Telecommunication Externality on Migration: Evidence from Chinese Villages[1]

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Abstract: We use a unique data set of 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, our difference-in-difference estimation shows that the access to landline phones increases the ratio of out-migrant workers by 2 percentage points, or about 51 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 on job opportunities and especiallytimely contact with left-behind family members. Our findings underscore the positive migration externality of expanding telecommunications access in rural areas, especially in places where migration potential is large.

JEL codes: J2, O15, O50

Keywords: Landline phones; telecommunications; migration; network effect; psychological costs; China.

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1 Introduction

The past decades have witnessed a surge in intranational migration. In the past three decades in China, 500 million people have flocked to the city, and during the period of 2000 to 2010 alone, China's urban population had expanded by 210 million (Wong 2012; World Bank and DRC, 2014). Outmigration is generally found to profoundly contribute to the welfare of both the recipient and sending destinations.Beegle, Weerdt and Dercon (2008), for instance, find that migration between 1991 and 2004 increasedconsumption growth rate in Tanzania by 36 percentage points. Despite the significant economic benefits, however, there are still substantial barriers to labor mobility. Why do so many potential migrants fail to act out their wishes? How can we facilitate outmigration?

The literature on the determinants of migration, starting from the classical Harris and Todaro (1970), emphasizes the rural-urban earning differentials as the key reason. Moreover, scholars recognize that a fundamental problem in migration is uncertainty. Potential migrants donot have full information about job opportunities, wages, and the quality of life in destinationcities, and new evidence on migrants' expectations in developing countries suggest how inaccurate these expectations can be (McKenzie, Gibson and Stillman 2013). The literature has recognized and provided evidences that an important way to reduce information problems is through the channel of networks (Barr and Oduro 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 2007; 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 Yamano2009).[2] 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 Yamano2009; 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 access to in landline phones, can loosen the constraints on potential migrants and lead to an increase in outmigration. How does access to telecommunications affect outmigration? We consider two reasons. First, telecom access allows potential migrants to access information on external labor markets, which substantially reduces their searching costs and increases the accuracy of their costs-benefits analysis of migration decisions. Second, telecom access allows migrants contacts with their left-behind family membersconveniently and timely, which substantially reduces the psychological costs of migration. This is especially important in China because of the prevailing policies limiting access to education and health care by migrants at destination cities, which results in adults largely leaving their families and migrating alone (Wong 2012; World Bank and DRC, 2014).

Using the *National Fixed Point Survey* conducted by the Ministry of Agriculture of China in 1993 and 1995–2000, we exploit 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, 35 had landline phones in 1993 (i.e., our initial year), 23 installed landline phones at different times during the sample period, and 3 remained without access to landline phones

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[2]For studies looking at how mobile phones reduce market information asymmetry in general, see Jensen (2007), Aker (2010), etc.

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 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 easilyhandled, and not surprisingly, 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-of-province migrant workers in total rural labor force by 1.5 to 2 percentage points, or 38.5 to 51 percent of the sample mean. The results are robust to a battery of validity checks, such as using DID coupled with matching, using a weighted regression, controlling for pretreatment effect, and using a flexible estimation method to account for differences in the time trend in outmigration of the treatment and the control groups. Two placebo tests also confirm our identification assumptions. First, 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 trend in the migration levels for villages always with telecom access and those never with telecom access. Second, if the telecom effects on outmigration reflect mechanismsother than the information or timely contact with left-behind families (as we hypothesize), telecom access will likely affect

Out-of-village, within-county migration, but we do not find that telecom access affects such short-distance migration.

We further shed light onour 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 out-migrants (a proxy for the information access through the network effect) and for villages with more non-labor population(a proxy for left-behind family members).

Our paper contributes to the literature 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 transparent and plausible strategy for identifying the effects of telecom on migration. The robustness of the results under a variety of specification checks renders support to the plausibility of our identification strategy. Second, our evidence comes from the country that has experienced the largest migration in the world and in the periods in which the migration process was in full swing (World Bank and DRC, 2014), and it is useful to know whether modern telecom has had 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, our results suggest that modern telecom may reduce the psychological costs of migration by allowing migrants to stay in touch with their family members 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 describe the data and the key variables. Section 4 presents the empirical strategy.Section 5shows ourempirical findings.Section 6concludes.

2 Background

2.1   Rural-to-Urban Migration in China

Since 1978, China has embarked on a great economic and social transformation, which has subsequently led to substantial changes in the rural-urban divide (Lau 2010). 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 (Baoand others 2011).As shown in Figure 1, the number of rural out-migrant workers rose from around 20 million in 1990 to 62 million in 1993,[3] 132 million in 2000, and nearly 160 million in 2011.[4]

[Insert Figure 1 Here]

Unlike migrations in many other countries, rural-to-urban migration in China has its own features. Most of these migrant workers, constrained by their lower education levels, work in the manufacturing and construction sectors in cities.Because of the presence of the household registration (or Hukou) system, rural migrants find it difficult to permanently settle down in recipient cities for a long time.[5]In particular, they are largely denied access to many of the social welfare programs, such as education and healthcare, to which their urban counterparts are entitled. Indeed, migrant workers on average return home two to three times annually and spend less than 9 months in recipient cities (Zhao 1999). Another feature of the 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.,Solinger1999; Zhao2003). This is also common in many other

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[3]According to the National Bureau of Statistics of China, rural out-migrant workers are defined as individuals who have rural household registration status (*hukou* in Chinese), but left their homeland and have worked outside the towns and counties for at least 6 months.

