3G Internet and Human Capital Development*

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Abstract

We study the impact of mobile internet expansion on student outcomes by exploiting the staggered global rollout of 3G between 2000 and 2018. We link geospatial data on 3G coverage to 2.5 million test scores from 82 countries and find that access to 3G substantially increased smartphone ownership and internet use among adolescents, yet reduced test scores in math, reading, and science by the equivalent of one-quarter of a year of learning. Negative effects are driven by exposure during adolescence and are concentrated among lower-achieving students. Mechanisms include increased passive online activities, reduced study efficiency, and weaker social connectedness.

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1 Introduction

The past two decades have witnessed an unprecedented increase in adolescents' access to mobile internet. Globally, one in three internet users is a child.[1] In the United States, 95 percent of teenagers have access to a cell phone, with nearly half reporting that they use the internet "almost constantly."[2] This ubiquitous internet access and constant digital connectivity are raising pressing concerns among parents, educators, and policymakers worldwide about their consequences (The New York Times, 2024; The Boston Globe, 2024; BBC News, 2024; UNESCO, 2024). In this paper, we ask: *How has the global expansion of mobile internet affected student learning and well-being?*

Theoretically, the answer to this question is ambiguous. On the one hand, greater connectivity can broaden access to information and resources that enhance student learning (The Economist, 2017). On the other hand, increased risks of procrastination, "digital addiction" (Allcott et al., 2022), anxiety, and reduced sleep (Braghieri et al., 2022; Billari et al., 2018; Golin, 2022) could hurt learning. Moreover, empirically, it is challenging to causally estimate the effect of mobile internet access on student outcomes as this is endogenous to family and country-level factors. Rigorous, well-identified evidence on the effect of mobile internet access remains scarce.

In this paper, we provide causal evidence, at the global scale, of the impact of mobile internet on student outcomes. We leverage the global rollout of 3G, a mobile network technology that provides high-speed wireless internet connectivity, between 2000 and 2018, combined with standardized test scores from over 2.5 million students in 82 countries, using data from the Programme for International Student Assessment (PISA). Our empirical strategy exploits variation in the timing and geography of 3G coverage at the subnational level to identify how mobile internet availability during adolescence affects student outcomes. Our approach addresses key identification challenges by using a difference-in-differences framework, employing a rich set of fixed effects to isolate the impact of differential 3G access on test scores. We find that 3G leads 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. Time spent on the internet, particularly out of school, increases by 5 hours per week. We also find evidence of a decline in a sense of belonging and

[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.

ease of making friends. Negative effects on test scores are largest at the bottom of the achievement distribution, and we find suggestive evidence for larger reductions among girls, students from disadvantaged backgrounds, and in students in low- and middle-income countries. Given that adolescence is a crucial time for human capital development (Cunha and Heckman, 2007), these findings not only have direct implications for ongoing debates around smartphone use in schools but also highlight that the risks of constant digital connectivity extend well beyond the classroom.

It is worth noting that 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 much 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 combine two main data sources in this paper. Our test score data on over 2.5 million students comes 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 measures 15-year-old students' mathematics, reading, and science skills in more than 80 countries across Europe, Asia, the Americas, and Africa. 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 well-being. Our geospatial data on 3G coverage comes from Collins Bartholomew. This data provides high-resolution indicators of 3G availability and offers consistent and comparable coverage estimates across countries and over time, allowing us to precisely link changes in mobile internet access to educational outcomes. For identification, we leverage the variation of 3G coverage within and across countries over time, combining geospatial data on 3G coverage with PISA

[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.

data, matching at the school-level urbanicity (village, town, city, large city).[4]

Our main specification is a difference-in-difference strategy leveraging within-country, within-urbanicity variation in 3G coverage over time, employing a rich set of fixed effects to isolate the impact of differential 3G access on test scores. Importantly, we include country-by-urbanicity fixed effects to control for time-invariant differences between urban and rural areas within each country (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, urbanization). We also control for key student demographic characteristics, parental education, and school characteristics.

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 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. Finally, we test and find null effects on a host of placebo outcomes such as student demographics (age, gender), parental background, and school characteristics.

Our main findings are as follows. 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 (7% increase) and 10 percentage points more likely to have a smartphone at home (11% increase). Students report spending an additional 5 hours on the internet each week (17% increase over a mean of 29 hours), the majority (4 hours) of which is driven by using the internet outside of school rather than in school. In turn, 3G expansion results in reductions in student test scores of approximately 0.04 to 0.08 standard deviations. Two benchmarks help contextualize the magnitude of the reduction in test scores. First, the magnitude of these reductions is roughly equivalent to the loss of one-quarter of a year of schooling based on estimates of annual learning (Bloom et al., 2008; Evans and Yuan, 2019). Second, Lavy (2015) finds

[4]PISA does not report precise student geolocations.

that one additional hour of weekly instruction raises test scores by 0.06 standard deviations. In comparison, we estimate that roughly five extra hours of weekly internet use leads to a decline of similar magnitude.

Importantly, the negative effects are larger at the lower end of the test score distribution. Additionally, we find that the negative effects of mobile internet are driven by exposure during adolescence rather than earlier exposure during childhood. Looking at the heterogeneity of effects, we find suggestive evidence that the effects are larger for girls, students whose parents are not college-educated, and students from low- and middle-income countries.

To understand the mechanisms driving the negative effects of 3G coverage on student learning, we examine its impact on students' engagement with schoolwork, attendance, digital leisure activities, home environment, social connectedness, well-being, and self-efficacy. We find that 3G expansion reshaped students' academic experiences by reducing both the quality of their study time and their sense of social connectedness. Although students report spending roughly 20 more minutes per week on homework following 3G rollout, they also become 6 percentage points less likely to use the internet for schoolwork and substantially more likely to use social media daily (17 percentage points). These patterns are consistent with digital distractions reducing the productivity of study time, in line with the observed declines in test scores. We find little evidence that absenteeism meaningfully contributes to these declines: missed school increases only marginally—equivalent to 18 minutes per month—and explains less than 6% of the drop in test performance. Instead, the more salient shift is toward passive online consumption, such as social media use, rather than more interactive activities like chatting or gaming. At the same time, we observe changes in the home learning environment, with fewer books available and indications of weaker parental involvement in students' daily lives. Correspondingly, we find that 3G coverage leads to a decline of 0.09 to 0.16 standard deviations in students' sense of belonging and ease of making friends. Overall, our results suggest that mobile internet access affects student outcomes not only through increased time online on passive consumption activities and reducing study efficiency, but also by eroding students' feelings of belonging and social connectedness.

Our research contributes to the literature on mobile internet connectivity and human capital development by providing the first global, comprehensive causal evidence to date 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 of broadband internet use, their study is limited to one US state and covers only the early home broadband expansion period (2000–2005). In contrast, we examine the effects of 3G-enabled *mobile* internet which allows anytime-anywhere, continuous, and often unsupervised exposure to online content, making digital engagement much more pervasive in students' daily lives than home broadband. Second, rather than looking at a single state or country, we leverage data on nationally representative data 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. 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 digital inclusion and regulation of smartphones in schools (United Nations, 2019).

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. Importantly, we find that it is the out-of-school time spent on the internet that has risen most sharply. This distinction is critical: while policy debates focus on regulating in-school screen time, our results show that the effects of constant mobile connectivity extend far beyond the classroom.

Our study also contributes to the literature on education technology by providing large-scale evidence on the general equilibrium effects of everyday exposure to (3G) mobile internet. Unlike prior research, which focuses on targeted experimental interventions in single-country settings, we examine how widespread, unrestricted 3G internet access affects adolescent learning. Previous studies have found mixed effects of technology on education—ranging from negative or null effects of home computers and laptops in Romania, California, and Peru (Malamud and Pop-Eleches, 2011; Fairlie and Robinson, 2013; Beuermann et al., 2015; Malamud et al., 2019) to positive effects of targeted remedial programs (Muralidharan et al., 2019) and computer-assisted learning

(Ganimian et al., 2020; Escueta et al., 2020; Rodriguez-Segura, 2022). However, unlike structured learning tools, mobile internet provides constant, unregulated connectivity, raising the potential for distraction at an unprecedented scale. Moreover, these experimental 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 technology and skill development.

Finally, our research contributes to the nascent literature on the socioeconomic and political effects of mobile internet expansion. Recent work has shown that 3G changed political attitudes, voting behavior (Guriev et al., 2021; Melnikov, 2021; Falck et al., 2014; Golin and Romarri, 2022), female labor force participation (Chiplunkar and Goldberg, 2022), and child injuries (Palsson, 2017).[5] We add to this literature by focusing on the impacts of 3G internet on education—a crucial but underexplored dimension of digital expansion—offering new insights into how mobile internet is transforming student behavior and learning worldwide. Our results provide timely evidence for ongoing education policy debates. While concerns about digital distraction have already motivated smartphone bans in schools, our findings caution that these policies alone may be insufficient as the bulk of the increase in time spent on the internet happens outside of school hours. Given that the effects are more severe for disadvantaged students and lower-achieving students, unchecked mobile internet use risks further widening existing achievement gaps.

The rest of the paper proceeds as follows. Section 2 describes the data. Section 3 outlines our methodology, and Section 4 presents our results, robustness checks, and heterogeneity analysis. 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.[6] In each country, PISA aims to

[5]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).

[6]A full list of participant countries can be found at PISA Country List.

obtain a sample that is representative of "15-year-old students attending educational institutions in grades 7 and higher."[7] We use student-level PISA data from seven rounds of testing between 2000 to 2018.[8]

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."[9] 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.[10]

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 participating 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 other aspects of students' lives, we additionally examine homework

[7] PISA 2018 Technical Report, Chapter 4.

[8] In response to the COVID-19 pandemic, the OECD postponed the scheduled PISA 2021 assessment.

[9] What is PISA?, About PISA.

[10] 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.

hours, absenteeism, indicators of their home learning environment, ease of making friends, sense of belonging, and self-efficacy. Not all of these variables are available for all years or all students. In cases where the scales or the text of the questionnaires vary across years, Appendix A details how we harmonize measures across PISA rounds.

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 in every country 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 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, Brazil, Canada, Italy, 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. This data has been widely used in recent economic studies looking at the impact of 3G on socio-economic and political outcomes (e.g., Guriev et al. (2021)). 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 the Gridded Population of the World and the Urban Centre Database, we use population estimates as of 2015 throughout our calculations.[11])

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 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

[11]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.

