Do Call Centers Promote School Enrollment? Evidence from India

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Abstract

Globalization has changed job opportunities in much of the developing world. In India, outsourcing has created a new class of high-skill jobs which have increased overall returns to schooling. Existing evidence suggests education may broadly respond to this change. We use microdata to evaluate the impact of these jobs on local school enrollment in areas outside of major IT centers. We merge panel data on school enrollment from a comprehensive school-level administrative dataset with detailed data on Information Technology Enabled Services (ITES) center location and founding dates. Using school fixed effects, we find that introducing a new ITES center causes a 4% to 7% increase in the number of children enrolled in primary school; this effect is localized to within a few kilometers. We show the effect is driven by English-language schools, consistent with the claim that the impacts are due to changes in returns to schooling, and is not driven by changes in population or income resulting from the ITES center. Supplementary survey evidence suggests that the localization of the effects is driven by limited information diffusion.

1 Introduction

In the last thirty years, globalization has dramatically changed job opportunities in the developing world. In many countries this change has increased the skill premium. In India, the focus of this paper, this change has been particularly striking. The number of individuals employed in outsourcing-related businesses has increased from roughly 50,000 in 1991 to over 2 million in 2010 (NASSCOM, 2010); these jobs demand employees with high levels of education and a good command of English, and pay high salaries by Indian standards. The availability of these new opportunities increases the return to education which may, in turn, increase school enrollment. Popular media

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suggests the availability of jobs of this type may have broad geographic impacts in India, including impacts outside of areas where these new jobs locate (Giridharadas, 2010). Understanding the magnitude of this change, and how widespread the impacts are, is important for understanding the consequences of globalization.[1]

Existing evidence on India suggests that cities and districts with a major IT presence have experienced changes in education patterns with the growth in these jobs. In this vein, Munshi and Rosenzweig (2006) demonstrate evidence of increased returns to English in Mumbai between 1980 and 2000, and simultaneous increases in English-language enrollment among groups best able to take advantage of new job opportunities. Shastry (2010) shows evidence that districts with greater IT growth over the 1990s have greater schooling growth following this period.

This paper makes two significant contributions to the evidence on globalization's impact on school enrollment. First, we estimate the effect of the introduction of these businesses outside of the major IT areas. This allows us to evaluate the validity of the popular claim that these businesses will have broad geographic impacts in all of India, and consider the question of whether these changes will ameliorate or exacerbate inequality across areas.[2] Our data is sufficient to allow us to distinguish the magnitude of impacts over quite small distances, and we argue we are able to make strong causal statements about the impact of call centers. As we detail below, we find that the impacts of call centers on school enrollment are large but very localized.

Second, with a more qualitative survey we are able to provide some preliminary evidence on the mechanisms behind these effects and their relatively narrow geographic range. We argue this effect is due to limited information dissemination across areas.[3] This suggests that in the absence of any intervention impacts may *not* be geographically broad, although better information provision about job opportunities could have large impacts.

The paper proceeds in two parts. We first use panel data on school enrollment and Information Technology Enabled Services (ITES) firms[4] to estimate the impact of new businesses on school enrollment, both overall and in English-language schools specifically. This estimation is done within school, using the sharp timing of ITES center introduction, and we argue the impacts we observe can

[1] This question echoes a very large existing literature on the returns to education and school enrollment in both the developing and developed world (e.g. Freeman, 1976; Katz and Murphy, 1992; Heckman, 1993; Kane, 1994; Foster and Rosenzweig, 1996; Griliches, 1997).

[2] This is related to broader issues of the impact of trade on inequality (i.e. Goldberg and Pavnik, 2007).

[3] This is consistent with Jensen (2010a) and Jensen (2010b) both of which suggest interventions which provide better information on job opportunities (in the former case, in a very similar setting) change schooling decisions.

[4] This is a class of business which includes call centers, as well as data processing, medical imaging and related services.

be interpreted causally. The second part of the paper uses data from a survey in one area which provides GPS data on the location of households and ITES centers and detailed data on the quality of information about these jobs. This allows us to estimate how information decays as people move further from the call center locations. Although it is more difficult to make causal claims in this case, we will be able to focus our estimation on people within a very small area (within 1 kilometer of a call center) which limits possible confounds.

We begin with panel data on enrollment at the school level from a comprehensive administrative dataset in three states in India (Karnataka, Andhra Pradesh and Tamil Nadu); each school is observed for a period of four to eight years between 2001 and 2008. We combine this with a newly collected dataset on ITES business locations and founding dates. Our ITES center data includes areas outside of Chennai, Hyderabad and Bangalore, which allows us to estimate the impact of jobs in areas which have not had an overwhelming IT presence. Our ITES center location data allows us to identify the PIN code (similar to a ZIP code) location of each center, which we can link to school location. We use a school fixed effects estimator to analyze how enrollment changes *within an individual school* upon the introduction of a new ITES center to the area.

We estimate the impact of ITES center introduction on schools in the same PIN code and find strong (and immediate) positive effects: the introduction of one additional ITES center to the PIN code is associated with between a 4% and 7% increase in number of children enrolled in the school in the year after the center introduction.[5] In addition to school fixed effects, we control for several time-varying school infrastructure controls and year fixed effects interacted with state dummies and area demographics. Our preferred specification is one in which we limit to areas most comparable to the areas with ITES centers: areas with English-language schools. This specification yields a coefficient of 7.0%. Our effects are robust to controlling for district specific trends and to limiting to areas which ever have ITES centers.

We next estimate the geographic range of these impacts, and find they are extremely localized. ITES centers in the two closest neighboring PIN codes (an average of 3.2 kilometers away) have a positive impact on school enrollment, but it is smaller and less significant than the own-PIN code effects. ITES centers in the 3rd to 5th closest neighboring PIN codes (an average of 5.8 kilometers away) have no significant impact.

An issue with interpreting these results causally is the possibility that the introduction of ITES

[5]This effect is driven in large part by older children, which is consistent with the fast impact.

centers anticipates increased school enrollment rather than causing it. The inclusion of school fixed effects in our specification addresses the concern that ITES center introduction is associated with some fixed area characteristic, but they do not address the concern that ITES centers might be introduced to areas which are *changing* more rapidly.[6] There are two specific concerns: (1) ITES center operators may be targeting areas where school enrollment is increasing, since those areas will produce more potential workers in the future and (2) there maybe changes in unobservables which drive both ITES center introduction and increased school enrollment.

To address both issues, we analyze the impact of ITES centers introduced in future years on current enrollment.[7] If ITES center operators are targeting areas which have more rapidly increasing schooling, future ITES centers should also correlate with changes in schooling. Similarly, if other variables are changing continuously and driving both variables, then future ITES centers should correlate with current changes. We do not find evidence for an impact of future ITES centers. The inclusion of the future ITES center measure does not affect our estimate of the impact of current ITES centers and future ITES centers are never a significant predictor of school enrollment. As a further test, we show that there are no trends in enrollment in years leading up to ITES center introduction.

We next explore whether these impacts vary by language of instruction. These jobs almost universally require knowledge of English in addition to high levels of education. Consistent with this, we find that enrollment in English-language schools increases by about 15% with the introduction of each ITES center, whereas there is no change for local-language schools. Further, we argue the impact for English-language schools should be particularly strong when the ITES center that is introduced is a call center (as opposed to data processing), and we find support for this in the data. Again, these results are if anything stronger when we limit the sample to areas which are more comparable to the areas which have ITES centers introduced.

These results point to a causal impact of ITES centers on school enrollment. In terms of magnitude, the results suggest that introducing an ITES center (median size of 80 employees) increases enrollment in the PIN code by 280 children. A lingering concern, given that our outcome

[6]We should note that we have no reason to think this type of endogenous placement is common. Conversations with ITES center operators suggested they choose where to locate primarily based on the level of infrastructure and the quality of possible employees, but there was no mention of locating based on anticipated increases in schooling or previous years schooling increases.

[7]This methodology has been used elsewhere to test for similar concerns (Jensen and Oster, 2009; La Ferrara, Chong and Duryea, 2009).

variable is number of children enrolled, is that this reflects changes in *population* which result from the ITES center opening.[8] We argue this is unlikely to be due to ITES center employees themselves: our ITES centers are small and only a very small share of employees either have children or migrate to work there. It is also possible that when people get jobs at ITES centers, they employ other individuals (e.g. drivers, maids) who migrate in. We provide a calibration and several pieces of direct evidence that this is not driving our results. Most concretely, the effects we observe do not scale with number of ITES center employees, which would be expected if they were driven by population changes. Two smaller concerns are that our results are driven by changes in the number of schools or by changes in income. We find no changes in number of schools, and argue that only a small fraction of our enrollment increase could be explained by income changes.

We therefore interpret our results as reflecting a reaction to changes in returns to schooling. In Section 6 we explore the mechanism by which this occurs. We distinguish two possibilities. First, the introduction of an ITES center may impact actual returns to schooling in the local area by providing new jobs at that center; this can explain our results only if labor markets are very localized. Alternatively, it may impact perceived returns to schooling by providing better information about these jobs in general; this can explain our results only if information is very localized.

To distinguish between these possible mechanisms, we conducted a survey in one district in Tamil Nadu (Madurai), which allows us to observe (a) the localization of the labor market and (b) how widely information diffuses. We find evidence in favor of the information story. Our data indicate that people do travel several kilometers for work, which suggests the narrow geographic range of ITES center impacts is not due to localization of labor markets. In contrast, knowledge is very localized. Even limiting the sample to individuals who live *within one kilometer* of an ITES center we find that those who live closer are more likely to report they know of a center in the local area and to correctly identify what qualifications are required for the job. Although less well identified, this provides suggestive evidence that the localized impacts we observe reflect slow information diffusion.

The findings in this paper relate to a large literature on what policies are effective in increasing school enrollment in the developing world (e.g. Duflo, 2001; Kremer, 2003; Kremer et al., 2005; Duflo, Hanna and Ryan, 2009; Burde and Linden, 2009). We are most closely related to two recent

[8] This would be a significant issue if, for example, we were talking about the opening of a major manufacturing plant in a small village.

papers using data from India. Shastry (2010) shows evidence that districts with greater IT growth in the 1990s have greater education growth in the 2000s. She uses a language-based instrument to predict where IT centers locate.[9] Relative to that paper, our more precise data on location and timing of ITES center entry and school enrollment allows us to say more about the mechanisms behind these effects and their geographic scope. This geographic specificity is also helpful in establishing causality. Jensen (2010b) shows that (randomly) providing call center jobs to women outside of Delhi improves conditions (including schooling) for girls. This paper is very complementary to ours and is consistent with the information mechanism that we argue for. However, we are able to evaluate how these impacts play out the market, without any direct intervention, which is valuable in evaluating the real-world impacts that we expect to see empirically.

In terms of policy, the results here suggest that the very presence of job opportunities may be enough to prompt changes in local area schooling, while supporting the claim that policies which provide better information may be effective in promoting school enrollment in areas further from new job options.[10] Although this paper focuses on the case of India, the results may well have implications for other countries. In the broadest sense, they suggest that poor understanding of job opportunities is a potentially important factor in limiting school enrollment in the developing world.

The rest of the paper is organized as follows. Section 2 provides some background on ITES centers, and describes the data. Section 3 describes our empirical strategy. Section 4 shows the central results of the paper, and Section 5 discusses robustness. Section 6 presents our survey data on and Section 7 concludes.

2 Background and Data

2.1 Background on ITES Centers

Although the concept of "outsourcing" business processes to low-wage countries has been around since the 1970s, the industry remained small until the late 1990s, as time and cost

[9] The instrument is non-linear: people who live in areas in which Hindi is the native language are also more likely to learn English, prompting more IT centers. However, among those who live in areas in which the native language is other than Hindi, people whose native language is *further* from Hindi are also more likely to learn English, also prompting more IT centers.