[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*.

developing countries (Barr and Oduro 2002; Munshi2003; 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 their jobsthrough village-based friends or relatives. Before the arrival of modern telecommunications technologies, such as landline and mobile phones, potential migrants largely 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.[6]

2.2   Development of Landline Phones in China

From 1949 to China's economic reform initiated in 1978, the government gave priority to developing heavy industry and largely neglected investment in telecommunications. In the late 1980s, economic reforms led to rapid growth in the economy. The booming economy started to call for better communications services, so the government developed telecom facilities in major cities and coastal areas. However, almost all subscribers to the telecom services were living in urban areas while people in remote rural parts of China remained unconnected.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 competitorsto the telecom market, and by gradually opening the telecom markets to foreign investors(also see Zheng and Ward 2011). As a result, service quality has dramatically improved and tariffs have fallen substantially, leading to a record growth in landline phone subscribers.[7]

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[6]Another way to obtain information is through mail system, which took days and weeks depending on the location and deliveryconvenience. Compounding the difficulty of this route is the fact that rural migrants tend not to be prolific letter writers.

[7]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.

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, other new technologiessuch 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. The number of Internet users has increased nearly 7 times between 2002 and 2011. However, between 1993 and 2000 (our sample period), landline phoneswere 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 willdemonstrate later, we are able to identifythe effects more convincingly than to identify the telecom effects of mobile phones, which involves individual- or household-level selectivity to a much greater extent.

3 Data and Variables

Our dataset is theNational Fixed Point Survey(NFPS) conducted by the Ministry of Agriculture of China in 1993 and 1995–2000,[8]during which periods it contains information on landline phones. Households were randomly sampled from six provinces (i.e., Gansu, Guangdong, Hubei, Liaoning, Shandong, and Yunnan provinces). The sample provinces cover diverse regions of China, ranging from coastal to inland areas and coveringnorthern, southern, western and eastern China; they also feature diverse levels of economic development, climate, natural endowment, and infrastructure.

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[8]Surveys were not conducted in 1994 because of financial reasons. Earlier periods are available but they do not contain information on landline phones.

We have 67 villages in 1993. Six villages were deleted because their location codes changed over time and we are not able to obtain consistent codes. Among the remaining 61 villages, 35 villages had landline phones in 1993; 23 villages installed landline phones during the sample period;and 3 villages had never installed landline phones duringthe sample period.

The key variables for our analysis are the measure of out-of-province migrant workers as our dependent variable, and an indicator of installation of landline phones as aregressor of interest. In NFPS, laborers or workersaredefined as males with agesbetween 16 and 60 and females between16 and 55. In the survey, a laborer is classified as the migrant worker if the individual has worked outside the village (including out-of-village and within-county, out-of-county and within-province, out-of-province and within-China, and overseas) for most of the time of a year.[9]Table 1 reports the summary statistics of our key variables. During the sample period (i.e., 1993, and 1995–2000), the overall ratio of out-of-province migrant workers to total labor force is 3.9 percent, and the overall ratio of out-of-village, within-county migrant workers is 5.2 percent. Meanwhile, our sample villages are quite poor with an average annual income per capita of 2076 *yuan* or US$333, small in population size (i.e., 479 households living in a7-square-kilometer area), and mostly located out of the mountains (i.e., 68 percent).

[Insert Table 1 Here]

The distances among the 61sample villagesare quite large, averaging 835 kilometers between any two villages. Suchlong distances make the spillover effect of landline phones from the treatment group to the control group quite unlikely.[10]

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[9]In China, the administrative hierarchy in the rural areas is central government, followed by provincial government, municipal government, county government, and then village government. There are 32 provinces, 345 municipalities, 2,856 counties, and 4,044,907 villages in April 2013.

[10]It is possible that non-sampled villages in between might also be treated, and there might be spillover effects from non-sampled treatment villages to our control villages. In this case, our estimation gives us the lower bound of the land-line phone effects.

We present summary statistics of landline phone accessibility in Table 2. The number of villages with landline phones in our data increased from 35 in 1993 to 58 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 difference-in-difference (DID) estimation method.

[Insert Table 2 Here]

4 Empirical Strategy

We rely on the DID approach to identify the causal effect of access to landline phones on outmigration. Our baseline estimation equation is as follows:

$$y_{vt} = \alpha_v + \gamma_t + \beta Tele_{vt} + \varepsilon_{vt}, \tag{4}$$

where y_{vt} is the ratio of Out-of-province migrant workers to the total labor force in village v at year t. $Tele_{vt}$ is equal to 1 if village v had landline phones at year t and 0 if not. 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 macro shocks common to all villages, such as the business cycle, macro level regulations, and the overall economic growth. ε_{vt} is the error term. To deal with potential heteroskedasticity and serial correlation, we cluster the standard error at the village level to avoid overstating estimation precision (Bertrand, Duflo, and Mullainathan, 2004).