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.[12] Many areas had non-zero 3G coverage in 2007, the first year for which we have 3G coverage data. 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.[13]

We combine these datasets to estimate 3G coverage for each country-by-urbanicity pair monthly, using linear interpolation between the yearly observations.[14] 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, we calculate each country-by-urbanicity cell's average population-weighted lightning frequency

[12] Unweighted averages would over-represent sparsely populated rural areas or remote regions. Our measure of regional 3G coverage takes into account differences in population density within regions to better capture access and exposure. Recent papers on the effects of 3G internet on political outcomes (e.g. Guriev et al. (2021)) also use this weighted measure for coverage.

[13] 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.

[14] Importantly, any measurement error from this aggregation would likely attenuate the estimated effects toward zero. Thus, while aggregation may introduce some noise, it biases our results conservatively rather than inflating the magnitude of the estimated declines.

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 are approximately -0.3 in the full sample and -0.1 in the ICT sample, with these representing simple unweighted averages across all students and years.

Both panels additionally summarize a set of variables related to homework, attendance, parental interactions, social connectedness, and well-being. As noted above, these questions were not administered universally across countries and PISA rounds.

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 85 percent used a smartphone at home. 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.[15] After increasing between 2000 and 2009, student test

[15] 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.

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 its 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.

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 in the 12 months preceding the exam.[16]

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 urban 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.[17] 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

[16]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.

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

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 Figure 4.

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 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.[18]

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

[18]Contemporaneous 2G availability does not have an independent effect on PISA test scores. These results are available upon request.

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 outline results from robustness checks and test for heterogeneous effects across different groups of students.

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 1.5 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.

Panels A through D of Table 2 show estimates of the effect of 3G on smartphone ownership and use. 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 A, are 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 B, are much larger and suggest that 3G coverage increases smartphone ownership by 10 percentage points.

In Panels C and D, we test for effects on student smartphone ownership and use, which are derived from the same responses used to construct outcomes in Panels A and B. The magnitudes of these estimates are slightly larger, indicating that 3G access increased smartphone use by 7 to 11 percentage points.

Panel E 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 roughly 7% of the sample mean of 0.53 (shown in Table 1).

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.[19] 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 Placebo Outcomes

First, we identify a set of placebo variables—characteristics that either predate the arrival of 3G or are unlikely to be affected by it. These include student gender and age, parents' years of education, whether the mother or father lives at home, number of bathrooms in the home (as a proxy for socioeconomic status), school funding sources, and the number of math periods per week. For each of these variables, we estimate Equation 1, excluding all control variables except for the fixed effects. Appendix Figure D.1 presents the results of these regressions, both with and without country-by-year fixed effects. Without country-by-year fixed effects, most estimates are statistically insignificant, although we find positive and significant effects for whether the student's

[19] 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.

mother or father lives at home. However, once country-by-year fixed effects are included, all estimates become statistically insignificant. These findings support our difference-in-differences identification strategy by showing no systematic changes in unrelated outcomes and underscore the importance of including country-by-year fixed effects to account for potentially confounding trends.

4.3.2 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 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.2. As can be seen in Appendix Figure D.2, 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.

4.3.3 Alternative Difference-in-Difference Estimators and Event Study Estimates

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. These estimates are shown in Figure 4. 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.

4.3.4 Instrumental Variables Estimates

We describe our instrumental variables results, which use lightning frequency and prior 2G coverage as instrumental variables as instruments for 3G coverage, in Appendix C.[20] 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

We explore treatment effect heterogeneity across different subsets of students, using three complementary analyses. First, we estimate the effects of 3G at different points on the student test score distribution. Second, we explore age-specific exposure to identify at what ages during childhood 3G exposure matters the most. Third, we estimate fully interacted models, allowing the effects of 3G to vary by student, family, or country characteristics.

4.4.1 Heterogeneity in Effects along the Student Test Score Distribution

To estimate distributional effects, we follow the same approach used in our alternative robust difference-in-differences estimators described above, collapsing the data to the country-by-urbanicity-by-year level (to simplify computation). Within each of these cells, we compute test score vigintiles (e.g., the scores at the 5th, 10th, ..., 95th percentiles). We then estimate Equation 1 separately for each vigintile and each subject.

Figure 5 presents the results. For both math and reading, we find a clear pattern: the negative effects of 3G are concentrated at the lower end of the student test score distribution, while effects at the upper end are smaller and statistically insignificant. For science, point estimates are also

[20] Our main TWFE results are robust to the inclusion of contemporaneous 2G availability. These results are available upon request.

negative across the distribution, but consistently include zero and do not display a strong visual pattern. These results suggest that the adverse effects of 3G exposure are concentrated among students with the lowest test scores.

4.4.2 Heterogeneity in Effects by the Time of Exposure during Childhood

Next, we assess whether the timing of 3G exposure during childhood matters by constructing age-specific measures of exposure prior to taking the PISA test at age 15. Specifically, we estimate effects separately for four developmental windows: ages 4–6, 7–9, 10–12, and 13–15. We prefer this approach over collapsing all years into a single cumulative measure, which would implicitly assume that exposure effects are linear and additive across childhood, which is unlikely in light of the cognitive development literature (Cunha and Heckman, 2007).

To implement this, we define the 3G entry year as the first year in which a country-urbanicity cell reached 10% 3G coverage.[21] Using this entry year and students' birth years, we compute the number of years of 3G exposure they experienced during each of four age ranges: 13–15, 10–12, 7–9, and 4–6. For example, a student who was 15 in 2018 and lived in an area where 3G was introduced in 2014 (when the student was 11) would have had three years of exposure at ages 13–15, two years at ages 10–12, and none at earlier ages.

We then re-estimate Equation 1, replacing the single 3G exposure variable with the four age-specific exposure measures in separate regressions. The results, shown in Figure 6, indicate that exposure before age 13 generally has weak or insignificant effects. In contrast, exposure at ages 13–15 has consistently negative and statistically significant effects: for math and reading, coefficients are significant at the 5% level; for science, at the 10% level. The estimated effects imply that an additional year of exposure during early adolescence reduces test scores by 0.025 standard deviations. These findings suggest that early teenage years are a particularly sensitive period for the effects of 3G exposure. The concentration of the effects in this age window also supports the interpretation that it is likely students' direct exposure to mobile internet—rather than broader factors that change in response to 3G access (e.g., downstream effects of political changes documented in Melnikov (2021))—that drives the decline in test scores.

[21] To improve precision, we allow the 3G entry year to be any calendar year, not just a PISA survey year.

4.4.3 Heterogeneity by Student, Family, and Country-Income Status

Finally, we consider heterogeneity across student, family, and country characteristics by fully interacting our TWFE models with these 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.[22]

Heterogeneity by Student Gender: Appendix Tables D.4 and D.6 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.

Heterogeneity by Parental Education: 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.

Heterogeneity by Country's Income Status: 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 parents' ability to utilize technology produc-

[22] 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.

tively for educational purposes.[23] 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.[24]

5 Potential Mechanisms

We now examine possible mechanisms that could drive the negative effects by studying how 3G coverage affected students' engagement with schoolwork, learning at home, attendance, digital leisure activities, social connectedness, well-being, and self-efficacy.

5.1 Engagement with Schoolwork and Attendance

The arrival of mobile internet potentially changes both the opportunity cost of studying and the productivity of study time. Table 4 presents results examining the effects of 3G coverage on students' academic time use, including time spent on homework and attendance.

Panel A of Table 4 shows the impact of 3G coverage on weekly homework hours. Results suggest that 3G coverage is associated with an increase of approximately 20 minutes per week in the time students spend on homework, statistically 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, increased internet access might lead to digital distractions and diminish the quality of study time despite the increase in hours spent on homework: students are 6 percentage points less likely to use the internet for schoolwork (significant at the 5% level) and slightly more likely to use mobile phones for homework (though not statistically significant). Additionally, we do not observe a significant increase in the use of mobile learning apps or websites (Panel B). Taken together, these findings suggest that while students may be allocating more time to homework, they do not necessarily use the internet more productively for school-related tasks. These results align with previous research suggesting that while more time may be allocated to homework, the effectiveness of that time could be reduced due to multitasking or interruptions (Allcott et al., 2022). Moreover, the decline in test scores is most pronounced among subgroups of students who browse more, particularly those from non-high-income countries and students

[23]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.

[24]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.

whose parents do not have a college education (Appendix Tables D.4 and D.6). These findings align with recent evidence on the importance of cognitive endurance (Brown et al., 2022; Reyes, 2023) and the adverse effects of digital distractions on learning efficiency (Allcott et al., 2022).[25]

We find little evidence that 3G affects school attendance. Panel E 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.8). Accordingly, we also do not see a significant increase in school administrators reporting that student absenteeism hindered learning "A lot" after the arrival of 3G. Therefore, the decline in test scores is unlikely due to increased student absenteeism.

5.2 Digital Leisure Activities: Chatting, Gaming, and Social Media

Table 5 presents results examining the effects of 3G coverage on students' digital leisure activities. Panels A and Panel B display estimates of the effect of 3G coverage on social media use. Panel A reports results for 2012–2018, the only years in which this question was asked. Panel B imputes zeros for 2000 and 2003. Both panels indicate that 3G expanded social media use; Panel B shows a substantial increase in daily social media use (an increase of 17 percentage points, significant at the 1% level). At the same time, Panels C and D show that 3G coverage does not significantly affect the frequency of online chatting or playing computer games daily. These results hint at the pervasive role of internet access in shaping students' leisure time. The overall trend suggests that 3G coverage drives a noticeable shift in time allocation toward passive consumption activities, such as social media use, rather than more interactive and social uses of the internet, such as chatting or gaming. This also aligns with the recent survey showing that YouTube is by far the most widely used platform among U.S. teens (90% in 2024), followed by social media platforms like TikTok, Instagram or Snapchat (55-60%), and much smaller shares who use communication-oriented platforms such as WhatsApp (23%), Facebook (32%) or Reddit (14%) (Pew Research Center, 2024).

[25] We explored testing measures of disengagement and cognitive endurance (e.g., non-response, rapid guessing), but data and comparability constraints across PISA waves prevent systematic analysis.

This underscores that much of teens' online time is spent on content consumption rather than interactive engagement.

5.3 Social Connectedness, Well-Being and Self-efficacy

Panels A, B, and C of Table 6 examine whether 3G coverage affects students' sense of social connectedness, well-being, and Self-efficacy. We standardize two indicators of social connectedness—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 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.

We also 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 6, we find that academic self-efficacy is also negatively associated with 3G coverage, but this effect is not statistically significant.

