Telecommunications Externality on Migration

Evidence from Chinese Villages

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Abstract

This paper uses a unique natural experiment in Chinese villages to investigate whether access to telecommunications—in particular, landline phones—increases the likelihood of outmigration. By using regional and time variations in the installation of landline phones, the difference-in-differences estimation shows that access to landline phones increases the ratio of out-migrant workers by 2 percentage points, or about 50

percent of the sample mean in China. The results remain robust to a battery of validity checks. Furthermore, landline phones affect outmigration through two channels: information access to job opportunities and timely contact with left-behind family members. The findings underscore the positive migration externality of expanding telecommunications access in rural areas, especially in places where migration potential is large.

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1 Introduction

The past decades have witnessed a surge in international and intranational migration. The United Nations (2010) documents that international migration increased from 145 million people to 191 million people during 1990–2005. In the past three decades in China, 500 million people have flocked to the city, and during 2000–10 alone, China’s urban population expanded by 210 million (Wong 2012). Outmigration is generally found to profoundly contribute to the welfare of both the recipient and sending destinations. In a literature survey, Clemens (2011) found that eliminating barriers to labor mobility can lead to a 67–147 percent increase in gross domestic product (GDP), while the corresponding numbers to eliminating all trade barriers and capital flow barriers are 0.3 to 4.1 percent and 0.1 to 1.7 percent, respectively. Meanwhile, a 10 percent increase in the remittance-to-GDP ratio is found to reduce the poverty ratio (measured by the share of residents living on less than \$1 a day) by 1.6 percent (Adam and Page 2005).² Despite the significant economic benefits, however, there are still substantial barriers to labor mobility. According to the Gallup World Poll (2010),³ 16 percent of the world’s adults, or 700 million adults, would like to migrate if given the opportunity. Why do so many potential migrants fail to act out their wishes? How can we increase the likelihood of outmigration?

The literature on the determinants of migration, starting from the classical Harris and Todaro (1970), emphasize the rural-urban earning differentials as the key reason. Moreover, scholars recognize that a fundamental problem in migration is uncertainty. Potential migrants do not have full information about job opportunities, wages, and the quality of life in destination cities, and some new evidence on migrants’ expectations in developing countries show how inaccurate these issues can be (McKenzie, Gibson and Stillman 2013). The literature has recognized and provided evidence that an important way to reduce information problems is through the channel of networks (Barr and Oduro

² See Hanson (2010) for more discussion on the impact of emigration on sending countries.

³ See Neil Esipova and Julie Ray, “700 Million Worldwide Desire to Migrate Permanently: U.S. Tops Desired Destination Countries,” <http://www.gallup.com/poll/124028/700-Million-Worldwide-Desire-Migrate-Permanently.aspx> (accessed March 16, 2013).

2002; Hanson and McIntosh 2010; Kilic and others 2009; McKenzie and Rapoport 2010; Munshi 2003; Uhlig 2006; Winters, de Janvry, and Sadoulet 2001; Yamauchi and Tanabe 2008). The literature also suggests that fast-changing information technology and its associated exposure on urban life would change the quality of information received by potential migrants and, therefore, their migration decisions. For instance, individuals exposed to foreign media and social media are more likely to migrate (Braga 2009; Komito 2011). Access to mobile phones increases the probability and intensity of rural-urban migration by offering more information about the labor market at the destination (Aker, Clemens, and Ksoll 2011; Muto and Yamano 2009). Moreover, the impact of mobile phone coverage expansion on migration depends on personal networks: the expansion of a mobile phone network strengthens the effect of the existing ethnic network on migration (Muto and Yamano 2009; 2011). But better information does not uniformly encourage migration—it depends on whether potential migrants over- or under-estimate the prospects of the potential destinations. If potential migrants over-estimate their employment and life prospects in the destination region, better access to information may decrease migration, as found by Farre and Fasani (2012).

In this paper, we investigate whether the availability of information technology, in particular, the investment in landline phones, can loosen the constraints on potential migrants and lead to an increase in outmigration. How does telecommunications access affect outmigration? We have two potential reasons. First, telecom technologies allow potential migrants to access external labor market information, which substantially reduces their searching costs and increases the accuracy of their costs-benefits analysis of migration decisions. Second, telecom access allows migrants convenient and timely contacts with their left-behind family members, which substantially reduces the psychological costs of migration. This is especially important in China because of the prevailing policies regarding access to education and health care, which discriminate against migrants and result in adults largely leaving their families and migrating alone (Wong 2012).

The empirical analysis draws on the data of the *Rural Permanent Sites Survey* conducted by the Ministry of Agriculture of China in 1993 and 1995–2000 and uses regional and time variations in the installation of landline phones to identify the causal effect of landline phones on outmigration. Out of 61 villages in our sample, 37 had landline phones in 1993 (i.e., our initial year), 21 installed landline phones at a different time during the sample period, and 3 remained without access to landline phones by 2000. Meanwhile, other telecom technologies, such as mobile phones and the Internet, only started to penetrate in the late 1990s and mostly in rich and coastal cities. Hence, our research setting allows us to separate the effect of landline phones from other competing telecom technologies. Furthermore, our identification is aided by directly controlling for the exposure to other information sources, such as newspapers and televisions. An important advantage of relying on landline phones to identify the effects of telecom technology is that we face a less serious challenge of endogeneity. Individuals can purchase mobile phones, and the access to mobile phones is closely related to personal ability, wealth, and demand for modern technology, which may be strongly related to the migration decision. In contrast, the installation of landline phones at the village level, as we document later, was largely related to several easily observable variables, and thus, its endogeneity for migration can be more easily dealt with. Perhaps because of this reason, our estimates of the telecom effects on migration are quite stable.