In our data, there are three types of villages: (1) some had landline phones through the whole sample period (referred to as *incumbents*); (2) some had no landline phones through the whole sample period (referred to as *outsiders*); and (3) some installed landline phones during the sample period at different time points (referred to as *switchers*). The staggered nature of landline installation provides us with useful identifying variations. In the baseline DID estimation, we first use the whole sample, and hence our DID identification essentially comes from the changes in the outcome for the switchers after the treatment relative to incumbents and outsiders, and from the changes in the outcome for the early installing switchers after the treatment relative to that for the late installing switchers during those early periods.Second, we use the sample of the switchers that are assumed to be more homogeneous, and the identification relies on the comparison of the early installing switchers with the later installing ones in those early periods.Note that there are only 23 villages in this robustness analysis. Hence, to adjust the inference issue of few clusters, we use the Wild cluster-bootstrap percentile-t procedure developed by Cameron, Gelbach, Miller (2008). As a further robustness checks, we take advantage of the fact that there is no status change in the incumbents and outsiders--the comparison between these two groups provides us with a good placebo test, i.e., for these two comparison groups without experiencing treatment, their differences in the outcome should be stable over time.Indeed, 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,

$$E\left[\varepsilon_{vt} \mid Tele_{vt}, \alpha_v, \gamma_t\right] = E\left[\varepsilon_{vt} \mid \alpha_v, \gamma_t\right]. \qquad (5)$$

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

4.1   Placement and Timing of Landline Phone Installation

A potential challenge to the DID specification is that the location and the 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 explainthe post-treatment divergence in the ratio of out-migrant workers, causing a spurious correlation between our explanatory variableof interest and the outcome variable. Thus, we need to understand what determines which village installed landline phones earlier to isolate the effect of landline phones on outmigration.

We conduct a regression analysis on the determinants of landline phone connection.Firstly, we consider the village's level of economic development (which may affect the degree of facility use), that is, the average income per capita and total population. We then consider special government policies, that is, whether the village was officially classified as apoverty village and whether it was officially classified as adisadvantaged village[11]—both categories of villages are supposed to enjoy compensatory policies. We also consider geographic features (that may affect the costs of installing landline phones), that is, the percentage of arable land, whether the village was in the mountainous area, the distance to the nearest county or municipal (or prefecture) government, and the distance to the main road. Lastly, we investigate whether the installation was triggered by the needs of out-migrant workers, and thus consider the number of both existing and potential out-migrant workers. We use the ratio of out-of-county migrant workers in 1991 to measure the existing out-migrant workers, and use the percentage of idle labors as a proxy for potential migrant workers, use the percentage of remittance income and the percentage of loan earnings as proxies for financial constraints of migration.All these determinant variables were measured in the pre-treatment stage in 1991.

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[11]Disadvantaged areas are regions that are occupied with vulnerable groups and minorities and tend to be far away from economic centers.

The regressions results are in Table 3. Columns (1) to (4) deal with the placement of landline phones, and the dependent variable is 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 experimented with the Cox proportional hazards model; the qualitative results are similar.[12]

[Insert Table 3 Here]

The findings suggest that 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)). Average income per capitaand total population together can explain around 23-30 percent of the total variations in the placement and timing of landline phone installation. Meanwhile, villages that were classified as "poor villages" or "disadvantaged villages" and were closer to the nearest county or municipal (or prefecture) government were more likely to have landline phones in 1993 and more likely to install landline phones earlier, while villages located in mountainousareas were less likely to have landline phones in 1993 and more likely to install landline phones later. None of the remaining determinants are statistically significant. In particular, the ratio of out-migrant workers in 1991 and proxies for potential migrant workersare consistently insignificant, suggesting that the installation of landline phones is not reversely caused by our outcome variable.

Our evidences in this subsectionthus show that the average income of a village, the population in a village, the "poor village"andthe "disadvantaged village" statuses,the distance to the nearest county or municipal (or prefecture) government, and the topographic conditions are important factors in determining whether the village installed landline phones and its timing. Meanwhile, conditional on these key determinants, other factors are found to be

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[12]The results are available upon request.

statistically insignificant, especially the pre-treatment ratio of outmigration villagers in total labor force. About 54 percent of the differences between treatment and control groups remain unexplained, and we are unable to locate other factors that significantly drive both the selection of landline phone installation and the post-installation differential in migration between these two groups.

4.2   Augmented Estimation Specification and Robustness Checks

We have shown that the treatment and the control groups *ex ante* differ significantly in the village's level of economic development. To alleviate the concern that pre-existing differences in economic development between these groups may generate the differential patterns of outmigration over time, we follow Gentzkow (2006) in controlling for a flexible time trend in outmigration generated by the preexisting village characteristics. Specifically, we interact a second-order polynomial function of time with the village's average income per capita in 1991, log total population in 1991, the "poor village" status in 1991, the "disadvantaged village" status in 1991, the indicator of being in mountainous area, and the distance to the nearest county or municipal (or prefecture) government.[13] Moreover, we also control for some other village characteristics and the exposure to other media which may also affect out-migration, including the number of firms, the percentage of non-labor population, the sex ratio, the number of newspapers and magazines subscribed per household, and the number of TV sets per household. However, since these control variables may be affected by the installation of landline phones, we use their values in 1993 and interact them with the second-order polynomial function of

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[13] We also use the distance to the nearest county or municipal government as a proxy to control for other infrastructure (such as roads/trains) and commuting time--migrant workers live closer to county or municipal government will be more easily able to travel between work and home location. Moreover, using fourth-order polynomial function of time in the interactions generates very similar results.

time. We do note that usingtheir interactions with forth-order polynomial function of time or the time-varying values of these control variables generate similar results.[14]

Hence, our augmented DID estimation specification becomes

$$y_{vt} = \alpha_v + \gamma_t + \beta Tele_{vt} + \mathbf{X}_{vt}'\eta + \varepsilon_{vt}, \qquad (6)$$

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

$$E\big[\varepsilon_{vt} \mid Tele_{vt}, \mathbf{X}_{vt}, \alpha_v, \gamma_t\big] = E\big[\varepsilon_{vt} \mid \mathbf{X}_{vt}, \alpha_v, \gamma_t\big]. \qquad (7)$$

5   Empirical findings

5.1   Main Results

Figure 2 shows the difference in the ratio of out-migrant workers between the treatment and the control groups over time. Clearly, the treatment and the 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, however, 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 (i.e., equation (6)) are reported in Table 4. We start with including only year and village fixed effects in column (1) of the upper panel. Here, landline phones have a positive and statistically significant coefficient, which is consistent with the findings in Figure 2. This result suggests that access to landline phones increases the ratio of outmigration by 2 percentage points.