[10] An caveat to this policy argument is that our results hinge on the fact that jobs in ITES centers require additional education; Atkin (2009) finds that growth in the export sector in Mexico actually leads to school *dropout* since export jobs pay well but do not require schooling.

restrictions were large. With the investment in trans-oceanic fiber-optic cables however, the costs of ITES off-shoring plummeted, and with its relatively educated English-speaking low-wage population, India emerged as the dominant provider of business services ranging from call centers to software development.

ITES center jobs are typically high-paying by Indian standards. The average starting salary at such firms is roughly 8,000 Rupees per month (about US$175), which is almost double the average per capita income of India (Ng and Mitter, 2005). These firms typically come in two types: (1) multinational corporations with subsidiaries or divisions located in India, and (2) Indian "third-party" firms that provide ITES centers and other services for Western companies. Jobs at the Indian firms tend to have lower wages, higher turnover, and less training than the "in-house" multinational corporation positions (Dossani and Kenney, 2004). The majority of ITES centers are in larger cities such as Bangalore, Delhi, and Mumbai, but they are spreading rapidly to smaller cities all over southern India.

Many of these firms are call centers, which focus on direct telephone interaction with Western customers. Workers make outgoing calls (for services like telemarketing), and take incoming calls (for customer service, tech support, and credit card activation, among other things) for large Western companies. At these centers, "voice" workers conduct calls almost entirely in English, primarily to the United States; thus, fluency is generally a requirement for entry-level positions.[11] Other, "non-voice" business processes outsourced to such firms range greatly in their skill-level, from data entry to software design. English proficiency may not be required for these jobs, though in our survey almost all non-voice centers reported that English was required.

From the perspective of this paper, there are at least two central features of ITES centers which we want to highlight. First, they require relatively high rates of education and pay high wages. To the extent that jobs of this type have not been available historically, their existence may well affect the returns to education (both perceived and actual). Second, the vast majority of these jobs require English skills, which is likely to affect the wage returns to learning English.

2.2 Data on School Enrollment

We use a large administrative dataset on primary school enrollment in India called the District Information System for Education (DISE). This dataset has been collected by the Indian government

[11] Indeed, many of these firms go to great lengths to train their workers to speak with American and British regional dialects, even adopting pseudonyms and memorizing idioms. Some workers report having to watch hours of American television programs to help perfect their speech patterns (Ng and Mitter, 2005).

since the late 1990s, although the data used in this paper begins in the early 2000s. Data collection is coordinated at the district level and involves surveys of schools. These school surveys have several parts. First, they collect data on primary school enrollment, including comprehensive data on number of enrolled students by age, grade, gender and caste. These data are designed to reflect statistics as of September 30th of the school year (which starts in the spring). Second, they collect data on features of the school, including language of instruction and physical plant characteristics. Each school is given a unique ID number, which allows us to follow schools over time.

The area-level survey is less comprehensive and less frequent, but includes some information on village or urban neighborhood characteristics. Most importantly, for most areas in this survey we observe the PIN code location of the school, which allows us to match the area to ITES center locations. A PIN code is similar to a ZIP code in the US; it is smaller than a census block.

The DISE data is collected by the district and then aggregated by each state government. We use data from three states that have been significantly impacted by globalization: Karnataka, Andhra Pradesh and Tamil Nadu.[12] The number of years of data varies across states. Panel A of Table 1 shows, for each state, the years of data coverage and the number of schools by year. In later years the dataset is more comprehensive, covering a larger share of schools; in particular, in two of the three states the first year of data collection was much less comprehensive. Although this means we do not have a balanced panel, by including school fixed effects we ensure that we compare the same schools over time.

Panel B of Table 1 provides some summary statistics on school enrollment and school characteristics. The average school in our sample is fairly small, with 144 students. This survey covers only primary schools, so we observe enrollment only up to eighth grade. The physical plant variables indicate schools are not in very good repair. In an average school, only 70% of classrooms are noted to be in good condition by surveyors. Half of the schools report having a boundary wall, half report having electricity and slightly above half have a toilet. Eleven percent of the sample reports at least some instruction in English (this is based on a question about what languages the school teaches in; they could list as many as they wanted).

This data has several limitations. First, as noted, the coverage of our sample differs somewhat across years. In general, the school fixed effects mean this is not a major issue. The one note of

[12]These three are also states in which we have a relatively long time series of data. Although there are of course many more areas of India, we argue these areas should be representative of areas most heavily impacted by these jobs.

caution is that if the schools we observe are different than the schools we do not observe, our results may have limited generalizablity. This is unlikely to be a serious issue, however, since our best estimates suggest we cover nearly all schools in India.[13]

A more important issue is that the data measures total number of children enrolled, not enrollment rates. This leads to the concern that our results reflect changes in population. We discuss this issue in greater detail in Section 5. For a small subset of school years the school also reported the total population of school-aged children in the area. The coverage of these data is limited, but in a robustness check we will use these data and the variable is summarized in Panel B of Table 1.

Finally, as noted, the DISE data covers only primary schools. It seems plausible, even likely, that much of the impact of ITES centers would be on enrollment in secondary school, since secondary school education is typically necessary for these jobs, and enrollment at that level is lower in general. Unfortunately, we cannot observe these enrollments; if anything, this may lead us to understate our impacts.

2.3 Data on ITES Centers

To match with the data on education, we collected data on ITES centers. We contracted with a firm in India that helps connect Western firms with Indian ITES centers to create a directory of ITES centers in Andhra Pradesh, Karnataka, and Tamil Nadu. They used their contacts, the Internet, and available directories to compile a list of firms, and called each to confirm their existence, the PIN code of their location and their founding date. Our data collection project focused on areas outside of Bangalore, Chennai and Hyderabad, although we did collect some information on centers there. This focus was in line with our desire to estimate the impacts of these firms outside of major IT centers.

This data collection project resulted in a dataset of 401 ITES centers. Figure 1 shows a histogram of ITES center founding dates; the incredible growth in number of centers over time is clear: in our sample, 68% are founded after the year 2000. As we note above, our data on schooling is collected in September, for the year spanning June to April and the ITES center founding dates are given as simply the calendar year of founding. We code the school year 2005-2006 as 2005, and match with ITES centers this way. A school in a PIN code with an ITES center introduced in 2005 is

[13]This is actually a somewhat difficult fact to measure. Official statistics on number of schools in India appear to be largely based on the same data we use here so there is no outside source that we can use to verify coverage. The fact that the Indian government uses this as the source of official statistics, however, gives us confidence that we are covering at least an extremely large share of total schools.

coded as having a new ITES center in the 2005-2006 school year.

The breakdown of number of ITES centers by state is presented in Panel A of Table 2. In Column 1 we show the count of all ITES centers; Andhra Pradesh is slightly less well-represented, but the number of ITES centers is fairly similar across states. In Column 2 we report these counts for areas outside the major cities of Bangalore, Chennai and Hyderabad (this is the sample we use for analysis). As expected, this limits the sample somewhat and we are left with 260 ITES centers.

We aggregate these ITES centers to the PIN code level. The first column in Panel B of Table 2 reports data on number of ITES centers by PIN code. The vast majority (97%) of PIN codes do not have any ITES centers; among those with at least one ITES center, the average is 2.6. Column 2 in Panel B shows these statistics with the data restricted to the sample we use (areas outside of the three major cities). This sample is similar, and the conditional mean is 2.5 ITES centers.

Panel C of Table 2 gives a better sense of the source of identification we use by showing three categories of schools. Our sample contains roughly 239,000 schools which are in PIN codes which never have ITES centers (or at least not ITES centers we observe). A further 172 schools are in PIN codes which have ITES centers, but do not add ITES centers during the survey period. Finally, we have 408 schools in PIN codes where the number of ITES centers changes over the course of the study. Given that our specifications will include school fixed effects, we are identifying off of these final 408 schools. In some specifications we will limit the comparison group to areas which are more comparable to those which have ITES centers.

In addition to this basic information on ITES center locations and founding dates, we undertook a follow-up survey of the centers in our sample. Although we attempted to survey all centers, in the end we were able to collect data on 83% (the remaining were missed largely due to refusal to answer survey questions). For these centers we have data on whether or not they are voice centers, whether they operate in English, the number of employees and several employee characteristics. Information on number of employees and whether they are voice or non-voice is available for all the centers we surveyed; demographic information is available for a subset.

The variables are summarized in Panel D of Table 2 (this summary data focuses on the centers we use in our identification, those outside the three large cities). The ITES centers are relatively small, with a median of 80 employees, and about half of them have voice operations. All ITES centers which handle voice calls operate at least in part in English. Employees are young (median age of 28), largely without children and mostly from the local area.

As a final note, in addition to ITES centers within the same PIN code as the school, we use two variables measuring slightly further centers: those in the two closest PIN codes, and those in the 3rd to 5th closest PIN codes. To calculate distance, we use GPS data on PIN code locations (the latitude and longitude are measured at the post office in each PIN code). We count the number of ITES centers in each of the two neighboring groups. The two closest neighboring PIN codes are an average of 3.2 kilometers away, and 1,117 schools ever have an ITES center in one of these closest neighbors. The further neighbors are an average of 5.8 kilometers away, and 1,935 schools ever have an ITES center in one of these areas.

2.4 Placement of ITES Centers

A central issue in our analysis is the fact that ITES centers are not placed randomly. Our analysis will take advantage of variation over time, so any fixed differences across areas will be adjusted for, but it remains important to understand what drives placement.

We undertake two strategies. First, we can get an initial sense of the magnitude of this threat based on discussion with ITES center operators about location choices. The primary issues they cited when deciding where to locate were infrastructure and transportation: areas with no electricity and roads were not appealing places to operate. In addition, center operators cited their need to get high quality employees cheaply in the local area. There was some sense of a trade-off: there are more qualified individuals in larger cities, but people outside these areas demand lower wages. These discussions certainly do not suggest that ITES centers are placed randomly; it is clear that center operators are thinking carefully about cost-benefit considerations. However, the central demographics discussed are very likely to be constant over time, at least over the short time frame of our study.

We are also able to evaluate this endogenous placement statistically using our data. To do this we estimate, at the neighborhood level: (a) the determinants of having an ITES center by the end of the sample in 2007 and (b) the determinants of adding an ITES center during the period we observe. We focus on variables cited by ITES center operators: whether the area has electricity, whether it is in a more urban area and whether there is an English-language school in the area. This last variable is intended to capture the availability of English-speaking individuals. We also include a control for total school enrollment and, in some cases, state fixed effects.

The results from these regressions are shown in Table 3. In general, the results support the interview evidence. More urban areas are more likely to have centers by 2007 and more likely to add

them during the sample; these effects hold with and without state fixed effects. Areas with English-language schools are also more likely to have centers and more likely to add them during the sample; again, these results are robust to state fixed effects. We see limited evidence that electricity matters, although this may be due to the high correlation with urbanization; enrollment also does not seem to have any impact.

The inclusion of school fixed effects means that any differences in levels of enrollment associated with these variables will not impact our results. However, if there are differential trends in enrollment across villages associated with these variables, this could impact our results. To address this, in the results below we will allow for separate year fixed effects for areas that are more urbanized and areas with any English-language schools; this is discussed in more detail below.[14]

3 Empirical Strategy

We estimate the impact of ITES centers on school enrollment using a fixed effects estimator. We observe enrollment in school i in PIN code j at year t; denote this variable n_{ijt}. In addition, we observe number of ITES centers in PIN code j at year t, which we denote c_{jt}. Our basic regression is shown in Equation (1) below

$$n_{ijt} = \alpha + \beta_1 c_{jt} + \gamma_i + \phi_t + \Psi X_{ijt} + \epsilon_{ijt} \tag{1}$$

where γ_i is a vector of school fixed effects and ϕ_t is a vector of date controls. These date controls include year fixed effects, and year fixed effects interacted with state fixed effects, village-level electricity, urbanization and controls for the number of English-language schools. Thus, we allow the year fixed effects to differ by state and by the variables that drive ITES center placement in Table 3. In addition to these fixed effects, we include a set of school-specific time-varying controls (X_{ijt}) measuring school-level infrastructure. The coefficient of interest is β_1, which captures the effect of ITES centers on school enrollment. This coefficient is identified off of schools in areas which add ITES centers during the sample. Throughout the analysis, we cluster our standard errors at the neighborhood level.[15] We will also estimate this overall regression including district-specific time

[14] We do not include separate trends in electricity or initial enrollment level since these do not impact placement; consistent with this lack of impact on placement, including these does not change our results.