5.4 Parental Interactions and Home Environment

Finally, we examine whether 3G affected students' home environment through changes in their interactions with their parents. Although data availability is limited—many outcomes draw on the parental questionnaire, which many countries did not administer—the overall pattern is consistent with weaker parental involvement in students' daily lives (Table 7). Students exposed to 3G are less likely to report everyday conversations with parents, less likely to receive homework help, and less likely to discuss politics or social issues at home, with all coefficients negative (except discussions about school performance, which shows a positive but statistically insignificant increase). These results complement our finding that 3G exposure reduces the number of books at home by 8 percentage points (significant at 1%), suggesting that the broader home learning environment also deteriorates (Table 4 Panel F). Taken together, these findings reinforce our interpretation that mobile internet access not only reshaped how adolescents allocate their own time but also their home learning environment.

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 3G internet access has fundamentally increased how much time adolescents spend online and this is causally associated with substantial reductions in student achievement, equivalent to nearly a quarter of a year's learning. These effects are concentrated among disadvantaged students and those at the bottom of the achievement distribution, raising further concerns about the potential for widening educational inequality in the digital era.

PISA remains the largest and most comprehensive international assessment, enabling analysis of educational outcomes, student behavior, and access to technology globally. Our findings contribute new and timely insights for ongoing debates on smartphone use in schools. While policymakers globally have responded by calling for restricting smartphone use in classrooms, our results show that the bulk of increased internet use occurs outside of school hours, where policies are far less developed. Addressing the risks of mobile internet for adolescents may therefore require a broader policy toolkit, including parental guidance and digital literacy initiatives. 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 and provide timely insights for global policymaking. In many low- and middle-income countries where mobile internet penetration is now rapidly accelerating, these findings highlight both the transformative potential of connectivity and the risks it may pose for equity in education.

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.

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Main Tables and Figures

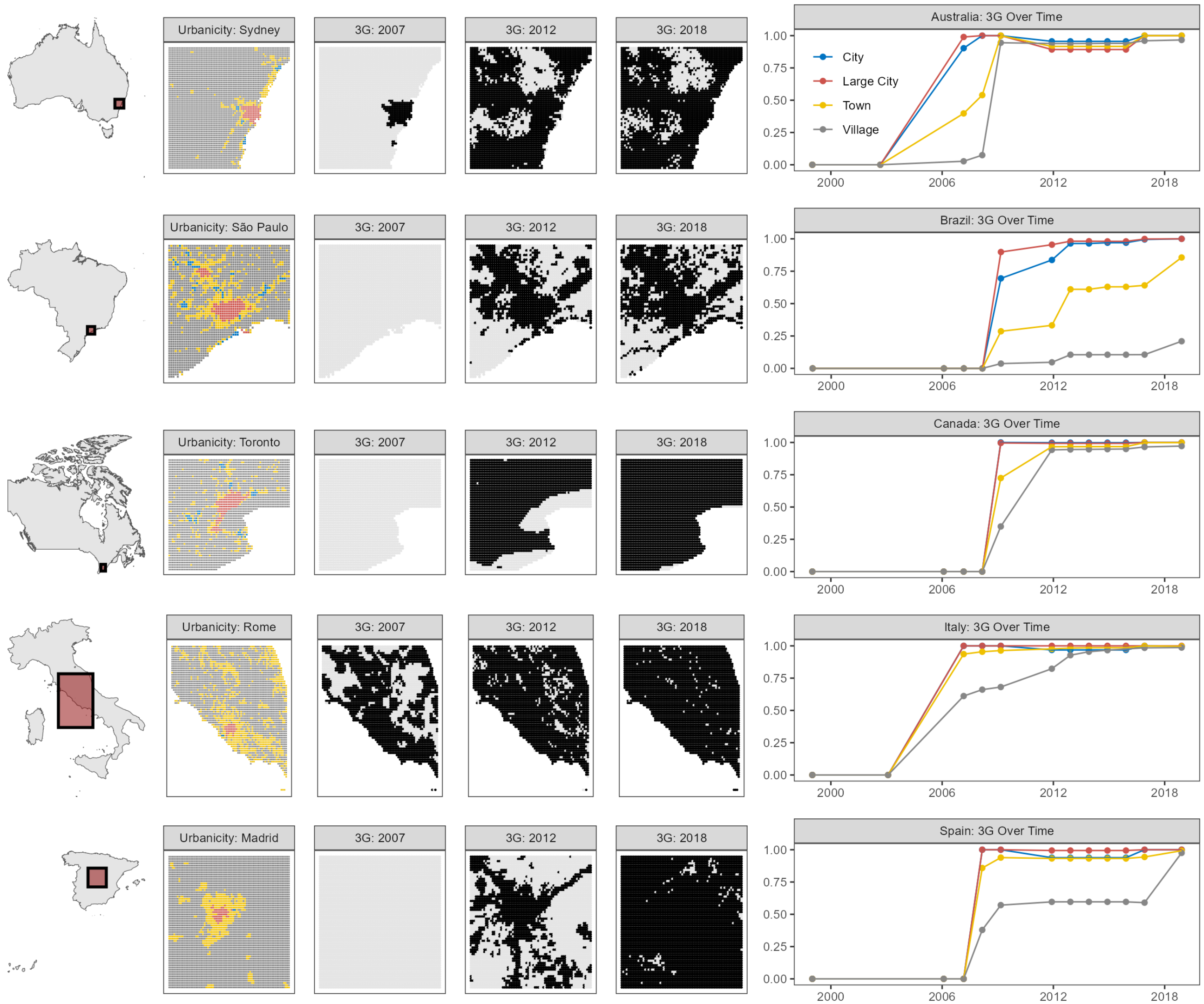

Figure 1: 3G Calculation Illustration

Note: Figure summarizes the calculations for 3G expansion in five countries: Australia, Brazil, Canada, Italy, 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.

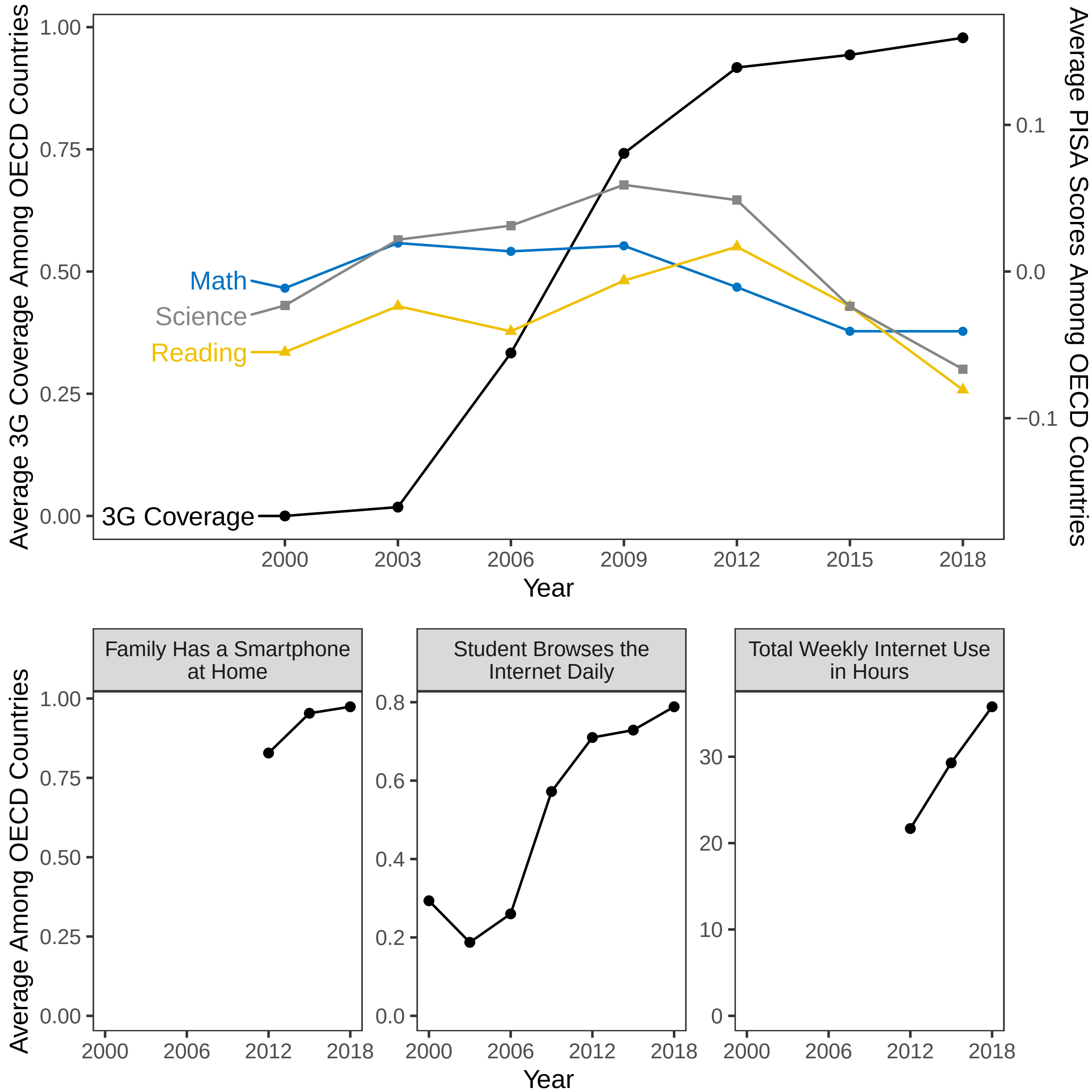

Figure 2: Trends in 3G Coverage, Test Scores, and Internet Access and Use in OECD Countries

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.