[Insert Figure 2 Here]

[Insert Table 4 Here]

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[14] The results are available upon request.

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In columns(2)-(3), we progressively add second-order polynomial function of time interacted with village characteristics in 1991/1993 to control for possible differences among the villages. Village characteristics include all the preexisting village characteristics and control variables as listed in section 4.2.Evidently, our estimated coefficient of landline phones not only remains statistically significant but also hasthe same magnitude—the landline phone effect on the outmigration ratio is 2 percentage points, or about 51 percent of the mean (i.e., 3.9 percent).

While in the lower panel of Table 4, we focus on the switchers group (those villages installed landline phones during our sample period), which are presumably more homogenous.The installation of landline phones is still found to positively and statistically significantly affect the out-migration ratio, with a slight decline in the magnitude. The landline phone effect on migration ratio is 1.5 percentage points, or about 38.5 percent of the sample mean.

5.2   Robustness Checks

In Table 5, we conduct several robustness checks on our identifying assumption (equation (7)).

[Insert Table 5 Here]

One potential challenge to our DID estimation is that even with a long list of controls (i.e.,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, variation in *hukou* access across villages in rural China, or the village's evolving policies on land reallocation when local residents migrate. Althoughsuch variables 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" dummy likely indicates that the landline phone effect merely reflects the influence of related confounding factors. To check the possibility of such confounding 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 phonesnext year is statistically insignificant, and our main coefficient remains robust to this control, supporting the validity of our DID estimation.[15]

Second, we use a more flexible specification for installing landline phones in the future and past, that is, replacing our regressor of interest ($Tele_{vt}$) with a series of time dummies indicating various distance in time to the landline installation year (the default time category is at least three years before the installation).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.Furthermore, we test whether the post-installation coefficients are different from pre-installation ones, the F-statistic for the joint test is 2.74, and we can reject the null hypothesis that all the post-treatment coefficients are equal to the pre-treatment coefficients at the 5 percent level, and thus further support a positive causal effect of landline phone installation.

5.3   Two Placebo Tests

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[15]As a rough but important additional test, we also exclude "Tele" in the same regression, and find that "landline phone anticipator" still remains statistically insignificant (the results are available upon request).

We now conduct two placebo tests to further support our key results. First, in the survey, we have three villages that had not installed landline phones by the end of the sample period (i.e., 2000). If outmigration is truly triggered by landline phones, we should see a similar trend in the ratio of out-migrant workers between villages with landline phones and those without landline phones throughout the entire sample period: their landline phone status did not change over time. In other words, the differences in the outcome betweenthese two comparison groups should be stable over time.Indeed, Figure 3shows that the differences between the two comparison groups, despite some fluctuations, remain similar over our sample period.

[Insert Figure 3 Here]

Second,in our theory, the effect of landline phones on outmigration originates from the sharing of job information and timely contacts with left-behind family members. Because villages within the same county are quite close, these two roles of landline phones may not be important, and we expect that landline phoneshave no impact on migration to different villages or towns within the same county. To test this implication, we construct a new outcome variable, *Within-county*, which is the percentage of Out-of-village, within-county migrant workers (in total village labor force), and we re-estimate equation (6) using this outcome variable (see column (3) of Table 5). Evidently, there is no statistically significant effect of landline phones on Out-of-village, within-county migration.

5.4   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 we then conduct a DID estimation based on these matched data. Specifically, matching is conducted based on the average income per capita, total population, whether being classified as "disadvantaged village", whether being

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in mountainous area, the distance to the nearest county or municipal (or prefecture) government, the number of firms in the village, the percentage of non-labor population, the sex ratio in the village, thenumber of newspapers andmagazines subscribed per household, and number of TV sets per households 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 (4) of Table 5, we find similar landline phone effect using this matched sample.

5.5 Weighted Regression

The estimates reported in the baseline give all villages equal weight, but the villages in the sample vary in size. As a robustness check, we use the average population size during our sample period as the weight variable. As shown in Table 5 column (5), our estimates from the weighted regression is similar to our baseline results, suggesting that access to the landline phone increases the outmigration rate by 2.5 percentage points. And our estimates are not sensitive to using the population size in the 1991 as the weight variable (see column (3) of Appendix Table 1).

5.6 Mechanism

We nowprovide evidence on thetwo 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(a proxy for the information access through the network effect). For the second channel

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to work, we expect the effect of landline phones to be stronger for villages having a larger number of non-labor population(a proxy for left-behind family members).

Regression results are reported in Table 6. In column (1), we add the interaction term between landline phone and the ratio of Out-of-province migrant workers in previous year. The coefficient of the interaction term is positive and significant, suggesting that the effects of landline phones on the ratio of out-migrant workers are larger in villages with more previous out-migrant workers. In column (2) of Table 6, we add the interaction of landline phone and the percentage of non-labor population in the previous year. Consistent with our conjecture, we find that the effects of landline phones on the ratio of out-migrant workers are larger for villages with more non-labor population. In column (3), we include both interactions in the same regression and conduct a horse race between the two channels in order to access theirrelative strength. As shown in the table, a one standard deviation increase in the ratio of previous out-migrant workers would lead to a 0.37 standard deviation increase in the effect of landline phones on Out-of-provincemigration ratio; and increasing the ratio of non-labor population in the previous year by one standard deviation would boost the effect of landline phones on Out-of-provincemigration ratio by2.14 standard deviation. Thus, the results render strong support to our conjectures that landline phone availability facilitates migration by reducing information costs on job searches and lessening psychological costs of leaving families behind. It appears that the strength of the psychological cost channel is quantitatively more important.