Based on the difference-in-difference (DID) approach, we find that the installation of landline phones leads to an increase in the ratio of out-province migrant workers in total rural labor force by 2 percentage points, or 51 percent of the sample mean. The results are robust to a battery of validity checks, such as using DID coupled with matching, controlling for time-varying province heterogeneity, controlling for pretreatment effect, using a flexible estimation method to account for differences in the time trend in outmigration of treatment and control groups, and using an alternative estimation method (i.e., a matched control group). Three placebo tests also confirm our identification assumptions. First, if the telecom effect on migration merely reflects changes in village characteristics, telecom access should affect in-migration (i.e., immigration into the village), but we do not find this to be the case. Second, if the

telecom effect merely reflects the time trend in relatively rich villages, then villages always with telecom access should have higher migration trends, but we find a similar difference in the migration levels for villages always with telecom access and those never with telecom access. Finally, if the telecom effect on outmigration reflects effects other than the information or psychological costs effect (as we hypothesize), telecom access will likely affect out-village, within-county migration, but we do not find that telecom access affects such short-distance migration.

We further test the two proposed mechanisms through which landline phones may increase outmigration, that is, information access and timely contact with left-behind family members. We find that the positive effect of landline phones on outmigration is greater for villages with a larger pool of previous outmigrants (a proxy for the information access through the network effect) and for villages with more young children (a proxy for left-behind family members).

Our paper is complementary to the existing literature on the determinants of migration in several ways. First, by using unique data on landline phone installation and by taking advantage of the predictive nature of landline phone installation at the village level, we have a relatively transparent and plausible strategy for identifying the effects of telecom on migration. The robustness of the results under a variety of specification checks testifies the plausibility of our identification strategy. Second, our evidence comes from the country that has experienced the largest migration in the world during which migration was in full swing, and it is useful to know whether modern telecom would have quantitatively important impact on migration. We find it is so. Third, there is little evidence of how family structure and psychological costs of migration affect migration, and in this paper, we find that modern telecom reduces the psychological costs of migration by allowing migrants to stay in touch with their children left behind in the villages.

The paper is organized as follows. In Section 2, we describe the rural-to-urban migration and the development of landline phones in China. In Section 3, we lay out the

theoretical model. Section 4 presents the empirical strategy; Section 5, the data; and Section 6, the empirical results. Section 7 concludes.

2 Background

Rural-to-Urban Migration in China

Because of food shortage and the great famine after the collapse of the Great Leap Forward in the early 1960s, the Chinese government started to restrict interregional migration, especially rural-to-urban migration, to ensure that sufficient resources stayed in agriculture production and to contain pressure for job creation in the cities. Specifically, the government adopted a household registration system (*hukou* in Chinese), which delineates where a person can live and what social welfare programs he or she is entitled to (e.g., Wu 1994; Zhao 2000). Without an urban residence permit (*urban hukou*), a farmer could not live and work in the city. It became nearly impossible for farmers to obtain urban *hukou* after the early 1960s (Naughton 2007). From 1949 to 1985, the average rural-to-urban migration rate for China was only 0.24, compared with a world average of 1.84 from 1950 to 1990 (Zhao 2000).

Since 1978, China has embarked on a great economic and social transformation, which has subsequently led to substantial changes in the rural-urban divide. In rural China, the household responsibility system emerged and eventually replaced the previous commune system, which greatly improved agricultural efficiency and generated surplus labor (Lin 1992; Zhao 2004). In urban areas, the development of a market-oriented economy, the establishment of special economic zones, the expansion of the nonstate sector, and the loosening of the urban employment policy created strong demand for migrant labor (Cai 2001; Meng and Zhang 2001). In addition, decades of rural-urban segregation and uneven economic growth led to a large income gap between urban and rural areas, which provided a stimulus for people to migrate to coastal and eastern China (Bao and others 2011). All these developments have contributed to China's surge in internal rural-to-urban migration.

According to the National Bureau of Statistics of China, rural out-migrant workers are defined as individuals who have rural household registration status but left their homeland and have worked outside the towns and counties for at least 6 months. As shown in Figure 1, the number of rural out-migrant workers rose from around 20 million in 1990 to 62 million in 1993, 132 million in 2000, and nearly 160 million in 2011.⁴ Most of these migrant workers, constrained by their lower education levels, work in the manufacturing and construction sectors in cities. However, rural-to-urban migration has significantly benefited both recipient and sending areas. For example, the Pearl Delta and Yangtze River Delta regions have emerged as one of the most important global manufacturing bases since the 1990s, partly because of the constant supply of cheap rural labor (Huang and Zhan 2005). And the large amount of remittances has greatly contributed to the economic development of inland rural areas through both consumption and investment and has helped reduce rural poverty since the late 1990s (de Brauw and Rozelle 2008).

Unlike many other international or internal migrations, rural-to-urban migration in China has its own features. Because of the presence of the household registration system, rural migrants find it difficult to permanently settle down in recipient cities for a long time.⁵ Also, they are largely denied access to many of the social welfare programs, such as education and medicine, to which their urban counterparts are entitled. Indeed, typically, migrant workers on average return home two to three times annually and spend less than 9 months in recipient cities (Zhao 1999). Another important feature of rural-to-urban migration in China is the emergence of the village-based migrant network. Because of decades of separation, rural households have limited ties with urban communities and little access to institutional supports at the destinations, making them rely on their origin-based networks to find jobs (e.g., Solinger 1999; Zhao 2003). This is also common in

⁴ Some other estimates are greater. For instance, Wong (2012) suggests that China's urban population expanded by 210 million.

