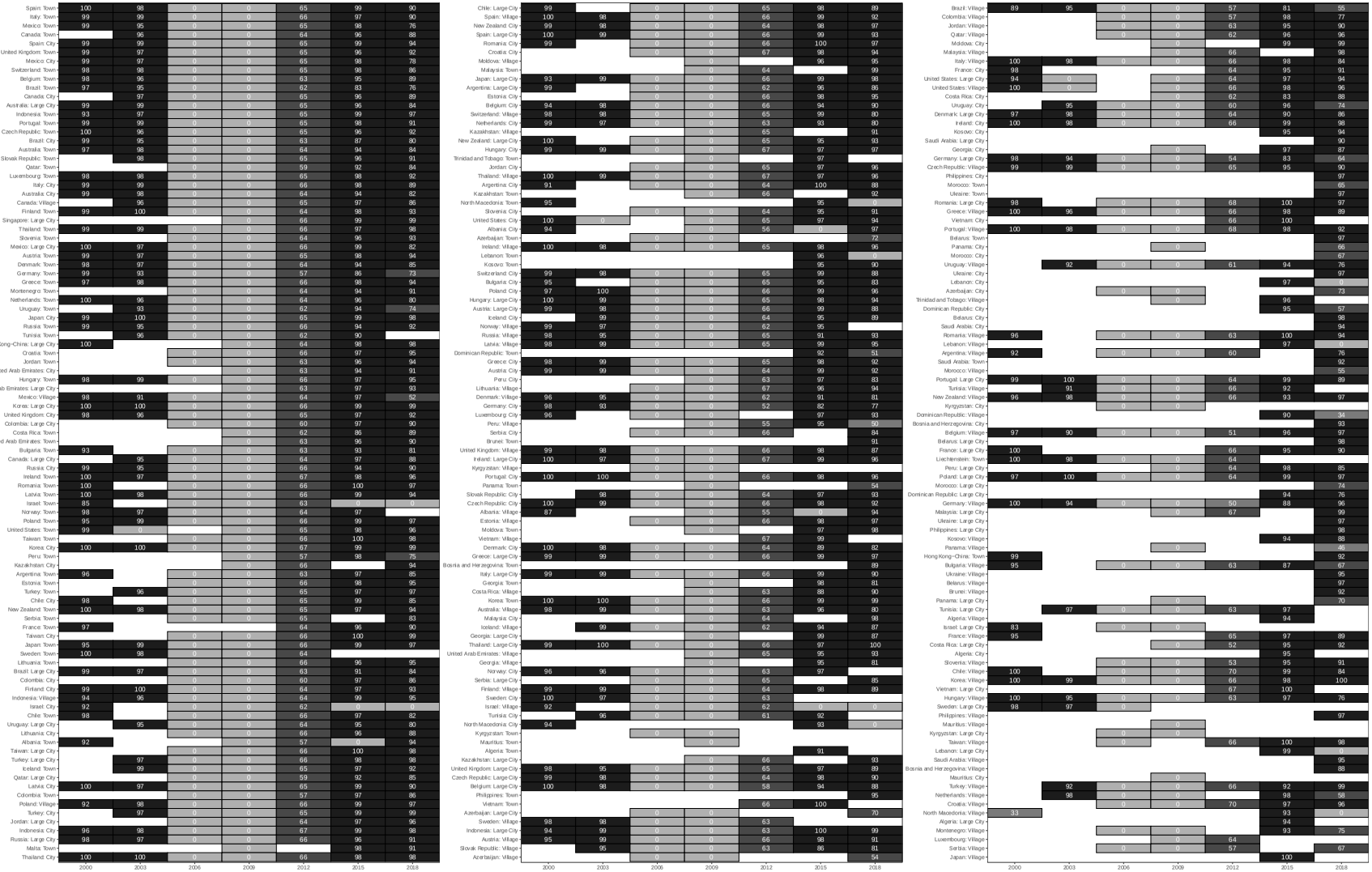


Figure E.6: Friendship Index: Data Availability by Country and Year

Note: Figure displays the share (in percentages ranging from 0 to 100) of full sample observations for which friendship index data is available, separately for each country-by-urbanicity cell.