[Insert Table 6 Here]

6 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 a substantial effect: increasing the probability of outmigration by 51 percent (or 2percentage points).The results are 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 identifying 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 previous period) and by allowing timely contacts with left-behind families (as we find the effect to be stronger in villages with more non-labor population the previous period).

Our paper implies that modern communication technology (landline phones, in our paper) has a large role to play in facilitating labor mobility between rural and urban sectors, and thus adds to recent literature that provides empirical evidence on the critical importance of modern telecom on economic development.[16]As we know, new telecommunication technologies, such as mobile phones and the Internet, began to penetrate China since late 1990s and have largely replaced the role of landline pones. As shown in Figure 1, the number of cellular phone users surpassed the number of landline phone users in 2003. Given the magnitude of the effects of access to telecommunication estimated here, and strong evidence of positive effect of internet elsewhere (Clarker, Qiang and Xu, 2015), the development of mobile phones and internet would have significant impacts in China.[17]

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

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[16]Harrison, Lin and Xu (2014) provide evidence that modern telecom is of key importance for explaining cross-country firm-level performance around the world, and African countries' disadvantage in this area partlyexplains their firm performance disadvantage.
[17]Clarke, Qiang and Xu (2015) provide firm-level evidence that a particular type of telecom, internet, serves as general-purpose technology that has widespread influence on firm performance in many industries and especially for smaller firms.

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 highly 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 morenon-labor population, it is perhaps important for telecom providers to internalize the extra benefits for providing telecommunications in such villages.

Figure 1: Time Trends of Migrant Workers and Development of Telecom in China
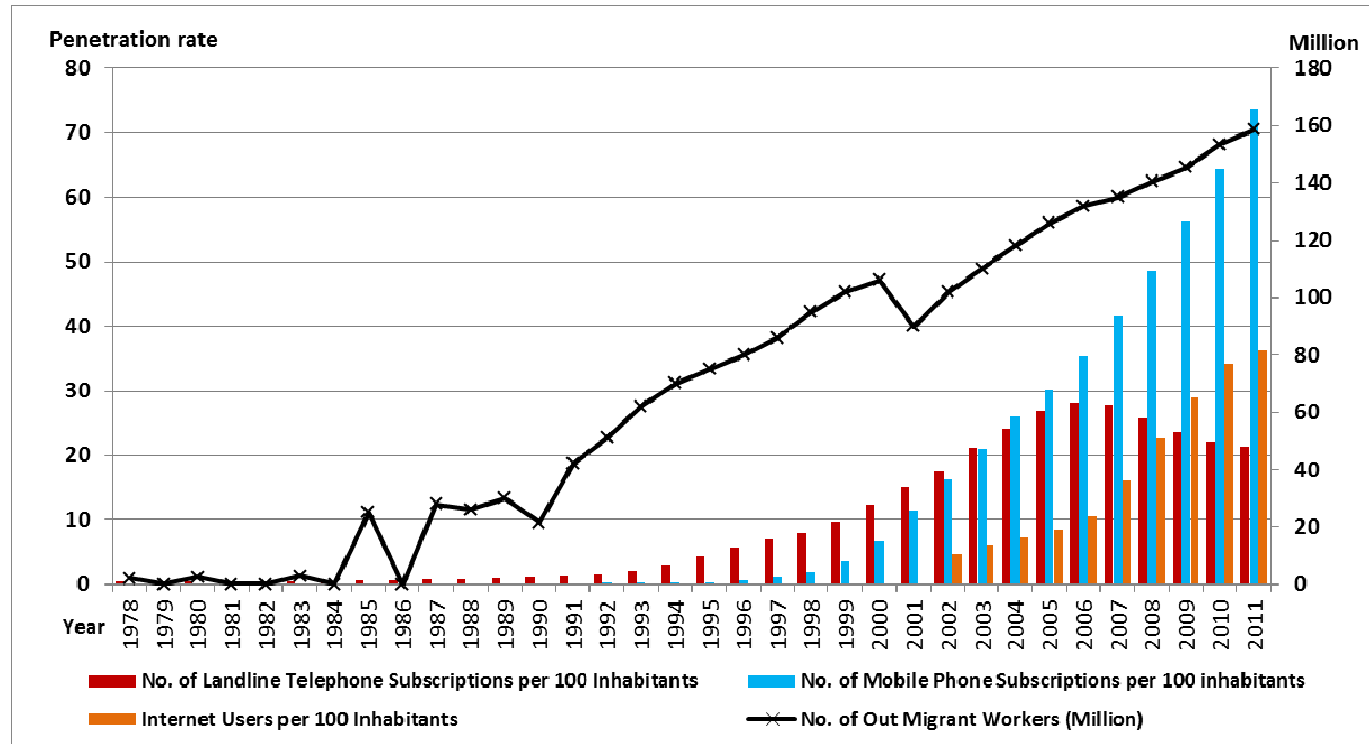
Sources: Second National Agricultural Census Key Data Bulletin (2008); Survey Monitoring Report of China's Migrant Workers (2011); and Contemporary Chinese Migrant Workers Flow Scale Expedition (2010) conducted by the National Bauru of Statistics of China.