[15] We choose to cluster at the neighborhood/village level (rather than at the school) since c_{jt} is the same for all schools within a village-year. In fact, the level of clustering makes relatively little difference – even clustering at the district level gives very similar standard errors. We should note that when we include district-specific trends in the regression we are

trends.

A concern with estimating this equation on all areas is that our impacts might be identified off of rural areas which are not at risk of having ITES centers, and these may not be appropriate comparisons for those areas which get ITES centers. Given that, we will estimate, and focus on, a specification in which we limit the sample to areas which have at least one English-language school; these are the areas most "at-risk" for getting ITES centers.[16] As a further robustness check, we will also limit to areas which ever have ITES centers during the sample period. Although our primary results use fixed effects, in a robustness check we will show our central estimates in first differences.

As noted in the introduction, we are concerned about the possibility that the results are driven by other variables which are changing over time and influence both ITES centers and school enrollment. A related issue is the possibility that ITES center operators consciously introduce centers in places where school enrollment is increasing. To address both of these issues, we estimate whether *future* ITES centers predict current enrollment using Equation (2) below.

$$n_{ijt} = \alpha + \beta_1 c_{jt} + \beta_2 c_{j,t+1} + \gamma_i + \phi_t + \Psi X_{ijt} + \epsilon_{ijt} \tag{2}$$

$c_{j,t+1}$ is a variable measuring number of ITES centers in PIN code j in year $t+1$. If $\beta_2 > 0$ this would indicate that areas which get ITES centers next year have higher enrollment in this year, relative to their previous enrollment. This would point to ITES centers being introduced in areas which are growing faster. In contrast, a finding that $\beta_2 = 0$ indicates that ITES centers are not introduced into areas in which school enrollment is growing for other reasons. This technique has been used elsewhere to address this concern (Jensen and Oster, 2009; LaFerrara, Chong and Duryea, 2009). We also estimate Equation (2) including a trend for years until a new ITES center is introduced. This allows us to look slightly more generally at whether enrollments are increasing in years up to a new ITES center introduction. It is important to note that this technique does not allow us to rule out the possibility that ITES centers are introduced at exactly the same time as another innovation, and that the other innovation drives school enrollment. However, this possibility seems more remote.[17]

not able to cluster at all given the large number of controls. This means the standard errors are likely biased downward in those regressions, although since the clustering does not make a large difference in general, it seems unlikely this bias is large.

[16] This is our strongest predictor of having an ITES center. Virtually all of our ITES centers are located in PIN codes which have at least one English-language school.

[17] We also cannot rule out the concern that ITES center operators are targeting areas which seems like they would have large enrollment *responses* to these centers. Under this theory, our results would be valid within sample but would overstate out-of-sample effects. This would require, however, that call center operators are choosing locations based

One important issue is the coverage of our ITES center dataset. Although we worked to cover as many ITES centers as possible, it seems extremely unlikely that coverage is perfect. There are very likely areas that have ITES centers that we do not observe. This means that our "control" group of non-changers also contains some schools that should be in the "treatment" group. To the extent that there is a positive effect of ITES centers on school enrollment, this imperfect coverage should bias our estimates of β_1 downward, since the changes in the control group will be more biased upward by the inclusion of "treatment" schools.

4 Results: Impact of ITES Centers on School Enrollment

This section presents our estimates of the impact of ITES centers on enrollment.

4.1 Baseline Results

We begin by showing the central result in the paper in graphical form, in Figure 2. To generate this figure, we focus on four groups of schools: (1) schools that always have an ITES center in their PIN code, (2) schools that add a center between the 2004-05 and 2005-06 school years, (3) schools that add a center between the 2005-06 and 2006-07 school years and (4) schools that never have any ITES centers.[18] For all four groups we isolate a balanced panel of schools which are observed for four years (2004-2007). Using this sample of schools, we regress log enrollment on year fixed effects and take the residuals; this removes any consistent year-by-year variation. These residuals are graphed in Figure 2, which show changes in these residuals relative to the level in 2004.

The key result in Figure 2 is that there are large year-on-year changes in enrollment in the two groups that add ITES centers during the sample, and these changes line up in terms of timing with the ITES center addition. In areas that add a center between 2004 and 2005, schools see a large increase in enrollment between these years, whereas there is only a small increase in schools that always have centers, and no change for schools that add centers later or never add them.[19] Further, for areas that add an ITES center between 2005 and 2006 there is a large increase in enrollment

the elasticity of primary school with respect to future returns. This seems extremely unlikely, given the difficulty of measuring these parameters and the insignificant effect they would have on short- and medium-run outcomes for the firms. Our conversations with ITES center operators also gave no indication of this type of consideration in placement.

[18] The two groups of changer schools here (add center in 2005 and add center in 2006) cover the vast majority of schools which add centers.

[19] As shown in Table 2, most schools never have any ITES centers. For this reason, the year fixed effects are largely identified off of these areas, so when we generate residuals removing these fixed effects, the average residuals in these area are very close to zero.

between these years, but no change in the year before. This is the only group with a large increase between 2005 and 2006. Overall, the figure demonstrates large changes in enrollment which correspond to ITES center introductions.

Panel A of Table 4 shows our statistical estimates of the effect of ITES centers on enrollment. Column 1 presents our results using the entire sample. The coefficient on ITES centers is positive and significant: adding one more ITES center increases school enrollment by 5.7%. Column 2 shows this regression with district-specific trends included, to address the concern that districts that have ITES centers introduced are trending differently than those that do not. The coefficient is slightly smaller (4.3%) but still highly significant. Our preferred estimate appears in Column 3, in which we limit to areas with at least one English-language school, which means our non-changer areas are most comparable to the areas which get ITES centers. Although this restricts the sample significantly, the coefficient is larger (7.0%) and highly significant. Finally, Column 4 limits further to areas that ever have an ITES center that we observe (including those that change and those that always have a center). The coefficient is again even larger and significant, despite the extreme sample size restriction.[20]

In Panel B of Table 4 we estimate the same regressions, but instead of estimating the impact of number of ITES centers, we focus on a dummy for whether the area has any centers. The impacts are of similar size but less precise. The lower precision may be due to having more limited scope for identification with the more limited number of changes. We should note that this suggests that the results we observe do not arise only from the first ITES center introduction.

The results in Panels A and B focus on the impact of ITES centers introduced into the same PIN code as the school. In Panel C of Table 4 we explore whether the introduction of ITES centers in the slightly broader surroundings matter. As described, we do this by estimating the impact of ITES centers in the two closest neighboring PIN codes, and slightly further neighbors (the 3rd to 5th closest). Panel C demonstrates that there are some impacts for ITES centers in the nearest neighbors. Focusing on our preferred specification in Column 3, we find one more ITES center in one of the closest neighboring PIN codes results in a 4.8% increase in enrollment, just slightly smaller than the impacts in the own-PIN code. However, the effects fall off very quickly. ITES centers in the slightly more distant neighbors have no significant impact; this is true even though these "distant" neighbors are still quite close, at an average of 5.8 kilometers away. This suggests effects are

[20] Appendix Table 1 shows the regressions in this table run in first differences; the results are extremely similar.

extremely localized.

The evidence in Table 4 suggests a strong connection between ITES centers and total number of children in school. In Appendix Table 2 we show these effects broken down by demographic group. We find the effects are similar for girls and boys.[21] They are larger for older than younger children, which suggests that much of the impact may be due to children staying in school rather than newly enrolling at the youngest ages. This is consistent with the immediecy of the impacts we observe, which seem more plausible if they reflect lack of dropout.

Future ITES Centers

The central threat to the validity of our estimates is the possibility that ITES center introduction anticipates schooling increases rather than causing them. This is related to the issue of endogenous ITES center placement. As discussed above, to the extent that endogenous placement reflects only characteristics which are constant over time this will not drive our results since we include school fixed effects. Further, if trends are different for areas which are urban, or have more English-language schools, we have also addressed this issue. The concern which remains unaddressed in our main specification is the possibility that ITES centers are located in areas that are changing in other ways that we do not observe. There are at least two specific concerns. One is that ITES centers are placed in areas where schooling is increasing more quickly, since center operators are targeting a future labor force (given that our estimates are for primary schools, this would be a fairly distant future). A second concern is that some other unobserved variable ("modernity", for example) drives both ITES center introduction and school enrollment.

To address this concern directly we estimate whether future ITES center placement predicts current enrollment. If it does, this would suggest ITES center introduction anticipates changes in schooling, which is our concern. Panel A of Table 5 replicates Panel A of Table 4, but includes a control for the number of ITES centers in the following year in addition to the indicator for current ITES centers. Adding the control for future ITES centers has only a small impact on our estimates of the effect of current ITES centers. In addition, and more importantly, the effect of future ITES centers is small and not statistically precise, suggesting no strong evidence of pre-trends. We should note, however, that we cannot reject equality between the coefficients.

[21] The fact that the impact for boys and girls is similar may seem puzzling, given the focus on the female nature of this work. In fact, in the ITES centers in our data, slightly less than half of the employees are women, which may explain the similar impact.

In Panel B of Table 5 we do a similar test, but rather than simply controlling for having an ITES center next year, we control for a time trend up to the year of ITES center introduction (the trend is defined so higher values indicate the center introduction is closer in time). If ITES centers are introduced into places where enrollment is increasing more quickly, we should see evidence of a positive trend. We do not see this. In our preferred specification in Column 3, the coefficient on the trend is -0.004, indicating a 0.4% decrease in enrollment for each year closer to the introduction of an ITES center. This is not significant, and is tiny relative to the estimated impact of introducing an ITES center in the current year. The trend is similarly insignificant and small in other columns. It is important to note that the results here do not indicate that ITES center placement is exogenous, but instead indicate that this endogenous placement does not drive our results.

4.2 Impacts of ITES Centers by Language of Instruction

The evidence above suggests that overall school enrollment increases in response to ITES center introduction. Here, we turn to separating the result by language of instruction. One of the central features of ITES centers in India is that the vast majority operate in English. In our survey, all of the voice ITES centers (which make up about half of our sample) use English; the majority of non-voice centers also require English. Given this, to the extent that what we observe reflects changes in schooling in response to job opportunities, these changes should disproportionally result in higher English-language school enrollment.

We separate our effects by language of instruction in Table 6. We generate new variables interacting the number of ITES centers with language of instruction and control separately for the impact of ITES centers on local language schools and on English-language schools.[22] Panel A of Table 6 shows our basic test of differences across school types. Column 1 reports impacts on total enrollment using the entire sample. We find the total impact of ITES centers in English-language schools is large and significant; the impact of ITES centers in local-language schools is effectively zero. In our preferred specification (Column 3), we find that enrollment in English-language schools increases by 14.9% for each ITES center introduced. The p-values reported at the bottom of the table indicate we can strongly reject the equality of the impacts in the two school types. One thing which is important to note is that we *do not* see decreases in enrollment in local language schools. The increase in enrollment in English-language schools does not appear to come at the expense of

[22]The two variables are mutually exclusive; each coefficient can be interpreted as the effect for that school type.

enrollment in local-language schools.

In Panel B of Table 6 we push the data on language further, and separate schools into three groups: those that do not teach in English at all, those that teach some in English and some in another local language and those that teach only in English. Consistent with the larger impact for English-language schools overall, we find the effects are largest for schools that teach exclusively in English. However, the difference between these and those that teach partially in English are small. The largest distinction appears to be between schools that teach at least some English and those that teach none.

We can further explore whether the enrollment changes seem to reflect response to the ITES center jobs by using the distinction between voice centers (i.e. call centers) and non-voice centers (i.e. data processing) in our sample. Speaking English well is particularly valuable for jobs in voice centers. Although most non-voice centers also require English, they are slightly less likely to do so and conversation with operators suggested that the necessary level of English proficiency is lower.