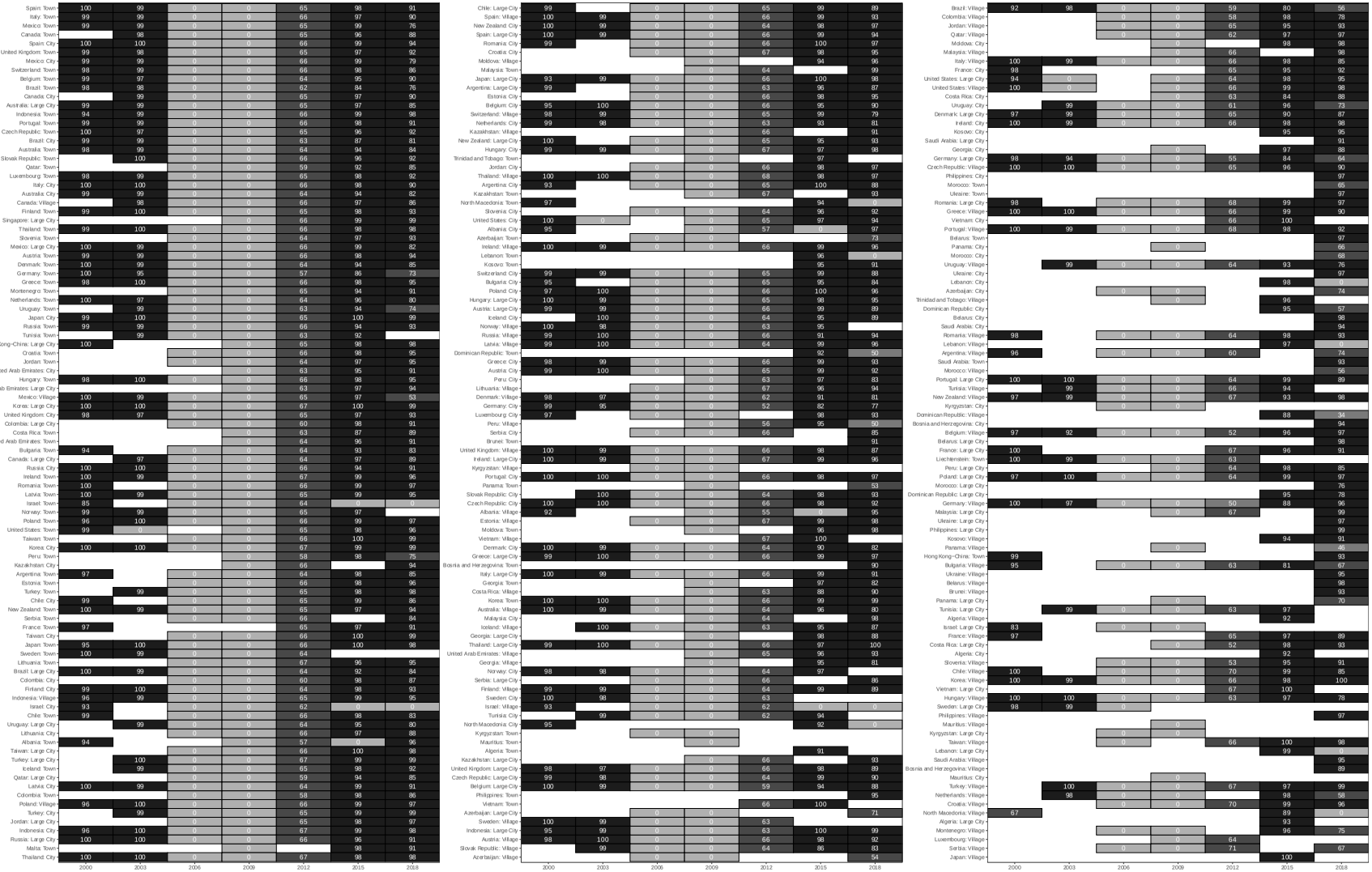


Figure E.7: Belonging Index: Data Availability by Country and Year

Note: Figure displays the share (in percentages ranging from 0 to 100) of full sample observations for which belonging index data is available, separately for each country-by-urbanicity cell.