⁵ This issue has changed substantially in the past few years. It has become easier for migrants to settle down in small cities, though the access to vital social services remains disadvantaged for migrants relative to residents with local urban *hukou*.

many other developing countries (Barr and Oduro 2002; Munshi 2003; Uhlig 2006; Winters, de Janvry, and Sadoulet 2001; Yamauchi and Tanabe 2008). Meng (2000) shows that 70 percent of rural-to-urban migrants in China found jobs through village-based friends or relatives. Before the arrival of modern telecommunications technologies, such as landline and mobile phones, potential migrants had to wait for temporary returns of previous migrants (such as during the Spring Festival) to obtain labor market information in cities, which generated substantial delays and high search costs.

Development of Landline Phones in China

When the People's Republic of China was established in 1949, the country had only 300,000 telephones, or 0.05 sets per 100 people. In addition, telecom facilities were largely outdated and concentrated in just a few large cities, such as Chongqing, Shanghai, and Wuhan (Wauschkuhn 2001). From 1949 to China's economic reform initiated in 1978, the government gave priority to developing heavy industry and largely neglected the investment in telecommunications. As a result, the number of telephones grew very slowly and stagnated relative to the growth in population: the tele-density in 1978 was only 0.38 sets per 100 people (see Figure 1).

In the late 1980s, the rapid growth in the economy originated from the policy of economic reform, which started to call for better communications services, the shortage of which clearly became a key bottleneck for further development. Thus, in the seventh five-year plan in 1985, the State Council, or the cabinet, stated that telecom development would become a national priority and the focus was to develop telecom facilities in major cities and coastal areas. Meanwhile, the government allowed telecom companies to borrow from state-owned banks and foreign sources and to enjoy preferential tax rates. As a result, the number of landline phones started to rise, growing at an average annual rate of 17 percent between 1986 and 1990 (Clegg, Kamall, and Leung 1996). However, the incentive schemes and federal support systems were only given to some specific areas (i.e., 14 open coastal cities and 5 special economic zones), which amplified the telecom advantage of the key cities against the rural areas. By 1991, these specific areas accounted for nearly 25 percent of telecom networks in China (Wu 2008), and almost all

subscribers were living in urban areas while people in remote rural parts of China remained unconnected.

For a long time before the late 1990s, the Ministry of Posts and Telecommunications was the regulator and main operator of telecom services, and telecom monopoly seriously constrained the development of the industry. In the late 1990s, partly following the worldwide trend (Li and Xu 2004), China started telecom deregulation and liberalization by granting more administrative autonomy to the Post and Telecommunications Bureaus at the regional and local levels, by introducing more competitors to market, and by gradually opening the telecom markets to foreign investors. As a result, the service quality has dramatically improved and charges have fallen substantially, leading to a record growth in landline phone subscribers.⁶ As shown in Figure 1, the number of landline phones per 100 people increased from less than 1 in 1990 to more than 12 in 2000, and the number continued to rise to 28.1 by 2006. Meanwhile, with the introduction of new technologies, other telecom modes, such as cellular phones and the Internet, began to penetrate China. For example, the number of cellular phone users surpassed the number of landline phone users in 2003 and peaked at 75 sets per 100 people by 2011. And the number of Internet users has increased nearly 7 times between 2002 and 2011. However, between 1993 and 2000 (our sample period), landline phones were the primary telecom tool, especially in rural areas. The exclusive reliance on landline phones in the rural areas thus allows us to focus on a single telecom technology. Moreover, because the introduction of a landline phone network was largely determined by village characteristics, as we will demonstrate later, we should be able to identify the effects more convincingly than to identify the telecom effects of mobile phones, which involves individual- or household-level selectivity to a larger extent.

⁶ Using cross-country data, Li and Xu (2004) find that both telecom privatization and competition facilitated telecom development, especially when both are done at the same time.

3 Model

In this section, we present a simple model to illustrate how landline phones may affect the likelihood of outmigration. While we emphasize two channels (the provision of external labor market information and timely communication with left-behind family members), we acknowledge that there could be other possible explanations. Here our purpose is to offer a simple framework to guide our empirical analysis. The model we use is based on a discrete choice framework proposed by Borjas (1987).

Consider the decision faced by individual i on whether to work in an outside city (migrate) or to stay in the village (stay). If she chooses to stay, her utility is assumed to be

$$U_s = w_s + \varepsilon_s, \quad (1)$$

where w_s is the wage rate in the village and ε_s is the idiosyncratic utility level for staying. If she chooses to migrate, her utility is assumed to be

$$U_m = f(T, N)w_m - C_m - g(T, H) + \varepsilon_m, \quad (2)$$

where w_m is the wage rate in the city; C_m captures the costs of migration, such as financial costs and so on. ε_m is the idiosyncratic term for migrating.