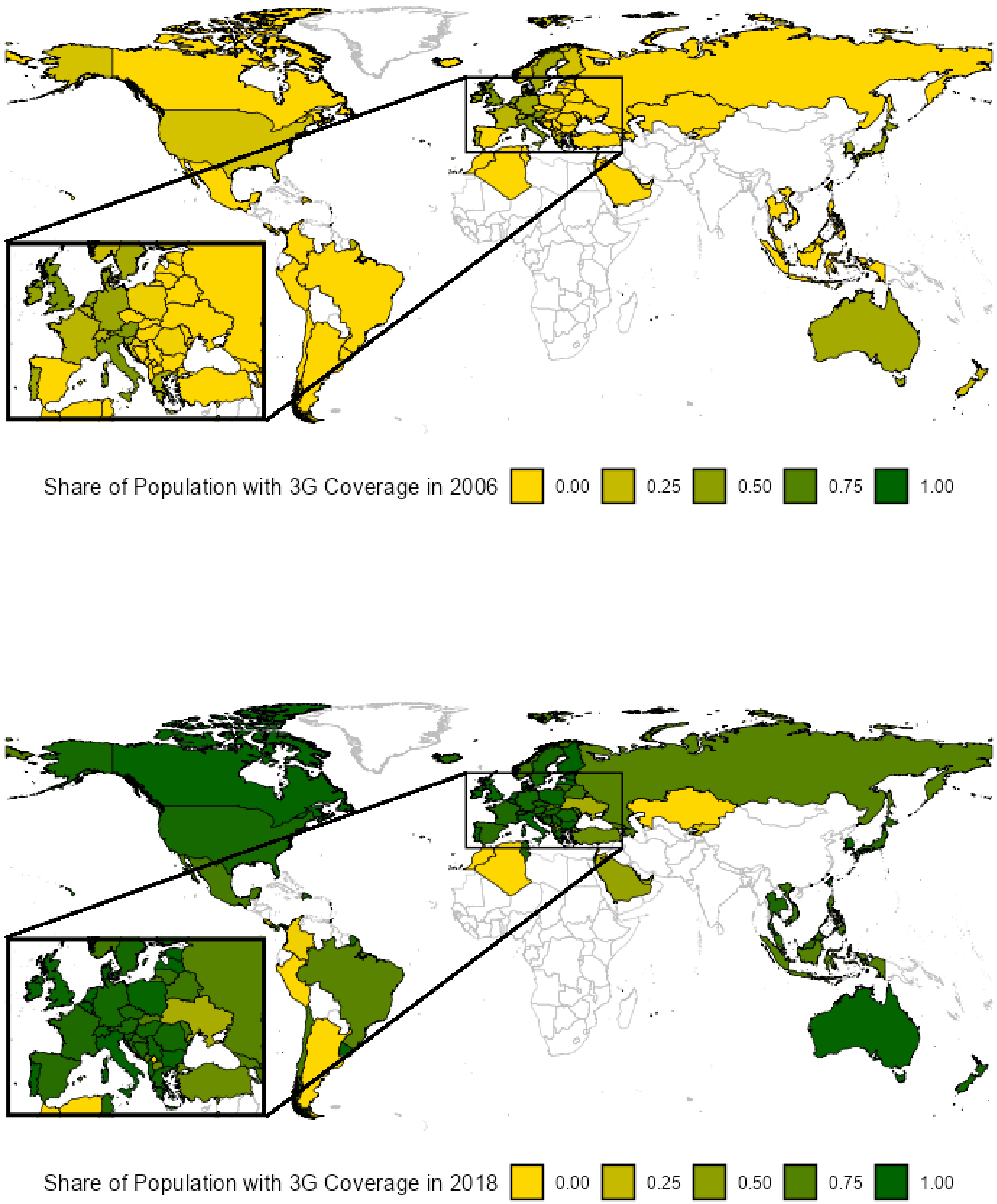

Figure 3: Country-Level 3G Coverage in 2006 and 2018

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

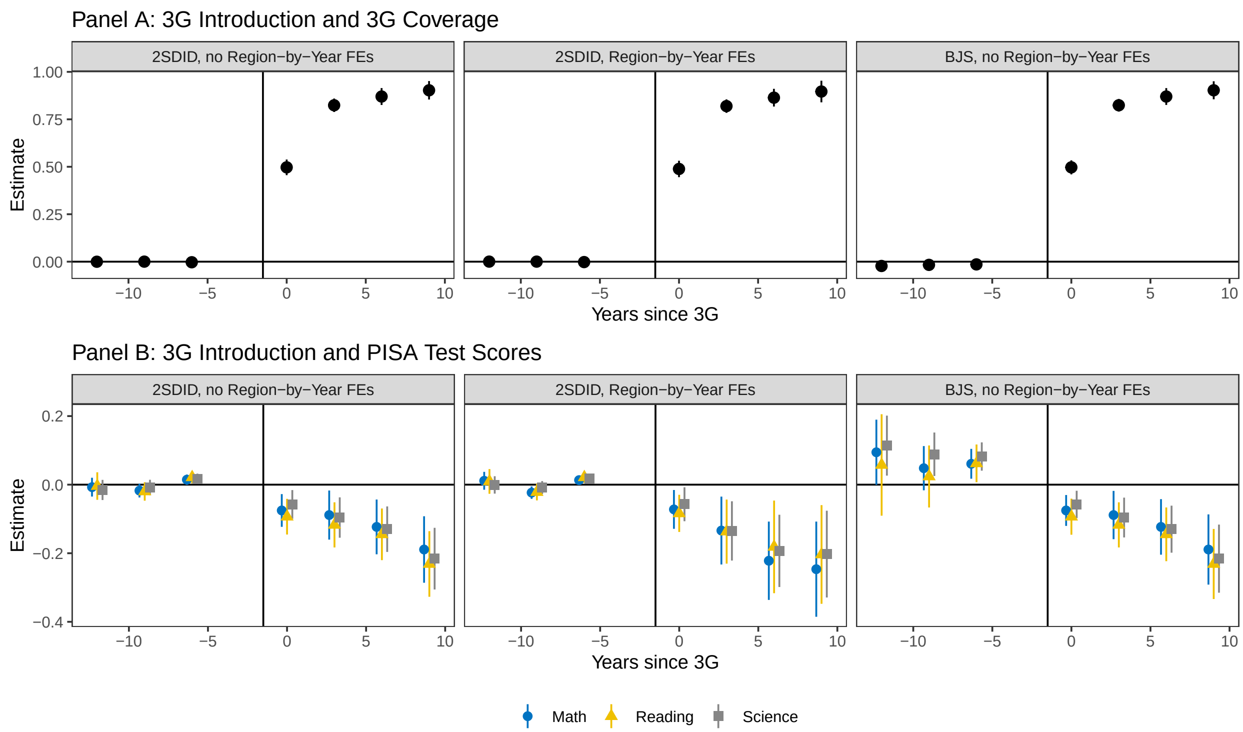

Figure 4: Dynamic Effect of 3G Entry on PISA Test Scores: Borusyak et al. (2024) and Gardner et al. (2023) Estimators

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 are clustered at the country-by-urbanicity level. Error bars indicate 95% confidence intervals. 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.

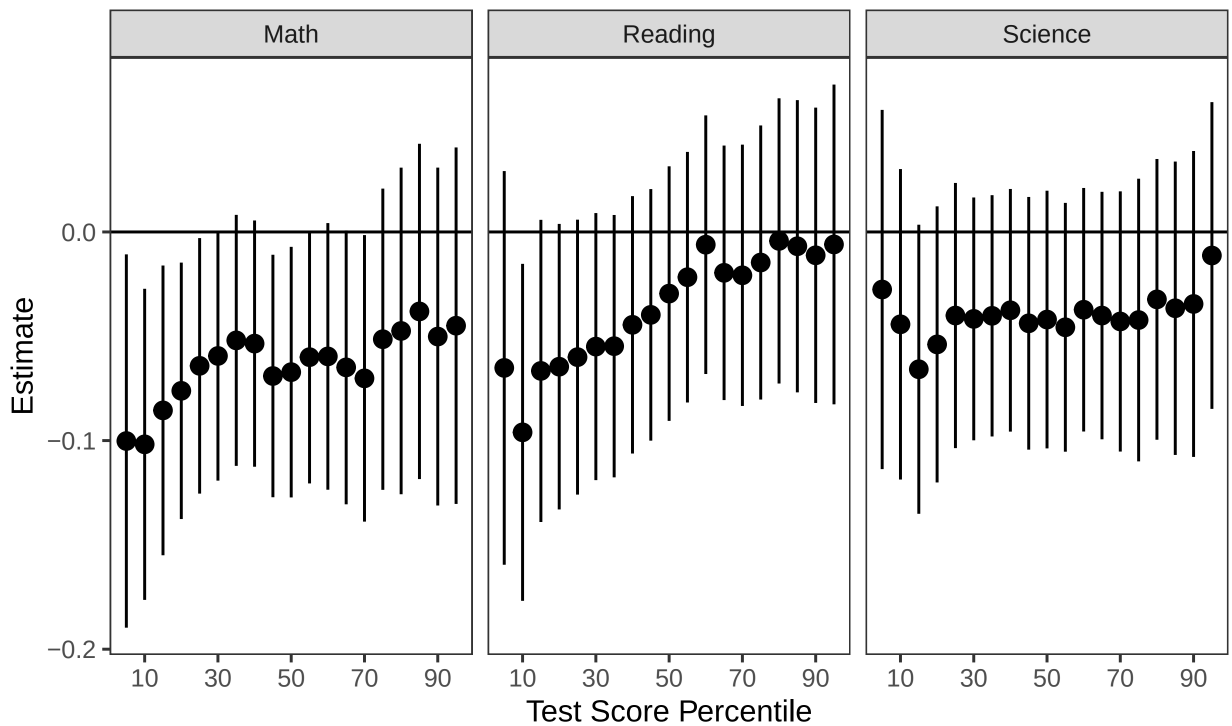

Figure 5: Distributional Heterogeneity in the Effects of 3G on PISA Test Scores

Note: Figure displays the distribution of the estimated effect of 3G coverage on test scores, separately by vigintile. Each point represents a separate regression estimating the effect of 3G on test scores at a specific point in the test score distribution within a country-urbanicity-year cell. All estimates include fixed effects for country-by-urbanicity, urbanicity-by-year, and country-by-year. Robust standard errors are clustered at the country-by-urbanicity level. Error bars indicate 95% confidence intervals. 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.