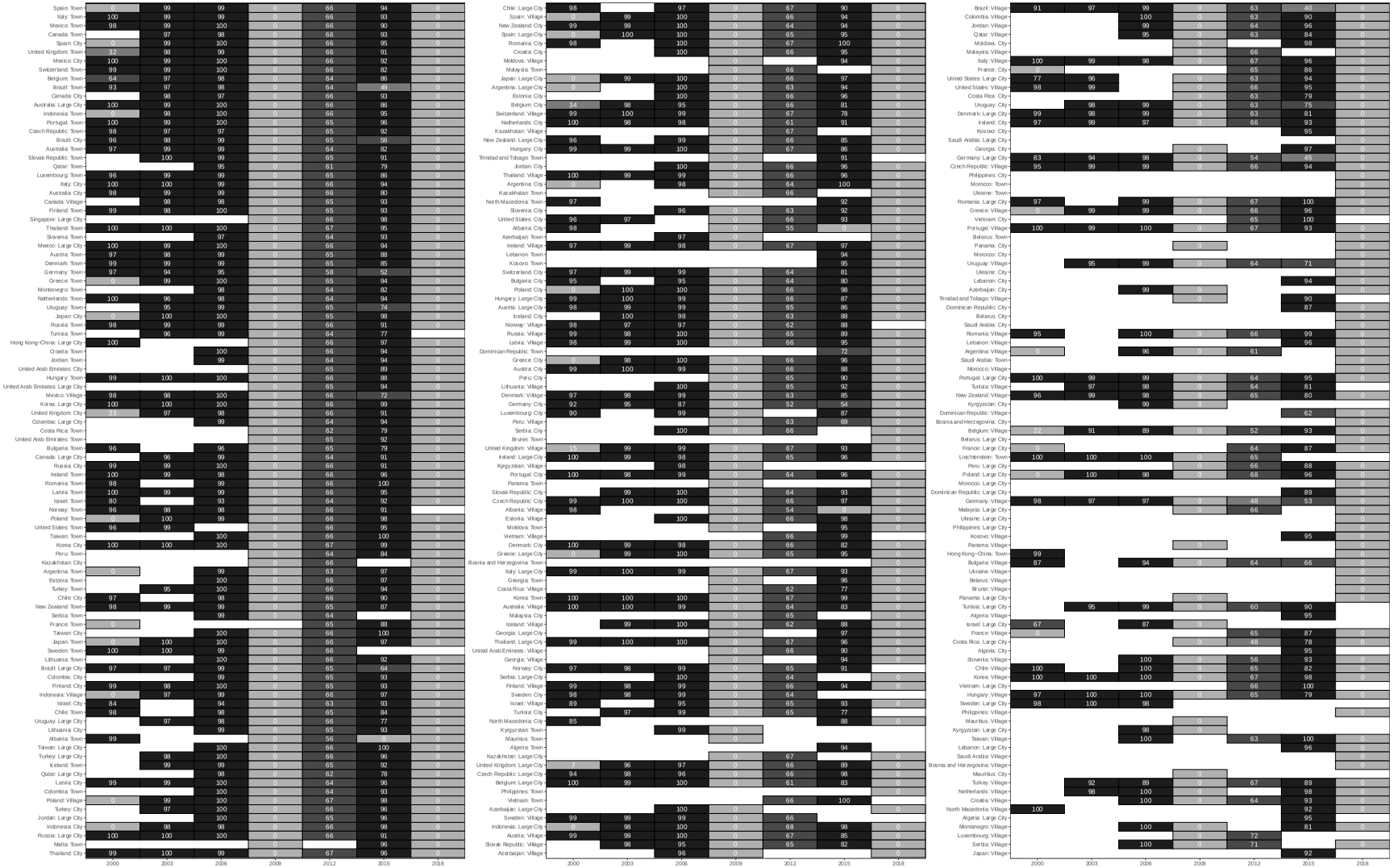


Figure E.8: Self-Efficacy Index: Data Availability by Country and Year

Note: Figure displays the share (in percentages ranging from 0 to 100) of full sample observations for which self-efficacy index data is available, separately for each country-by-urbanicity cell.