The effect of landline phones is captured by the two functions, $f(T, N)$ and $g(T, H)$. $f(T, N)$ measures the labor market information available to the individual, which is affected by the access to landline phones (T) and the information network (measured by the existing number of migrant workers in the same village, N). It is assumed that $f_T > 0$, $f_N > 0$, and $f_{TN} > 0$, that is, the access to landline phones not only increases the availability of labor market information but also magnifies the effect of networking. The assumption on the interaction term is plausible since the same network effect is realized much faster and much more cheaply when village residents have access to a phone network than without the phone network (Barr and Oduro 2002; Hanson and McIntosh 2010, McKenzie and Rapoport 2010; Kilic and others 2009; Munshi 2003;

Uhlig 2006; Winters, de Janvry, and Sadoulet 2001; Yamauchi and Tanabe 2008). The second function, $g(T, H)$, measures the psychological costs faced by a migrant worker, which include those associated with leaving family members behind. The number is assumed to decrease with access to a landline phone (T) and increase due to the number of left-behind family members, such as children (H). In addition, the availability of landline phones should reduce the negative impact of the number of left-behind family members. Thus, $g_T < 0$, $g_H > 0$, and $g_{TH} < 0$. The assumption of a negative interaction term is based on the intuition that talking and advising over the phone to the left-behind family members in the village allow the migrant to ease the pains of not seeing the family and help the migrant to react more quickly in cases of emergency.

Hence, the probability of migrating is

$$\begin{aligned}
P &= \Pr[U_m > U_s] \\
&= \Pr[f(T, N)w_m - C_m - g(T, H) + \varepsilon_m > w_s + \varepsilon_s] \\
&= \Pr[\varepsilon_s - \varepsilon_m < f(T, N)w_m - C_m - g(T, H) - w_s] \\
&= \int I(\tilde{\varepsilon}_{sm} < f(T, N)w_m - C_m - g(T, H) - w_s) j(\tilde{\varepsilon}_{sm}) d\tilde{\varepsilon}_{sm} \\
&= J(f(T, N)w_m - C_m - g(T, H) - w_s),
\end{aligned} \tag{3}$$

where $\tilde{\varepsilon}_{sm} \equiv \varepsilon_s - \varepsilon_m$; $I(\cdot)$ is the indicator function; and $J(\cdot)$ and $j(\cdot)$ are the cumulative and probability distribution function of $\tilde{\varepsilon}_{sm}$, respectively.

With the properties of $f(\cdot)$ and $g(\cdot)$, we obtain three propositions.

Proposition 1: The effect of landline phones on the probability of outmigration is positive,

that is, $\frac{\partial P}{\partial T} > 0$.

Proposition 2: The positive effect of landline phones on outmigration is magnified by the

existence of networks, that is, $\frac{\partial^2 P}{\partial T \partial N} > 0$.

Proposition 3: The positive effect of landline phones on outmigration is magnified by the number of left-behind family members, that is, $\frac{\partial^2 P}{\partial T \partial H} > 0$.

4 Empirical Strategy

To identify the causal effect of access to landline phones on outmigration, we use the DID method. Specifically, we compare the ratio of outmigrant workers (in total rural labor force) in the treatment group (i.e., the villages that installed landlines during the sample period) with that in the control group (i.e., the villages that had landlines at the beginning of the sample period) before and after the installation of the landlines. While we also have sample villages that never had landline phones, the sample is too small—only three villages—so we do not use them in our control group. Our baseline estimation equation is as follows:

$$y_{vt} = \alpha_v + \gamma_t + \beta Treatment_v \times Post_{vt} + \varepsilon_{vt}, \quad (4)$$

where y_{vt} is the ratio of out-province migrant workers in village v at year t . $Treatment_v$ is the indicator of the treatment group, which is equal to 1 if village v installed landline phones during the sample period and 0 if village v had landline phones throughout the whole sample period or never had landline phones during the whole period. $Post_{vt}$ is the indicator of post-treatment period, which is equal to 1 if $t \geq t_{vt}^0$ where t_{vt}^0 is the year the treatment village v installed landline phones and 0 otherwise. α_v is the village fixed effect, capturing all time-invariant village heterogeneity, such as the distance away from coastal regions, culture, village inequality, and so on. γ_t is the year fixed effect, capturing all yearly shocks common to all villages, such as the business cycle, macro level regulations, and the trend in income growth. ε_{vt} is the error term. To deal with the potential heteroskedasticity and serial correlation, we cluster the standard error at the village level to avoid overstating estimation precision (Bertrand and Mullainathan 2004).

The identifying assumption associated with the DID estimation equation (4) is that conditional on the controls, our regressor of interest (i.e., the interaction between the treatment status indicator and post-treatment period indicator) is uncorrelated with the error term. That is,

$$E[\varepsilon_{vt} | Treatment_v \times Post_{vt}, \alpha_v, \gamma_t] = E[\varepsilon_{vt} | \alpha_v, \gamma_t]. \quad (5)$$

In the remaining part of this section, we discuss potential violations of our identifying assumption and our remedies, as well as several robustness checks.

Placement and Timing of Landline Phone Installation

A potential challenge to the DID estimation specification is that the place and timing of the installation of landline phones are not random. For example, more remote and poorer villages could install landline phones later than coastal and richer villages. One may then be concerned that such preexisting differences across treatment and control groups may explain the post-treatment divergence in the ratio of out-migrant workers, causing a spurious correlation between our regressor of interest and the outcome variable. Thus, one must understand what determines which village installed landline phones earlier to isolate the effect of landline phones on outmigration.

To this end, we first conducted an intensive online research on how China Telecom Corporation, the monopoly of telecommunications in the 1990s and early 2000s, decided which village was connected to the landline phone network first in the 1990s.⁷ Unfortunately, there is not much discussion online about the determinants of landline phone connection. Among the sporadic pieces of information we found, most websites cite total income as the main reason. We then interviewed one China Telecom Corporation employee to get first-hand information about landline phone installation. We were told that when choosing which village was connected to the landline phone network

⁷ The search engine we used is *Baidu*, the Chinese version of Google and the best in searching Chinese websites.

first, the company mainly considered the degree of facility use, which is closely related to village wealth.