Figure 2: Differences in Ratio of Out-Migrant Workers between Treatment and Control Groups over Time
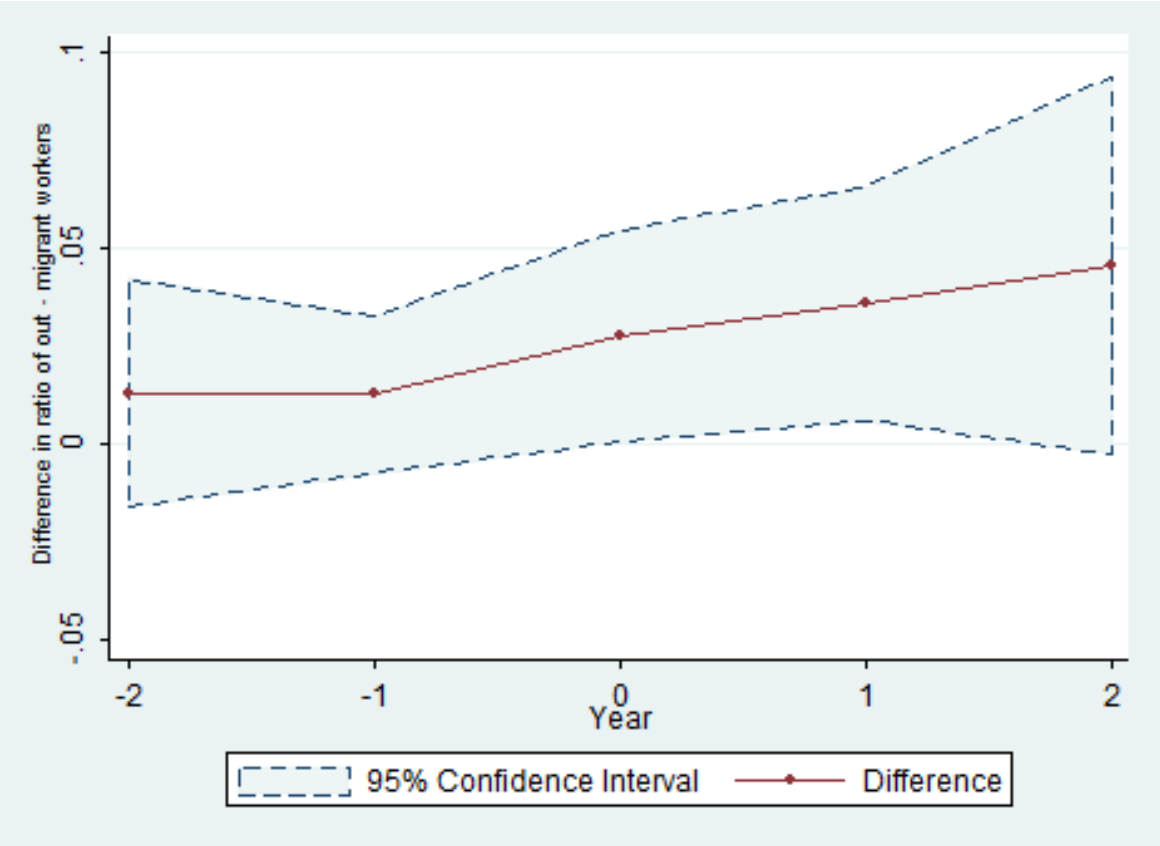
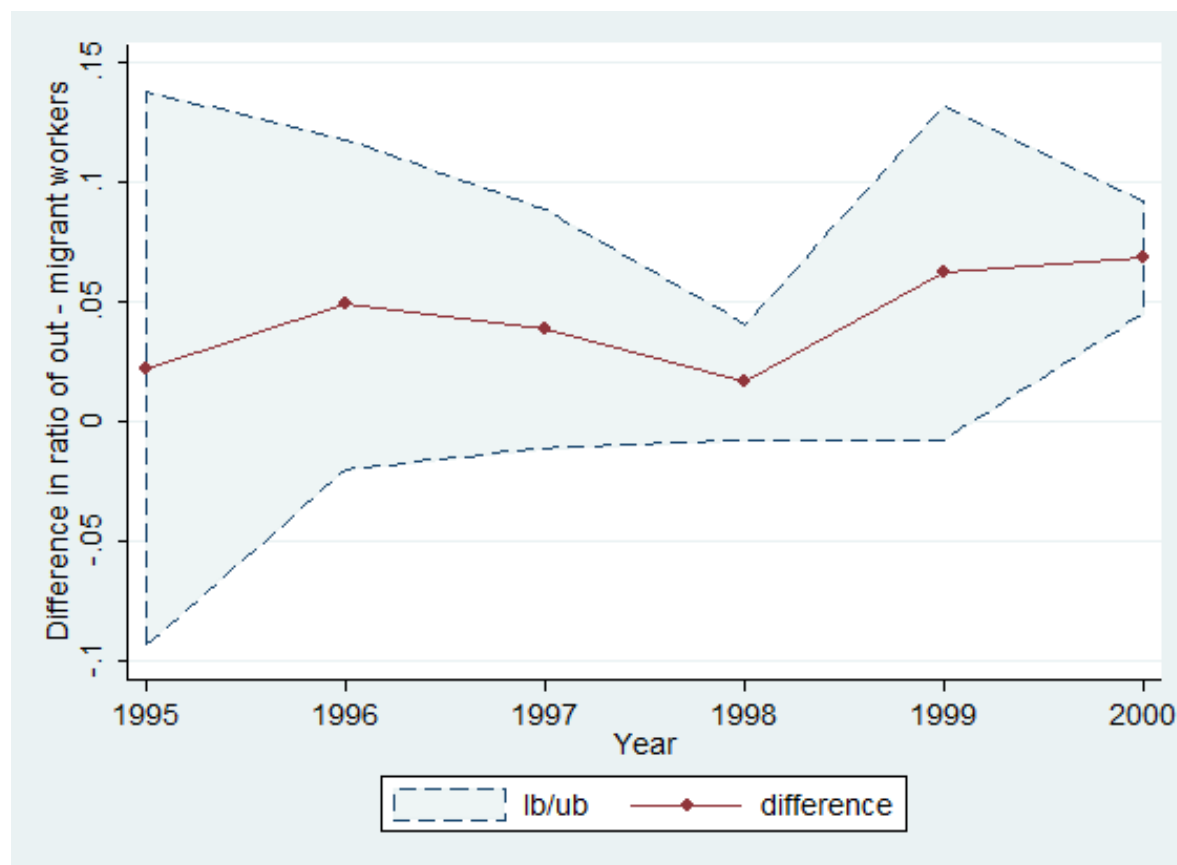Note. The time in the x-axis represents the distance (in years) to the year of landline installation.