We generate new variables measuring the number of voice and non-voice ITES centers in each PIN code; we define a center as a "voice center" if at least 50% of employees handle voice calls.[23] We interact each of these new variables with school language. These regressions are reported in Panel C of Table 6. In all four columns, the coefficients tell a similar story. For non-voice centers, the effects on English-language schools are slightly larger, but the difference is small. Both types of schools see enrollment increases from non-voice centers. For voice centers, however, the impact for English-language schools is much larger than for local-language schools. We can typically reject that the difference in effects for English-language and local language schools is the same for voice and non-voice center introductions. Interestingly, in some specifications we see evidence of a reduction in enrollment in local-language schools when a voice ITES center is introduced; this could reflect students substituting between school types, which is not an effect we observe when we consider the impact of all ITES centers combined.

[23] As noted, we have voice/non-voice data for 83% of ITES centers. For the centers with data, we generate indicator for whether they are voice centers or non-voice centers. For the purpose of this analysis only, we exclude areas that have a center for which we do not observe the type. The results are similar if we assume they are one or the other.

5 Robustness: Changes in Population and Income

This section addresses several key robustness issues. In particular, we evaluate whether it is possible that our results are simply driven by mechanical changes in number of schools, population or income deriving from the ITES center introduction.

5.1 Changes in Population

A key downside of our data on education is that we observe number of students enrolled, not enrollment rates. This introduces the possibility our results could be driven by population increases. The controls thus far rule out the concern that ITES center are introduced to more populous areas or areas which are growing faster. However, if the ITES center itself increases population, this could produce our result. This would be a concern if we were, for example, considering the impact of introducing a large manufacturing plant to an isolated area. In this case, however, we argue this is unlikely to explain more than a very small fraction of the effect we observe.

To begin, it seems appropriate to calibrate the magnitude of our results in terms of the change in number of students. Focusing on on preferred specification in Column 3 of Tables 4 we find a 7.0% increase in enrollment after the introduction of an ITES center. Based on a median school size of 143, this is 11 students per school, which aggregates to about 280 students in the PIN code overall (if we include the impacts on the two closest PIN codes, this figure is over 600 students).

The first question is whether in-migration among the employees of the ITES center themselves could be driving this change. This is extremely unlikely. ITES centers tend to employ young, childless individuals. This can be seen in anthropological and ethnographic work on ITES centers in India (i.e. Ng and Mitter, 2005) and directly in our ITES center survey data. In the average center in our sample, managers reported than 10% of employees have children (see Table 2), so the potential increase in children in the area even if all employees were new to the area is small. Further, relocation for work in ITES centers is also relatively rare (12.2% of employees). Even if we assume *all* this relocation is by people with children we find an average of 5.6% employees with children relocate. At the median ITES center, which has 80 employees, this amounts to just 4.4 people with relocated children. In fact, this number is likely to be an upper bound; in reality, the individuals with children are generally the *least* likely to relocate. We argue that this calibration effectively rules out the possibility that migration by the ITES center employees themselves drives anything more

than a tiny fraction of our effect.

There remains a concern that the introduction of an ITES center may bring with it other service jobs, which could increase population.[24] This could mean other jobs in the ITES center itself (although this should be captured in our employment measure) or, more likely, jobs working for ITES center employees (e.g. drivers, maids, cooks). If people migrate into the towns for these jobs, this could result in population changes. The first argument against this is again calibration-based. As we note, the total student increase is 280, and the median ITES center employs 80 people, of whom about 12% migrate in. If we assume that relocation for work in support jobs is as frequent as for work in the ITES centers themselves[25], then even if there were three new support employees for each ITES center employee, those who migrated in would have to each bring nine children between the ages of 6 and 14 in order to explain our results. This is implausible (in fact, probably impossible). Put differently, if we assume that each ITES center worker hires two new servants, and 10% of those individuals migrate in with two school-aged children, this would explain about 15% of our effect, still very small despite the fact that these are very generous assumptions.

As a second, related, calibration, we note that migration in general in India is fairly limited. In other household survey data (the 1998 and 2005 National Family and Health Survey) we can estimate what share of school-age children report migrating to a new area within the last year. For 88% of clusters in the NFHS sample there are zero in-migrants in the last year among school-aged children.[26] Even the clusters at the 90th percentile on this measure still have only 3.5% of school-aged children who are in-migrants in the last year. This further bolsters the argument that population changes are unlikely to drive our results.

In addition, we show two analyses which test the population mechanism directly. First, in Appendix Table 3 we show, for the subset of areas for which the school reports total area (neighborhood, village) population, the impact of controlling for population on our results (these population data are recorded by the school, and vary across years).[27] We do not want to lean very

[24]Yet another, perhaps less plausible issue is that the introduction of an ITES center is associated with an overall increase in other types of businesses, which bring in more migrants. The evidence in Table 5 above limits this concern; for this to drive our results, it must be the case that these other businesses enter at exactly the same time. This is also largely addressed by our evidence following this paragraph.

[25]In fact, this is an overstatement: based on nationally representative data from India, migration is least likely among low-skill individuals and most likely among those with high skills.

[26]In the NFHS a survey cluster typically covers a single village or area within a town and includes a randomly selected subsample of individuals.

[27]Since there are multiple schools in each area, we cannot generate enrollment rates off of these data, since the population reported is an area-level population not simply the population relevant for that school. The fact that this is true should also be clear from the coefficient on population; it is much lower than one, which at least partially reflects

heavily on the evidence in these regressions since we observe population only for a small subset of the sample and it is unclear how the school estimated population. However, this table demonstrates that including a control for population in the regressions does not significantly impact our estimates. The coefficients are noisier, but this seems to be due to changes in sample: there is very little difference between Panel A (restricted sample only) and Panel B (restricted sample, with population control).

Second, we note that an important implication of any mechanical mechanism like migration is that larger ITES centers should have larger effects on enrollment. This need not be the case if the effects are driven by changes in information provided by the existence of the ITES center. In fact, we find that the impacts we observe **do not** scale with the size of the ITES center. In Table 7 we show our primary analyses but include in the regression a control for number of employees.[28] If enrollment effects were scaling with the size of the ITES center, the coefficient on employees should be positive, but it is negative and indistinguishable from zero in all specifications.

As a final argument, we note that if migration was driving the increase in school enrollment, we would expect to see similar gains across age groups in schooling, since it would just reflect the ages of the workers' children. As we discussed previously, and shown in in Appendix Table 2, increases are not homogeneous across age groups, but in fact are larger for older students, which is inconsistent with the population-driven story.

ITES Center Driven Changes in Number of Schools A related, but more minor, concern is that our results are driven by changes in the number of schools in the area. If the introduction of an ITES center causes a decrease in the number of schools then the remaining schools could see enrollment increases even if the total enrollment rate in the area remains constant. We evaluate this by estimating the impact of ITES center introduction on the count of schools in the village. Estimates are shown in Appendix Table 4. The results indicate that changes in school count are not a concern: the impact on number of schools is very small and not significant.

5.2 Changes in Income

A second concern with our results is the possibility is that ITES centers drive enrollment because they increase income and schooling is a normal good. We note that this seems unlikely, given the results in Table 7, which show no impact of number of employees on enrollment effects, since the

the fact that as the area population increases, not all of that increase goes to a given school.

[28] We collected this information for most, but not all, of our ITES centers as detailed in Section 2.

total income increase should be greater for larger ITES centers. Still, we take advantage of the fact that existing literature has provided estimates of the income elasticity of school enrollment in similar contexts to estimate the magnitude of predicted enrollment increase resulting from increased income from ITES centers.

This is done in Table 8. We begin by showing (in the top row) several estimates from existing literature of the income elasticity of school enrollment in the developing world (Alderman et al., 2001; Glick and Sahn, 2000; Glewwe and Jacoby, 2004; Orazem and King, 2007). In the second row, we show our estimate of the percentage increase in income generated by ITES centers; this is the same in all columns. Details of this calculation are in Appendix A. This increase is very small. Although ITES centers pay quite well, they simply do not employ a large number of people.

The first and second rows together allow us to calculate the expected change in enrollment given the change in income. The predicted percent increase in enrollment ranges from 0.14% to 0.72%. Our actual enrollment increase (Column 3 of Table 4) is 7.0%, an order of magnitude larger; even at the largest elasticity estimates in the literature, it is unlikely that much more than a tenth of the effect is explained by income increases.

Similar to the case of population, an auxiliary concern is that the ITES center brings other businesses, which also increase income. It is more difficult to rule this out than in the population case. However, that the fact that we do not see evidence of pre-trends suggests that these new businesses would need to arrive at exactly the same time as the ITES centers. In addition, given the very small share of the effect which is plausibly explained by ITES center income, in order for income overall to explain a larger share, these other businesses would need to swamp the ITES centers in their income contribution, which seems unlikely.

It is important to note that our argument in this section is *not* that migration and income changes play no role in our results. This seems implausible. However, our analyses here show that any reasonable estimation of these mechanical effects suggest they are very small and unlikely to explain more than a tiny faction of our effects.

6 Mechanisms: Localized Information versus Localized Returns

We draw several conclusions based on the results in Sections 4 and 5. The introduction of an ITES center to an area results in an increase in school enrollment and this increase is concentrated in

English-language schools. The observed increase does not appear to be driven by mechanical changes in the number of schools, population or income. Finally, these changes are very localized: ITES centers even slightly further away have little or no impact on enrollment. Based on these results, we argue that the effects we observe reflect responses to changes in the perceived returns to schooling after the introduction of new local job opportunities.

In this section we provide some initial evidence on the mechanisms that drive this effect. We distinguish two possibilities. First, the introduction of an ITES center may impact actual returns to schooling by providing new jobs at that center. Alternatively, it may impact perceived returns to schooling by providing better information about these jobs in general, even if the change in actual job opportunities is limited. This distinction is potentially important for thinking about policy implications. In this section we use a supplementary dataset which we collected in Madurai District (in Tamil Nadu) to provide some evidence on this question.

To fix ideas, consider the simplest model of schooling decision-making in a context with no information frictions. Assume there are two locations, A and B, both of which begin with no ITES centers and otherwise identical job opportunities and education costs. Assume education is a binary choice which carries some positive wage returns. At some date, an ITES center is introduced into area A and (because we are assuming information is shared fully) it is immediately observable to individuals in both areas. The existence of this center increases the wage returns to education while education costs remain the same.

For individuals in area A, the value of education increases by the full amount of the increased wage returns. For individuals in area B, however, the increase is less because to take advantage of the new jobs, they would need to migrate to area A. Assuming the cost of migration is positive, the reaction of individuals in area B to the ITES center should be smaller than in area A; how much smaller depends on migration costs. Note that these migration costs could be the cost of moving to live in a new area, or the cost of travel to work in that area.

Now consider adding information frictions so the information about the increased returns diffuse only partially (or not at all) between areas A and B. In this case, the response in area B will be less than in area A *even if costs of migration are small*; how much less will depend on how limited information diffusion is. This suggests that the key to distinguishing between these models is to first get a sense of whether migration is possible or common across these local areas. To the extent that the costs of migration appear to be small, we turn to the information story, and analyze how quickly

information decays with ITES center distance. It is very important to keep in mind the distances we are discussing are very small, with the impact of ITES centers decaying significantly within a few kilometers. This means when we consider the costs of migration or information decay, we are considering these factors over a very small distance.

Survey Data from Madurai

The DISE data do not provide evidence that would allow us to observe either costs of migration/travel or information. To get at these mechanisms, therefore, we fielded a short survey in Madurai District in Tamil Nadu. Madurai is a small city about 450 kilometers from Chennai with several ITES centers. We surveyed 1000 individuals: 500 in Madurai itself and 250 in each of two smaller towns, Thirumangalam and Peraiyur, which were about 20 and 50 kilometers away, respectively. We collected data including distance to work, future plans for children and knowledge of ITES centers. Importantly, we collected GPS data on location of households and ITES centers, allowing us to calculate exact distances. Details of the survey appear in Appendix B.