We next conducted a regression analysis on the determinants of landline phone connection based on the aforementioned anecdotal evidence. Specifically, we first considered the village's level of economic development, that is, total income and total population. We then considered special government policies, that is, whether the village was classified as a poverty village and whether it was classified as a remote village—both categories of villages are supposed to enjoy compensatory treatment over many policies. We also considered geographic features (that may affect the costs of installing landline phones), that is, the percentage of arable land and whether the village was in the mountains. Lastly, we investigated whether the installation was triggered by the needs of out-migrant workers. All these determinant variables were measured in the pretreatment stage in 1991.

Regressions results are reported in Table 1. Columns (1) to (4) deal with the placement of landline phones, that is, we examine whether a village installed landline phones in 1993. Columns (5) to (8) concern the timing of the installation, and the dependent variable is the number of years from the initial year (i.e., 1993) of the sample until the year that the village installed landline phones. We also experiment with the Cox proportional hazards model and the ordered probit model; the qualitative results are similar.⁸

The findings are consistent with our anecdotal evidence about the key importance of local income level. Indeed, richer villages are more likely to have landline phones in 1993 (columns [1] to [4]) and more likely to install landline phones earlier (columns [5] to [8]). Total income itself can explain around 27 percent of the total variations in the placement and timing of landline phone installation. None of the remaining determinants are statistically significant. In particular, the ratio of out-migrant workers in 1991 is

⁸ The results are available upon request.

consistently insignificant, suggesting that the installation of landline phones is not reversely caused by our outcome variable.

In summary, our evidence in this subsection, both qualitatively and quantitatively, shows that the total income of a village is an important factor in determining whether the village installed landline phones and its timing. Meanwhile, conditional on this key determinant, other factors are found to be not statistically significant, especially the pretreatment ratio of outmigration villagers. Although 73 percent of the differences between treatment and control groups are unexplained, we cannot locate other factors that significantly drive both the selection of landline phone installation and the post-installation differential in migration between these two groups.

Augmented Estimation Specification and Robustness Checks

We have just established that the treatment and control groups *ex ante* differ significantly in total income. To alleviate the concern that preexisting differences in village wealth between the treatment and control groups may generate the differential patterns of outmigration over time, we control for a flexible time trend in outmigration generated by the preexisting village wealth. Specifically, we interact the village total income in 1991 with a fourth-order polynomial function of time. Moreover, we further include the interactions between the fourth-order polynomial function of time with the percentage of arable land in 1991 and whether the village was in the mountains—the two variables having a nontrivial t-statistics (i.e., greater than 1) though not being statistically significant in the determinants equation. Finally, we control for province-time fixed effect (and, hence, the comparison is within province, across villages) and some other time-varying village characteristics, such as total population (in logarithm form), sex ratio, budget surplus, electricity, and exposure of other medias (such as newspapers and televisions). Hence, our augmented DID estimation specification becomes

$$y_{vt} = \alpha_v + \gamma_t + \beta Treatment_v \times Post_{vt} + \mathbf{X}_{vt}' \eta + \varepsilon_{vt}, \quad (6)$$

where $\mathbf{X}_{vt}$ is a vector of additional controls discussed earlier. The new identifying assumption is

$$E[\varepsilon_{vt} | Treatment_v \times Post_{vt}, \mathbf{X}_{vt}, \alpha_v, \gamma_t] = E[\varepsilon_{vt} | \mathbf{X}_{vt}, \alpha_v, \gamma_t]. \quad (7)$$

5 Data

The data come from the Rural Permanent Sites Survey conducted by the Ministry of Agriculture of China in 1986–1991, 1993, and 1995–2000.⁹ Because surveys in 1986–1991 do not contain information on landline phones, we restrict our analysis to the 1993 and 1995–2000 surveys. The survey sites were randomly sampled from six provinces (i.e., Gansu, Guangdong, Hubei, Liaoning, Shandong, and Yunnan provinces). As shown in Figure 2, the sample provinces (in red) are spread out across China, ranging from coastal to inland areas and covering northern, southern, western and eastern China; they also feature diverse levels of economic development, climate, natural endowment, and infrastructure. To capture province heterogeneity, we control in the regressions for province-year dummies so that our identification relies on variations across villages within a province-year cell.

We have a total of 67 villages in 1993. Six villages were deleted because they changed location codes over time, for which we cannot trace. Among the remaining 61 villages, 37 had landline phones in 1993, and they are used as the control group; 21 installed landline phones during the sample period, and they are used as the treatment group); three had no landline phones installed even at the end of our sample period, and they are used in a placebo test.

Table 2 reports the summary statistics of our key variables. During the sample period (1993, 1995–2000), the overall ratio of out-province migrant workers is 3.7 percent, and the overall ratio of out-village, within-county migrant workers is 5.2 percent. Interestingly, the ratio of immigrant workers (i.e., those who came to the village to work from other places) is quite large, around 12.9 percent. Meanwhile, our sample villages are quite poor with an average annual income per capita of 2119 yuan or US\$340, small (i.e.,

⁹ Surveys were not conducted in 1992 and 1994 because of financial reasons.