Figure 3: Differences Between Villages with and those without Landline Phones throughout the Whole Sample Period

Table 1. Summary Statistics

	Mean	S.D.	# Obs
A. Proportion of migrant workers in total rural labor force			
Out-of-province	0.039	0.066	422
Out-of-village, within-county	0.052	0.073	422
B. Village characteristics			
Average income per capita (Yuan)	2075.910	1353.469	422
No. of households	479.401	356.767	422
Land area (Km^2)	7.172	10.970	422
Total population	1895.962	1289.815	422
Arable land area/total land area	0.516	0.333	422
Proportion of villages located in mountain areas	0.318	0.466	422
Proportion of villages classified as "Poor Village"	0.104	0.306	422
Proportion of villages classified as in " Disadvantaged Areas" in 1991	0.305	0.464	59
No. of newspapers per household	0.230	0.263	422
No. of TV sets per household	0.808	0.235	422
No. of non-labor population/total population	0.497	0.096	422
C. Distance between villages (km)			
Distance to the nearest county/municipal/prefecture government	23.319	18.817	422
Distance to the main road	1.775	2.739	422
Distance between every two villages	834.530	464.830	3721

Table 2. Summary Statistics on Landline Phone Availability

A. Landline telephone availability by year	
Year	Number of villages
1993	35
1995	37
1996	43
1997	50
1998	53
1999	56
2000	58

B. Landline telephone availability by province

Province	Year 1993	Year 2000	Number that add landline
Liaoning	0.83	1.00	2
Shandong	0.60	1.00	4
Hubei	0.40	1.00	9
Guangdong	0.90	1.00	1
Yunnan	0.80	1.00	1
Gansu	0.00	0.75	6

C. Number of villages with new access

Year	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	5	1	0	0	7
1998	0	1	0	0	1	1	3
1999	1	1	0	0	0	2	4
2000	0	0	0	0	0	1	1

Table 3. 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			
Average income per capita in 1991	0.021***	0.020***	0.015***	0.013**	-0.096***	-0.096***	-0.070**	-0.059**
	(0.006)	(0.006)	(0.006)	(0.005)	(0.030)	(0.034)	(0.028)	(0.025)
Log population in 1991	0.296***	0.310***	0.329***	0.388***	-1.312***	-1.397***	-1.213***	-1.481***
	(0.077)	(0.078)	(0.091)	(0.101)	(0.366)	(0.374)	(0.451)	(0.493)
Being classified as a poor village in 1991(0/1)		-0.201	-0.233	-0.289*		0.869	1.101*	1.351*
		(0.155)	(0.148)	(0.168)		(0.711)	(0.624)	(0.677)
Being classified as a disadvantaged village in 1991(0/1)		0.097	0.248*	0.251*		-0.671	-1.796**	-1.941**
		(0.140)	(0.125)	(0.134)		(0.699)	(0.697)	(0.736)
Percentage of arable land in 1991			-0.097	-0.156			0.639	0.617
			(0.203)	(0.220)			(0.900)	(0.842)
Mountains area (0/1)			-0.161	-0.238			1.423*	1.829**
			(0.137)	(0.155)			(0.779)	(0.815)
Distance to the nearest county/city (km)			-0.009***	-0.009***			0.042**	0.040**
			(0.003)	(0.003)			(0.016)	(0.015)
Distance to the main road (km)			0.030	0.031			-0.034	-0.037
			(0.019)	(0.021)			(0.142)	(0.149)
Out-county migrant workers / Total labor force in 1991				-0.217				-2.975
				(1.074)				(4.483)
Idle labors / Total labor force in 1991				0.017				-0.287
				(0.035)				(0.196)
Remittance income / Total income in 1991				-1.415				9.111
				(1.198)				(5.994)
Loanearnings/ Total income in 1991				-1.444				6.565
				(0.921)				(3.911)
Observations	59	59	59	59	59	59	59	59
R-squared	0.288	0.312	0.426	0.459	0.234	0.259	0.394	0.440

Note: Standard errors in parentheses are clustered at village level. *** p<0.01, ** p<0.05, * p<0.1