Evidence on Costs of Travel and Migration

Our DISE evidence is on enrollment of children in primary school. In order to conclude that large costs of migration explain the decay of the effect over distance, two things must be true. First, it must be unusual for people to travel more than a few kilometers for work; if a large share of people travel more than that, it suggests the labor market is not as localized as the ITES center impacts. Second, it must *also* be unusual for children to live more than a few kilometers away when they leave home. If it is possible or likely that a large share of children will live some distance away when they are working as adults, and people have perfect information, they should respond to returns in more distant areas.

By and large, the data does not support either of these requirements. The median person in our sample who is working reports working 2 kilometers away from where he lives; 25% work more than 6 kilometers away. Among people with at least ten years of schooling – presumably most likely to work at high wage jobs like those at ITES centers – the median person reports working 4 kilometers away and 25% work more than 10 kilometers away. This suggests that it is not unusual to travel reasonable distances to work.[29]

[29] As a side note, this also supports the argument in Section 5 that our estimates are not driven by population or

Further evidence comes from the data on child migration. Among children of the sample participants who are over 18, roughly 40% of them live away from home, and 25% live more than 5 kilometers away. Further, there is evidence that parents expectations line up with this: when asked where they expect their child to live after they are married, 54% of respondents report they expect the child to live at least in a different neighborhood, and 25% report they expect the child to live in a different district.

The evidence on migration is echoed by larger datasets. Data from the National Family and Health Survey show that among working individuals ages 20-35 with at least a secondary school education, roughly 30% have moved in the last five years. Similarly, in the 2001 Census, 29.9% of all persons were living in a town other than that of their birth.

Taking this evidence together, it seems very unlikely that our DISE results reflect localized changes in actual returns; the evidence simply does not support the claim of such localized labor markets.

Evidence on Information Diffusion

We turn now to the question of whether limited information diffusion might explain our results. We focus on relating distance to an ITES center (calculated based on GPS coordinates) to two pieces of data reported by the households: knowledge about ITES centers and whether parents plan on ITES center jobs for their children. At the end of this subsection we show some auxiliary evidence on distance and reported "returns to schooling."

Knowledge of ITES Centers We focus on five knowledge variables: whether the respondent reports knowing anyone who works in an ITES center, whether they report that there is an ITES center within the "local area"[30] and three measures of their knowledge about ITES center job qualifications. The job qualification questions listed a set of characteristics (e.g. speak English, college graduate) and asked individuals whether these were "required" for jobs in an ITES center; in some cases the correct answer was yes, and in others it was no. We generate three measures of knowledge: the share of questions for which individuals reported they "didn't know" whether the qualification was required, the share of the true qualifications they correctly identified and the share

income. Since people travel for work, any income impacts would be less localized than the ITES center impacts we estimate.

[30]The definition of "local" was up to the respondent; the idea was to get a sense of whether they knew of any ITES centers.

of the false qualifications they correctly identified.[31] Appendix B reports summary statistics on the variables.

We begin by looking at how knowledge varies between Madurai, the district capital (which has some ITES centers), and the two smaller towns of Thirumangalam and Peraiyur (neither of which have any ITES centers). Panel A of Table 9 reports summary statistics for each of the three areas. As expected, we find knowledge of ITES centers is much greater in Madurai than in the other two areas. For example, 34% of respondents in Madurai report knowing someone who works in an ITES center, versus 9.6% in Thirumangalam and 6.4% in Peraiyur. Those in Madurai are more likely to know of an ITES center in the local area. In the case of knowledge, nearly all individuals in all areas are able to correctly identify the true required qualifications, but those in Madurai are more likely to reject the false qualifications, consistent with having better information.

Although somewhat informative, the evidence in Panel A relies on comparing across areas which are different for other reasons (e.g. income, distance to major cities), which could drive these differences. Moreover, the evidence in the DISE data points to variation in response over much smaller distances; if information explains this variation, we should see differences in information even within Madurai. To explore this, we estimate regressions within Madurai with simple demographic controls. Panel B of Table 9 uses all of the Madurai data and estimates coefficients on two dummies: being within a half a kilometer of the closest ITES center and being between 0.5 and 1.5 kilometers away; the omitted category is between 1.5 and 3 kilometers away. We see that knowledge is the highest in areas within a half a kilometer of the ITES center on four of the five measures, which is consistent with the evidence from the DISE data of effects decaying over relatively short distances.[32] The exception is when we explore impacts on the share of people who correctly identify true qualifications where nearly everyone gets a perfect score.

We can get a visual sense of the patterns within Madurai in Appendix Figures 1-5, which show smoothed plots of the knowledge outcomes against distance from the closest ITES center. For the

[31] The correct qualifications were: speak English, use a computer and be a college graduate. The incorrect qualifications were: politically active, have a driver's license and be a woman.

[32] Though the negative effect for the middle category (0.5-1.5 km away) is somewhat curious, it is likely being driven by an outlying call center. Honeywell, by far the largest call center in Madurai, is located on the outskirts of town. Unlike the smaller centers, people close to Honeywell all have relatively high basic knowledge of what call centers are, and have heard of someone who works there. The deeper knowledge, however, such as qualifications for working there, decays at the same rate as for other ITES centers. This is likely due to the fact that Honeywell is simply much more visible in the neighborhood than a typical call center in our sample. Because Honeywell is located relatively far outside the city center, everyone close to it is more than 1.5 km away from any other call center. Thus, a large percent of the people who live near enough to Honeywell to be impacted by it actually fall into the "farthest" category in these regressions.

same four measures, we see strong evidence that information deteriorates quickly in the area right around the ITES center. Between 0 and 2 kilometers, moving further away decreases knowledge. Consistent with the estimate on the second dummy in the regressions in Panel B, there is some evidence that people who live much further away (between 2 and 3 kilometers) have better information.

In Panels C and D of Table 9 we take this analysis one step further and estimate the impact of distance to an ITES center on knowledge within more limited areas. As we squeeze the data in on smaller areas we increase the comparability across individuals, as well as the comparability across the ITES centers to which they are exposed. Our ideal is to get as close as possible to the "experiment" of comparing two individuals who are slightly different distances from a single ITES center. We approximate this ideal with a sample restriction. Panel C limits the data to households within 1 kilometer of an ITES center; Panel D limits to those within 0.5 kilometers.

Despite the small sample sizes (especially in Panel D), in all regressions we see a highly significant relationship between distance and knowledge. Individuals who are closer to an ITES center (so distance is smaller) are more likely to report knowing someone who works at one of these businesses, and more likely to report one in the local area. Further, those who are closer to an ITES center are less likely to report they don't know what qualifications are required and more likely to reject the false qualifications. Consistent with the evidence in Panels A and B, there is less impact on whether they correctly identify true qualifications. This suggests that the largest impact of the ITES center is to correct perceptions about what is required to work there. The evidence in Panel D suggests that even *within* a half a kilometer of an ITES center, being closer increases knowledge, this suggests that information decays with distance even within an extremely small area.

Child Job Choices Our second piece of evidence on information focuses on job choices for children. In the survey, we asked individuals about the most likely jobs for their child; they were given a list of possible jobs and asked to list three options. We focus on whether they choose the job "Call Center/BPO Worker" as the most likely job and analyze how proximity to an ITES center impacts this outcome. Since enrollment declines as children age, there is more selection in the older sample; given this, we run regressions on the whole sample and limited to children ages 5-10.

Again, we start by comparing Madurai with the other two towns. Respondents in Madurai much more frequently report this as the most likely job for their children: 7.1% of respondents in Madurai report this, versus 1.9% in Thirumangalam and 1.2% in Peraiyur. As in the case of

knowledge, the more interesting comparison is by distance within Madurai. Panel A of Table 10 reports regression results from the Madurai-only sample. Column 1 uses the entire sample and estimates coefficients on the two distance dummies; controls are child sex and age, head of household education and whether the respondent reports that call centers are one of the three listed jobs with the highest wages. This first regression shows no evidence that proximity to ITES centers matters for whether parents envision this job for their children. In Column 2, however, when we limit to younger children we see a strongly positive impact of being close to an ITES center.

The difference across age groups could reflect differential selection. It is also possible that this difference reflects the fact that schooling choices are more malleable for younger children – for example, it might still be possible to switch them to an English school. To get some sense of this, in Columns 3 and 4 we interact distance with whether the child is enrolled in an English-language school (controlling for the overall English-language impact). These results are more striking. For both the overall sample and for the younger children, we observe that for children enrolled in an English-language school, proximity to an ITES center strongly impacts whether the parent reports that an ITES center job is likely. The fact that this occurs for the overall sample in addition to the younger children suggests that the lack of impact for the total sample in Column 1 is due to lack of language flexibility among older children.

Overall, this table suggests that there is an increase in perceived chance of ITES center jobs for children when an ITES center is closer. Again, this points to a very sharp decay of information about these jobs even over small distances.

Returns to Schooling and Reported Changes in Behavior As a final note, we present two more speculative pieces of evidence that are supportive of the information story. The first is on returns to schooling. We asked individuals their "best guess" about the the monthly wage in the area for someone with a secondary school degree and for someone with only primary school; we define "returns to schooling" as the simple difference between these two values. Panel B of Table 10 regresses this returns to schooling measure on the distance measures, both the distance dummies (Column 1) and the measure of distance to the closest center, with the sample limited to ares within 1 kilometer (Column 2). In both cases, we see evidence that proximity to an ITES center impacts perceived returns. Areas within a half a kilometer of an ITES center report monthly returns that are are 350 Rs higher than more distant areas. This is a large effect: the average in the areas more than half a kilometer away is 733 Rs. Even within 1 kilometer of the ITES center (Column 2) moving

closer increases perceived returns.

The second piece of evidence comes from the last question on the survey. For the 131 individuals in the sample who reported knowing of an ITES center in the local area, we asked whether they had made any change in response to that center introduction. Of course, it is extremely difficult to interpret responses to questions like this, especially given that it was asked at the end of the survey, which leaves open concerns about priming. However, the results are striking. About 50% report intentions to increase schooling for their children, some of whom cite specifically that they will enroll their children in English-language schools. It is interesting to note that this is the only behavior change reported – there is no mention of individuals getting jobs at ITES centers – which is consistent with the evidence in Section 5 that these centers probably do not have large impacts on current income.[33]

We argue that the evidence in this section suggests that the localized impact of ITES centers that we observe in the DISE data reflect limited information diffusion rather than localized labor markets. The distances traveled for work, and the chance that children live some distance away when they are older, are sufficiently high that it seems implausible that there are large costs of "migration" to jobs a few kilometers away. On the other hand, the evidence on information suggests that knowledge of ITES centers and expectations about the possibility of children working there decay very rapidly as households move away from these centers, which is exactly what we would expect if information diffuses slowly or not at all across space.

7 Conclusion

In this paper we argue that the introduction of ITES centers in India have large impacts on school enrollment, and these impacts are concentrated in the very local areas around the ITES centers. We argue this effect is causal, and is not driven by pre-trends or mechanical changes in area-level population or income. The very local nature of our analysis and the fine timing of the effects are helpful in ruling out the concern that endogenous placement or trends in unobservables drive the

[33]Of 131 individuals who know of an ITES center in their local area, when asked if they will change their behavior because of the ITES center, 47 answered no, 9 answered that they will make their child study more or longer, 19 answered that they will make their child learn English, 38 answered that they will make their child learn computer skills or typing, and 18 indicated that there will be a change, but do not specify further. Not one parent answered that they would try to get a job at this center, or a nearby business, or any other change that was not related to investment in human capital for their child. While there is some danger of priming with this question (it was asked at the end of a survey about education and call centers, among other things), it is consistent with our assertion that these ITES centers are causing increased enrollment directly, through information dispersion about returns to schooling.

impacts we see. Further, we provide some suggestive evidence that the very localized nature of the impacts may reflect limited information about non-local job opportunities; we argue this is more likely than the claim that these new job opportunities only impact local returns to schooling.