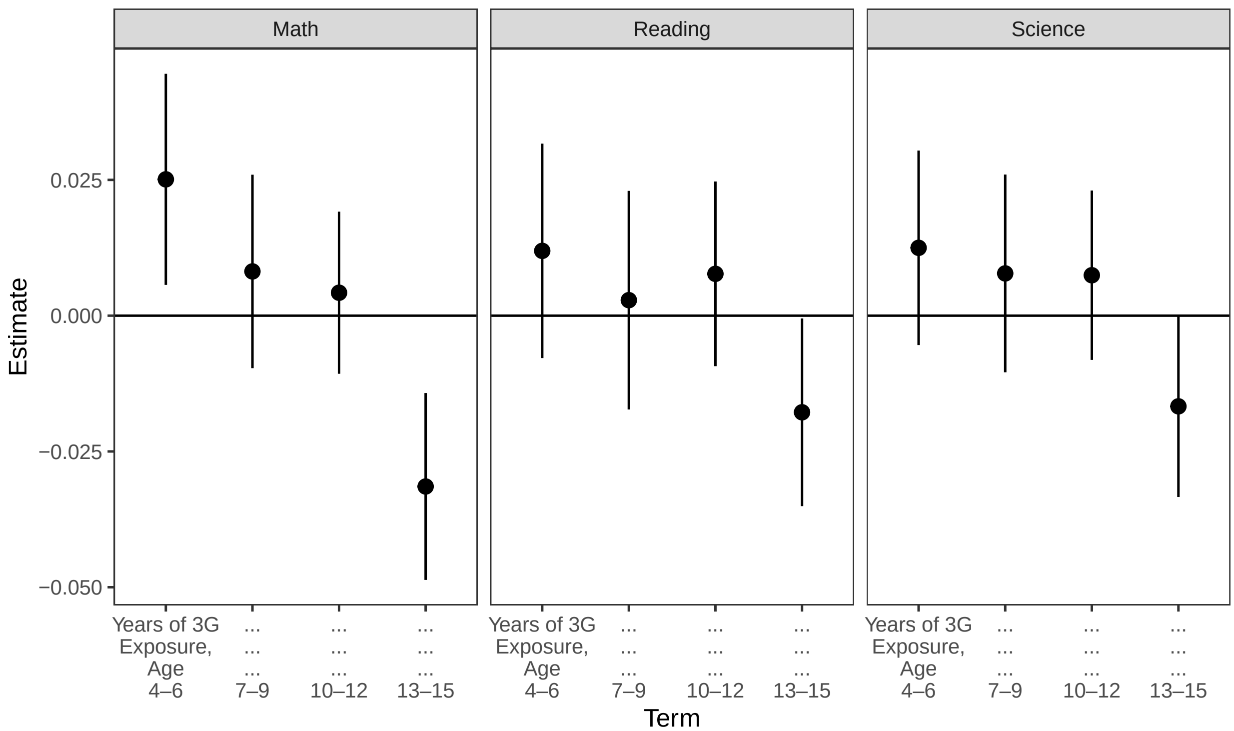

Figure 6: Effects of Age-Specific 3G Exposure on PISA Test Scores

Note: Figure displays age-specific estimates of the effect of 3G coverage on test scores. All estimates include fixed effects for country-by-urbanicity, urbanicity-by-year, and 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. Robust standard errors are clustered at the country-by-urbanicity level. Error bars indicate 95% confidence intervals. 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.

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
log(Number of Books at Home) (2003-2018)	2,493,799	3.877	1.468
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	1,516,674	0.494	0.500
Age	1,516,674	15.784	0.291
Is Immigrant	1,516,674	0.047	0.212
Father's Years of Education	1,516,674	13.286	3.879
Mother's Years of Education	1,516,674	13.332	3.938
School is Private, Independent	1,516,674	0.054	0.227
School is Private, Non-Independent	1,516,674	0.110	0.313
Weekly Homework Hours (2000-2006, 2012)	590,492	6.432	5.391
Days Skipped Past 2 Weeks (2012-2018)	750,087	0.733	1.378
log(Number of Books at Home) (2003-2018)	1,485,679	4.097	1.424
Friendship Index (2000-2003, 2012-2018)	874,403	−0.008	0.986
Belonging Index (2000-2003, 2012-2018)	878,835	0.005	1.002
Self-Efficacy Index (2000-2006, 2012-2015)	859,606	0.016	1.051
Math Score	1,516,674	−0.097	0.938
Reading Score	1,516,674	−0.114	0.942
Science Score	1,516,674	−0.063	0.936
Share of Population with 3G Coverage	1,516,674	0.617	0.418
Has a Smartphone at Home (2012-2018)	788,037	0.897	0.304
Has & Uses a Smartphone at Home (2012-2018)	788,037	0.841	0.365
Total Weekly Internet Use in Hours (2012-2018)	788,037	29.212	22.288
Browses the Internet Daily	1,516,674	0.534	0.499
Ever Uses Mobile Phone for Homework (2015, 2018)	524,728	0.585	0.493
Ever Uses Internet on Comp./Dig. Device for Schoolwork (2009-2018)	1,069,870	0.848	0.359
Any Mobile Learning Apps (2015, 2018)	524,728	0.537	0.499
Uses Social Media Daily (2012-2018)	788,037	0.720	0.449
Chats Online Daily (2003-2018)	1,498,897	0.550	0.498
Plays Computer Games Daily (2000-2012)	991,946	0.264	0.441

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: Has a Smartphone at Home (2012-2018)		
3G	0.032	0.040
	(0.038)	(0.039)
Num.Obs.	788037	788037
R2	0.124	0.134
Panel B: Has a Smartphone at Home (2012-2018; 2000, 2003 Set to 0)		
3G	0.099**	0.096**
	(0.046)	(0.045)
Num.Obs.	993872	993872
R2	0.775	0.777
Panel C: Has & Uses a Smartphone at Home (2012-2018)		
3G	0.062	0.071*
	(0.041)	(0.041)
Num.Obs.	788037	788037
R2	0.141	0.151
Panel D: Has & Uses a Smartphone at Home (2012-2018; 2000, 2003 Set to 0)		
3G	0.115**	0.114**
	(0.050)	(0.050)
Num.Obs.	993872	993872
R2	0.683	0.686
Panel E: Browses the Internet Daily		
3G	0.032*	0.038**
	(0.017)	(0.017)
Num.Obs.	1516674	1516674
R2	0.258	0.265
Panel F: Total Weekly Internet Use in Hours (2012-2018)		
3G	5.093**	4.964**
	(2.007)	(2.005)
Num.Obs.	788037	788037
R2	0.131	0.149
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. Data is restricted to the ICT Sample. 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 Academic Time Use and Home Learning Inputs

	(1)	(2)
Panel A: 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 B: Ever Uses Mobile Phone for Homework (2015, 2018; 2000, 2003 set to 0)		
3G	0.103*	0.091
	(0.058)	(0.060)
Num.Obs.	730563	730563
R2	0.418	0.424
Panel C: Ever Uses Internet on Comp./Dig. Device for Schoolwork (2009-2018)		
3G	-0.057**	-0.057**
	(0.026)	(0.027)
Num.Obs.	1069870	1069870
R2	0.095	0.105
Panel D: Any Mobile Learning Apps (2015, 2018; 2000, 2003 set to 0)		
3G	0.051	0.051
	(0.052)	(0.054)
Num.Obs.	730563	730563
R2	0.376	0.382
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: log(Number of Books at Home) (2003-2018)		
3G	-0.077***	-0.078***
	(0.027)	(0.029)
Num.Obs.	2493799	2493799
R2	0.245	0.261
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 academic time use and home learning inputs. 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. Data used for Panels B, C, and D is restricted to the ICT Sample. 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 5: TWFE Estimates: Effect of 3G on Digital Leisure Activities

	(1)	(2)
Panel A: Uses Social Media Daily (2012-2018)		
3G	0.023	0.031
	(0.034)	(0.032)
Num.Obs.	788037	788037
R2	0.080	0.092
Panel B: Uses Social Media Daily (2012-2018; 2000, 2003 Set to 0)		
3G	0.173***	0.172***
	(0.043)	(0.043)
Num.Obs.	993872	993872
R2	0.477	0.483
Panel C: Chats Online Daily (2003-2018)		
3G	-0.012	-0.010
	(0.016)	(0.016)
Num.Obs.	1498897	1498897
R2	0.174	0.181
Panel D: Plays Computer Games Daily (2000-2012)		
3G	-0.013	-0.004
	(0.011)	(0.011)
Num.Obs.	991946	991946
R2	0.136	0.159
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 digital leisure activities. 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. Data is restricted to the ICT Sample. 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 6: TWFE Estimates: Effect of 3G on Social and Mental Well-Being

	(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
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 ease of making friends (captured by the Friendship Index), feelings of belonging in the school (captured through the Belonging Index), and academic self-efficacy. These measures are described in greater detail in the Appendix Section A. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: TWFE Estimates: Parental Interactions

	(1)	(2)
Panel A: Ever Discusses Politics/Soc. Issues with Parents (2000, 2009, 2018)		
3G	-0.066*	-0.068*
	(0.038)	(0.037)
Num.Obs.	195231	195231
R2	0.099	0.129
Panel B: Discusses School Performance with Parents Every Day (2000, 2009-2018)		
3G	0.037	0.028
	(0.028)	(0.030)
Num.Obs.	370498	370498
R2	0.120	0.142
Panel C: Eats Main Meal with Parents at Home Every Day (2000, 2009-2018)		
3G	-0.059	-0.035
	(0.036)	(0.034)
Num.Obs.	370353	370353
R2	0.072	0.095
Panel D: Just Talks with Parents Every Day (2009-2018)		
3G	-0.019	-0.007
	(0.030)	(0.030)
Num.Obs.	330147	330147
R2	0.104	0.110
Panel E: Ever Receives Homework Help from Parents (2000, 2009-2018)		
3G	-0.043	-0.032
	(0.043)	(0.040)
Num.Obs.	368180	368180
R2	0.079	0.101
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 parental interactions. These measures are described in greater detail in the Appendix Section A. 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.

A PISA Data 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 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.3 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'.

A.4 Number of Books at Home

In all relevant PISA rounds, students were asked, "How many books are there in your home?" With the exception of the 2000 round, the available response categories have been consistent: 0–10, 11–25, 26–100, 101–200, 201–500, and more than 500 books. We convert these responses to numeric values by assigning the midpoint of each range (treating “more than 500” as 500) and then taking the logarithm.

A.5 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, in addition to analyzing the composite Belonging index constructed by PISA, we also separately examine student responses to one of the components of this index—“I make friends easily at school” — separately. We analyze it separately as this statement has a clear, standalone interpretation and offers a direct measure of students’ own perceived ability to connect with others, an important precursor for the expansion of social networks. Eligible responses were “strongly agree," "agree," “disagree," and “strongly disagree." We transform these responses into integers 0 through 3 and standardize these values such that it has a mean of 0 and a standard deviation of 1. We refer to this standalone item as the Friendship Index throughout the paper.

A.6 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.[26]

A.7 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

[26] 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.

- 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.8 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 for browsing. 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™>).
- **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™>).

We construct a binary variable indicating whether a student reports browsing 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.9 Use of Mobile Phone for Homework

In 2015 and 2018, PISA asked students “How often do you use digital devices for the following activities outside of school? [...] Doing homework on a mobile device." We construct a binary variable indicating whether a student reports ever using a mobile device for homework outside of school.