489 households living in a 7 square kilometer area), sexually balanced (i.e., sex ratio is 1.03), and mostly located out of the mountains (i.e., 70 percent). But these villages have substantial annual government budget (i.e., 1.7 million yuan or US\$210,000) and good infrastructures (i.e., 80.9 percent of households have televisions and 97.9 percent of households have electricity).

Note that the distances among 58 sample villages are quite large, averaging 263 kilometers between any two villages and 241 kilometers between a treatment village and a control village. Such long distances make the spillover effect of landline phones from the treatment group to the control group quite unlikely.

We present summary statistics of landline phone accessibility in Table 3. The number of villages with landline phones in our data increased from 37 in 1993 to 57 in 2000 (panel A). Except for Gansu province, all villages in the other provinces in our data had access to landline phones by the end of 2000 (panel B). Finally, the timing of installing landline phones varies across our sample villages and time (panel C). For example, most landline phones in Hubei province were installed in the early years of our sample period (i.e., 1993, 1995–1996), while installation occurred much late in Gansu province (i.e., 1998–2000). Such variations afford us a good opportunity to identify the causal effect of landline phones by using the DID estimation method.

6 Empirical findings

Main Results

Figure 3 shows the difference in the ratio of out-migrant workers between treatment and control groups over time. Clearly, the treatment and control groups have similar ratios of out-migrant workers 2 years and 1 year before the installation of landline phones in treatment villages. Right after the installation of landline phones, treatment villages experience an increase in the ratio of out-migrant workers, and the trend continues for at least 2 more years.

Regression results using the DID specification (equation [6]) are reported in Table 4. We start with including only year and village fixed effects in column (1). Here, landline phones have a positive and statistically significant coefficient, which is consistent with the findings in Figure 3. This result suggests that access to landline phones increases the ratio of outmigration by 2.1 percentage points.

In the remaining columns, we progressively add fourth-order time polynomial interacted with village characteristics in 1991, time-varying village characteristics, and province-time dummies. Village characteristics include the percent of land that is arable and whether the village is mountainous. Time-varying village characteristics include total population (in logarithm form), sex ratio, budget surplus, electricity, number of newspapers, and number of televisions. In our preferred and most conservative specification (column [5]), the comparison is among villages with and without experiencing the treatment in the same province-year (due to the control for province-time dummies) after holding constant other time-varying controls (i.e., log total population, sex ratio, budget surplus, electricity, number of newspapers, and number of televisions), and village-characteristics-specific time trends (i.e., in 1991, time polynomials interacted with log village income, percent of arable land, and whether mountainous). Evidently, our estimated coefficients of landline phones not only remain statistically significant but also have similar magnitude—now the landline phone effect on migration ratio is 1.9 percentage points, or about 50 percent of the mean (i.e., 3.7 percent).

Robustness Checks

In this subsection, we conduct several robustness checks on our identifying assumption (equation [7]). Regression results are reported in Table 5.

One potential challenge to our DID estimation is that even with a long list of controls (i.e., province-year dummies, village dummies, various time-varying village characteristics, and so on), there may remain some unobserved time-varying village characteristics that drive both the installation of landline phones and changes in the ratio

of out-migrant workers. Such local characteristics would include local government officials' attitude toward outmigration or the village's evolving policies on land reallocation when local residents migrate. Although variables such as these are likely to change gradually over time rather than suddenly or all at once, their effects are likely to appear as if changes in outmigration would anticipate the installation of landline phones (Jensen and Oster 2009). This is similar to the preprogram test in labor economics (Heckman and Hotz 1989), and the significance of the "landline phone anticipator" likely indicates that the landline phone effect merely reflects the influence of related confounding factors. To check the possibility of such compounding effects, we include an indicator for installing landline phones next year in the regression. As shown in column (1) of Table 5, the effect of installing landline phones is statistically insignificant, and our main coefficient remains robust to this control, supporting the validity of our DID estimation.

Second, we use a more flexible specification for installing landline phones in the future and past, that is, replacing the post-treatment period indicator ($Post_{vt}$) with a series of time dummies that indicate various time distance to the landline installation year (the default time category is at least three years before the installation).¹⁰ Such an exercise can shed light on whether the treatment and control groups are comparable until the time of treatment and become different after that time. As shown in column (2) of Table 5, we find similar patterns in outmigration between treatment and control groups before the installation of landline phones, but they diverge right after the installation, with much larger magnitudes. Although individually the post-treatment variables are not statistically significant, the F statistic for the joint significance of the three post-treatment variables is 3.13, significant at the 5 percent level.

¹⁰ In effect, the omitted variable is the time-invariant "treatment" status, which is absorbed in the village fixed effect.

Three Placebo Tests

We now conduct three placebo tests to offer further support to our key results. First, we look at migration into the villages—that is, instead of outmigration, we examine in-migration. If the installation of landline phones and changes in outmigration reflect the changes of underlying village environments, we expect landline phone installation to have “effects” on in-migration. Here the landline phone effect on in-migration is of course just spurious correlation—our theory implies effects of having access to landline phones at the migration source, not the migration destination—and its significance would cause second thoughts on our specification. However, as shown in column (3) of Table 5, the estimated coefficient of landline phones on in-migration is nowhere near being statistically insignificant.

Second, in the survey, we have three villages that had not installed landline phones by the end of the sample period (i.e., 2000). If the outmigration is truly triggered by landline phones, then we should see a similar difference in the ratio of out-migrant workers between villages with landline phones and those without landline phones throughout the entire sample period, because their landline phone status did not change over time. In other words, for two comparison groups without treatment, their differences in the outcome should be stable over time. Indeed, Figure 4 shows that the differences between the two comparison groups, despite some fluctuations, remain similar over our sample period.