These results relate to a larger literature on promoting schooling increases in the developing world. To get a sense of magnitude, we can compare the results here to other interventions designed to increase schooling. Our overall estimate indicates that an additional ITES center prompts a 5.7% increase in school enrollment; based on an enrollment rate of around 80% this amounts to an increase in enrollment rate of around 4.1 percentage points. This number is comparable to enrollment effects of other interventions designed to increase schooling in the developing world. For example, the conditional cash transfers in PROGRESA increased schooling 3.4-3.6 percentage points (Schultz, 2004). A program in Kenya which provided school uniforms to girls in Kenya (worth about 1.75% of average yearly income) increased enrollment by 6 percentage points (Evans, Kremer and Ngatia, 2008). Miguel and Kremer (2004) found that administering deworming drugs decreased absence by 7 percentage points, although they do not report effects on enrollment.

From a policy standpoint, the results provide support for interventions which inform students about returns to schooling (as in Jensen, 2010a and 2010b). In the absence of this type of policy, we would expect short-term gains in enrollment to be concentrated around areas with local ITES centers; the evidence in Section 6 suggests this concentration could be limited by broader information sharing.

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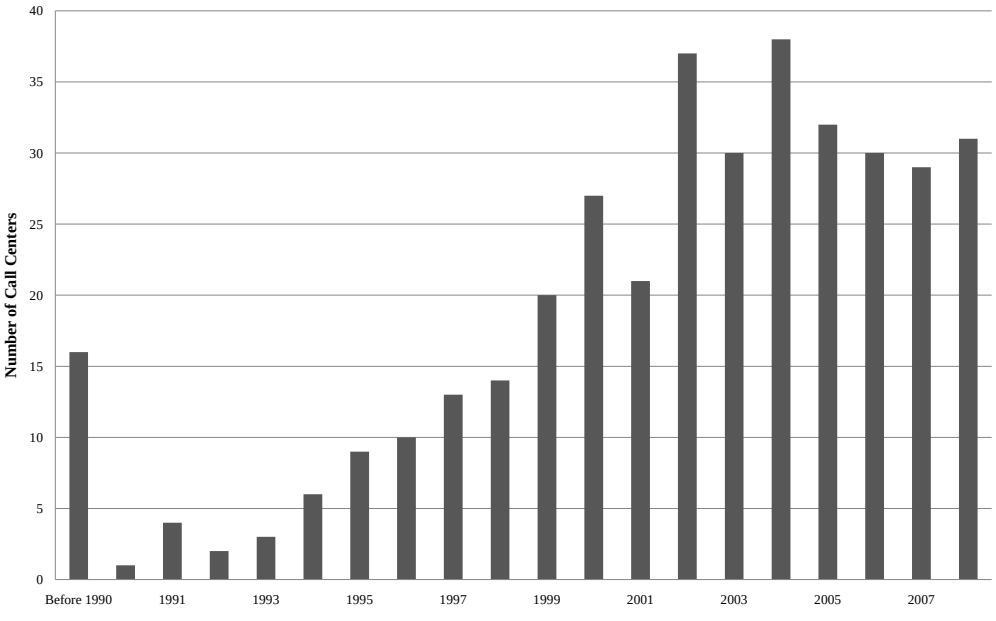
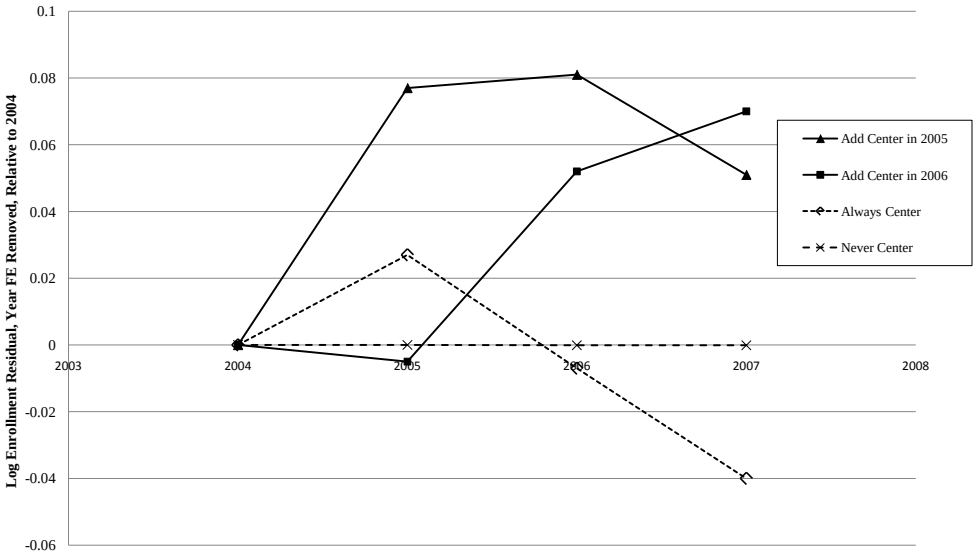
Figure 1:
Distribution of ITES Center Founding Dates



Notes: This figure shows the distribution of ITES center founding dates among centers in our sample.

Figure 2:
Impact of ITES Centers on School Enrollment



Notes: This figure shows changes in enrollment over time for four balanced panels of schools. All enrollment numbers are residuals from a regression of log enrollment on year fixed effects and values represent changes relative to the residual values in 2004. The year 2004 refers to the 2004-2005 school year (beginning in June 2004, ending in April 2005); enrollment is recorded as of September 2004. Schools are coded as adding a center in 2005 if an ITES center is founded in the area any time during 2005.

Table 1: **DISE Summary Statistics**

Panel A: Years of Coverage and Number of Schools			
	Andhra Pradesh	Karnataka	Tamil Nadu
Years of Data Coverage	2004-2007	2001-2007	2003-2007
Number of Schools in:			
2001-02		27,136	
2002-03		43,409	
2003-04		44,636	43,662
2004-05	59,121	48,515	48,391
2005-06	89,474	50,207	48,283
2006-07	99,017	50,889	50,696
2007-08	98,485	52,369	51,548
Panel B: School Summary Statistics			
	Mean	Std. Dev.	Observations
Total Enrollment	143.8	166.4	905,838
Grade 1-2 Enrollment	40.0	47.3	905,838
Grade 3-4 Enrollment	38.5	47.6	905,838
Grade 5-6 Enrollment	37.6	49.7	905,838
Grade 7-8 Enrollment	27.7	67.6	905,838
Girl Enrollment	69.4	88.9	905,838
Boy Enrollment	74.4	96.4	905,838
% Classrooms in Good Condition	70.7	37.2	905,838
% Schools with Electricity	49.0	50.0	905,838
% Schools with Boundary Walls	51.3	50.0	905,838
Teach in English (0/1)	0.11	0.32	905,717
Total School-Age Population	163.1	1,404	255,355

Notes: Panel A shows years of data coverage and summary statistics by state for the three states in our data set. Panel B shows summary statistics on enrollment and school characteristics for the sample of schools used in the analysis. Population is recorded by the schools for only a subset of schools and years.

Table 2: **ITES Center Summary Statistics**

Panel A: Number of ITES Centers By State		
	Number of ITES Centers	
	Including Cities	Excluding Cities
Andhra Pradesh	100	74
Karnataka	144	121
Tamil Nadu	157	65
Panel B: Number of ITES Centers by PIN Code		
	Number of PIN Codes	
Number of ITES Centers	Including Cities	Excluding Cities
0	5,219	
1	68	47
2	29	22
3	12	10
4	11	8
5+	25	5
Panel C: Number of Schools by Category		
	Number of Schools	
Never Had an ITES Center	238,986	
Has Same Number of ITES Centers	172	
Has Change in Number of ITES Centers	408	
Panel D: ITES Center Survey Results		
Median Number of Employees	80	
Median Age	28	
Share with Voice Employees	.625	
Share with English Voice Employees	.620	
Share Employees with Children	.10	
Share Employees Relocated for Work	.122	

Notes: This table shows summary statistics for our sample of ITES centers. Location (PIN code) and founding year were collected in a primary survey; only centers with both location and founding date were included in the sample. Panel D reports data from a phone survey which covered 83% of these centers and asked more detailed questions on employees and services.

Table 3: **Placement of ITES Centers**

Dependent Variable:	*Number of ITES Centers, 2007*		*Add ITES Center During Sample*	
	(1)	(2)	(3)	(4)
Ever Had Electricity	-.0002	-.0001	-.0002	.0001
	(.0005)	(.0005)	(.0003)	(.0003)
Urban	.010***	.010***	.004***	.004***
	(.001)	(.001)	(.001)	(.001)
Any English School (0/1)	.007***	.006***	.003***	.003***
	(.0008)	(.0008)	(.0004)	(.0004)
Log Enroll. First Survey Year	-.00005	-.0001	.000001	-.0001
	(.0001)	(.0002)	(.0001)	(.0001)
State Fixed Effects	NO	YES	NO	YES
R-squared	0.002	0.003	.002	.002
Observations	71,667	71,667	71,890	71,890

Notes: This table shows the effects of pin code characteristics on ITES center placement. The left hand side variable in Columns 1 and 2 is the number of ITES centers in 2007; in Columns 3 and 4 it is whether any centers were added during the sample period. Standard errors in parentheses, clustered at the PIN code level. *significant at 10% **significant at 5% ***significant at 1%.

Table 4: **Effect of ITES Centers on School Enrollment**

Dependent Variable:	*Log Enrollment*			
Panel A: Number of ITES Centers in PIN Code				
Sample:	All Schools		In PIN Code with Any English Schools	Ever Had an ITES Center
Controls:	*Standard*	*District Trends*	*Standard*	*Standard*
	(1)	(2)	(3)	(4)
Number of ITES Centers	.057**	.043***	.070**	.109***
	(.024)	(.015)	(.028)	(.035)
Observations	918,136	918,136	276,044	2,123
Panel B: Any ITES Center in PIN Code				
Sample:	All Schools		In PIN Code with Any English Schools	Ever Had an ITES Center
Controls:	*Standard*	*District Trends*	*Standard*	*Standard*
	(1)	(2)	(3)	(4)
Any ITES Center	.055*	.034**	.070*	.108
	(.034)	(.017)	(.041)	(.066)
Observations	918,136	918,136	276,044	2,123
Panel C: Number of ITES Centers in Neighboring PIN Codes				
Sample:	All Schools		In PIN Code with Any English Schools	Ever Had an ITES Center
Controls:	*Standard*	*District Trends*	*Standard*	*Standard*
	(1)	(2)	(3)	(4)
ITES Centers in PIN Code	.052**	.039***	.063***	.056**
	(.026)	(.015)	(.023)	(.022)
# ITES Centers in 1st, 2nd	.037***	.039***	.048***	.044***
Nearest Neighbors	(.013)	(.011)	(.012)	(.008)
# ITES Centers in 3rd-5th	.013	.008	.019	.020
Nearest Neighbors	(.013)	(.009)	(.012)	.012
Observations	918,136	918,136	276,044	11,253
Standard controls: School fixed effects, time-varying school plant characteristics, year dummies interacted with dummies for state, urban, and English language school in PIN code.				
District Trend controls: Standard controls plus district-specific trends.				

Notes: This table shows our primary estimates of the impact of ITES centers on school enrollment. The independent variable measures the number of ITES centers in the same PIN code as the school. Columns 1-2 include all schools. Column 3 is limited to PIN codes with any English schools. Column 4 is limited to schools which ever have an ITES center in their PIN code (either always have the same number or change during the sample). Standard errors (in parentheses) are clustered at the neighborhood level in Columns 1, 3 and 4; clustered errors could not be estimated when district trends are included in Column 2. *significant at 10% **significant at 5% ***significant at 1%.