A.10 Use of Internet for Schoolwork

Between 2009 to 2018, PISA asked students whether they used the internet for schoolwork. These questions are listed below, separately by year.

- **2009**: How often do you do the following at home? [...] Browse the Internet for schoolwork (e.g. preparing an essay or presentation)
- **2012**: How often do you use a computer for the following activities outside of school? [...] Browsing the Internet for schoolwork (e.g. for preparing an essay or presentation).
- **2015**: How often do you use digital devices for the following activities outside of school? [...] Browsing the Internet for schoolwork (e.g. for preparing an essay or presentation).
- **2018**: How often do you use digital devices for the following activities outside of school? [...] Browsing the Internet for schoolwork (e.g. for preparing an essay or presentation).

We construct a binary variable indicating whether a student reports ever browsing the internet for schoolwork.

A.11 Use of Mobile Learning Apps

In 2015 to 2018, PISA asked students whether they used the mobile learning apps. These questions are listed below, separately by year.

- **2015**: How often do you use digital devices for the following activities outside of school? [...] Downloading learning apps on a mobile device.
- **2018**: How often do you use digital devices for the following activities outside of school? [...] Using learning apps or learning websites on a mobile device

We construct a binary variable indicating whether a student reports ever using mobile learning apps.

A.12 Social Media Use

Between 2012 and 2018, PISA asked students various questions about social media use. These questions are listed below, separately by year.

- **2012**: How often do you use a computer for the following activities outside of school? [...] Participating in social networks (e.g. <facebook>, <MySpace>).
- **2015**: How often do you use digital devices for the following activities outside of school? [...] Participating in social networks (e.g. <Facebook>, <MySpace>).
- **2018**: How often do you use digital devices for the following activities outside of school? [...] Participating in social networks (e.g. <Facebook>, <MySpace>).

We measure these responses in the form of a binary indicator identifying whether the respondent uses social media daily.

A.13 Online Chat Use

Between 2003 and 2018, PISA asked students various questions about online chat use. These questions are listed below, separately by year.

- **2003** How often do you use: [...] a computer for electronic communication (e.g. e-mail or “chat rooms")?
- **2006**: How often do you use computers for the following reasons? [...] For communication (e.g. Email or “chat rooms”)
- **2009**: How often do you use a computer for following activities at home? [...] <Chat on line> (e.g. <MSN®>)
- **2012**: How often do you use a computer for the following activities outside of school? [...] <Chatting online> (e.g. <MSN®>).
- **2015**: How often do you use digital devices for the following activities outside of school? [...] <Chatting online> (e.g. <MSN®>).
- **2018**: How often do you use digital devices for the following activities outside of school? [...] <Chatting online> (e.g. <MSN®>).

We measure these responses in the form of a binary indicator identifying whether the respondent uses online chat tools daily.

A.14 Computer Games

Between 2000 and 2012, PISA asked students various questions about computer games. These questions are listed below, separately by year.

- **2000**: How often do you use each of the following kinds of computer software? [...] Games
- **2003**: How often do you use: [...] games on a computer?
- **2006**: How often do you use computers for the following reasons? [...] Play games
- **2009**: How often do you use a computer for following activities at home? [...] Play one-player games [...] Play collaborative online games
- **2012**: How often do you use a computer for the following activities outside of school? [...] Playing one-player games. [...] Playing collaborative online games.

We measure these responses in the form of a binary indicator identifying whether the respondent plays computer games daily.

A.15 Discussing Politics or Social Issues with Parents

In 2000, PISA asked students: "In general, how often do your parents: discuss political or social issues with you?" In 2009 and 2018, PISA asked parents: “How often do you or someone else in your home do the following things with your child? Discuss political or social issues." We construct a binary indicator equal to 1 if the respondent reported ever discussing political or social issues, and 0 otherwise.

A.16 Discussing School Performance with Parents

In 2000, PISA asked students: "In general, how often do your parents: discuss how well you are doing at school?" In 2009, 2012, 2015, and 2018, PISA asked parents: “How often do you or someone else in your home do the following things with your child? Discuss how well my child is doing at school." We construct a binary indicator equal to 1 if the respondent reported discussing school performance every day, and 0 otherwise.

A.17 Eating Main Meal with Parents

In 2000, PISA asked students: "In general, how often do your parents: eat <the main meal> with you around a table?" In 2009, 2012, 2015, and 2018, PISA asked parents: “How often do you or someone else in your home do the following things with your child? Eat <the main meal> with my child around a table." We construct a binary indicator equal to 1 if the respondent reported eating the main meal together every day, and 0 otherwise.

A.18 Just Talking with Parents

In 2009, 2012, 2015, and 2018, PISA asked parents: “How often do you or someone else in your home do the following things with your child? Spend time just talking to my child." We construct a binary indicator equal to 1 if the respondent reported spending time just talking ever day, and 0 otherwise.

A.19 Receiving Homework Help from Parents

In 2000, PISA asked students: "How often do the following people work with you on your <schoolwork>? Your mother," and separately asked: "How often do the following people work with you on your <schoolwork>? Your father." Between 2009 and 2018, PISA asked parents the questions below:

- **2009**: How often do you or someone else in your home do the following things with your child? Help your child with his/her homework.
- **2012**: How often do you or someone else in your home do the following things with your child? Help my child with his/her mathematics homework.
- **2015**: How often do you or someone else in your home do the following things with your child? Help my child with his/her science homework.
- **2018**: How often do you or someone else in your home do the following things with your child? Help my child with his/her reading and writing homework

We construct a binary indicator equal to 1 if the respondent reported ever receiving/providing homework help, and 0 otherwise.

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) estimators 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.

Figure 4 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.2. Across subjects and estimators, these estimates are consistently negative and statistically significant. Magnitudes are slightly larger than our main TWFE