Third, in our theory, the effect of landline phones on outmigration originates from the sharing of job information and timely contact with left-behind family members. Because villages are quite close within the same counties in which these two roles of landline phones may not play significant parts, we should expect that there is no effect of landline phones on the migration to different villages or towns within the same county. To test this implication, we construct a new outcome variable, *Out-village*, which is the percentage of out-village, within-county migrant workers (in total rural labor force), and we reestimate equation (6) using this outcome variable (see column [4] of Table 5).

Evidently, there is no statistically significant effect of landline phones on out-village, within-county migration.

Using DID Matching

To ensure that our results are not driven by functional form assumptions, we use an alternative estimation method. That is, we use the propensity score matching method to locate an *ex ante* similar control village for each of our treatment villages, and then we conduct a DID estimation based on these matched data. Specifically, matching is conducted based on the mean values of average household disposable income, percentage of arable land, total population, sex ratio, budget surplus, electricity, number of newspapers, and number of televisions from the initial year of the sample until the village installed landline phones. For the treatment villages that installed landline phones in each sample year between 1995 and 2000, we conduct one-to-one propensity score matching to locate a similar control village from a group of villages that had landline phones before that year. As shown in column (5) of Table 5, we find similar results for the landline phone effect using this matched sample.

Mechanism

We now check for the two mechanisms through which landline phones affects outmigration, that is, the provision of information on outside job opportunities and timely contact with left-behind family members.

For the first channel to work, we expect the effect of landline phones to be stronger for villages having a larger stock of pretreatment, out-migrant workers. For the second channel to work, we expect the effect of landline phones to be stronger for villages having a larger number of young children.

Regression results are reported in Table 6, with odd columns including time and village fixed effects and even columns including all the controls. Consistent with our argument, we find that the effects of landline phones on the ratio of out-migrant workers are larger for villages with more previous out-migrant workers and for villages with more young children. Thus, the results render strong support to our conjectures on how

landline phone availability facilitates migration by reducing information costs on job searches and lessening psychological costs of leaving families behind.

7 Conclusion

Using a unique natural experiment in which the installation of landline phones in a village was easily explained by obvious village factors with the added advantage of no other telecommunications options available to confound our analysis, we rely on the DID approach to identify the effect of access to landline phones on outmigration. We find that the effect is substantial, increasing the probability of outmigration by roughly 50 percent (or 2 percentage points). The results are quite robust to alternative controls and an alternative estimation approach. Our various specification checks, such as placebo tests, pretreatment tests, and so on, also render support to our assumptions based on the DID approach. Perhaps more convincingly, we find that the landline phone effects are achieved mainly through two channels: by reducing job search costs (as we find the effect is stronger when the village had a large migrant network in the pretreatment period) and by reducing psychological costs of leaving behind families (as we find the effect to be stronger in villages with more children).

Our paper implies that modern communication technology has a large role to play in facilitating labor mobility between rural and urban sectors. Since labor movement from low- to high-productivity sectors is a primary avenue for economic growth (e.g., Ngai and Pissarides 2007; Robinson 1971), the government should take into account the positive externality of having modern telecom technology installed in rural areas. Such externality is unlikely to be taken into account when telecom is monopolized by specific ministries, as in the case of China—in this case the ministry is likely to consider only the economic benefits of installing additional landline phones (or mobile towers) for the ministry or the telecom operators. Indeed, in our specification checks, we find that the decision to install a landline phone network only depended on local income level, but not the previous migrant network. Because our findings suggest that the benefits of a telecom

network would be higher in areas with a larger migrant network and more children, it is perhaps important for telecom providers to internalize the extra benefits for providing telecommunications in such villages.

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Figure 1, Time Trends of Migrant Workers and Development of Telecom in China

Sources: Second National Agricultural Census Key Data Bulletin (2008); Survey Monitoring Report of China's Migrant Workers (2011); and Contemporary Chinese Migrant Workers Flow Scale Expedition (2010) conducted by the National Bureau of Statistics of China.

Figure 2, Distribution of Sample Villages in People's Republic of China

Figure 3, Differences in Ratio of Out-Migrant Workers between Treatment and Control Groups over Time

Note. The time in the x-axis represents the distance (in years) to the year of landline installation.

Figure 4, Differences Between Villages with Landline phones and Those without throughout the Whole Sample Period

Table 1. Place and Timing of Landline phone Installation

Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Had landline phone in 1993				Timing of landline phone installation			
lnincome_91	0.238*** (0.082)	0.256*** (0.094)	0.233** (0.102)	0.233** (0.103)	-0.968** (0.384)	-1.027** (0.486)	-0.838* (0.453)	-0.841* (0.458)
lnpopulation_91	0.036 (0.141)	0.014 (0.150)	0.023 (0.155)	0.028 (0.155)	-0.165 (0.642)	-0.082 (0.709)	-0.179 (0.709)	-0.162 (0.718)
poor_91		0.089 (0.184)	0.040 (0.178)	0.033 (0.181)		-0.382 (0.786)	-0.099 (0.731)	-0.123 (0.757)
remote_91		-0.005 (0.153)	-0.043 (0.176)	-0.044 (0.177)		0.096 (0.727)	-0.043 (0.885)	-0.045 (0.896)
% arable_91			-0.213 (0.197)	-0.223 (0.204)			1.052 (0.907)	1.020 (0.920)
mountains			-0.096 (0.176)	-0.102 (0.177)			1.066 (0.895)	1.046 (0.900)
migrate_91				-0.402 (1.087)				-1.320 (4.061)
Observations	57	57	57	57	57	57	57	57
R-squared	0.270	0.274	0.286	0.287	0.236	0.240	0.271	0.271