Table 4.Baseline Results

Dependent variable	[1]	[2]	[3]
Regressors:	Ratio of Out-of-province migrant workers		
Panel A: Full Sample			
Tele	0.020**	0.020**	0.020**
	(0.008)	(0.008)	(0.008)
Year fixed effect	Y	Y	Y
Village fixed effect	Y	Y	Y
Time polynomial interactions with:			
Determinant variables in 1991	N	Y	Y
Control variables in 1993	N	N	Y
Observations	422	411	411
R-squared	0.067	0.152	0.168
Number of villages	61	59	59
Panel B: Sample of Switchers			
Tele	0.014	0.016***	0.015***
	(0.009)	(0.000)	(0.000)
Year fixed effect	Y	Y	Y
Village fixed effect	Y	Y	Y
Time polynomial interactions with:			
Determinant variables in 1991	N	Y	Y
Control variables in 1993	N	N	Y
Observations	160	160	160
R-squared	0.775	0.836	0.850
Number of villages	23	23	23

Notes: 1. Determinant variables in 1991 include average income per capita, log population, being classified as disadvantaged areas, being classified as poor village, distance to the nearest county or municipal (or prefecture) government, being in mountains areas;
2. Other control variables in 1993 include sex ratio, ratio of non-labor population, no. of firms in the village, newspaper per household, and TV sets per household;
3. In the upper panel, standard errors in parentheses are clustered at the village level; whereas in the lower panel, standard errors in parentheses are calculated using the Wild cluster-bootstrap percentile-t procedure developed by Cameron, Gelbach, and Miller (2008). *** $p<0.01$, ** $p<0.05$.

Table 5.Robustness Checks

Specification	[1]	[2]	[3]	[4]	[5]
	Ratio of Out-of-province migrant workers		Within county	Matching	Weighted
Tele	0.023***		0.003	0.018*	0.025**
	(0.008)		(0.010)	(0.010)	(0.011)
Tele next year	-0.005				
	(0.005)				
2 years before installation		0.012			
		(0.015)			
1 year before installation		0.008			
		(0.011)			
time of installation		0.024*			
		(0.014)			
1 year after installation		0.034**			
		(0.016)			
2 years + after installation		0.044			
		(0.028)			
Year fixed effect	Y	Y	Y	Y	Y
Village fixed effect	Y	Y	Y	Y	Y
Time polynomial interactions with:					
Determinant variables in 1991	Y	Y	Y	N	Y
Control variables in 1993	Y	Y	Y	N	Y
Province time dummies					
F test for treatment × {time 0,		2.74**			
post1, post2+}		0.038			
Observations	411	376	411	170	411
R-squared	0.169	0.225	0.138	0.238	0.182
Number of villages	59	54	59	51	59

Notes:1. Determinant variables in 1991 include average income per capita, log population, being classified as disadvantaged areas, being classified as poor village, distance to the nearest county or municipal (or prefecture) government, being in mountains areas;
2. Other control variables in 1993 include sex ratio, ratio of non-labor population, no. of firms in the village, newspaper per household, and TV sets per household.
3. Standard errors in parentheses are clustered at village level.   *** $p<0.01$, ** $p<0.05$.

Table 6. Mechanisms

Dependent variable	[1]	[2]	[3]
	Ratio of Out-of-province migrant workers		
Tele	-0.010	-0.075	-0.080*
	(0.010)	(0.060)	(0.044)
Tele × Ratio of migrant workers in previous year	0.456**		0.445**
	(0.200)		(0.191)
migrant workers/ labor force in previous year	0.380*		0.365*
	(0.224)		(0.202)
Tele × Ratio of non-labor population in previous year		0.168	0.147*
		(0.114)	(0.077)
Non-labor population/ total population in previous year		0.002	-0.080
		(0.101)	(0.077)
Year fixed effect	Y	Y	Y
Village fixed effect	Y	Y	Y
Time polynomial interactions with:			
Determinant variables in 1991	Y	Y	Y
Control variables in 1993	Y	Y	Y
Observations	292	292	292
R-squared	0.412	0.146	0.428
Number of villages	59	59	59

Notes:1. Determinant variables in 1991 include average income per capita, log population, being classified as disadvantaged areas, being classified as poor village, distance to the nearest county or municipal (or prefecture) government, being in mountains areas;
2. Other control variables in 1993 include sex ratio, ratio of non-labor population, no. of firms in the village, newspaper per household, and TV sets per household.
3. Standard errors in parentheses are clustered at village level. *** $p<0.01$, ** $p<0.05$.

Appendix Table 1. Robustness Checks

Specification	[1] Ratio of Out-of-province migrant workers	[2]	[3] Weighted
Tele			0.026**
			(0.011)
Tele next year			
2 years before installation	0.013	0.012	
	(0.014)	(0.015)	
1 year before installation	0.013	0.008	
	(0.010)	(0.011)	
time of installation	0.028**	0.024*	
	(0.014)	(0.014)	
1 year after installation	0.036**	0.034**	
	(0.015)	(0.016)	
2 years + after installation	0.046*	0.044	
	(0.024)	(0.028)	
Year fixed effect	Y	Y	Y
Village fixed effect	Y	Y	Y
Time polynomial interactions with:			
Determinant variables in 1991	N	Y	Y
Control variables in 1993	N	Y	Y
Province time dummies			
F test for treatment × {time 0, post1, post2+}	2.18** (0.084)	2.74** 0.038	
Observations	387	376	411
R-squared	0.099	0.225	0.188
Number of villages	56	54	59

Notes:1. Determinant variables in 1991 include average income per capita, log population, being classified as disadvantaged areas, being classified as poor village, distance to the nearest county or municipal (or prefecture) government, being in mountains areas;
2. Other control variables in 1993 include sex ratio, ratio of non-labor population, no. of firms in the village, newspaper per household, and TV sets per household.
3. In Column [3], the population size in the 1991 is used as the weight variable.
4. Standard errors in parentheses are clustered at village level.   *** $p<0.01$, ** $p<0.05$.

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