Table 5: **Effects of Future ITES Centers**

Dependent Variable:	*Log Enrollment*			
Panel A: Number of ITES Centers Next Year				
Sample:	All Schools		In PIN Code with Any English Schools	Ever Had an ITES Center
Controls:	*Standard*	*District Trends*	*Standard*	*Standard*
	(1)	(2)	(3)	(4)
ITES Centers	.054*	.040**	.065**	.099**
	(.028)	(.018)	(.033)	(.042)
ITES Centers Next Year	.006	.005	.007	.018
	(.018)	(.017)	(.020)	(.024)
p-value, This Year=Next	0.234	0.261	0.207	0.163
Observations	918,136	918,136	276,044	2,123
Panel B: Trends Leading Up to ITES Center Entry				
Sample:	All Schools		In PIN Code with Any English Schools	Ever Had an ITES Center
Controls:	*Standard*	*District Trends*	*Standard*	*Standard*
	(1)	(2)	(3)	(4)
ITES Centers	.063***	.060*	.068***	.084***
	(.019)	(.032)	(.020)	(.027)
Years To Entry	.002	.005	-.0004	-.008
	(.007)	(.008)	(.009)	(.013)
Observations	918,136	918,136	276,044	2,123
Standard controls: School fixed effects, time-varying school plant characteristics, year dummies interacted with dummies for state, urban, and English language school in pin code.				
District Trend Controls: Standard controls plus district-specific trends.				

Notes: This table tests whether future ITES centers predict current enrollment. The independent variable in Panel A measures the number of ITES centers in the same PIN code as the school either in the current year or the following year. The independent variable in Panel B measured the number of ITES centers in the same PIN code as the school, along with the linear trend in enrollment leading up to the entry of an ITES center. Columns 1-2 include all schools. Column 3 is limited to PIN codes with any English schools. Column 4 is limited to schools which ever have an ITES center in their PIN code (either always have the same number or change during the sample). Standard errors (in parentheses) are clustered at the PIN code level in Columns 1, 3 and 4; clustered errors could not be estimated when district trends are included in Column 2. *significant at 10% **significant at 5% ***significant at 1%.

Table 6: **Effects by Language of Instruction**

Dependent Variable:	*Log Enrollment*			
Sample:	All Schools		In PIN Code with Any English Schools	Ever Had an ITES Center
Panel A: Impact of ITES Centers by School Language of Instruction				
Controls:	*Standard*	*District Trends*	*Standard*	*Standard*
# Centers × Local Lang.	-.011	-.023	-.014	.030
	(.022)	(.019)	(.030)	(.037)
# Centers × English	.149***	.133***	.149***	.213***
	(.039)	(.022)	(.038)	(.048)
p-value, English=Local Language	0.000	0.000	0.001	0.001
Observations	918,014	918,014	275,981	2,121
Panel B: Impact of ITES Centers by Detailed School Language of Instruction				
Controls:	*Standard*	*District Trends*	*Standard*	*Standard*
# Centers × Local Lang.	-.011	-.023	-.014	.033
	(.022)	(.019)	(.030)	(.037)
# Centers × Some English	.129**	.118***	.125**	.181***
	(.058)	(.038)	(.058)	(.061)
# Centers × All English	.160***	.142***	.162***	.235***
	(.044)	(.028)	(.043)	(.053)
Observations	918,014	918,014	275,981	2,121
Panel C: Impact of Voice and All ITES Centers by School Language of Instruction				
Controls:	*Standard*	*District Trends*	*Standard*	*Standard*
# Non-Voice × Local Lang.	.091**	.094*	.099*	.128***
	(.040)	(.056)	(.053)	(.036)
# Non-Voice × English	.124***	.104***	.139***	.182***
	(.009)	(.039)	(.010)	(.061)
# Voice × Local Lang.	-.036*	-.056**	-.051*	-.019
	(.021)	(.023)	(.027)	(.052)
# Voice × English	.164**	.147***	.158**	.205***
	(.070)	(.030)	(.071)	(.074)
p-value:V.English - V.Local = N.V.English - N.V.Local	0.052	0.015	0.068	0.099
Observations	917,955	917,955	275,969	2,062
Standard controls: School fixed effects, time-varying school plant characteristics, year dummies interacted with dummies for state, urban, and English language school in pin code.				
District Trend Controls: Standard controls plus district-specific trends.				

Notes: This table shows the impact of ITES centers by school language of instruction. Panel A shows the differential effects for English and local language schools. Panel B shows the effect for local language schools, schools with some English instruction, and schools with exclusive English instruction. Panel C shows the effects by school language for voice and non-voice ITES centers. Voice centers are defined as ITES centers where at least half of employees handle voice calls. Columns 1-2 include all schools. Column 3 is limited to PIN codes with any English schools. Column 4 is limited to schools which ever have an ITES center in their PIN code. Standard errors (in parentheses) are clustered at the PIN code level in Columns 1, 3 and 4; clustered errors could not be estimated when district trends are included in Column 2. *significant at 10% **significant at 5% ***significant at 1%.

Table 7: **Robustness: Number of Employees**

Dependent Variable:	*Log Enrollment*			
Sample:	All Schools		In PIN Code with Any English Schools	Ever Had an ITES Center
Controls:	*Standard*	*District Trends*	*Standard*	*Standard*
	(1)	(2)	(3)	(4)
Number of ITES Centers	.082***	.088***	.086***	.114***
	(.018)	(.028)	(.020)	(.025)
Log Number of Employees	-.007	-.013	-.005	-.002
	(.008)	.007	(.010)	(.015)
Observations	918,077	918,077	277,043	2,064
Standard controls: School fixed effects, time-varying school plant characteristics, year dummies interacted with dummies for state, urban, and English language school in pin code.				
District Trend Controls: Standard controls plus district-specific trends.				

Notes: This table shows our the effect of the number of employees of a call center on school enrollment. The independent variables measures the number of ITES centers in the same PIN code as the school, and the natural log of the number of employees in the center, respectively. Columns 1-2 include all schools. Column 3 is limited to PIN codes with any English schools. Column 4 is limited to schools which ever have an ITES center in their PIN code (either always have the same number or change during the sample). Standard errors (in parentheses) are clustered at the PIN code level in Columns 1, 3 and 4; clustered errors could not be estimated when district trends are included in Column 2. *significant at 10% **significant at 5% ***significant at 1%.

Table 8: **Robustness: Income Effects**

	(1)	(2)	(3)	(4)
Existing Estimates of Income Elasticity	0.25	0.40	1.00	1.25
Estimate of % Change in Income from ITES Introduction	0.57%	0.57%	0.57%	0.57%
Predicted Percentage Point Increase in Enrollment	0.14%	0.23%	0.57%	0.72%
Actual Increase in Enrollment	7.0%	7.0%	7.0%	7.0%
Percent of Actual Enrollment Increase Explained	2.0%	3.3%	8.1%	10.3%

Notes: Income elasticity estimates from (1) Glewwe and Jacoby (2004), (2) Glick and Sahn (2000), (3) Orazem and King (2007), and (4) Alderman et al. (2001). Appendix A reports details on how we calculate Row 2.

Table 9: **Knowledge of ITES**

Panel A: Knowledge of ITES by Taluk

	Heard of Someone who works at ITES	Know of ITES in local area	% ITES Ques. "Don't Know"	% True Qual. Answer Correct	% False Qual. Answer Correct
Madurai [N=500]	.335	.266	17.6	90.0	45.9
Thirumangalam [N=250]	.096	.012	18.3	90.9	39.9
Peraiyur [N=250]	.064	.004	15.3	88.3	32.7

Panel B: Knowledge of ITES by Distance to Closest, Within Madurai

	Heard of Someone	Know of ITES	% "Don't Know"	% True Correct	% False Correct
ITES <0.5 km away	.212***	.230***	-.099**	-.015	.265***
	(.082)	(.078)	(.042)	(.042)	(.061)
ITES 0.5-1.5 km away	-.129***	-.110***	-.009	-.021	.015
	(.040)	(.037)	(.041)	(.021)	(.030)
Controls	YES	YES	YES	YES	YES
Observations	494	496	498	498	498

Panel C: Knowledge of ITES by Distance to Closest, Within 1 km of an ITES Center

	Heard of Someone	Know of ITES	% "Don't Know"	% True Correct	% False Correct
Distance to ITES	-.641***	-.488***	.215***	-.052	-.445***
	(.130)	(.131)	(.065)	(.070)	(.111)
Controls	YES	YES	YES	YES	YES
Observations	137	139	139	139	139

Panel D: Knowledge of ITES by Distance to Closest, Within 0.5 km of an ITES Center

	Heard of Someone	Know of ITES	% "Don't Know"	% True Correct	% False Correct
Distance to ITES	-2.20***	-1.81***	.768***	-.633*	-.652
	(.549)	(.642)	(.212)	(.312)	(.437)
Controls	YES	YES	YES	YES	YES
Observations	32	32	32	32	32

Notes: Data comes from the survey run in Madurai District, Tamil Nadu. Panels B, C and D are limited to areas within Madurai. Dependent variables are the same in all panels (titles abbreviated in Panels B-D). Controls: head of household education, whether the household head speaks any english and number of assets held by household (television, radio, refrigerator, and toilet). In Panel B, the omitted category is more than 1.5 km away from an ITES center. Thirumangalam is located 20 km from Madurai, and Peraiyur is 50 km away. Columns 3-5 rely on answers to a set of six questions about qualifications which are "required" for job in an ITES center. Details are in Section 6. Standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%.

Table 10: **Future Expectations and Distance to ITES**

Panel A: ITES as Job Possibility for Child				
	Dependent Variable: ITES listed first as possible job for child			
	All Children	Children Ages <10	All Children	Children Ages <10
Within 0.5 km of an ITES	-.004	.167**	-.072	-.035
	(.065)	(.080)	(.075)	(.095)
Within 1 km of an ITES	-.0008	.026	-.063	-.024
	(.039)	(.042)	(.049)	(.051)
Enrolled in English School			-.100*	-.092
			(.057)	(.060)
Within 0.5 km of an ITES × English			.292*	.586***
			(.153)	(.168)
Within 1 km of an ITES × English			.175**	.129
			(.081)	(.082)
Observations	170	81	169	81
Controls: Child age, child sex, head of household education, whether respondent reports call center as highest wage job, asset ownership (television, radio, refrigerator, toilet).				

Panel B: Returns to Schooling		
	Dependent Variable: Estimated Return to Secondary School	
	All Madurai	Within 1 km of a ITES
Within 0.5 km of an ITES	351.3**	
	(138.1)	
Within 1 km of an ITES	88.1	
	(67.8)	
Minimum Distance to ITES		-534.3*
		(314.9)
Observations	497	138
Controls: Head of household education and age, asset ownership (television, radio, refrigerator, toilet).		

Notes: Data comes from the survey run in Madurai District, Tamil Nadu. All regressions are limited to households within Madurai. The omitted distance category is more than 1.5 kilometers away. In Panel B, returns to schooling is calculated based on questions in the survey about what the respondent thinks the average person with a secondary school education earns in a month in Madurai, and the same for the average person with a primary school education. Our measure of the estimated returns to secondary school is simply the difference between these two answers. Standard errors in parentheses. *significant at 10% **significant at 5% ***significant at 1%.

Appendix Tables

Appendix Table 1: ***Impact of ITES Centers on Enrollment in First Differences***

Dependent Variable:	*Change in Enrollment*			
Sample:	All Schools		In PIN Code with any English-Language Schools	Ever Had an ITES Center
Controls:	*Standard*	*District Trends*	*Standard*	*Standard*
	(1)	(2)	(3)	(4)
Change in # ITES Centers	.052**	.044**	.060**	.076**
	.021	(.018)	(.029)	(.029)
Observations	687,336	687,336	78,795	1,529
Standard controls: Time-varying school plant characteristics, year dummies interacted with dummies for state, urban, and english language school in village.				
District Trend controls: Standard controls plus district-specific trends.				

Notes: This table shows the effect of a change in the number of ITES centers on the change in school enrollment. Columns 1-2 include all schools. Column 3 is limited to villages with any English schools. Column 4 is limited to schools which ever have an ITES center in their PIN code (either always have the same number or change during the sample). Standard errors (in parentheses) are clustered at the village level in Columns 1, 3 and 4; clustered errors could not be estimated when district trends are included in Column 2. *significant at 10% **significant at 5% ***significant at 1%.