Robust standard errors in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table 2. Summary Statistics

	Mean	S.D.	# Obs
A. Proportion of migrant workers in total rural labor force			
Out-province	0.037	(0.07)	403
Out-village, within-county	0.052	(0.07)	403
Immigrants within village	0.129	(0.41)	403
B. Village characteristics			
Average income per capita (Yuan)	2119.494	(1364.52)	403
No. of households	488.913	(362.00)	403
Land area (Km ²)	7.234	(11.19)	403
Proportion of villages located in mountain areas	0.303	(0.46)	403
Sexratio (Male/Female)	1.029	(0.11)	403
Government' budget surplus (100 Yuan)	1737.623	(14019.87)	403
No. of newspapers per household	0.234	(0.27)	403
Proportion of household having TV	0.809	(0.24)	403
Proportion of household using electricity	0.979	(0.11)	403
C. Distance between villages (km)			
Distance between every two villages	262.730	(663.98)	1653
Distance between one treatment village and one control village	240.868	(122.73)	777

Table 3. Summary Statistics on Landline phone Availability

Year	A. Landline phone availability by year						
1993	37						
1995	41						
1996	44						
1997	50						
1998	53						
1999	56						
2000	57						
B. Landline phone availability by province							
Province	% villages with landline installed, 1993 & 2000		Number that add landline				
	Year 1993	Year 2000					
Liaoning	0.92	1.00	1				
Shandong	0.60	1.00	4				
Hubei	0.47	1.00	8				
Guangdong	0.90	1.00	1				
Yunnan	0.80	1.00	1				
Gansu	0.00	0.67	6				
Year	C. Number of villages with new access						
	Liaoning	Shandong	Hubei	Guangdong	Yunnan	Gansu	Total
1995	0	2	2	0	0	0	4
1996	0	0	2	0	0	2	4
1997	1	0	4	1	0	0	6
1998	0	1	0	0	1	1	3
1999	0	1	0	0	0	2	3
2000	0	0	0	0	0	1	1

Table 4. Main Results

Dependent variable	(1)	(2)	(3)	(4)	(5)
	Ratio of out-province migrant workers				
Landline phone	0.021** (0.009)	0.022** (0.009)	0.021** (0.009)	0.020** (0.008)	0.019** (0.008)
<i>Year fixed effect</i>	Y	Y	Y	Y	Y
<i>Village fixed effect</i>	Y	Y	Y	Y	Y
<i>Other time-varying controls</i>	N	N	N	Y	Y
<i>Province time dummies</i>	N	N	N	N	Y
Time polynomial interactions with:					
<i>Log income in 1991</i>	N	Y	Y	Y	Y
<i>% arable land in 1991</i>	N	N	Y	Y	Y
<i>Mountains area (0/1)</i>	N	N	Y	Y	Y
Observations	403	397	397	397	397
R-squared	0.064	0.094	0.106	0.139	0.304
Number of villages	58	57	57	57	57

Note. Robust standard errors, clustered at the village level, in parentheses: *** p<0.01, ** p<0.05, * p<0.1

Table 5. Robustness Checks

Specification	(1)	(2)	(3)	(4)	(5)
		Migrant workers Out-village			Matched
		Out-province	Immigrants	within county	data
Landline phone	0.019*** (0.007)		-0.017 (0.025)	-0.007 (0.009)	0.022*** (0.008)
Landline phone next year	-0.001 (0.005)				
treatment x 2 years before		0.005 (0.014)			
treatment x 1 year before		0.003 (0.011)			
treatment x time of installation		0.018 (0.013)			
treatment x 1 year after		0.023 (0.016)			
treatment x 2 years after and onwards		0.032 (0.026)			
<i>Year fixed effect</i>	Y	Y	Y	Y	Y
<i>Village fixed effect</i>	Y	Y	Y	Y	Y
<i>Other time-varying controls</i>	Y	Y	Y	Y	N
<i>Province time dummies</i>	Y	Y	Y	Y	Y
<i>Time polynomial interactions</i>	Y	Y	Y	Y	N
F-test for treatment x {time 0, post1, post2}		[3.07]**			
Observations	397	397	397	397	292
R-squared	0.304	0.310	0.139	0.243	0.280
Number of villages	57	57	57	57	42

Note. Robust standard errors, clustered at the village level, in parentheses: *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Mechanisms

	(1)	(2)	(3)	(4)
Dependent variable:	Ratio of Out-province migrant workers			
Landline phone	0.009** (0.004)	0.006 (0.006)	0.020** (0.008)	0.018*** (0.007)
Landline phone * # out-migrants in 1991	0.933*** (0.328)	0.886*** (0.288)		
Landline phone * # children in 1991			0.019* (0.010)	0.019*** (0.007)
<i>Year fixed effect</i>	Y	Y	Y	Y
<i>Village fixed effect</i>	Y	Y	Y	Y
<i>Other time-varying controls</i>	N	Y	N	Y
<i>Province time dummies</i>	N	Y	N	Y
<i>Time polynomial interactions</i>	N	Y	N	Y
Observations	286	282	403	397
R-squared	0.245	0.489	0.077	0.168
Number of villages	58	57	58	57

Robust standard errors, clustered at the village level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1