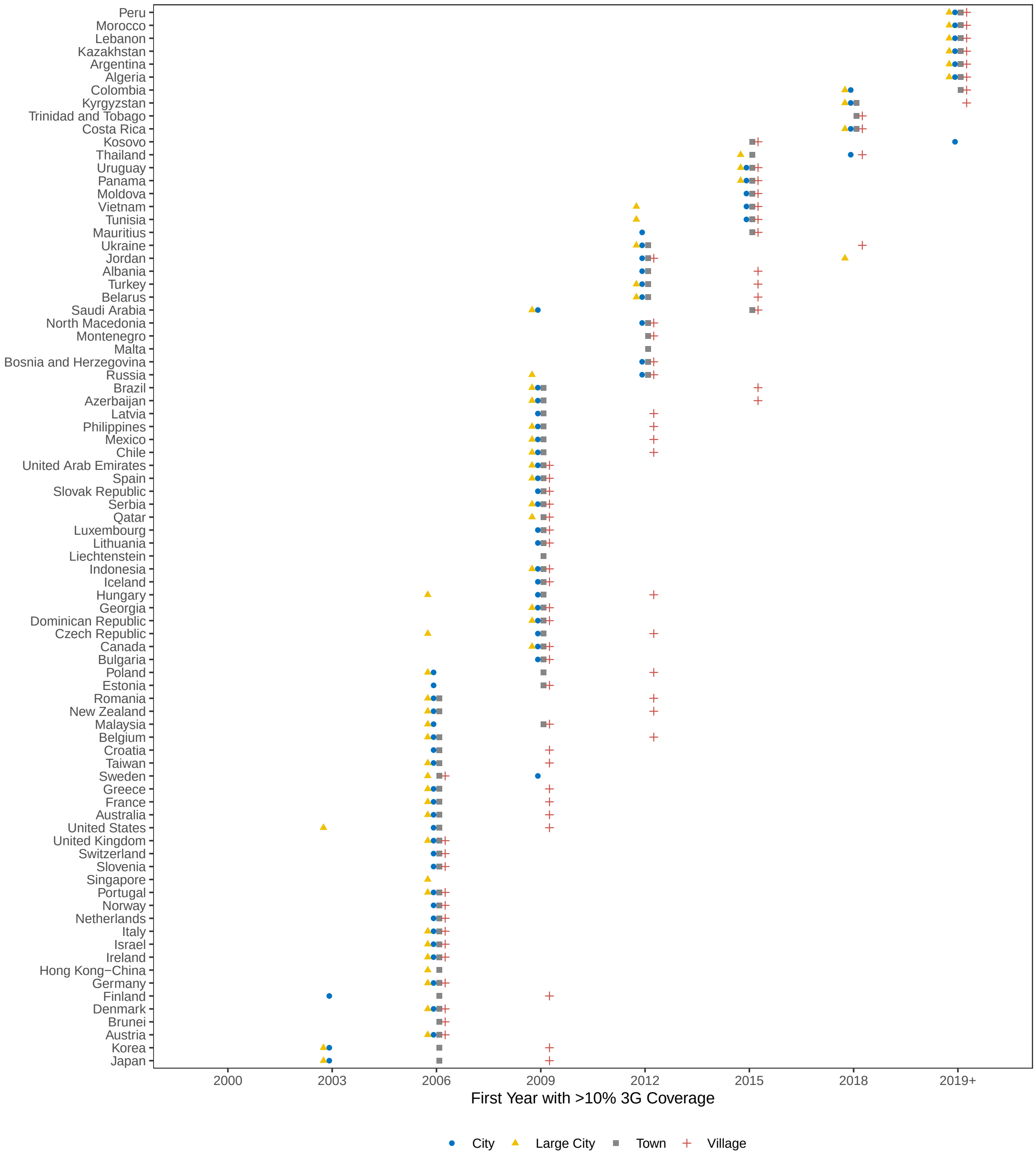

Figure B.1: Distribution of 3G Entry Dates within and Across Countries

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

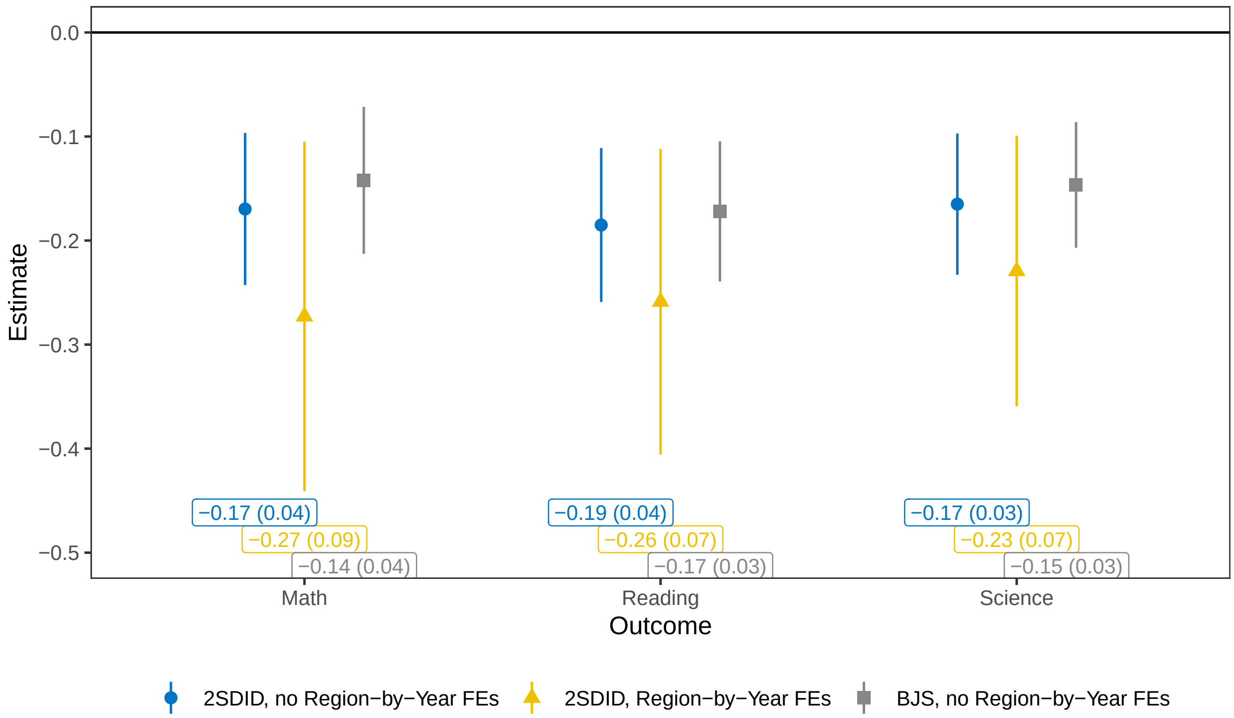

Figure B.2: Static Effect of 3G Entry on PISA Test Scores: Borusyak et al. (2024) and Gardner et al. (2023) Estimators

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. Error bars indicate 95% confidence intervals. 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.

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

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 limited over-time availability of most ICT variables, we focus on effects on smartphone ownership and use, setting 2000 and 2003 values to zero. Appendix Table C.1 documents positive and statistically significant effects on smartphone ownership using this approach. Effect sizes are larger than those presented in our main estimates, but standard errors are also much larger.

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 × 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 Smartphone Use

	(1)	(2)	(3)
	FS	RF	IV
Dep. Var	3G	Tech. Use	Tech. Use
Panel A: Has a Smartphone at Home (2012-2018; 2000, 2003 Set to 0)			
Lightning × Year	-0.004***	-0.002***	
	(0.001)	(0.001)	
2G × Year	0.012***	0.001	
	(0.003)	(0.001)	
$\hat{3G}$			0.302***
			(0.110)
F-Stat			12.84
Num.Obs.	993872	993872	993872
R2	0.950	0.770	0.768
Panel B: Has & Uses a Smartphone at Home (2012-2018; 2000, 2003 Set to 0)			
Lightning × Year	-0.004***	-0.002***	
	(0.001)	(0.001)	
2G × Year	0.012***	0.001	
	(0.003)	(0.001)	
$\hat{3G}$			0.407***
			(0.132)
F-Stat			12.84
Num.Obs.	993872	993872	993872
R2	0.950	0.676	0.672
Country-by-Urbanicity FEs	✓	✓	✓
Year FEs	✓	✓	✓
Baseline Controls	✓	✓	✓

Note: Table displays instrumental variables results estimating the effect of 3G coverage on smartphone 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. Data is restricted to the ICT Sample. 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)	
$\hat{3G}$				-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)	
$\hat{3G}$				-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)	
$\hat{3G}$				-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)
Dep. Var	TWFE Score	FS 3G	RF Score	IV Score
Panel A: Math				
3G	-0.034			
	(0.034)			
2G × Year		0.007	0.008	
		(0.005)	(0.006)	
$\hat{3G}$				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)	
$\hat{3G}$				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)	
$\hat{3G}$				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

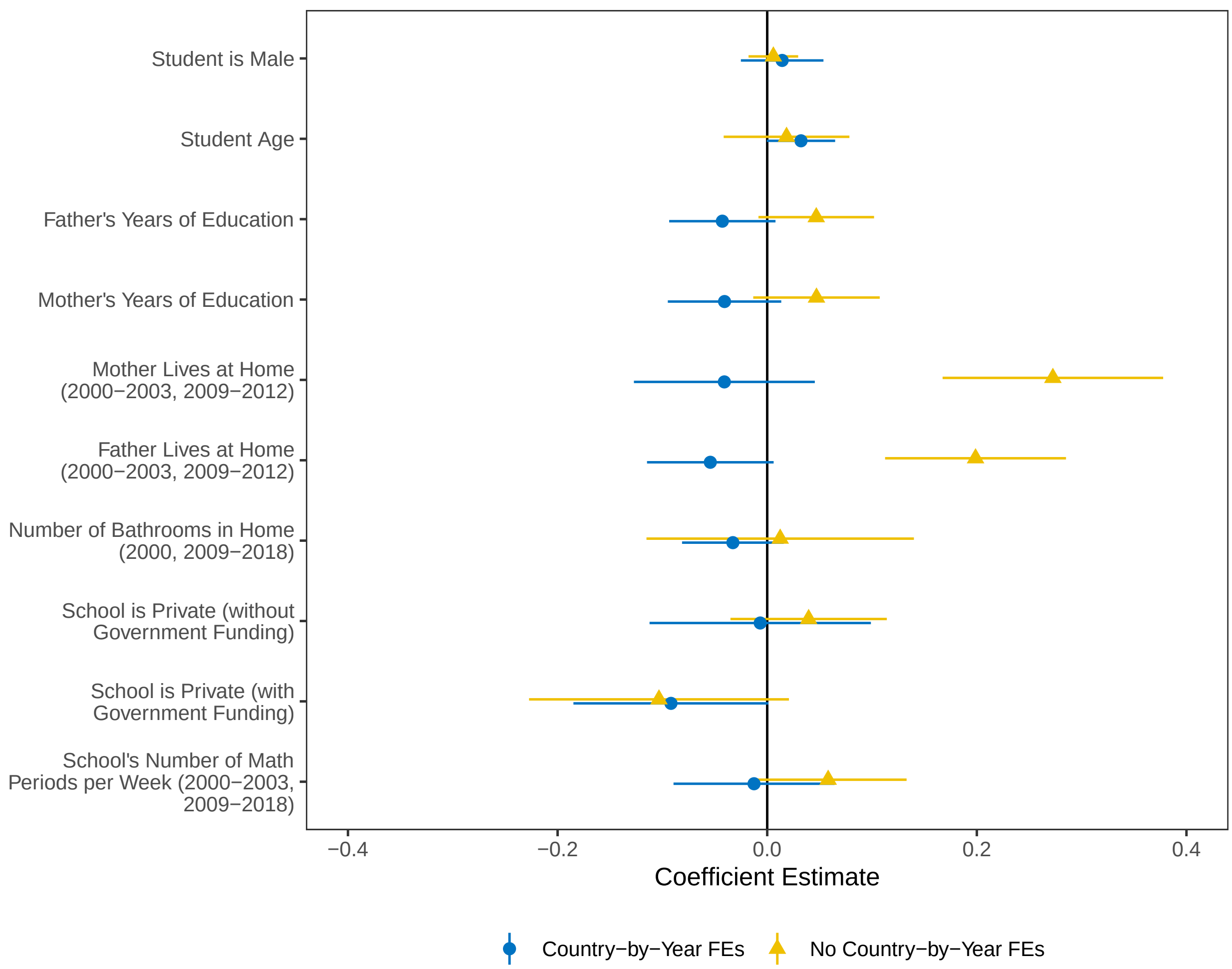

Figure D.1: Estimated Effect of 3G on Placebo Outcomes

Note: Figure displays the TWFE results estimating the effect of 3G coverage on various placebo outcomes, with and without Country-by-Year fixed effects. Dependent variables are indicated on the vertical axis. Robust standard errors are clustered at the country-by-urbanicity level. Error bars indicate 95% confidence intervals. 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.

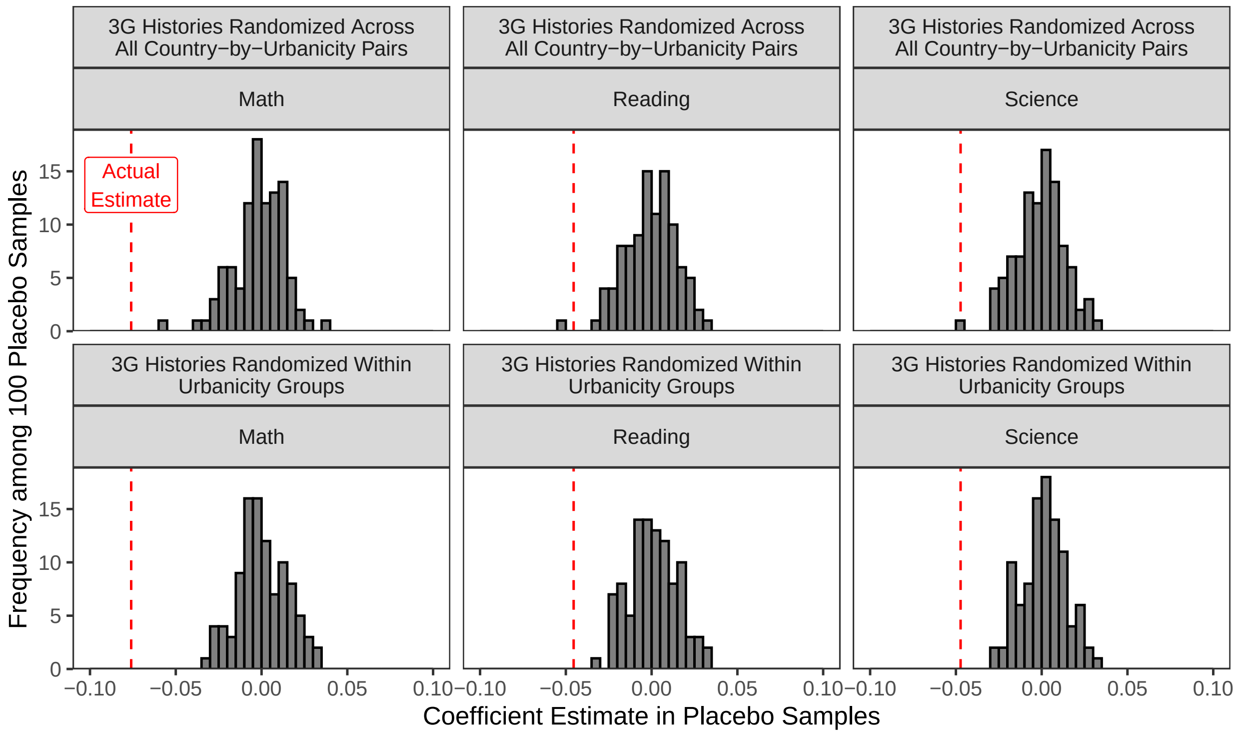

Figure D.2: Permutation Test: Distribution of Estimated Test Score Effects under Placebo 3G Histories vs. Actual Estimates