Appendix Table 2: ***Enrollment Effects by Demographic Group, State***

Dependent Variable:		*Log Enrollment*		
		Number of ITES Centers	Standard Error	Observations
(1)	Boy Enrollment	.059**	.023	902,616
(2)	Girl Enrollment	.050**	.024	907,438
(3)	Grades 1-2 Enroll.	.034	.027	828,895
(4)	Grades 3-4 Enroll.	.069**	.035	817,038
(5)	Grades 5-6 Enroll.	.043	.029	857,209
(6)	Grades 7-8 Enroll.	.097***	.030	341,845

Notes: This table shows coefficients on number of ITES centers from regression of the form in Column 1 of Table 4 but with variation in the left hand side variable. All regressions include the standard controls: School fixed effects, time-varying school plant characteristics, year dummies interacted with dummies for state, urban, and English language school in village. Standard errors in parentheses, clustered at the neighborhood level. *significant at 10% **significant at 5% ***significant at 1%.

Appendix Table 3: ***Effects on Enrollment with Population Controls***

Dependent Variable:	*Log Enrollment*			
Panel A: Restricted Sample, No Population Control				
Sample:	All Schools		In Village with Any English-Language Schools	Ever Had an ITES Center
Controls:	*Standard*	*District Trends*	*Standard*	*Standard*
	(1)	(2)	(3)	(4)
Number of ITES Centers	.076*	.054***	.092*	.137
	(.043)	(.018)	(.051)	(.126)
Observations	327,740	327,740	120,547	1,075
Panel B: Restricted Sample, with Population Control				
Sample:	All Schools		In Village with Any English-Language Schools	Ever Had an ITES Center
Controls:	*Standard*	*District Trends*	*Standard*	*Standard*
	(1)	(2)	(3)	(4)
Number of ITES Centers	.074*	0.052***	.091*	.131
	(.043)	(.018)	(.050)	(.121)
Log Village Population	.006***	.006***	.004	-.019
	(.002)	(.0008)	(.003)	(.029)
Observations	327,740	327,740	120,547	1,075
Standard controls: School fixed effects, time-varying school plant characteristics, year dummies interacted with dummies for state, urban, and english language school in village. *District Trend controls: Standard controls plus district-specific trends.*				

Notes: This table shows the impact of ITES centers controlling for population. Population is reported by a subset of school-years, and is reported by the school as the village population. In cases where the school does not report population but other schools in the village do report population we use the average population among reporter schools as population for all schools in the village. Panel A does not control for population but limits the sample to school-years in which population is observed. Standard errors (in parentheses) are clustered at the village level. *significant at 10% **significant at 5% ***significant at 1%.

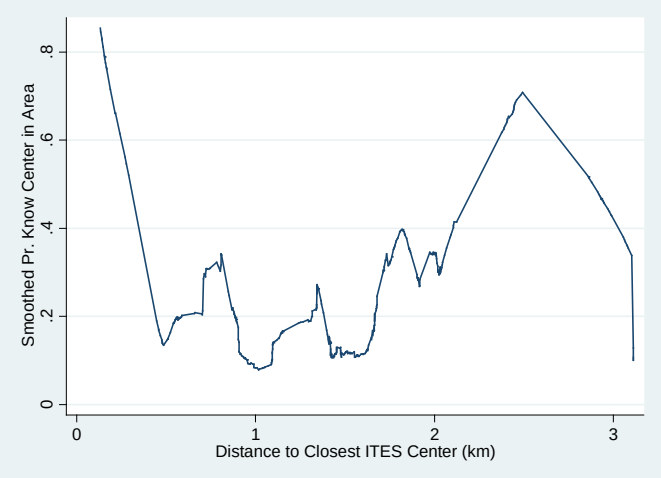
Appendix Table 4*: **Robustness: Number of Schools***

Dependent Variable:	*Count of Schools in Village*			
Sample:	All Schools		Villages with Any English-Language Schools	Ever Had an ITES Center
Controls:	*Standard*	*District Trends*	*Standard*	*Standard*
Number of ITES Centers	.007	.010	.001	.014
	(.053)	(.020)	(.104)	(.092)
Observations	356,796	356,796	32,301	477
Standard controls: Village fixed effects, time-varying school plant characteristics, year dummies interacted with dummies for state, urban, and English language school in village.				
District Trend controls: Standard controls plus district-specific trends.				

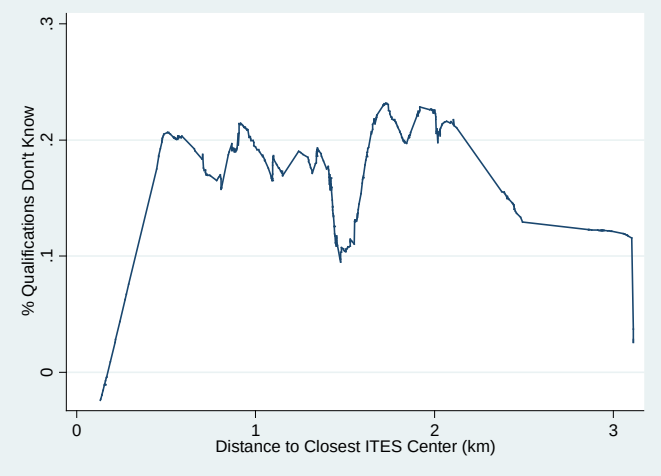
Notes: This table shows the effect of ITES centers on number of schools in a village. Columns 1-2 include all schools. Column 3 is limited to villages with any English schools. Column 4 is limited to schools which ever have an ITES center in their PIN code (either always have the same number or change during the sample). Standard errors (in parentheses) are clustered at the village level in Columns 1, 3 and 4; clustered errors could not be estimated when district trends are included in Column 2. *significant at 10% **significant at 5% ***significant at 1%.

Appendix Figure 1:
Distance to Nearest ITES Center and Knowing Someone who Works In One

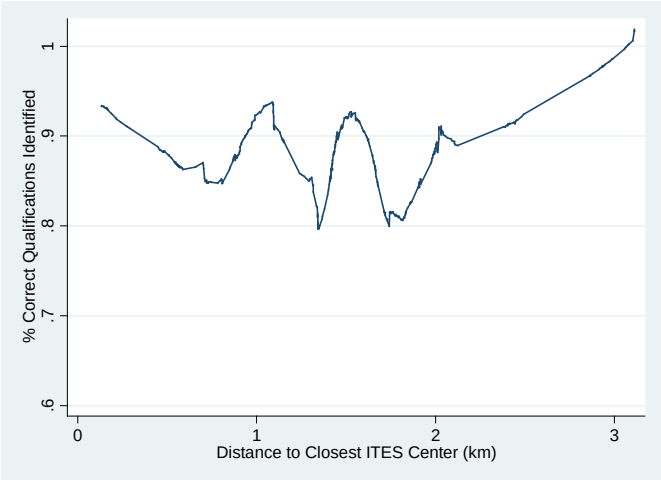
Appendix Figure 2:
Distance to Nearest ITES Center and Knowing Center in Local Area



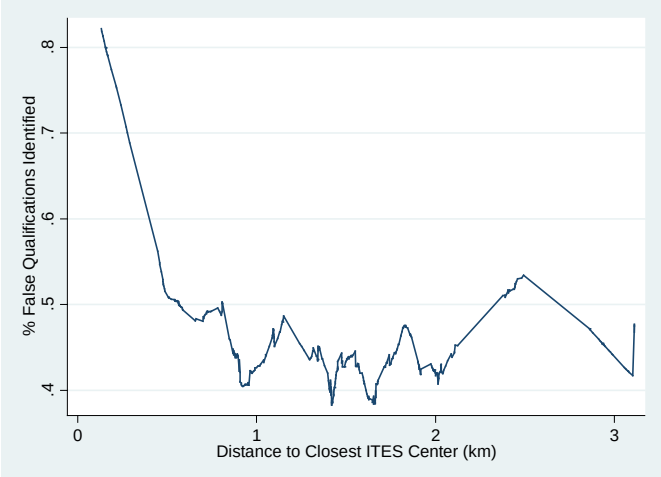
Appendix Figure 3:
Distance to Nearest ITES Center and % of Qualifications Report "Don't Know"



Appendix Figure 4:
Distance to Nearest ITES Center and % of Correct Qualifications Identified



Appendix Figure 5
Distance to Nearest ITES Center and % of False Qualifications Identified as False



Appendix A: Calculating ITES Center Impact on Income

The second row of Table 8 reports an estimated percentage increase in income due to ITES centers. We use a simple back-of-the-envelope calculation to generate this number. Although this is very unlikely to be perfectly accurate, we argue it is an upper bound on what the impact might be. To generate this number, we focus on the median PIN code and the median ITES center (in terms of size). We observe the number of children enrolled in school in the median PIN code; this number is roughly 10,000. We use this number to estimate the average number of people in the median PIN code. To do this, we note that the Indian census indicates children aged 6-12 make up roughly 15% of the overall population, and from the National Family and Health Survey (which is consistent with other sources) we observe that roughly 85% of children in this age range are enrolled in school. Combining these figures we argue that roughly 12.75% of individuals are children enrolled in school, so we expect the median PIN code to have roughly 78,000 people.

The median per capita income in these states is US$659. Applying this value to the population, we estimate total income of the median PIN code at about US$51 million.

The median ITES center in our sample has 80 employees and based on a survey of a sub-sample of ITES centers, pays roughly US$2100 per year in starting wages. This is more than twice the median per capita income. We calculate the increase in income due to ITES centers assuming that the income from ITES center employees is simply added to total income in the PIN code; we note this is likely to be an *overestimate,* since these individuals probably substitute into ITES center jobs away from some lower paying job, not from doing nothing.

We calculate the increased income, and then calculate the percentage increase implied by this; the resulting figure is 0.57% as reported in Table 8.

Appendix B: Madurai Survey

We conducted a survey of approximately 1,000 households in Madurai district in Tamil Nadu. Of these, 500 households were in the city of Madurai, 250 were in Thirumangalam, a town approximately 20 kilometers away, and 250 were in Peraiyur, a town approximately 50 kilometers away. We surveyed households in groups of 10: at the start, 100 households were randomly selected from election rolls. These 100 households were each surveyed along with their 9 closest neighbors.

The survey included a household roster, with the names of each member of the household (including those who did not live at home), along with age, highest grade completed, enrollment status, employment status, job, and distance to work. In addition, we asked the household questions about assets, language, and how long they have lived in the area. We also asked questions about earnings for individuals with primary school and secondary school in the area, as well as a series of questions about ITES centers. This latter set of questions included information on whether the individual knew anyone who worked at one of these centers, whether they knew of any of these businesses in the local area and a series of questions about what qualifications were required for this job. If households reported knowing of a BPO in the local area, we included an open response question about whether they had made any changes because of the call center.

If the household included at least one child between the ages of 5 and 15 and enrolled in school, we randomly selected one of the children for more detailed questions about schooling and future job and marriage. These included questions on type of school attended (e.g. public, private) and language of instruction. We asked parents to choose the three most likely jobs for their child to have from a list of 15. We asked these questions before any mention of BPOs or call centers, in order to avoid leading the respondents in any way. All schooling (and other) questions were asked of the head of household, typically the father.

In addition to this, we recorded the GPS location of each household surveyed, as well as the GPS locations of BPOs in the local area in order to accurately calculate distance from the nearest call center. Since all of the BPOs in the district were located in Madurai city, the households in Peraiyur and Thirumangalam were necessarily at least 20 kilometers away from any ITES center, but within the city of Madurai there was significant variance in distance to the nearest BPO.

Madurai Survey Summary Statistics

	Mean	Std. Dev.	Observations
Information:			
Heard of someone who has worked at an ITES (share)	.207	.405	995
Knows of an ITES center in the local area (share)	.137	.344	996
% ITES Ques. "Don't Know"	.172	.246	1000
% True Qual. Answer Correct	.894	.239	1000
% False Qual. Answer Correct	.411	.316	1000
Expectations:			
BPO listed as first job for child	.045	.208	398
Estimated return to secondary school	818.15	1911.2	1000