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		0.027**		0.037***
		(0.011)		(0.010)
Browses Internet Daily			0.148***	0.206***
			(0.013)	(0.012)
Num.Obs.	788037	788037	788037	788037
R2	0.332	0.322	0.327	0.341
Panel B: Reading				
Total Weekly Internet Use in Hours	-0.004***			-0.005***
	(0.000)			(0.000)
Has a Smartphone at Home		0.046***		0.042***
		(0.012)		(0.010)
Browses Internet Daily			0.225***	0.279***
			(0.014)	(0.013)
Num.Obs.	788037	788037	788037	788037
R2	0.282	0.275	0.285	0.298
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			0.185***	0.244***
			(0.013)	(0.012)
Num.Obs.	788037	788037	788037	788037
R2	0.291	0.281	0.289	0.303
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	0.962	0.898
	(0.760)	(0.794)
Num.Obs.	788037	788037
R2	0.096	0.108
Panel B: Out-of-School Weekly Internet Use in Hours (2012-2018)		
3G	4.131***	4.066***
	(1.570)	(1.543)
Num.Obs.	788037	788037
R2	0.114	0.134
Panel C: Total Weekly Internet Use in Hours (2012-2018)		
3G	5.093**	4.964**
	(2.007)	(2.005)
Num.Obs.	788037	788037
R2	0.131	0.149
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. Data is restricted to the ICT Sample. 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	-0.112***	-0.088***
	(0.032)	(0.029)
Num.Obs.	1516674	1516674
R2	0.323	0.348
Panel B: Reading		
3G	-0.063*	-0.042
	(0.033)	(0.030)
Num.Obs.	1516674	1516674
R2	0.291	0.315
Panel C: Science		
3G	-0.080***	-0.058**
	(0.030)	(0.028)
Num.Obs.	1516674	1516674
R2	0.274	0.299
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)		Has & Uses a Smartphone at Home (2012-2018)		Has & Uses a Smartphone 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)	(9)	(10)	(11)	(12)
Panel A: By Student Gender												
3G	0.047	0.046	0.092**	0.092**	0.090**	0.090**	0.113**	0.113**	0.027	0.026	5.134**	5.272**
	(0.033)	(0.035)	(0.042)	(0.043)	(0.042)	(0.044)	(0.047)	(0.047)	(0.019)	(0.019)	(2.095)	(2.122)
3G × Female	-0.025	-0.012	0.017	0.014	-0.048	-0.034	0.009	0.005	0.012	0.018	0.144	-0.530
	(0.037)	(0.038)	(0.020)	(0.020)	(0.038)	(0.038)	(0.022)	(0.023)	(0.020)	(0.019)	(2.162)	(2.153)
Num.Obs.	788037	788037	993872	993872	788037	788037	993872	993872	1516674	1516674	788037	788037
R2	0.125	0.130	0.775	0.777	0.142	0.148	0.684	0.685	0.261	0.263	0.135	0.144
Panel B: By Parental Education												
3G	0.047	0.045	0.114**	0.115**	0.085**	0.082*	0.137***	0.138***	0.067***	0.068***	5.060**	5.070**
	(0.034)	(0.035)	(0.051)	(0.052)	(0.042)	(0.043)	(0.052)	(0.053)	(0.022)	(0.022)	(2.313)	(2.362)
3G × Either Parent has Tert. Ed.	-0.035	-0.033	-0.073*	-0.082*	-0.061	-0.062	-0.096**	-0.102**	-0.068***	-0.062**	-1.596	-1.473
	(0.037)	(0.036)	(0.041)	(0.043)	(0.041)	(0.040)	(0.041)	(0.042)	(0.024)	(0.025)	(2.414)	(2.446)
Num.Obs.	788037	788037	993872	993872	788037	788037	993872	993872	1516674	1516674	788037	788037
R2	0.128	0.130	0.776	0.777	0.144	0.147	0.684	0.685	0.261	0.263	0.137	0.145
Panel C: By Country Income Level												
3G	0.049	0.046	0.090*	0.091*	0.087**	0.083*	0.104*	0.104*	0.049***	0.050***	5.878***	5.792**
	(0.041)	(0.045)	(0.048)	(0.049)	(0.042)	(0.046)	(0.053)	(0.054)	(0.018)	(0.019)	(2.235)	(2.284)
3G × Country is High Income in 2000	-0.101*	-0.102	-0.122**	-0.112**	-0.174**	-0.145*	-0.165***	-0.140**	-0.107***	-0.090***	-3.444	-1.530
	(0.058)	(0.066)	(0.052)	(0.054)	(0.072)	(0.082)	(0.061)	(0.062)	(0.028)	(0.028)	(6.273)	(6.339)
Num.Obs.	788037	788037	993872	993872	788037	788037	993872	993872	1516674	1516674	788037	788037
R2	0.125	0.122	0.776	0.775	0.142	0.140	0.683	0.683	0.260	0.258	0.135	0.139
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. Data is restricted to the ICT Sample. 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: All Subjects

	(1)	(2)	(3)	(4)
3G	-0.056**	-0.037	-0.063**	-0.052
	(0.023)	(0.027)	(0.026)	(0.032)
3G × Female		-0.042*		
		(0.025)		
3G × Either Parent has Tert. Ed.			0.028	
			(0.030)	
3G × Country is High Income in 2000				0.057
				(0.053)
Num.Obs.	7800402	7800402	7800402	7800402
R2	0.347	0.358	0.355	0.349
Country-by-Urbanicity FEs	✓	✓	✓	✓
Urbanicity-by-Year FEs	✓	✓	✓	✓
Country-by-Year FEs	✓	✓	✓	✓
Baseline Controls	✓	✓	✓	✓
Subject FEs	✓	✓	✓	✓
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 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.7: 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.8: 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

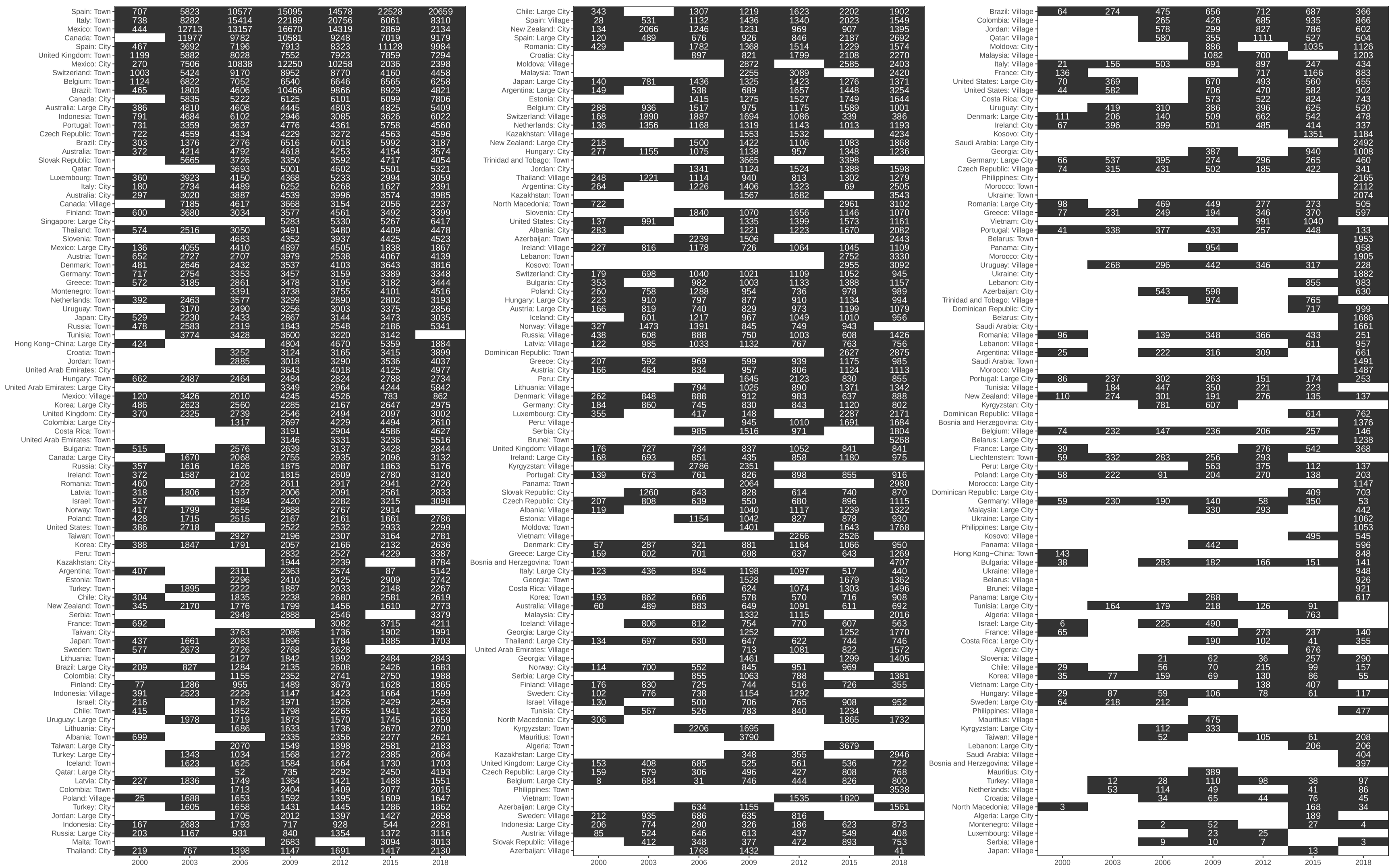

Figure E.1: Data Coverage by Country and Year

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.